My Struggles with the Block Universe

Selected Correspondence, January 2001 – May 2011

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In honor of 10 May 2011, the 10th anniversary of my last Cerro Grande posting:
A day that should have seen some of the most significant struggles passed.

Blake C. Stacey, Editor

VERY ROUGH, VERY VERY INCOMPLETE DRAFT

Build of: 16 July 2013

What does determinism profess? It professes that those parts of the universe already laid down absolutely appoint and decree what the other parts shall be. The future has no ambiguous possibilities hidden in its womb; the part we call the present is compatible with only one totality. Any other future complement than the one fixed from eternity is impossible. The whole is in each and every part, and welds it with the rest into an absolute unity, an iron block, in which there can be no equivocation or shadow of turning.

Indeterminism, on the contrary, says that the parts have a certain amount of loose play on one another, so that the laying down of one of them does not necessarily determine what the others shall be. It admits that possibilities may be in excess of actualities, and that things not yet revealed to our knowledge may really in themselves be ambiguous. Of two alternative futures which we conceive, both may now be really possible; and the one become impossible only at the very moment when the other excludes it by becoming real itself. Indeterminism thus denies the world to be one unbending unit of fact. It says there is a certain ultimate pluralism in it.

— *William James*

Chance is a purely negative and relative term, giving us no information about that of which it is predicated, except that it happens to be disconnected with something else—not controlled, secured, or necessitated by other things in advance of its own actual presence. What I say is that it tells us nothing about what a thing may be in itself to call it "chance." All you mean by calling it "chance" is that this is not guaranteed, that it may also fall out otherwise. For the system of other things has no positive hold on the chance-thing. Its origin is in a certain fashion negative: it escapes, and says, Hands off! coming, when it comes, as a free gift, or not at all.

This negativeness, however, and this opacity of the chance-thing when thus considered *ab extra*, or from the point of view of previous things or distant things, do not preclude its having any amount of positiveness and luminosity from within, and at its own place and moment. All that its chance-character asserts about it is that there is something in it really of its own, something that is not the unconditional property of the whole. If the whole wants this property, the whole must wait till it can get it, if it be a matter of chance. That the universe may actually be a sort of joint-stock society of this sort, in which the sharers have both limited liabilities and limited powers, is of course a simple and conceivable notion.

— *William James*

Introduction

This document is the second installment of three in the *Cerro Grande Fire Series*. It is a collection of emails compiled in the same spirit as my previous `quant-ph/0105039`, *Notes on a Paulian Idea: Foundational, Historical, Anecdotal and Forward-Looking Thoughts on the Quantum*. One could joke that it represents the loquacious side of my quantum-foundations research program, but even that would be an understatement. Nearly every one of my emails from the period January 2001 – May 2011 having anything at all to do with the conceptual side of quantum theory—from my growing understanding of it, to fights over its meaning, advocacy[1], dreams, "business" opportunities, jokes, even simple reverence and awe—anything and everything on the subject—has some representation in this volume. To zeroth-order approximation, the compilation represents a grand exercise in "intellectual waste not, want not." But, I bring it forward with a more serious goal in mind.

One of the correspondents in my earlier collection wrote me just before its release, "I would not object to [your selection of our correspondence] in your volume ... but I wonder if our amicable banter would really be of value/interest to a wider readership!?" The question of interest, of course, can only be answered by the reader. But on the question of value for those who are interested in issues of quantum interpretation, my answer is a resounding yes. I am convinced that the email compositions collected here provide an essential ingredient, unavailable anywhere else, for turning the point of view of quantum theory put forth in technical detail here

`arXiv:1003.5209v1 [quant-ph]`

and

`arXiv:1301.3274v1 [quant-ph]`.

into a "live option" within the vast spectrum of quantum foundational thought.

The papers cited above comprise the most developed statements of the "Quantum Bayesian" research program, or QBism, to date, but they are no substitute for the kind of understanding—the living feeling—that can only be gotten by the actual process of fighting one's way day after day, sleepless night after sleepless night, through an inchoate and only slowly forming worldview.

No textbook on any subject can leave a student knowing the subject from a mere reading of its pages. The student must actively work at understanding. He must live through the struggles the book's exercises mean to provide, always testing his understanding, always practicing how to respond if confronted on this or that issue. Yet, all this is only the start of a process; it is just the glimmer of knowing. Ultimately, the student must set the book's subject matter to work in his wider stream of activities. But if this is so for "knowing" a textbook's subject, so much more so it must be for a worldview—a vision of the thrust and character of the world itself.

To get oneself into seeing or comprehending a worldview at all, one must gain a feel for its proponents' temperaments. William James—the veritable mascot of this volume—makes the point most forcefully in his 1907 monograph *Pragmatism*:

[1] And, my gosh, will one find advocacy in this volume. Be prepared for repetition. If the despicable years of George W. Bush's presidency had anything positive which I incorporated into my own palette, it was, "Stay on message."

> The history of philosophy is to a great extent that of a certain clash of human temperaments. Undignified as such a treatment may seem to some of my colleagues, I shall have to take account of this clash and explain a good many of the divergencies of philosophies by it. Of whatever temperament a professional philosopher is, he tries, when philosophizing, to sink the fact of his temperament. Temperament is no conventionally recognized reason, so he urges impersonal reasons only for his conclusions. Yet his temperament really gives him a stronger bias than any of his more strictly objective premises. It loads the evidence for him one way or the other . . . just as this fact or that principle would. He *trusts* his temperament. Wanting a universe that suits it, he believes in any representation of the universe that does suit it. He feels men of opposite temper to be out of key with the world's character, and in his heart considers them incompetent and 'not in it,' in the philosophic business, even though they may far excel him in dialectical ability.
>
> Yet in the forum he can make no claim, on the bare ground of his temperament, to superior discernment or authority. There arises thus a certain insincerity in our philosophic discussions: the potentest of all our premises is never mentioned.

which itself builds on his 1879 essay "The Sentiment of Rationality":

> Why does [W. K.] Clifford fearlessly proclaim his belief in the conscious-automaton theory, although the 'proofs' before him are the same which make Mr. Lewes reject it? Why does he believe in primordial units of 'mind-stuff' on evidence which would seem quite worthless to Professor Bain? Simply because, like every human being of the slightest mental originality, he is peculiarly sensitive to evidence that bears in some one direction. It is utterly hopeless to try to exorcise such sensitiveness by calling it the disturbing subjective factor, and branding it as the root of all evil. 'Subjective' be it called! and 'disturbing' to those whom it foils! But if it helps those who, as Cicero says, "vim naturae magis sentiunt,"[2] it is good and not evil. Pretend what we may, the whole man within us is at work when we form our philosophical opinions. Intellect, will, taste, and passion co-operate just as they do in practical affairs . . .

The year 2013 rings no differently. David Deutsch calls the Everett interpretation of quantum theory an "implication of science." Yet, where he, David Wallace, and Simon Saunders—to name some of Everett's most striving and eloquent disciples—can see no evidence against such a reading of quantum theory, I can see nothing particular about the theory's formalism that compels the view at all. In fact, I see nothing that even suggests it. In counterpoint: The identities and analogies between quantum theory and Bayesian probability theory that come alive for me, seem to be for them but dead afterthoughts of a formalism handed down from on high. There is something about their mindset that is as foreign to me as mine is to them.

The conceit of this document (as it cannot be otherwise if it is to exist at all) is that *QBism vis naturae magis sentit.* The 2100 or so pages to follow are the only ways I know to convey the varieties of "intellect, will, taste, and passion" needed to make a view like QBism sensible and deemed worth pursuing. Plenty in the pages herein is

[2] "feel the force of nature more"

indeed banter, but taking note of the statement and style, both of the banter and the more seriously considered exegeses, is, as I see it, the most honest way to expose the potentest of QBism's premises. In all, these pages taken together form an expression of one temperament as it confronts a world that will either accommodate it, bend it, or potentially smite it down.

If one were to look for a single most-encompassing statement of QBism's vision of the world—the crucial extract of this whole document—it might well be captured by this passage from page 2058:

> QBism says that every quantum measurement is a moment of creation, and the formal apparatus of quantum theory is an aid for each agent's thinking about those "creatia" she is involved with. But surely a Copernican principle applies just as much to QBism as to any other science. QBism's solution starts by saying the last point just that much more clearly: "Quantum measurement represents those moments of creation an agent happens to seek out or notice." It does not at all mean that there aren't moments of creation going on all around, unnoticed, unparticipated in by the particular agent, all the time. The larger world of QBism is something aligned with James's vision of a pluriverse where "being comes in local spots and patches which add themselves or stay away at random, independently of the rest."

or a little more fully by this one on page 1915:

> Three characteristics set QBism apart from other existing interpretations of quantum mechanics. First is its crucial reliance on the mathematical tools of quantum information theory to reshape the look and feel of quantum theory's formal structure. Second is its stance that two levels of radical "personalism" are required to break the interpretational conundrums plaguing the theory. Third is its recognition that with the solution of the theory's conundrums, quantum theory does not reach an end, but is the start of a great journey.
>
> The two levels of personalism refer to how the "probabilities" and "measurement events" of quantum theory are to be interpreted. With regard to quantum probabilities, QBism asserts that they be interpreted as personal, Bayesian degrees of belief. This is the idea that probability is not something out in the world that can be right or wrong, but a personal accounting of what one expects. The implications of this are deep, for one can see with the help of quantum information theory that it means that quantum states, too, are not things out in the world. Quantum states rather represent personal accounting, and two agents speaking of the same quantum system may have distinct state assignments for it. The second level of personalism appears with the meaning of a quantum-measurement outcome. On this QBism holds with Pauli that a measurement apparatus must be understood as an extension of the agent herself, not something foreign and separate. A quantum measurement device is like a prosthetic hand, and the outcome of a measurement is an unpredictable, undetermined "experience" shared between the agent and external system. Quantum theory, thus, is no mirror image of what the world is, but rather a "user's manual" that any agent can adopt for better navigation in a world suffused with creation: The agent uses it for her little part and participation in this creation.

But these formulations are the fruit of a tree somewhere 10-years-up a very steep slope. They could not have been said at the time of my *Notes on a Paulian Idea*.

[MORE TO COME.]

Much of what is recorded here concerns a fairly major transition in my thought. Once upon a time, I expressed my position with the slogan, "Quantum States are States of Knowledge." But I ultimately realized this phrase was inconsistent with a thoroughgoing Bayesian conception of probabilities—the conception of Frank P. Ramsey, Bruno de Finetti, and Richard Jeffrey. Worse perhaps, it was inconsistent with the idea that physical systems have *no* telepathy!—i.e., that there is no spooky action at a distance. The remediation of these problems required ... This document tracks my thoughts as I tried to formulate a corrective to the conception ... which in turn led the way to *ontological* considerations that were like a hammer to the "block universe" conception of the world.

[MUCH MORE TO COME.]

This time a large fraction of the emails are significantly more technical in flavor and argumentation, though certainly as personal as ever. Thus by necessity, the layout of the book is more continuous in thread than previously.

[MUCH MORE TO COME.]

Correspondents:

Index to the More Significant Arguments and Ideas

- Certainty
- Dutch book arguments: coherence vs. strict coherence
- Indeterminism
- The mark of external reality
- Pragmatist theories of truth
- Quantum measurement as metaphorically like "working the philosopher's stone"
- Radical probabilism
- Subjectivity of quantum states
- Subjectivity of quantum operations
- Objectivity of the quantum formalism
- Malleability of reality
- John Archibald Wheeler
- Wigner's friend

DISCLAIMERS:

I. This document represents a unique, and hopefully entertaining, method to communicate my more recent foundational thoughts on quantum theory. For precisely this reason, however, it carries a great danger to my friends. It is after all a collection of correspondence. There are two things that should not be mistaken: 1) The potential of my memory to be faulty when reporting the views of others, and 2) that the quotes taken from my correspondents were composed in anything other than a casual manner for *private* use only. With regard to the latter, I assert the right of my correspondents to deny—without apologies!—that their quotes represent accurate accounts of their thoughts. I have tried to guard against misrepresentation by keeping the number of quotes and correspondent-replies to a minimum: The ones that are used, are used mainly as springboards for *my* tendentiousness.

II. Various deletions of text have been made to the original letters. The purpose of the vast majority of these is to spare the reader of the "merely personal" in my life. A smaller fraction are for the sake of protecting the innocent, protecting the correspondents, and protecting the illusion that I am good-natured. The same holds as well for a small number of explicit changes of phrase (in my own writings, *never* the correspondents). In most cases, I have tried to make the process look as seamless as possible, with no evidence that the text may have been otherwise. In my own writings, bare ellipses should be interpreted as punctuation; bracketed ellipses indicate true editorial changes.

III. There is no claim that all the ideas presented here are coherent. The hope is instead that the incoherent ones will earn their keep by their entertainment value.

ACKNOWLEDGEMENTS:

I thank Scott Aaronson, Ingemar Bengtsson, Paul Benioff, Jeffrey Bub, Adán Cabello, Richard Campos, Carlton Caves, Åsa Ericsson, Robert Garisto, Lucien Hardy, Osamu Hirota, Adrian Kent, Eugen Merzbacher, Klaus Mølmer, Ulrich Mohrhoff, Michael Nielsen, Y. Jack Ng, Marcos Pérez-Suárez, Itamar Pitowsky, Arkady Plotnitsky, Damian Pope, Steve Savitt, Ben Schumacher, and Bill Wootters for helping me believe a project like this is not *too* crazy. I thank Marcus Appleby, Hans Christian von Baeyer, Eric Cavalcanti, Greg Comer, Steven van Enk, Andrei Khrennikov, Matthew Leifer, David Mermin, John Preskill, Huw Price, Rüdiger Schack, Maximilian Schlosshauer, and Rob Spekkens for helping me believe it might even be worthwhile.

Beyond all others, however, I thank Blake Stacey for his constant encouragement and the living evidence he gave that the document really might make a difference. With the dozens and dozens of typos he caught at the same time ("samizdata" he called them), the title of "editor-in-chief" would not be out of place for him.

There is a major difference between this collection and my last: It is not so busy with letters and replies to Asher Peres. He is sorely missed. In all, the tone of the book is a little lonelier without him.

Now my own suspicion is that the Universe is not only queerer than we suppose, but queerer than we can suppose.

— J. B. S. Haldane

05-01-01 *In Defense of Interactionism* (to J. M. Renes)

Renesism 1: *I've been reading up on some quantum logic, and finally have some idea of what it is and how it might be useful in this "research direction" – but in some ways cuts against the Bayesian grain, and that might be an opportunity to do some interesting (further) research. I say further because I think our Gleason-like result is a piece in this puzzle; the puzzle being something like "what does quantum logic look like when using POVMs instead of projectors?" That's probably misleading a bit, so "starting from where Hilbert-space-projector quantum logic does, where do POVMs take you? What are the logical structures and rules which differentiate classical and quantum mechanics? How does the weirdness of quantum mechanics arise from these rules and structures?" I'm sure a lot of the answers to these questions are trivial replacements of PVMs with POVMs, but not always.*

I don't know what Carl has said, but I like the sound of your "research direction." I doubt any of the references below will be overly useful, but I met a guy at the quantum information tutorials in Edinburgh who has spent a lot of time generalizing quantum logics to effect algebras. His name is Roberto Giuntini. I just looked up the following articles on Web of Science.

I say I doubt it will be overly useful, because I gathered that, in his heart, he too was looking for the holy grail of most quantum logicians: To find a way of pinning observer-independent properties on the world via algebraic properties of the theory's surface terms (i.e., states, observables, Hamiltonians). In opposition to Carl—I won't say it's heresy—I think that's the wrong way to proceed. I think we'll ultimately find *a quantum reality*, but we'll have to dig deeper than that.

All that said, the reason I like your research direction is because I think understanding the purely algebraic properties of POVMs is an important link nevertheless. I would like to think that quantum mechanics is more about our interface with the world than the world itself ... and that it's at that interface that we'll find our glimpse of a "quantum reality." What is it about the interface that makes our best Bayesian predictions of the quantum mechanical form? That can be an algebraic question just as much as trying to pin naive properties on the world (regardless of how contrived the logic). In that sense, there is some chance of maybe learning something from the Giuntinis of the world.

On another topic—not completely unrelated—I talked to my boss about your summer position here. It's not completely 100% yet, but it's pretty damned close (maybe 99.9%). So, we should have a good summer together.

- Dalla Chiara ML, Giuntini R
  Paraconsistent ideas in quantum logic
  SYNTHESE 125: (1-2) 55-68 2000

- Cattaneo G, Dalla Chiara ML, Giuntini R, et al.
  Effect algebras and para-Boolean manifolds
  INT J THEOR PHYS 39: (3) 551-564 MAR 2000

- Cattaneo G, Dalla Chiara ML, Giuntini R
  How many notions of "sharp"?
  INT J THEOR PHYS 38: (12) 3153-3161 DEC 1999

- Giuntini R, Pulmannova S
  Ideals and congruences in effect algebras and QMV-algebras
  COMMUN ALGEBRA 28: (3) 1567-1592 2000

- Cattaneo G, Giuntini R, Pilla R
  BZMV(dM) algebras and stonian MV-algebras (applications to fuzzy sets and rough approximations)
  FUZZY SET SYST 108: (2) 201-222 DEC 1 1999

- Giuntini R
  Quantum MV-algebras and commutativity
  INT J THEOR PHYS 37: (1) 65-74 JAN 1998

- CATTANEO G, GIUNTINI R
  Some Results on BZ Structures from Hilbertian Unsharp Quantum Physics
  FOUND PHYS 25: (8) 1147-1183 AUG 1995

- GIUNTINI R
  Quasi-Linear QMV Algebras
  INT J THEOR PHYS 34: (8) 1397-1407 AUG 1995

- DALLACHIARA ML, GIUNTINI R
  Partial and Unsharp Quantum-Logics
  FOUND PHYS 24: (8) 1161-1177 AUG 1994

- GIUNTINI R
  3-Valued Brouwer-Zadeh Logic
  INT J THEOR PHYS 32: (10) 1875-1887 OCT 1993

08-01-01 *Little Miracles* (to R. Jozsa)

I ran across the funniest thing by accident a minute ago. I'll place the announcement below. I especially got a chuckle from the last speaker's talk title. Maybe that's been the problem between you and me all along!

> Conference Announcement
>
> Numbers, Sets and Structures
> University of Bristol
> Saturday 18th November 2000
>
> Speakers:
>
> "The origin and status of our conception of number"
> Bill Demopolous (Western Ontario)
>
> "Can you be sure the number three isn't Julius Caesar?"
> Fraser MacBride (St. Andrews)

"Is there a unique natural number system?"
John Mayberry (Bristol)

"Anti-realists and classical mathematicians cannot get along"
Stewart Shapiro (Ohio State)

08-01-01 *Lunch?* (to S. J. van Enk)

Sorry, but I'm going to try hard not to exist until observed by Norbert.

There was a young man who said, "God
Must think it exceedingly odd
If he finds that this tree
Continues to be
When there's no one about in the Quad."

REPLY
Dear Sir:
Your astonishment's odd:
I am always about in the Quad.
And that's why the tree
Will continue to be,
Since observed by
Yours faithfully,
NORBERT.

09-01-01 *Please Come* (to L. Hardy)

The little blurb below is self-explanatory, and I'm being too lazy to personalize it. But you're in the first round of people I'm contacting. The reference to Montréal refers to the previous meeting I tried to invite you to. I hope you can come to this one; I'm especially eager to talk about your new paper (and I think Mermin is too). Unfortunately, Wootters can't make it, but I'm hoping that some of the other guys (from the previous meeting) can. If you have some grant money to spend, that'd be great, and it will help me get some of the more needy folk there. But most importantly, I hope you can come either way. Please let me know your thoughts as soon as possible. Once I get a better picture of who can make it, I'll start contacting more of the room-and-board crowd.

June 11–16, 2001, there's going to be a conference in Växjö, Sweden titled, "Quantum Theory: Reconsideration of Foundations," and I've been asked to organize a session on "quantum foundations in the light of quantum information" (or at least that's my take on it). I've been given the go-ahead to invite 3–4 people, all expenses paid, and 5 people with local expenses taken care of (but no travel). I'm hoping hard that you'll say yes to being one of the 3–4 (or one of the 5 if you have enough grant money and are feeling generous). In any case, *I'm hoping you'll say yes.* In flavor and constitution, I plan to make the session as much like our little meeting in Montréal as I can (given the larger, more diverse audience of the conference). The other people that I presently know to also be attending are Enrico Beltrametti, Jeff Bub, Arthur Fine, Ed Nelson, Pekka Lahti, and Asher Peres . . . but as a I say there will be a further contingent of quantum information people and a load of others beside that. Please let me know your thoughts at your earliest convenience:

I've been told to get back to the main organizer (Khrennikov) with my recommendations by next Sunday (Jan 14).

Your madly-optimistic-for-the-quantum friend,

Chris

09-01-01 *To Capitalize or Not To Capitalize?* (to S. J. van Enk)

That is the question. If you were me, would you capitalize the Greek letter starting the sentence below?

> The words "quantum state" are used here, just as in the previous formulation: One cannot get away from that. However, there are no unknown quantum states. $\rho^{(N)}$ is known by the experimenter if no one else. More importantly, the experimenter must be in a . . .

Don't you wish you had my troubles?

10-01-01 *Foundation-Sensitive Quantum-Information People* (to A. M. Steane)

I really, really enjoyed my conversation with you in Mykonos this summer: It prompts the invitation below. The blurb should be self-explanatory, so I'm going to be a little lazy and not personalize it. But you *are* in the first round of people I'm contacting; it's just that most of those guys were at a previous meeting Gilles Brassard and I organized this spring. Anyway, I hope you'll come. I think some of your ideas are quite deep and deserve a wider circulation with people who enjoy this sort of thing. To give you a flavor of the sort of people I'm seeking out for this session, I'll also place the *Montréal* meeting blurb further below. (So, don't get confused by there being two conference announcements in this note.) I don't know who I can get yet, but if things turn out like at the previous meeting, I think you'll have quite a bit of fun.

Finally, if you would like to come and have some grant money to spend, that'd be great, and will help me get some of the more needy folk there too. But most importantly, I hope you can come either way. Please let me know your thoughts as soon as possible. Once I get a better picture of who can make it, I'll start contacting more of the room-and-board crowd.

14-01-01 *Title and Abstract for Quantum Tutorials* (to J. E. Mazo)

Title and abstract for the talks (all three) below.

> Title: Enough Quantum Mechanics To Get Up and Running
>
> Abstract: I have a single goal in mind, to get as many people involved and cranking out research in quantum information science as possible. The motivation is self-serving: I really do believe there is nothing more exotic, more interesting, more important than quantum information. These talks (three or so) will be designed with that goal in mind—to get the communication theorist thinking about quantum stuff. If you can come to the lectures with any small memory of linear algebra (what a vector space is, what an inner product is, etc.), then I'll try to get you out with a workable knowledge of

what you might accomplish in the quantum domain. The lectures will be predominantly about the rules of the game from the communication perspective, but they will also be peppered liberally with simple relevant examples—quantum key distribution, quantum teleportation, Bell inequalities (no pun!), etc.

17-01-01 *How About This?* (to A. M. Steane)

That's too bad you can't come. My confirmed list of sessioners is now: Bub, Caves, Hardy, Jozsa, Lahti, Mermin, Peres, Schack, and Schumacher. (Still waiting on Preskill.) Almost surely now, we'll change the dates to June 17-22. You would have been a great addition! I'd really like to get some representation of your paper "A Quantum Computer Needs Only One Universe." So, how about this idea? Do you have a cohort sufficiently versed in the ideas (and convinced of their utility) to carry the torch? If you can't talk on your paper, maybe that person could do just as well. Any recommendations?

17-01-01 *Shannon meets Bohr* (to A. Y. Khrennikov)

Here's where it stands so far. I've drawn up a significantly more detailed synopsis for you. What do you think? I think the handwriting is on the wall as far as this group is concerned: the best dates for the meeting would be June 17–22. Shall we make those dates firm?

As I've told you already, what I've been doing in inviting people is giving them the choice to pay for part of the way out of their own grants. I figure we can get a lot more interesting people that way, and not sacrifice any quality. Indeed, the list is starting to include some of the best of the best in quantum information and still a lot of them are willing to pay their own travel expenses (especially, they say, if it will make for a better meeting).

So, how do your finances look given this new information? Would you like me to pursue some more of my (relevant) friends? Rarely do we find a congenial opportunity to talk about quantum foundations with each other—so this opportunity is quite a nice thing for us. The way I would proceed is roughly according to the list I laid out below. I say, "the more, the merrier!" But *you* are the financier; so it's *your* decision.

I read today about your meeting last Nov-Dec on *Probability in Physics*! It looked fascinating; I wish I had been there.

Anyway, for the present meeting, it looks like Shannon meets Bohr!

17-01-01 *Up Your . . .* (to H. J. Bernstein)

Alley. Up your alley, Herb.
Have a look at this article:

> The Environment and the Epistemological Lesson of Complementarity
> Folse HJ
> Environmental Ethics **15**(4) 345–353 WIN 1993
>
> Document type: Article Language: English Cited References: 23 Times Cited: 1
>
> Abstract: Following discussions by Callicott and Zimmerman, I argue that much of deep ecology's critique of science is based on an outdated image of natural science. The significance of the quantum revolution for environmental issues does not lie in

its alleged intrusion of the subjective consciousness into the physicists' description of nature. Arguing from the viewpoint of Niels Bohr's framework of complementarity, I conclude that Bohr's epistemological lesson teaches that the object of description in physical science must be interaction and that it is now mistaken to imagine that physical science aims to represent nature in terms of properties it possesses apart from interaction.

KeyWords Plus: normative naturalism, intrinsic value, quantum-theory, science

Addresses: Folse HJ, Loyola Univ, Dept Philosophy, 6363 St Charles Ave, New Orleans, LA 70118.

18-01-01 *Indeed I Have* (to A. Plotnitsky)

Plotnitskyism 1: *I hope you have been thinking about Pauli and all in general (naturally, no rush with sending anything).*

Indeed I have, but not nearly to the extent that I had hoped to. Getting established in this new environment has been quite something else. Did I ever write you after Mykonos? I can't find any record of it. OK, in the next note I'll send you a *very* incomplete compendium on Paulian ideas. Perhaps that will help you get better oriented.

In the present note, I'll also put some thoughts prompted by Mykonos. They were in a letter to David Mermin. And then still further below that, I'll place a passage that my note to Mermin refers to, "Genesis and the Quantum". [See 10-09-02 note "Don't Forget Your Problem Sets" to the Communards.] I hope you can sort it all out. It would be interesting to hear to your thoughts. I think there is some connection between the "efficacity" you spoke of and my "Zing!". (I think we even spoke about this in Mykonos.)

By the way, could you send me the full bibliography of your writings on quantum stuff?

Excerpt from 20 July 2000 note titled "Zing!" to N. David Mermin

Let me give you yet another suggestion for reading (that'll you'll probably ignore 2/3 of ... you told me you only read about 1/3 of what I suggest). I really enjoyed this one on my flight home the other day.

> H. J. Folse, "Niels Bohr's Concept of Reality," in *Symposium on the Foundations of Modern Physics 1987: The Copenhagen Interpretation 60 Years after the Como Lecture*, edited by P. Lahti and P. Mittelstaedt (World Scientific, Singapore, 1987), pp. 161–179.

As always, I almost surely liked it because it was saying something I wanted to hear.

Somehow I feel that I had an epiphany in Mykonos. Do you remember the parable of "Genesis and the Quantum" from my Montréal problem set? [See note to Gilles Brassard, titled "Problem Set," dated 15 May 2000.] And do you remember my slide of an empty black box with two overlays. The first overlay was of a big $|\psi\rangle$ (hand drawn in blue ink of course). I put the slide of the box up first, and said "This is a quantum system; it's what's there in the world independent of us." Then I put the first overlay on it and say, "And this symbol stands for nothing more than what we know of it. Take us away and the symbol goes away too." I then remove the $|\psi\rangle$. "But that doesn't mean that the system, this black box, goes away." Finally I put back up the $|\psi\rangle$ over the box, and the final overlay. This one says: "Information/knowledge about what? The consequences of our experimental interventions into the course of Nature."

Well, now I've made another overlay for my black box slide. At the top it asks, "So what is real about a quantum system?" In the center, so that it ends up actually in the box, is a very stylistic version of the word "Zing!" And at the bottom it answers, "The locus of all information-disturbance tradeoff curves for the system." In words, I (plan to) say, "It is that zing of the system, that sensitivity to the touch, that keeps us from ever saying more than $|\psi\rangle$ of it. This is the thing that is real about the system. It is our task to give better expression to that idea, and start to appreciate the doors it opens for us to shape and manipulate the world." What is it that makes quantum cryptography go? Very explicitly, the zing in the system. What is it that makes quantum computing go? The zing in its components!

Anyway, I'm quite taken by this idea that's getting so close to being a technical one—i.e., well formed enough that one might check whether there is something to it. What is real of the system is the locus of information-disturbance (perhaps it would be better to say "information-information") tradeoff curves. The thing to do now is to show that Hilbert space comes about as a compact description of that collection, and that it's not the other way around. As I've preached to you for over two years now, this idea (though it was in less refined form before now) strikes me as a purely ontological one ... even though it takes inserting an Alice, Bob, and Eve into the picture to give it adequate expression. That is, it takes a little epistemology before we can get to an ontological statement.

I looked back at your original Ithaca Interpretation paper, and I'll be bold enough to say that this idea satisfies all your most important desiderata: (1), (2 suitably modified), (3), and (5).

Part of this, by the way, is why I liked so much Folse's paper. Also, believe it or not, for a moment while reading it I thought I could finally "SEE" correlation without correlata. (Not lying.) But then I thought I liked the phrase "Interaction without Interactoids" even more. My wife just thought I was being silly. Maybe you will too.

Arkady's Reply, "Bibliography"

Here is my "quantum-mechanical" bibliography. It only lists books and articles primarily on Bohr and the epistemology of quantum mechanics. There are another dozen or so articles dealing with the relationships among quantum epistemology, philosophy, and literature, as well some reviews in scholarly and popular press (*Chicago Tribune*).

Books:

- *The Knowable and the Unknowable: Modern Science and Nonclassical Thought*, Ann-Arbor, Mich.: University of Michigan Press, forthcoming in 2001. Contains substantial chapters on Bohr and Heisenberg.
- *Complementarity: Anti-Epistemology After Bohr and Derrida*, Durham, NC: Duke Univeristy, 1994.

Articles:

- "Reading Bohr: Complementarity, Epistemology, Entanglement, and Decoherence," forthcoming in the *Proceedings of the NATO Advanced Research Workshop on "Decoherence and its Implications for Quantum Computation,"* eds. Antonios Gonis and Patrice Turchy (Dordrecht: Kluwer, 2001)

- "Quantum Physics and Philosophy 1900–2001," forthcoming in *Disciplinarity at the Fin-de-Siècle*, eds. Amanda Anderson and Joseph Valente (Princeton, NJ: Princeton University Press, 2001)
- "Landscapes of Sibylline Strangeness: Complementarity, Quantum Measurement and Classical Physics," *Metadebates*, eds. G. C. Cornelis, J. P. Van Bendegem, and D. Aerts (Dordrecht: Kluwer, 1999)
- "Techno-atoms: The Ultimate Constituents of Matter and the Question Concerning Technology in Quantum Mechanics," *Tekhnema: Journal of Philosophy and Technology*, Winter-Spring 1999
- "Complementarity, Idealization, and the Limits of the Classical Conceptions of Reality," in *Mathematics, Science and Postclassical Theory*, eds. Barbara H. Smith and Arkady Plotnitsky (Durham, NC: Duke University Press, 1997)

18-01-01 *Berkeley's Clones* (to A. Peres)

Asherism 1: *BTW, can God clone an unknown quantum state?*

Funny that you ask this. I just addressed this question in the paper I'm writing with Caves and Schack:

> There is hardly a paper in the field of quantum information that does not make use of the idea of an "unknown quantum state." Unknown quantum states are teleported [Bennett1993, Experiments1998], protected with quantum error correcting codes [Shor1995, Steane1996], and used to check for quantum eavesdropping [Bennett1984, CryptoExperiments]. The list of uses grows each day. But what can the term unknown state possibly mean? In an information-based interpretation of quantum mechanics the term is an overt oxymoron: For there, quantum states in their essence are states of knowledge and not states of nature [Hartle1968]. If a quantum state is used to describe a system, then it is *known* by someone—at the very least, by the describer himself.
>
> This message is the main point of our paper. If a phenomenon ostensibly invokes the concept of an unknown state in its formulation, then the unknown state had better be a shorthand for a more fundamental situation, even if that more fundamental situation still awaits a complete analysis. This means that, for any phenomenon using the idea of an unknown quantum state in its description, a consistent information-based interpretation of quantum mechanics demands that either
>
> 1. The owner of the unknown state—a further decision-making agent or observer—must be explicitly identified. In this case, the unknown state is just a stand-in for the unknown *state of knowledge* of an essential player that was skipped over previously. Or,
> 2. If there is clearly no further decision-making agent or observer on the scene, then a way must be found to reexpress the phenomenon with the term "unknown state" consistently banished throughout its formulation. In this case, the end-product of the effort will be a single quantum state used for describing the phenomenon—namely, the state that captures the describer's state of knowledge.
>
> The use of unknown states we actually analyze in depth has to do with the measurement technique known as quantum-state tomography [Vogel1989b, Smithey1993,

Leonhardt1995]. The usual description of tomography is this. A device of some sort, a laser say, repeatedly prepares many instances of a quantum system in a fixed quantum state ρ (pure or mixed). An experimentalist who wishes to characterize the operation of the device or calibrate it for future use may be able to perform measurements on the systems it prepares even if he cannot get at the device itself. This may be useful because he may have some prior knowledge of the device's operation that can be translated into a probability distribution over states. Thus learning about the state will also be learning about the device. Most importantly, though, it is assumed that the precise state ρ is unknown. The goal of the experimenter, therefore, is to perform enough measurements, and enough kinds of measurements, on a large enough sample, to estimate the identity of ρ.

This is clearly an example where there is no further player to pin the unknown state upon as a state of knowledge. Any attempt to do so would be quite unnatural: Where would the player be placed? On the inside of the device the tomographer is trying to characterize? [3] The only strategy open to us is the second one listed above, i.e., to banish the idea of the unknown quantum state completely from quantum-state tomography's formulation.

19-01-01 *Industrial Boys* (to A. Kent)

Kentism 1: *But might you want to circulate a criticism of the article itself, independent of the value of its John-component?*

Thanks for the offer, but I think I have to shy away from that. John has always been among my physics heroes. That's why I was so incensed when I found his name attached to (1) mathematical Platonism, (2) some form of an Everett interpretation, (3) a hope for a final theory, and (3) the idea that the observer plays no role in the measurement, ALL in opposition to everything he's written for 30 years. My lashing was a lashing out at Max. I don't want drag John any further through the dirt.

20-01-01 *Sleeping Around . . .* (to G. Brassard)

Maybe I should have completed the title of this note with, "with Shannon and Bohr." Well anyway, you'll understand the relevance of the title when you read the invitation below! I really hope you can come. You, more than anyone, are probably responsible for this session's existence: Without your financing last year, I wouldn't have been propelled into this position of *infamy*!!

[3] This move would be little more respectable than George Berkeley's famous patch to his philosophical system, a difficulty captured engagingly by the limerick of Ronald Knox and an anonymous reply: "There was a young man who said, 'God must think it exceedingly odd if he finds that this tree continues to be when there's no one about in the Quad.' "

Dear Sir:
Your astonishment's odd:
I am always about in the Quad.
And that's why the tree
Will continue to be,
Since observed by
Yours faithfully,
God.

(Nor would I have these continued chances to see my dream come true.) I think your talk on the communication cost of Bell inequality violations would be perfect for this crowd. But on top of that, at least Bub and Hardy (and soon to be many more I think) are taking our ideas about "QM from QKD and no QBC" seriously.

You'll note that Bill isn't in the list below; I already contacted him and he couldn't make it this time. You'll also note that Charlie's not in there either: In this case, it's because he refused to talk about foundations last time! (You did too, but I won't hold that against you.)

So, come!

> Dear friends,
>
> I recently found out that I've gotten another opportunity to gather some friends in an exotic place to thrash out the idea of "Quantum Foundations in the Light of Quantum Information." (This is starting to become a habit for me.) Anyway, this time I've been asked to organize a session at a larger conference in Växjö, Sweden titled "Quantum Theory: Reconsideration of Foundations." The main organizer is Andrei Khrennikov, and he is planning to have 40-50 people attending, with further sessions on Bohmian mechanics, GRW mechanics, and other issues in quantum foundations (Bell inequalities, Kochen-Specker theorems, etc.). Also I've been allowed to ask a small contingent of philosophers.
>
> In flavor and constitution, I plan to make our session much like the meeting Gilles Brassard and I held in Montréal last spring. There the theme was organized around the strong feeling that we'll find the greatest things and technologies to come out of quantum mechanics when we finally grasp the parts of it that make us feel the most uncomfortable. The theory is begging us to ask something new and profound of nature.
>
> This time the list of quantum information invitees (so far) includes: Gilles Brassard, Jeffrey Bub, Carl Caves, Lucien Hardy, Richard Jozsa, David Mermin, Asher Peres, John Preskill, Rüdiger Schack, John Smolin, and Ben Schumacher. The philosophical invitees I'm in charge of trying to get are: Doug Bilodeau, Henry Folse, Itamar Pitowsky, Arkady Plotnitsky, and Abner Shimony. Depending upon how the money holds out, I might be able to call up even a few more in the community. (Also, as I say, there are other sessions and thus other invitees, but I don't know who they are yet.)
>
> I think we'll have a double-edged opportunity at this meeting, so I'm looking quite forward to it. First, we'll get a chance to continue the lines several of us started in Montreal—at that time, the attendees were Bennett, Bernstein, Brassard, Bub, Hayden, Jozsa, Mermin, Schack, Schumacher, and Wootters. But second, we may be able to play the role of educators to a larger community interested in dabbling in these same issues. That is, we will have an opportunity to get them to think of quantum information as a TOOL for exploring quantum foundations.
>
> The dates for the meeting are June 17 to 22. We think this choice will help make for quite a pleasant time: Midsummer's eve is June 21, and Mermin has pointed out that picking wild strawberries in the midnight twilight is huge fun. Furthermore, there are significant local festivities planned around that time of year.
>
> The information I'd like to get from you (as soon as possible!) is:
>
> 1) Can you confirm positively that you would like to come?
>
> and
>
> 2) What kind of financial resources will it take for us to get you there?
>
> a) Full expenses paid?

b) Local expenses paid?
c) Nothing?

Concerning question 2), you can count this letter as an invitation, so feel free to answer any of the three options a), b), or c). However, the more money I can get the invitees to throw into the pot (IF they have it available), the more interesting people I can get to Sweden to keep us entertained. I've been told that I can have 3-4 invitees all expenses paid and 5 invitees with local expenses paid from the quantum information group. You can see that I'm *stretching* my limits! How am I going to do this? I'm *hoping* that a significant number of invitees will be able to volunteer to pay their nonlocal expenses, i.e. their travel expenses. Money is flowing pretty well into quantum information in Canada, the US, and the UK right now; and I do know that at least some of you have a surplus. On the other hand, the most important thing is that you attend, regardless of your funding status.

So please come: I think we'll have a lot of fun. Please let me know your thoughts as soon as you can! The sooner Khrennikov and I can get this preliminary information, the sooner we can start throwing stones at all the other problems.

All the best,

Chris

23-01-01 *It's Really Real* (to the Växjö Invitees)

The main organizer of the Växjö meeting, Andrei Khrennikov, now has all your financial-need information, etc., in hand. The word is, the total session below will go through—physicists, philosophers, and all. You should be hearing from him with an official invitation and conference announcement next week (when he returns from a conference in Italy). So, please mark the dates in your calendars!

Think John Wheeler thoughts!: "I want you ... to jolt the world of physics into an understanding of the quantum because the quantum surely contains—when unraveled—the most wonderful insight we could ever hope to have on how this world operates, something equivalent in scope and power to the greatest discovery that science has ever yet yielded up ..."

Looking forward to seeing you all in Sweden!

Quantum Theory: Reconsideration of Foundations Växjö, Sweden, 17–22 June 2001

Session: *Shannon meets Bohr: Quantum Foundations in the Light of Quantum Information*

The Physicists (Shannon meets Bohr)	Institution	Confirmed Participation
Gilles Brassard	U. Montréal	??
Jeffrey Bub	U. Maryland	yes
Carlton Caves	U. New Mexico	yes
Christopher Fuchs	Bell Labs	yes
Lucien Hardy	Oxford U.	yes

Richard Jozsa	U. Bristol	yes
Pekka Lahti	U. Turku	??
David Mermin	Cornell U.	yes
Asher Peres	Technion	yes
Itamar Pitowsky	Hebrew U.	yes
John Preskill	Caltech	tentative
Ruediger Schack	U. London	yes
Ben Schumacher	Kenyon College	yes, −1st day
John Smolin	IBM Research	yes

The Philosophers (Bohr meets Shannon)	Institution	Confirmed Participation
Doug Bilodeau	U. Indiana	yes
Henry Folse	Loyola U.	yes
Arkady Plotnitsky	Purdue U.	yes
Abner Shimony	Boston U.	??

Student Contingent	Institution	Confirmed Participation
Joe Renes	U. New Mexico	yes
Daniel Terno	Technion	yes

24-01-01 *Split Personality?* (to S. J. van Enk)

December 12, 2000, Tuesday
Science Desk
New York Times

ESSAY: ‘A Practical Tool,’ But Puzzling, Too
By JOHN ARCHIBALD WHEELER

What is the greatest mystery in physics today? Different physicists have different answers. My candidate for greatest mystery is a question now a century old, “How come the quantum?”

What is this thing, the “quantum”? It’s a bundle of energy, an indivisible unit that can be sliced no more. Max Planck showed us a hundred years ago that light is emitted not in a smooth, steady flow, but in quanta. Then physicists found quantum jumps of energy, the quantum of electric charge and more. In the small-scale world, everything is lumpy.

And more than just lumpy. When events are examined closely enough, uncertainty prevails; cause and effect become disconnected. Change occurs in little explosions in which matter is created and destroyed, in which chance guides what happens, in which waves are particles and particles are waves.

Despite all this uncertainty, quantum physics is both a practical tool and the basis of our understanding of much of the physical world. It has explained the structure of atoms and molecules, the thermonuclear burning that lights the stars, the behavior of semiconductors and superconductors, the radioactivity that heats the earth, and the comings and goings of particles from neutrinos to quarks.

Successful, yes, but mysterious, too. Balancing the glory of quantum achievements, we have the shame of not knowing "how come." Why does the quantum exist?

My mentor, the Danish physicist Niels Bohr, made his peace with the quantum. His "Copenhagen interpretation" promulgated in 1927 bridged the gap between the strangeness of the quantum world and the ordinariness of the world around us. It is the act of measurement, said Bohr, that transforms the indefiniteness of quantum events into the definiteness of everyday experience. And what one can measure, he said, is necessarily limited. According to his principle of complementarity, you can look at something in one way or in another way, but not in both ways at once. It may be, as one French physicist put it, "the fog from the north," but the Copenhagen interpretation remains the best interpretation of the quantum that we have.

Albert Einstein, for one, could never accept this world view. In on-again, off-again debates over more than a dozen years, Bohr and Einstein argued the issues – always in a spirit of great mutual admiration and respect. I made my own effort to convince Einstein, but without success. Once, around 1942, I went around to his house in Princeton to tell him of a new way of looking at the quantum world developed by my student Richard Feynman.

Feynman pictured an electron getting from point A to point B not by one or another possible path, but by taking all possible paths at once. Einstein, after listening patiently, said, as he had on other occasions, "I still cannot believe God plays dice." Then he added, "But maybe I have earned the right to make my mistakes."

Feynman's superposed paths are eerie enough. In the 1970's, I got interested in another way to reveal the strangeness of the quantum world. I called it "delayed choice." You send a quantum of light (a photon) into an apparatus that offers the photon two paths. If you measure the photon that leaves the apparatus in one way, you can tell which path it took.

If you measure the departing photon in a different way (a complementary way), you can tell if it took both paths at once. You can't make both kinds of measurements on the same photon, but you can decide, after the photon has entered the apparatus, which kind of measurement you want to make.

Is the photon already wending its way through the apparatus along the first path? Too bad. You decide to look to see if it took both paths at once, and you find that it did. Or is it progressing along both paths at once? Too bad. You decide to find out if it took just one path, and it did.

At the University of Maryland, Carroll Alley, with Oleg Jakubowicz and William Wickes, took up the challenge I offered them and confirmed that the outcome could be affected by delaying the choice of measurement technique – the choice of question asked – until the photon was well on its way. I like to think that we may one day conduct a delayed-choice experiment not just in a laboratory, but in the cosmos.

One hundred years is, after all, not so long a time for the underpinning of a wonderfully successful theory to remain murky. Consider gravity. Isaac Newton, when he published his monumental work on gravitation in the 17th century, knew he could not answer the question, "How come gravity?" He was wise enough not to try. "I frame no hypotheses," he said.

It was 228 years later when Einstein, in his theory of general relativity, attributed gravity to the curvature of spacetime. The essence of Einstein's lesson can be summed up with the aphorism, “Mass tells spacetime how to curve, and spacetime tells mass how to move.” Even that may not be the final answer. After all, gravity and the quantum have yet to be joined harmoniously.

On the windowsill of my home on an island in Maine I keep a rock from the garden of Academe, a rock that heard the words of Plato and Aristotle as they walked and talked. Will there someday arise an equivalent to that garden where a few thoughtful colleagues will see how to put it all together and save us from the shame of not knowing “how come the quantum”? Of course, in this century, that garden will be as large as the earth itself, a “virtual” garden where the members of my imagined academy will stroll and converse electronically.

Here, a hundred years after Planck, is quantum physics, the intellectual foundation for all of chemistry, for biology, for computer technology, for astronomy and cosmology. Yet, proud foundation for so much, it does not yet know the foundation for its own teachings. One can believe, and I do believe, that the answer to the question, “How come the quantum?” will prove to be also the answer to another question, “How come existence?”

26-01-01 *Report: Wallace, ‘Worlds in the Everett Interpretation’* (to J. N. Butterfield, C. Pagonis, and D. Wallace)

Anyone who knows me knows that I am rather down on attempts to interpret quantum mechanics along Everett-like lines. I think the most funny and telling statement of this in the present context is that, whereas Mr. Wallace speaks of “Everettians,” I often speak of “Everettistas.” Thus, I am almost surprised that you sent me this paper to referee.

My difficulties come not so much from thinking that an Everett-like interpretation is inherently inconsistent or that parallel worlds tax the imagination too much. It’s more that this line of thought strikes me, at best, as a complete dead end in the physical sense. At worst, I fear it requires us to tack on even more ad hoc structures to quantum theory than we already have. (Here, I’m thinking of a preferred basis for the Hilbert space and a preferred tensor-producting of it into various factors.) For these reasons, among umpteen others, I have always been inclined to an epistemic interpretation of the quantum state. Doing this has helped me (personally) to focus the issue to asking, “What is this *property* of the quantum world—i.e., reality—that keeps us from ever knowing more of it than can be captured by the quantum state?” To that extent, I consider myself something of a realist who—just as David Deutsch—takes the wavefunction absolutely seriously. *But* absolutely seriously as a state of knowledge, not a state of nature. I do well believe we will one day shake a notion of reality from the existing theory (without adding hidden variables, etc.), but that reality won’t be the most naive surface term floating to the top (i.e., the quantum state). When we have it, we’ll really have something; there’ll be no turning back. Physics won’t be at an end, but just at the beginning. For then, and only then, will we be able to recognize how we might extend the theory to something bigger and better than quantum mechanics itself.

All that said, you’re going to be surprised by my evaluation of this paper. Without hesitation, I recommend you publish it! Of all the things I’ve read on Everett-like interpretations over the years, this paper has struck me as the most reasonable of the lot. This is a nice paper. It changed none of my views, but it caused me deep pause for thought. What better honor can one have from a sparring partner?

I am not in a position to judge whether the paper is a significant step forward over the many Saunders papers it cites—I’ve never read any of them—but I do know this much: I have walked away from three of Simon Saunders' talks and not had a clue what he was talking about. So, even if my evaluation (as an outsider) of the paper's technical merit leaves something to be desired, I think Mr. Wallace's paper clearly serves a purpose within our community. The analogies between the Everett-like structure he proposes and the spacetime of general relativity are indeed intriguing and worthy of thought. For me personally—whatever their ultimate merit—they give a new stone on which to hone the arguments for an epistemic interpretation of the quantum state.

Let me just make a few minor comments to round out this report.

1) I very much enjoyed the discussion: "So there are no theory-neutral observations: rather, there is an existing theory in terms of which our observations are automatically interpreted, and which we must take as our starting point when interpreting the theories of physics." I've seen a discussion like this before, and it probably should be cited. Here are two references from my personal archive:

- W. Heisenberg, *Physics and Beyond: Encounters and Conversations*, translated by A. J. Pomerans (Harper & Row, New York, 1971), pp. 63–64. Heisenberg reports Einstein as having once said the following to him:

  > It is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe. You must appreciate that observation is a very complicated process. The phenomenon under observation produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness—we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions. When we claim that we can observe something new, we ought really to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our consciousness—function in such a way that we can rely upon them and hence speak of "observation."

  This is discussed further in:

- M. Jammer, "The Experiment in Classical and in Quantum Physics," in *Proceedings of the International Symposium, Foundations of Quantum Mechanics in the Light of New Technology*, edited by S. Kamefuchi, H. Ezawa, Y. Murayama, M. Namiki, S. Nomura, Y. Ohnuki, and T. Yajima (Physical Society of Japan, Tokyo, 1984), pp. 265–276.

2) Equation 1: There's a typo. "Atom decayed" also appears in the second term of the superposition.

3) Section 10 (the big table). This may have been my favorite part of the paper. But I would do this: Put the relativity column on the left and the Everett column on the right. Somehow, I found it much easier to read in that manner. It just seemed more natural to recognize a feature in relativity first, and then look for the analogous feature in Everett.

4) There's a typo in the fifth entry of that table. The words "choice of choice of" appear in the right-hand column.

27-01-01 *The Test Particle* (to D. Wallace)

I presume by now you've seen my thoughts on your Everett article. I meant it all; it's a very nice paper. I'd like to encourage you to place the article on the `quant-ph` archive. I'd like to refer some of my friends to it to the get a debate going. Having the paper easily accessible will help get that off the ground.

One piece of analogy (or disanalogy) that you didn't explore very much is the geodesic. Is there an analogue to the geodesic in your Everettian system? One thing that strikes me is that you might start seeing a conceptual divergence here. It's not clear to me that one can concoct a good notion of "test particle" for this game: even adding the smallest system possible to an existing multiverse (I hate that term) doubles its Hilbert space dimension (acting as if the Hilbert space is finite to begin with).

31-01-01 *Ah Midsummer . . .* (to R. Jozsa)

Thanks for the report on Butterfield. I just refereed a paper by David Wallace (for Butterfield's journal) on a variant of many worlds. It's a paper that Butterfield is very sympathetic to. The impressive thing about it was that maybe it made more sense than anything else I've read on the idea. So I accepted it for publication. I made it very clear that I continue to think that MWI is a dead-end, contentless idea. But I feel morally obligated to think that only of (what might be) their best shot!

31-01-01 *Corrections FUCHS Manuscript 060103PRA* (to *Physical Review A*)

Along with this note, I will fax back my corrections listed directly on the page proofs. All page numbers below refer to the marked up proofs. On the faxed copy, all modified pages are marked with a star in the bottom right corner.

The vast majority of your changes improved the readability of the paper. I thank you for making them. However, please note that I am fairly adamant about my remarks concerning pages 1 and 4, which have turned into something of a bone of contention because of a previous experience. Please consider my point of view with respect.

<u>Page 1)</u>

b) First sentence of abstract. I wish Physical Review would review its policy of blindly deleting all uses of the word "new" in titles and abstracts. Sometimes authors make use of this term for exactly the opposite of self-aggrandizing purposes. In my original draft I started with the sentence:

> "In this paper we give a new way to quantify the folklore notion that quantum measurements bring a disturbance to the system being measured."

You changed it to:

> "In this paper we provide a way to quantify the notion that quantum measurements bring a disturbance to the system being measured."

Written in this manner (without any extra qualification) it makes the authors appear arrogant. Any reader who knows anything whatsoever of quantum mechanics will think with disdain, "As if that's never been quantified before?!?!" Having said my piece, however, I will accept your deletion of the word "new."

What I cannot accept is your complete deletion of the word "folklore." It plays an important role in that sentence, and I ask that you replace it in its adjectival form "folkloric." The original word (barring only its mistaken grammatical form) was placed there quite carefully. It conveys the idea that we are speaking about something that is taken to be common knowledge, even when it has never been quantified. Quantifying it is the point of our paper. "FOLKLORE: the traditional beliefs, myths, tales, and practices of a people." Physicists, being human, are as susceptible to folklore as anyone else. You write, "Author–Please use more literal, meaningful modifiers." It was meant to be taken literally: there is no word that captures the concept better.

The fight over this word is a funny one with *Physical Review*. Asher Peres and I in our paper PRA **53**, 2038 (1996) had to fight for it in our opening sentence then. Finally Bernd Crasemann (then editor) accepted our point. Here is the original conversation, drawn from my email archive:

Peres:

> Dear Sirs:
>
> I just received the proofs of this article, and I am shocked that the fourth word of the first paragraph, "folklore", was changed into "convention" by the copy-editor, who added a comment "we favor more literal and accurate terminology". The authors have carefully chosen the word "folklore" because it has, in the present context, a slightly derogative meaning. This is exactly the message we want to convey to the readers of this article. I understand that the role of a copy-editor is to correct typos, style and grammar errors, and the like. There should be no distortion of the original meaning of the text. The authors, not APS or AIP, are responsible for the contents of their article.
>
> Please confirm that you are willing to accept the term "folklore" that was chosen by us. In case of a negative answer, we shall have to reconsider whether to publish our work in *Physical Review*, or withdraw it and submit it to another journal (we would prefer *Physical Review*).

Crasemann:

> You convincingly justify your use of the word "folklore" and we will certainly let it stand, even though it is not listed in the index of your admirable book, *Quantum Theory: Concepts and Methods*, which sits on my desk and has been a source of great edification.
>
> Permit me, however, to rise in defense of our staff's unceasing struggle to guard the integrity of the journal's scientific language. It was not a copy editor, but a Senior Assistant to the Editor, endowed with enviable Sprachgefuehl, who suggested the change in an effort to expunge unduly colloquial terminology. While no one is perfect, our colleagues in the Editorial Offices do a truly outstanding job safeguarding the journal's style.

I see no reason why the decision should be different this time around.

Page 2)

a) Second paragraph, third sentence. I originally wrote, "If there were a set of hidden variables underneath ..." You changed it to "If there is a set ..." This question was meant to be a hypothetical, and as such the verb should be "were." There is no question in my mind that there *are no* hidden variable underneath quantum mechanics. Please replace "were."

Page 3)

a) Second paragraph, sixth sentence. I wrote "What is novel here is that the encoding ..." You changed "novel" to "interesting," again charging that I should use more literal wording. "NOVEL: strikingly new, unusual, or different." "INTERESTING: arousing or holding the attention." Novel is the appropriate word for that sentence (describing a discovery of 1970); please reinstate it.

Page 4)

a) Second paragraph, fifth sentence. I accept your point that the phrase "founding fathers" is a colloquialism. However, substituting "original description" does not capture the proper idea either. Please replace "founding fathers" with "common folklore" (in line with my remarks above).

b) Third paragraph, first sentence. Again, please substitute "folklore" for "the founding fathers."

Page 12)

a) Sentence just above Eq. (16). You changed "... one can think of the interaction as causing the system to further unitarily evolve to" to "... to evolve further unitarily to." I understand you don't want me to split my infinitive, but "unitarily evolve" is the proper atomic verb. Please change the phrase to "... to unitarily evolve further to."

Page 13)

a) First paragraph, second to last sentence. Indeed "get a handle on" is a colloquialism, almost a Texanism. But "comprehend" doesn't capture the right idea either. Please replace the sentence with, "Finally, it stands to reason that if we can delineate the tradeoff ..."

Page 15)

a) Second paragraph, third sentence. I wrote, "When this obtains, the Shannon ..." You wrote, "When this answer is obtained, the Shannon ..." Completely different meaning. Please replace it with, "When this is the case, the Shannon ..."

Page 19)

a) Very top of page. Consistency is the hobgoblin of my small mind. 99.9% of all "non" words in the English language are nonhyphenated. Check a *Webster's* dictionary; check an *American Heritage* dictionary. It seems only in the physics community that this standard rule is broken. Please reinstate "nonnegative" in place of "non-negative."

31-01-01 *Tomography* (to S. J. van Enk)

The second reference below sounds a lot like it's doing what you say?

- K. Vogel and H. Risken, "Determination of Quasiprobability Distributions in Terms of Probability Distributions for the Rotated Quadrature Phase," Phys. Rev. A **40**, 2847 (1989).
- D. T. Smithey, M. Beck, M. G. Raymer, and A. Faridani, "Measurement of the Wigner Distribution and the Density Matrix of a Light Mode Using Optical Homodyne Tomography: Application to Squeezed States and the Vacuum," Phys. Rev. Lett. **70**, 1244 (1993).
- U. Leonhardt, "Quantum-State Tomography and Discrete Wigner Function," Phys. Rev. Lett. **74**, 4101 (1995).

01-02-01 *Oh Translator* (to S. J. van Enk)

Can you translate the German in this [quote of E. T. Jaynes]?

> For some sixty years it has appeared to many physicists that probability plays a fundamentally different role in quantum theory than it does in statistical mechanics and analysis of measurement errors. It is a commonly heard statement that probabilities calculated within a pure state have a different character than the probabilities with which different pure states appear in a mixture, or density matrix. As Pauli put it, the former represents '... eine prinzipielle *Unbestimmtheit*, nicht nur *Unbekanntheit*'. But this viewpoint leads to so many paradoxes and mysteries that we explore the consequences of the unified view, that all probability signifies only human information.

Steven's Reply

Literally it says

> a principle undeterminedness, not just unknownness.

where principle here is used as an adverb meaning something like fundamental.

Does that help?

01-02-01 *Zoom, Zoom, Zoom* (to G. Brassard)

Can you come, can you come, can you come??!?!?!?!

01-02-01 *Zing, Zing, Zing* (to G. Brassard)

Please come, please come, please come!!

02-02-01 *Zub, Zub, Zub* (to G. Brassard)

Please come, please come, please come!

02-02-01 *Zurek Criticisms* (to A. Kent)

Considering the careful critic that you are, you must have at some point written up your thoughts on Zurek's view of the quantum interpretation problem (to the extent that he has a well-defined view). Where can I find that? Alternatively, do you know of any other good critique papers by others along those lines?

05-02-01 *The Eerie Parallel* (to A. Kent)

Kentism 2 : *[Functionalism is] a view which has little to recommend it except a pleasing sense of answering a deep question with no work, and so naturally has become very widely held and respected among philosophers of mind. In this, it eerily parallels the Everett interpretation. It is only fitting that the two should be combined into a grander exercise in question-begging.*

At times in the past I've found myself wanting to be able to write like Mark Twain, and like William James. Both had this way of putting things—very different ways—that made me shiver from seeing the truth in their thoughts. Today I found myself wanting to be able to write like you!

06-02-01 *Interesting Coincidence* (to A. S. Holevo)

I'm writing up a paper (finally!) on this quantum de Finetti stuff you saw in Cambridge two years ago. And while building the bibliography, I had to place the following two contiguous entries:

- A. S. Holevo, "Information-Theoretical Aspects of Quantum Measurement," Prob. Info. Trans. **9**, 110 (1973).
- E. Prugovečki, "Information-Theoretical Aspects of Quantum Measurements," Int. J. Theo. Phys. **16**, 321 (1977).

I cited them for different reasons, and the two papers have quite distinct contents, but isn't it funny how the citations fell next to each other!

06-02-01 *Zoink!, Zoink!, Zoink!* (to G. Brassard)

Will you come? Will you come? Will you come?
(Every time I do this I have visions of the video version of *Horton Hears a Who.*)

07-02-01 *GHZM* (to N. D. Mermin)

Weren't you the first person to show that there's something weird about the state $000 + 111$ (even though everyone calls that a GHZ state)? If so, can you give me the full reference for that paper: including title and page numbers. If no, can you still give me the full reference for *that* paper: including title and page numbers.

David's Reply

Very scholarly of you to notice. The GHZ state is not in the GHZ paper. But they deserve the glory. I only constructed my version after hearing about their result and thinking that there ought to be a simpler way to make the same point. (Standing on the shoulders of GHZnts.)

I wrote about GHZ in two places. One is a *Physics Today Reference Frame* column (the only Reference Frame column I know of to receive citations in the technical literature) where I launch the sloppy scholarship by suggesting that GHZ invented the $000+111$ version of the argument as well as the general idea; the other's an AJP article.

- "What's Wrong with These Elements of Reality?," Phys. Tod., 9–11, June, 1990.
- "Quantum Mysteries Revisited," Am. J. Phys. **58**, 731–734, 1990.

There was a reason for my sloppy scholarship, by the way. I was quite enchanted with what I had boiled their argument down to and realized that if I attributed it to them, I could praise it extravagantly. But if I presented it as mine, I'd have to be boringly objective.

07-02-01 *The Scholar and the Cut* (to N. D. Mermin)

Merminition 1: *Very scholarly of you to notice.*

I am nothing if not a scholar. (Too bad I haven't yet been a good physicist too. I keep waiting for the day . . .)

07-02-01 *Wow!* (to A. Plotnitsky)

Yesterday I received the package you mailed to me! I can't express how grateful I am. I never imagined you would send me your book!! I'll make you a promise: I'll work very hard to digest all of it before meeting you again. I feel that a lot of our thoughts run fairly parallel, but I know that I have a lot of room for growth. (You, by the way, couldn't have gotten a better endorsement in my eyes than that John Wheeler was interested in your book.)

07-02-01 *Reports and Chutzpah* (to A. Kent)

Kentism 3: *I gather you'd already read David's article: maybe in fact that prompted the Zurekian query?*

Yeah, I was pretty open with my report. Might as well share it with you too; I'll paste it below. [See 26 January 2001 entry titled "Worlds in the Everett Interpretation" in *Coming of Age with Quantum Information*.]

But, no, that's not what prompted my Zurekian query. That was prompted by the paper I'm writing for the NATO meeting last summer. (I'm a little late!) The title will be "Quantum Foundations in the Light of Quantum Information," and mostly it will be devoted to laying out my program (rather than reporting results). But because the meeting was on decoherence and its connection to foundations, I thought it behooved me to say in a crisp way that it has *no* connection to foundations before going into my own spiel.

09-02-01 *A Little GHZM with Your States, Sir?* (to S. L. Braunstein)

I was thinking about a conversation we once had, and it dawned on me that you're probably the only one I dare tell this joke to. You once asked me for confirmation that David Mermin was actually the inventor of the three-particle GHZ state: GHZ, in their original paper, only talked about a four particle state. (I'll place the full story of this in David's words below.) [See 07-02-01 note "GHZM" to N. D. Mermin.] Anyway, I was toying with the idea of calling the three-particle GHZ state the GHZM state—maybe you once did too?—but how do you think that would end up being pronounced? "`!@?$`" is the first thing that comes to mind . . . but we can't have that at professional conferences!

10-02-01 *Historical Accuracy* (to N. D. Mermin)

While I'm digging for complete historical accuracy . . . Can you give me the complete reference for the very first appearance of GHZ's four-party state. Was it some AJP article including Shimony? There must have been something before that.

In any case, can you give me the complete reference including the title of the paper. Sorry to keep bugging you.

David's Reply

> The very first GHZ paper was: "Going Beyond Bell's Theorem", in *Bell's Theorem, Quantum Theory, and Conceptions of the Universe* (! — conceptions of the laboratory would have been good enough for me), ed. M. Kafatos (Kluwer Academic, Dordrecht, The Netherlands, 1989) pp. 69–72.
>
> The AJP article you mention is "Bell's Theorem without Inequalities", Daniel M. Greenberger, Michael A. Horne, Abner Shimony, Anton Zeilinger, Am. J. Phys. **58**, 1131–1143 (1990).

10-02-01 *Our Time with Brick and Keech* (to D. B. L. Baker)

You know, the years I spent with you during our youth make you partly to blame for this! I just wrote the following two footnotes for a paper by Caves, Schack and me, slated to appear in the *prestigious American Journal of Physics*. Tell me if you get the joke. The greatest mark of success will be if you (and readers like you) get it ... but the editor doesn't and therefore lets it slip past his red pen.

Let me know.

- `\bibitem{Mermin1990}`

  N. D. Mermin, "What's Wrong with These Elements of Reality?," Phys. Tod. **43**(6), 9 (1990); N. D. Mermin, "Quantum Mysteries Revisited," Am. J. Phys. **58**, 731 (1990). Though Mermin was the first to point out the interesting properties of this three-system state (following the lead of Ref. `\cite{Greenberger1989}` where a similar four-system state was proposed), we call attention to the pronunciational perils of calling the state a "GHZM state" and, thus, defer to the more common label GHZ.

- `\bibitem{Greenberger1989}`

  D. M. Greenberger, M. Horne, and A. Zeilinger, "Going Beyond Bell's Theorem," in *Bell's Theorem, Quantum Theory and Conceptions of the Universe*, edited by M. Kafatos (Kluwer, Dordrecht, 1989), p. 69.

12-02-01 *AMS Abstract* (to me)

Title: The Power of Generalized Measurements (POVMs)

Abstract: It has become customary in quantum information theory to think of "generalized measurements" or "positive-operator valued measures" (POVMs) as a derivative concept. This is because it is known that a POVM can always be built up from a standard von Neumann measurement on an ancillary system that has previously interacted with the target system. In this talk we point out the utility of taking the POVM as a basic notion. This strategy greatly reduces the difficulty of proving many very basic theorems in quantum mechanics and gives a deeper insight into why "entanglement" has the exact structure that it does.

14-02-01 *Shannon and Bohr* (to E. Merzbacher)

Since I haven't heard back from you concerning the "Shannon meets Bohr" meeting, I'm going to assume that you cannot come. In any case, our funds are fairly exhausted now. But I would have loved to have had you there, especially to expand on this wonderful thing you wrote me in 1999:

Merzbacherism 1: *For a long time I have thought about writing an essay on what is meant by the term "physical system", including some history of that slippery concept. I'll probably never do it, but your opinion piece has brought this matter back to my mind.*

Do it some day! You'd automatically have an audience of at least one. (That's something I cannot say for some of my papers!)

17-02-01 *Saturday with Some Jazz* (to A. S. Holevo)

Holevo-ism 1: *The conference was interesting, among people you know were Milburn, Massar, van Dam, Werner, ... Quite remarkable was great interest from classical probabilists and mathematical statisticians to learn quantum information. Eurandom is a good place and we should keep it in mind. ... The highlight was supposed to be the lecture of a Nobel-price winner 't Hooft .... The point of his lecture was that on Planck distances, where gravitational interaction becomes important, one may convert to nonlocal hidden variables in place of quantum theory. It was interesting to see how classical prejudices are strong even in the most distinguished physicists' minds.*

Thanks for the report of the Europhys conference. Indeed your remark about 't Hooft was most interesting. Today I am taking a leisurely day, thinking about philosophical things and compiling further my Bohrish-Paulian compendium (I now have 355 references), and thus thinking about such things.

We had a very busy week here at Bell Labs. Chris King gave a wonderful talk, and much of Jim Mazo's information theory group was in attendance. Mary Beth outlined a proof of the equality conditions for the strong subadditivity and this led to some good discussion about notions of Markov chains for quantum states. Serap and Gerhard gave a talk about "quantum coding" for commutative alphabets. And then while Peter Shor was visiting, Emina Soljanin presented what she thinks is a proof of the achievability of (your) χ in the noncommutative case. (Recall that Barnum and several of us proved χ to be a lower bound; M. Horodecki also proved it independently.) Unfortunately I had to miss that talk. Finally, I gave my final tutorial talk on basic quantum mechanics (from the quantum information slant) to Mazo's group; when I return from Japan I will start a set of tutorials on quantum information proper (for the smaller, more prepared audience).

17-02-01 *My Bohr-Pauli Compendium* (to K. Barad)

Your quantum writings have been brought to my attention by Herb Bernstein and Mike Fortun, and I wanted to tell you that your one article that I did read was wonderful! (I'll place the passages that took me the most below.)

Mainly I'm writing this letter, though, in hopes that I can get you to send me some more of your writings or at least fill in the missing information for the two references (far) below. Any way you can help would be most appreciated. I am compiling a bibliographical compendium of sensible writings about quantum mechanics, and I would like to include all your work there.

- K. Barad, "A Feminist Approach to Teaching Quantum Physics," in *Teaching the Majority: Breaking the Gender Barrier in Science, Mathematics, and Engineering*, edited by S. V. Rosser (Teachers College Press, New York, 1995), pp. 43–75.

> The Newtonian worldview is compatible with an objectivist epistemology, in which the well-prepared mind is able to produce a privileged mental mirroring of the world as it exists independently of us human beings. That is, what is "discovered" is presumed to be unmarked by its "discoverer." The claim is that the scientist can read the universal equations of nature that are inscribed in [God's] blackboard: Nature has spoken. Paradoxically, the objects being studied are given all the agency, even and most especially when they are seen as passive, inert objects moving aimlessly in the void. That is, these cultureless agents, existing outside of human space-time, are thought to reveal their secrets to patient observers watching and listening through benignly obtrusive instruments. Notice that agency is not attributed to human beings; once all subjective contaminants have been removed by the scientific method, scientists simply collect the pure distillate of truth.
>
> The Newtonian worldview is still so much a part of contemporary physics culture that it infects the teaching of post-Newtonian physics as well. That is, the stakes are so high in maintaining the mirroring view of scientific knowledge that quantum physics is presented as mysticism.

and

> Notice that particular experimental arrangements can be used to give more or less definite meaning to each of the complementary variables, but due to the lack of object-instrument distinction . . . it is not possible to assign the value obtained to the object itself. The "property" being measured in a particular experimental context is therefore not "objective" (that is, a property of the object as it exists independently of all human interventions), but neither is it created by the act of measurement (which would belie any sensible meaning of the word *measurement*). Bohr speaks of this "interaction" between "object" and "instrument" as a "phenomenon." The properties then are properties of phenomena. That is, within a given context, classical descriptive concepts can be used to describe phenomena, our intra-actions within nature. (I use the term *intra-action* to emphasize the lack of a natural object-instrument distinction, in contrast to *interaction*, which implies that there are two separate entities; that is, the latter reinscribes the contested dichotomy. . . . That is, the ambiguity between object and instrument is only temporarily contextually decided; therefore, our characterizations do not signify properties of objects but rather describe the intra-action as it is marked by a particular constructed cut chosen by the experimenter (see Ref. [Barad95] for more details).
>
> The notion of "observation" then takes on a whole new meaning according to Bohr: "[B]y an experiment we simply understand an event about which we are able in an unambiguous way to state the conditions necessary for the reproduction of the phenomena" (quoted in Ref. [Folse85], p. 124). According to the analysis of the previous section, this is possible because, in performing each measurement, the experimenter intervenes by introducing a constructed distinction between the "object" and the "measuring device" (e.g., deciding whether the photon is part of the object or the instrument). The claim is that unambiguous, reproducible measurements are possible through the introduction of constructed cuts. Notice that "[n]o explicit reference is made to any

individual observer": Different observers will get the same data set in observing any given phenomenon. Therefore, reproducibility, not some Newtonian notion of objectivity denoting observer independence, is the cornerstone of this new framework for understanding science.

For Bohr, the uncertainty principle is a matter of the inadequacy of classical description. Unlike the "mirroring" representationalism inherent in the Newtonian-Cartesian-Enlightenment framework of science, scientific concepts are not to be understood as describing some independent reality. A post-Newtonian framework sees these constructs as useful (i.e., potentially reproducible) descriptions of the entire intra-action process (the phenomenon, which is context dependent by definition), not of an isolated object. The implications of this finding are profound. In Bohr's own words:

> The extension of physical experience in our own days has ... necessitated a radical revision of the foundation for the unambiguous use of elementary concepts, and has changed our attitude to the aim of physical science. Indeed, from our present standpoint, physics is to be regarded not so much as the study of something a priori given, but rather as the development of methods for ordering and surveying human experience. (Bohr, Ref. [Bohr63c], p. 10)

In other words:

> These facts not only set a limit to the extent of the information obtainable by measurements, but they also set a limit on the meaning which we may attribute to such information. We meet here in a new light the old truth that in our description of nature the purpose is not to disclose the real essence of [physical objects] but only to track down, so far as it is possible, relations between the manifold aspects of our experience. (Bohr, Ref. [Bohr63a], p. 18)

and

Bohr's philosophy of physics involves a kind of realism in the sense that scientific knowledge is clearly constrained, although not determined, by "what is out there," since it is not separate from us; and given a particular set of constructed cuts, certain descriptive concepts of science are well-defined and can be used to achieve reproducible results. However, these results cannot be decontextualized. Scientific theories do not tell us about objects as they exist independently of us human beings; they are partial and located knowledges. Scientific concepts are not simple namings of discoveries of objective attributes of an independent Nature with inherent demarcations. Scientific concepts are not innocent and unique. They are constructs that can be used to describe "the between" rather than some independent reality. (Why would we be interested in such a thing as an independent reality anyway? We don't live in such a world.) Consideration of mutually exclusive sets of concepts produces crucial tensions and ironies, underlining a critical point about scientific knowledge: It is the fact that scientific knowledge is socially constructed that leads to reliable knowledge about "the between"—which is just what we are interested in. This shifting of boundaries deconstructs the whole notion of identity: Science can no longer be seen as the end result of a thorough distillation of culture. There is an author who marks off the boundaries and who is similarly marked by the cultural specificities of race, history, gender, language, class, politics, and other important social variables. Reproducibility is not a filter for shared biases. In stark contrast to the objectivist representationalism that is usually transmitted to students, the new framework inspired by Bohr's philosophy of physics is robust and intricate. In particular, there is an explicit sense of agency and therefore accountability. And so I refer to this Bohr-inspired framework, which shares much in common with central concerns in contemporary feminist theories, as "agential realism."

Karen's Reply

Thank you for your interest in my work. As per your request, below is a list of some of my other articles further elaborating Bohr's philosophy-physics. I hope you find these useful.

- K. Barad, "Meeting the Universe Halfway: Realism and Social Constructivism without Contradiction," in *Feminism, Science, and the Philosophy of Science*, edited by L. H. Nelson and J. Nelson (Kluwer, Dordrecht, 1996), pp. 161–194.
- K. Barad, "Getting Real: Technoscientific Practices and the Materialization of Reality," in Differences: A Journal of Feminist Cultural Studies **10**(2), 87–126 (1998).
- K. Barad, "Agential Realism: Feminist Interventions in Understanding Scientific Practices," in *The Science Studies Reader*, edited by M. Biagioli (Routledge, NY, 1998), pp. ??–??.
- K. Barad, "Reconceiving Scientific Literacy as Agential Literacy: Or, Learning How to Intra-act Responsibly Within the World," in *Doing Science + Culture*, edited by R. Reid and S. Traweek (Routledge, NY, 2000), pp. 221–258.
- K. Barad, "Agential Realism," in *Routledge Encyclopedia of Feminist Theories*, edited by L. Code (Routledge, NY, 2000).
- K. Barad, "Re(con)figuring Space, Time, and Matter," in *Feminist Locations*, edited by M. DeKoven (Rutgers University Press, New Brunswick, NJ, 2001), pp. ??–??.
- K. Barad, "Scientific Literacy → Agential Literacy = (Learning + Doing) Science Responsibly," in *A New Generation of Feminist Science Studies*, edited by M. Mayberry, B. Subramaniam, and L. Weasel (Routledge, NY, 2001), pp. ??–??.

21-02-01 *There Are No Quantum States* (to A. Peres)

Asherism 2 : *There are no quantum states (in a relativistic theory). Therefore it is pointless to discuss collapse, there is no EPR paradox, etc. What the theory is about is propagators (or rather superpropagators = completely positive maps) from initial preparation to final observation. However, an explicit evaluation of these superpropagators is best done by introducing fake notions (quantum states), just as we use a vector potential in electrodynamics, or a spacetime metric in general relativity, because it is cumbersome not to use these gauge dependent objects. I don't know what these ideas will bring, but at least I have an impression that now I understand better.*

My thesis, paradoxically, and a little provocatively, but nonetheless genuinely, is simply this:

QUANTUM STATES DO NOT EXIST.

> The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time, ..., or Fairies and Witches, was an essential step along the road to scientific thinking. The quantum state, too, if regarded as something endowed with some kind of objective existence, is no less a misleading conception, an illusory attempt to exteriorize or materialize the information we possess.
>
> — *the ghost of Bruno de Finetti*

21-02-01 *There Are No Quantum States, 2* (to A. Peres)

Asherism 3: *Where did you write that?*

I've never put it in real print yet, only in my philosophical samizdat. Maybe I'll find a way to get it into real print soon.

What am I up to? Too much and not enough at the same time!

21-02-01 *A Reference* (to P. Pearle)

If I were going to cite one (and only one) reference on spontaneous collapse ideas, which one should it be? I just want something, even a popular exposition, that can serve as a point of departure for my readers.

To give you a flavor of what I'm looking for I cited the following in connection with the Einselectionists, the Bohmians, the Everettistas, and the Consistent Historians.

1. W. H. Zurek, "Decoherence, Einselection and the Existential Interpretation (The Rough Guide)," Phil. Trans. R. Soc. Lond. A **356**, 1793–1821 (1998).

2. J. T. Cushing, A. Fine, and S. Goldstein, editors, *Bohmian Mechanics and Quantum Theory: An Appraisal*, (Kluwer, Dordrecht, 1996).

3. D. Deutsch, *The Fabric of Reality: The Science of Parallel Universes—and its Implications*, (Allen Lane, New York, 1997).

4. R. B. Griffiths and R. Omnès, "Consistent Histories and Quantum Measurements," Phys. Today **52**(8), 26–31 (1999).

26-02-01 *THE Book* (to D. P. DiVincenzo)

I just wrote to Barbara explaining what a heel I was for forgetting to ask how she made it home during the big snow storm (after her interview here). But I'm also a heel for not writing earlier to thank you for the wonderful Jammer book you bought me. I really, really appreciate it. It is THE classic in quantum foundations.

27-02-01 *Claude Shannon's Death* (to the "Shannon meets Bohr" Invitees)

To those friends I contacted for the "Shannon meets Bohr" session in Växjö:

We at Bell Labs were all deeply moved yesterday to learn of Claude Shannon's death over the weekend. Like for many Alzheimer's patients, however, we knew that it may have been a blessing in disguise. You can read more about Shannon's life in the *New York Times* obituary section today.

Some weeks ago, I found myself using the following words for a recommendation letter I was asked to write:

> As it turns out, today January 16, we had a dedication ceremony at Bell Labs for a bronze bust of Claude Shannon, the founder of classical information theory. That struck me as symbolic. Since joining the laboratory, I have been asking myself over and over what role I might play in furthering the legacy of Shannon?

Those words hold just as true for me for this midsummer's meeting. Let us use information theory as a sword and finally defeat this mystery of the quantum.

28-02-01 *History's Mysteries — A Real One!* (to family and old friends)

Dear Family and Friends (mostly from my old home town ... I'm too embarrassed to tell my colleagues),

I likely made a good fool of myself, but if you have cable TV, you might be on the lookout for my ugly mug on the History Channel the week of March 12. I got the announcement below from them. The time 8pm may be referring to California time, I don't know. They interviewed me for about an hour—stuff to do with quantum teleportation—but I suspect they're going to use only a minuscule amount of that footage. Steven Weinberg and Jeff Kimble will also be making appearances in the capacity of "official, knowledgeable scientists."

> We have an air date!! The show will air on the History Channel on the show, 'History's Mysteries: The Philadelphia Experiment' Monday, March 12 at 8pm. For more show information, you can access the History Channel web site at
>
> `www.thehistorychannel.com`
>
> The show will repeat throughout the week and you can get that air info from the web site.

06-03-01 *Pauli-ish Compendium* (to H. Atmanspacher)

I am compiling a rather large compendium to be titled, "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature," and will ultimately be submitting it to the journal *Studies in History and Philosophy of Modern Physics*. Perhaps the subject of the compendium needs no explanation given the title. It presently has about 400 items listed in it.

In any case, I wonder if I can ask of your assistance. In the compendium, I presently have you listed with three titles:

- H. Atmanspacher, "Wolfgang Pauli und die Alchemie," Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie **34**, 3–32 (1992).

- H. Atmanspacher, H. Primas, and E. Wertenschlag-Birkhäuser, eds., *Der Pauli-Jung-Dialog und seine Bedeutung für die moderne Wissenchaft*, (Springer, Berlin, 1995).

- H. Atmanspacher and H. Primas, "The Hidden Side of Wolfgang Pauli: An Eminent Physicist's Extraordinary Encounter with Depth Psychology," J. Consc. Stud. **3**, 112–126.

Unfortunately, I have not been able to obtain these materials. Might I ask you to send me copies of these papers (in the case of the book, maybe you wrote an introduction)? Also, if there is anything else you've written on the subject since then, I would be most appreciative if you could send that too. I will place my professional address below.

I obtained your email address from Andrei Khrennikov who just placed me on the advisory board to his ICMM in Växjö. I am in Germany at least once a year to visit my wife's parents in Munich; perhaps I will get a chance to visit with you sometime during such a visit (the next will be in August or September).

Harald's Reply

I am happy to give you a list of publications of mine on Pauli, see below. Copies of the articles will be sent to you.

Concerning the title of your project, it is somewhat unclear to me why you connect Pauli so tightly with Wheeler. You will have your reasons to join those names. Personally, I would not connect Pauli and Wheeler on a more than superficial level. Maybe you have insights which I am lacking.

Whenever you are in Germany, please let me know if you see a chance to come to Freiburg. As I spend some part of my time in Munich, it's possible we can meet there as well.

1. H. Atmanspacher.
   Alchemie und moderne Physik bei Wolfgang Pauli.
   In *Unus Mundus.* Edited by T. Arzt, M. Hippius-Gräfin Dürckheim, and R. Dollinger. Peter Lang Verlag, Frankfurt, 1992, pp. 135–181.
2. H. Atmanspacher.
   Wolfgang Pauli und die Alchemie, Teil I: Biographische und historische Aspekte.
   Z. Grenzgeb. Psych. **34**, 3–32 (1992).
3. H. Atmanspacher.
   Wolfgang Pauli und die Alchemie, Teil II: Das opus alchymicum.
   Z. Grenzgeb. Psych. **34**, 131–162 (1992).
4. H. Atmanspacher.
   Wolfgang Pauli und die Alchemie, Teil III: Impulse für die modernen Naturwissenschaften.
   Z. Grenzgeb. Psych. **35**, 1–27 (1993).
5. E. Wertenschlag und H. Atmanspacher.
   Das Irrationale in den Naturwissenschaften: Wolfgang Paulis Begegnung mit dem Geist der Materie (Tagungsbericht & Kommentar).
   Gaia **2**, 214–225 (1993). Reprinted in *Z. Grenzgeb. Psych.* **35**, 221–230 (1993).
6. H. Atmanspacher, H. Primas, und E. Wertenschlag (editors).
   Der Pauli-Jung-Dialog und seine Bedeutung für die moderne Wissenschaft.
   Springer Verlag, Berlin, 1995.
7. H. Atmanspacher.
   Raum, Zeit und psychische Funktionen.

In *Der Pauli-Jung-Dialog und seine Bedeutung für die moderne Wissenschaft.* Edited by H. Atmanspacher, H. Primas, und E. Wertenschlag. Springer, Berlin, 1995, pp. 239–274.

8. H. Atmanspacher.
Paulis wissenschaftlicher Briefwechsel 1940–1949 (Book Review).
Physik. Blätter **51**, 1104–1105 (1995).
9. H. Atmanspacher and H. Primas.
The hidden side of Wolfgang Pauli.
Journal of Consciousness Studies **3**, 112–126 (1996).
Reprinted in *Journal of Scientific Exploration* **11**, 369–386 (1997).
10. H. Atmanspacher.
Paulis wissenschaftlicher Briefwechsel 1950–1952 (Book Review).
Physik. Blätter **53**, 349 (1997).
11. H. Atmanspacher.
Wolfgang Pauli – nicht nur Physiker.
In *Jenseits von Kunst.* Edited by P. Weibel. Passagen, Wien, 1997, pp. 246–248.
12. H. Atmanspacher.
Paulis wissenschaftlicher Briefwechsel 1953–1954 (Book Review).
Physik. Blätter **55**(9), 79 (1999).

07-03-01 *Wonderful* (to M. A. B. Whitaker)

Thank your for your kind invitation to speak at the Belfast meeting. I am flattered, and of course I will make time on my calendar to be there. Indeed "Quantum Foundations in the Light of Quantum Information" has been one of my pet peeves lately, and I have spent quite a bit of energy in promoting that direction in our community. (First organizing a small meeting in Montréal last year with Brassard, and then a small meeting in Sweden this year.) I think there is probably no way quantum information will make a deeper contribution to physics than in finally bringing to a satisfactory conclusion the foundation problem.

So please mark me down for a talk—I have a pretty good one now devoted solely to this idea that not many in the foundations community have seen—and please keep me abreast of the further developments.

07-03-01 *Pauli and Wheeler* (to H. Atmanspacher)

What a wealth of information! Thank you so much. I didn't realize that you had written so extensively on Pauli! If only I could read German! But, still, your sending those things will be most useful: with the help of some friends I will be able to get the abstracts and some key pieces of the text translated for inclusion into the compendium.

You ask why I connect Pauli and Wheeler so tightly? Well, really the whole document is centered around a theme to do with the potential "malleability" of our world. I use Pauli and Wheeler as two figureheads, but really the document is much more about the overall theme. There is also much material on Bohr, quite a lot on Rosenfeld, stuff to do with ideas about *evolutionary* physical laws à la Peirce, Bergson, James, etc., a sprinkling of historical things on alchemy, and so forth. I'll place the (present) abstract and two quotes (one by Pauli, one by Wheeler) below to show why I've chosen those two men as figureheads.

Thank you again for your help!

09-03-01 *Banach Conference* (to D. Petz)

Thank you for the invitation to speak at the Banach Center meeting. I am flattered that you would think of me in this regard. If I were to give a talk there I would try to make sure that the subject was more along the lines of "Some Nontrivial Theorems in Quantum Mechanics" rather than as in my previous visit!! Beside visiting you again, I would be specially intrigued to meet Csiszar and discuss his "secrecy capacity" in the quantum context.

May 21–26 is just potentially possible for me.

13-03-01 *FYI – The History Channel* (to S. K. Stoll)

Stollicism 1: *On a related topic, is it true you gave an interview to Swedish media about teleportation of "souls"? I'm puzzled.*

Why do you ask? It wasn't to the Swedes, it was before that. It may have been a Japanese crew. I tried to explain that what is teleported is the quantum state, not a material object. And of course, they said, "What's that?" And I said, "It's more like the soul than the body." Or some such ting.[4]

Did I actually make an appearance on the show? Or did they completely cut me?

I'm in Tokyo right now. Akira came for Steven's and my talks. That guy is amazing: It took him a two hour trip each way, just to come sit through our talks. Friday we're going to go visit his laboratory. He's an associate professor at U. Tokyo now.

13-03-01 *Is Nice nice?* (to S. L. Braunstein)

What's the business about below? It seems they put my name on their flyer before I had a chance to answer them. Oh well. A little extra advertisement can't hurt. Now we'll just have to see whether I'm free during that time. Thanks for promoting me.

Is Nice a nice place? I'm in Tokyo right now. Charlie Bennett and I are sharing a room, and both up with jetlag. The lights are off and the laptops are glowing. A little like two fireflies in the night.

Sam's Preply

Dear Dr. Touddaint and Dr. Plet,

[4]Actually it was a Brazilian television crew. On 7 November 1998, I wrote to Asher Peres, "Teleportation has surely grasped the media's attention. Wednesday a Brazilian television crew came here and shot 1.5 hours of footage of the lab, Kimble and me. All that to make a five minute TV segment on some prime-time show. The interviewer assured me that 70 million people would be seeing my face and hearing my thoughts. It's funny, but when they told me that, all I could think of was the poor sick woman who sent Charlie Bennett a police report of some minor accident she had had about ten years earlier. She had read enough of Wheeler's thoughts on quantum mechanics—with phrases like, "reality is a function of the community of communicators," etc.—to think that if not more people knew about the smallness of the accident, it might turn into something really big and horrible. I thought, "I'm not starting to feel any more real yet. I wonder if it'll happen when the viewers actually see the film?" Oh well, I guess I don't know what it means to feel real anyway! The interviewer was fixated on whether the soul would also get carried over by a teleportation device. I'm sure my remarks will go over well in a Catholic country!"

Mr. Anders Hansson was correct about my interest in giving a presentation at DASIA 2001, unfortunately, other commitments have been made before I knew what dates were involved.

Might I suggest an alternative (whom I have approached):

- Dr. Chris Fuchs at Lucent
  cafuchs@research.bell-labs.com

His especial interests are quantum cryptography, which I think you would find very interesting; he is is also an excellent speaker and would be worth getting to come.

Sincerely,

Sam Braunstein

16-03-01 *Jammin' Jammer* (to H. Barnum)

Thanks for thinking of me. Actually, I was just given a copy of Jammer's 1974 book (*The Philosophy of Quantum Mechanics*) by David DiVincenzo. But I don't have Jammer's 1966 book yet (*The Conceptual Development of Quantum Mechanics*). I'm not completely sure which one you were thinking of.

And by the way, thanks for sending me a report on Jeff Bub's seminar. I was dancing a little jig for a week after.

17-03-01 *RCFoC* (to J. B. Lentz)

Thanks for the article. I'm still in Japan. I'll be here until Thursday. The History Channel show was an episode of History's Mysteries. The title was something about "The Philadelphia Experiment," some silliness about a supposed teleportation of a navy ship from Philadelphia to Norfolk, VA sometime in WWII. From what I understand, of the hour-long interview they gave me, they only used about a minute of the footage (at most).

17-03-01 *Pounding Rice Cakes* (to H. Barnum)

The allusion is this: One of my greatest eye-opening experiences was in learning that when the Japanese look at the moon they see a rabbit pounding rice cakes. Ever since being told that, I've been able to see it too. It's plain as day. Thirty-three years of not seeing it once, and then, boom, someone tells me of it and I see it all the time. Likewise, I'll bet the next big step in physics will only require that we see something right here in front of us. It'll be something no big multi-billion dollar particle accelerator will be needed for. We just have to figure out how to take note of it. I'm banking that a hint is already written down in the quantum.

18-03-01 *The Copenhagen Picnic* (to H. J. Folse)

Folsesm 1: *What is it that you're looking for in the Archives? I spent six months going through them 26 years ago. Of course there's more there nowadays. But I doubt that there is any undiscovered gold to find, and if there is it's deeply buried.*

Ultimately, I plan to dig deeper into whatever can be found in the US repositories you mentioned, but for the present Ben and I thought it might make a nice picnic with which to end our time in Scandinavia.

What am I looking for? Basically, just fun historical tidbits to do with this train of thought I've been telling you about. You never know what you might find ... especially when seen through "prepared" eyes. If I need to pull out an official reason for being there, I can use this large compendium I'm putting together—if it doesn't create too much copyright trouble, I plan to have it appear in *Studies in History and Philosophy of Modern Physics*. And I'm sure I can get any number of letters vouching for my historical integrity: you perhaps?, Mermin, Butterfield (editor of SHPMP), or maybe some more appropriate people if I think harder.

Speaking of the compendium, would you like to see it as it stands? It's now got about 400 references in it and a nonnegligible amount of annotation. It stands presently at 80 pages. I'm projecting about 500 references and 150 pages for the completed project. There's a whole lot of editing left to be done, but you might find it fun even at this stage. And maybe you can tell me about any glaring omissions that you know of.

For the present, let me attach a note I wrote you on my way to Japan. You never answered my two questions, even though you replied to later emails. So maybe you never got it.

From a 09 March 2001 note to Henry Folse, "The Right Choice"

Sorry for my long delay in getting back to you. I'm off to Japan in about five hours for two weeks, and *trying* to get things tied up here before leaving has made the week pretty hectic.

Anyway, the most important message I wanted to tell you is that I'm gettin' damned happy I invited you to Växjö. Reading your papers has been a really pleasant experience. I think I hit my first dozen last night at about this time. (But maybe the bigger question is, why am I up at this time?!?) Below, I'll place my Folse compendium as it stands—every word typed in lovingly with my own little fingers! Of course, I have a few quibbles with some things I've read, but I think I'll save my comments until I've read the complete body of work. 27 years is a long time, and you could well have changed your mind about some things: I'll give you the benefit of the doubt for now.

Folsesm 2: *I should certainly warn you that very few in this community agree much with my reading of Bohr.*

I couldn't care less about that: I like it (or most of it), and that's all that counts for me. Besides, I've read Bohr myself—fairly carefully I've always thought—and your view significantly coincides with my memory of that.

Folsesm 3: *I'm more concerned about my lack of knowledge of anything about information theory.*

You need not be too concerned, but of course it wouldn't hurt you to do a little reading on the side if you've got some time. Somewhere below, you wrote:

> In describing the phenomena of observational interactions, quantum theory describes them as being caused by the interactions between the observing systems and microsystems. The fact that we can form no representation, no mechanical picture, of the atoms on which the mechanistic description of

> the phenomenal world is based, hardly reveals that we are ignorant of what these entities *are*. Rather it testifies to what we have learned about them – that they cannot be so represented – through explorations of the atomic phenomenon in which their strange behavior is revealed to human experience.

It is my strongest opinion that the great fruits of quantum information and computing get at precisely this point ... and in spades! The point is that this "nonrepresentability" in actual fact boils down to a positive statement rather than a negative one. So it would do you well to learn a little about our field. (And, luckily, most of what we do is not abstruse stuff: it's just basic quantum mechanics, viewed mostly from a new point of view with a new set of goals in mind.) Where to start? Maybe a good place would be three "recent" *Physics Today* articles:

1. Gottesman and Lo, "From Quantum Cheating to Quantum Security," PT November 2000 (don't have the page numbers)
2. Preskill, "Battling Decoherence: The Fault-Tolerant Quantum Computer," PT June 1999, p. 24
3. Bennett, "Quantum Information and Computation," PT October 1995, p. 24 (interesting coincidence)

If you get that far, let me know, and I'll suggest a couple of really *mild* technical articles that'll be worth their weight in gold in insight.

But why do I think it would do you well? Because I think you have an honest heart. And, because while I believe Bohr and his gang certainly started to point us in the right direction, I think we have a long, long technical way to go before we can claim a particularly deep understanding of the quantum structure. Here's how I put it in exasperation to David Mermin once:

> What's your take on this passage? Can you make much sense of it? What does he mean by "providing room for new physical laws?" What "basic principles of science is he talking about?" What five pages of derivation are lying behind all this business?
>
> It nags me that Bohr often speaks as if it is clear that the structure of quantum theory is derivable from something deeper, when in fact all the while he is taking that structure as given. When did he ever approach an explanation of "Why complex Hilbert spaces?" Where did he ever lecture on why we are forced to tensor products for composite systems? It's a damned shame really: I very much like a lot of elements of what he said, but as far as I can tell all the hard work is still waiting to be done.

The issue in my mind is *not* to *start* with complex Hilbert space, unitary evolution, the tensor product rule for combining systems, the identification of measurements with Hermitian operators, etc., etc., and *showing* that Bohr's point of view is *consistent* with that. Instead it is to start with Bohr's point of view (or some variant thereof) and see that the *precise* mathematical structure of quantum theory *must* follow from it. Why complex instead of real? Why unitary, rather than simply linear? Indeed, why linear? Why tensor product instead of tensor sum? And, so on. When we can answer *these* questions, then we will really understand complementarity.

I'm banking my career on the idea that the tools and issues of quantum information theory are the proper way to get at this program.

OK, I've got to get some sleep. I have this dream that I'm going to get work done all the way to Japan. But if I don't get some sleep, I'll certainly be kidding myself.

Henry's Reply

Yes, if you have some particular theme to look for beforehand, then you can perhaps improve your chances of finding something of interest. Actually when I first started exploring the Archives my interest was totally different, and I probably didn't see many things that might strike me as important now. My original target was Heisenberg, not Bohr, and my original interest was the influence of Kantianism on German scientists, not realism or interpreting quantum theory. I thought of Bohr as a positivist, but when I started reading more stuff, I came to believe that Bohr had been seriously misrepresented by philosophers in general, and so I ended up going in this direction. Also they're probably in a lot better order today than they were then. At that time Bohr's last secretary, Fru Hellman, was still very much alive and running the show. She had personal knowledge of much of the files, but beyond that there was just a list of file names and dates. I'm sure at that time there were a lot of pages of manuscripts that were more or less misidentified or otherwise inscrutable. It was really an old-world European kind of thing. The last time I was there it seemed they had made a lot of progress, but still had a ways to go. I'd be interested to see how modern they might be nowadays. I imagine I'll go thru Copenhagen prior to Sweden; I'm afraid I have to get home ASAP after the conference is over.

18-03-01 *Hello from Tokyo* (to J. D. Sanders & L. Sanders)

Here's a funny story about Cuero. The last time I was in town, Kiki and I dropped in to the little bookshop/coffeeshop on Main street. I picked up an old copy of Arthur Eddington's *Our Mysterious Universe* for 50 cents. When I bought it, I told the lady—and this is a true story—"You know, this is the first book I ever bought in Cuero!" I had bought comic books in Cuero as kid, but never a real book (even a paperback). All my real books had come from Victoria and the like. But there was another weird coincidence. That night there was an author there having a book signing. It turned out, she works in Morristown (where I live presently) and lives in Murray Hill (where I work presently). A small world, and it keeps getting smaller!

20-03-01 *The Transcript Archive* (to B. W. Schumacher)

That is a really nice site!

Thanks again for coming to Japan. Charlie, Steven, and John are all gone now. Only I'm left. Things are much quieter now, and I am starting to get back to the hard job of writing some things down.

Ben's Preply

This is pretty exciting stuff.

I've been looking into the Archives for the History of Quantum Physics (AHQP) stuff. There is a web site at which one can find out more:

`http://www.amphilsoc.org/guides/ahqp/`

> Included is a list of everyone about whom there is material in the AHQP, including interviews. There are copies of the transcripts in the U.S. at various institutions, but it would of course be much cooler to visit them at the Bohr Institute.

22-03-01 *Nonorthogonal States* (to R. W. Spekkens and J. E. Sipe)

I just pulled up your new paper, `http://arxiv.org/abs/quant-ph/0003092`. It looks like it's going to be interesting even for nonmodal people!

It dawned on me when I saw your title this morning—I'm jetlagged greatly just having got back from Japan—that maybe I never told you about my paper: C. A. Fuchs, "Nonorthogonal Quantum States Maximize Classical Information Capacity," Phys. Rev. Lett. **79**(6), 1162–1165 (1997).

(You can also find it on `quant-ph`.) It shows that there is a certain sense (motivated in an operational way by information transmission problems) in which nonorthogonal states are sometimes more stable against noise than orthogonal ones. Peter Shor, John Smolin, and I even have examples now where it is best to pull the states from a set of linearly dependent ones!

I don't know if these examples have any impact on your proposed ontology, but it might be worth thinking about.

By the way, you misspelled Wootters.

24-03-01 *Annoyed* (to S. K. Stoll)

I'm back from Japan now, and I decided to finally watch that history channel thing this morning at 4:00 AM! You're right, I certainly do wish I had never been on it.

Stollicism 2: *In the last 5 minutes of the program, the photography of the lab was great, they show that first, along with a panning shot of the "Unconditional Quantum Teleportation" paper, with voice over and a clip of you saying something like, "this experiment is so fantastical that it is almost in the realm of science fiction ... " (I kid you not). Then it has a clip of Jeff explaining how technically it is impossible to teleport a material object in the foreseeable future because of the massive bits of information necessary to do so, or some such. Then the narrator says something about how Einstein started all this talk of teleportation and it flips backs to you explaining that his '35 paper was an argument against quantum mechanics.*

More precisely, I said:

> "This is something that is so fantastic ... scientifically ... at this point in time that it almost is in the realm of science fiction."

But that was targeted not at Akira's experiment; it was about that silly ship teleportation stuff. They certainly gave a sense though that I was talking about Jeff's lab. Then the other thing about Einstein really grates. The larger part of my career is devoted to debunking the idea that entanglement is about "spooky action at a distance." It is just about information: what information Alice and Victor can obtain about Bob's site. The only thing that is teleported in quantum teleportation is Victor's *predictions* ... from being about one particle to being about another. But that is not only the larger part of my career, it was the larger part of my interview!! And they just dropped that completely. They played up this "spooky action" stuff, and that is just the opposite of what I had intended.

I live and I learn.

26-03-01 *Home Sweet Home* (to O. Hirota)

I apologize for my delay in sending you word of my safe arrival. Since arriving in New Jersey, most of my time has been spent in trying to wake up, battling a small cold, and becoming reacquainted with my family. I've hardly looked at email at all.

There is no need to thank *me* for *my* involvement in the Tokyo party. I don't deserve that honor. We all owe *you* many thanks for giving us this wonderful opportunity. I had a tremendous time, and was struck once again by the inner strength of the Japanese culture. I also particularly thank you for helping to get me out of trouble my final day there. My daughter became so happy with her "Japanese gifts"; it was an opportunity I would have been ashamed to miss.

To continued progress in quantum information!

26-03-01 *Big Daddy on the Way* (to H. J. Folse)

I arrived back in my office today to find your newest mail to me. Thanks so much for going to that trouble.

In the next email, I'll send you a PostScript file of "The Activating Observer"—the compendium I told you about. Please keep in mind that it is a very tentative version … full of typographical errors, gaps, and other inanities. In particular, the Introduction is still very much under construction. I may well take out some of the nasty things I said about Whitehead for instance.

As of yesterday, I believe I only have about three of your papers to go reading-wise! It's been a very nice experience. In particular, over the weekend, I enjoyed your 1978 paper "Kantian Aspects of Complementarity." In this connection, I have quite a pressing question for you. Do you happen to have the full text of all the letters between Bohr and Pauli on the phrase "detached observer?" (The ones I know of from your paper are dated 15 February, 2 March, and 25 March 1955. But maybe there are more.) If so, have you (or someone) translated those texts into English? When you open my compendium, you'll see why I'm particularly interested in this. Those texts would be an invaluable addition to it.

Thanks again for going to such trouble for me.

26-03-01 *Clifton Proposition* (to N. D. Mermin)

Have you thought about this? [See 02-04-01 note "Present State of Thought" to R. Clifton.] I've thought about combining contributions from Montréal and Växjö as a *small* possibility, but then I was thinking as the target journal our new one on quantum information. But as I say, I was only thinking of this in a small way: most of my thoughts were purely self-serving … namely, using it as a way to extract Schumacher's "Doubting Everett" paper from his head. (Bennett with his Everettista fatigues was really getting on my nerves in Japan last week: there's only so many days I can room with such a person! Ben was my only breath of fresh air.)

David's Reply

> All my life I have conscientiously declined invitations to edit anything and have never regretted it. I'm inclined not to abandon that successful policy at this late stage, but I told Rob Clifton (who incidentally is one of the more solid philosophers of quantum mechanics) that I'd chew it over with you in Växjö this June.
>
> On the other hand a volume that was Montréal+Växjö might be a useful contribution to western civilization. I like the idea of it being in Hist Phil Sci, because it would reach

a bunch of readers who ought to be more interested in this subject than they currently are. (Look at the profound effect you had on Bub.)

I don't think there's any hurry with this.

29-03-01 *French Philosophers* (to J. M. Renes)

Do you remember you once told me about a French philosopher who said something like: neither the subject is real on its own, nor the object is real on its own; only their interface is real. Or some such thing. Can you give me a reference to that?

29-03-01 *There's At Least One More* (to H. J. Folse)

Looking through your book (I picked up a copy from `Powells.com`), I found at least one more letter in that exchange on the "detached observer" that I'm interested in. It's dated 11 March 1955, from Pauli to Bohr.

I sure hope you have copies of these letters in their entirety! (And I keep my fingers crossed that you've translated them.) This is quite exciting to me.

Unfortunately, I'm going to have to slow down on my metaphysical project for the time being. The mundane matters in my life are starting to pile up for the month. Too many papers that desperately need finishing and—more frighteningly for me—I've got to meet with the president of Bell Labs in three weeks. He's asked for a pitch on where we as a company should be going with quantum information. The philosopher meets the executive: that ought to be pretty.

Henry's Reply, "Bohr-Pauli Exchange"

You're quite right that this an interesting exchange between Bohr and Pauli. I suspect that many have ignored them because of the late date.

The letters are in English and typed, but at least on one Pauli inserted several comments in handwriting. Since it's about 15 pages or so, I've photocopied and mailed all four of them to you at your Bell Labs address. The copies are too poor to scan very easily. The Pauli letters are photocopies of the originals; the Bohr letters are photocopies of Bohr's carbon copies.

Hope you enjoy reading them.

W. Pauli, letter to Niels Bohr, dated 15 February 1955, photocopy obtained from the Niels Bohr Institute via Henry Folse.

Dear Bohr,

It is with great pleasure that I received your nice letter and above all, the text of your lecture on "Unity of Knowledge". The general outlook of it is of course the same as mine. Under your great influence it was indeed getting more and more difficult for me to find something on which I have a different opinion than you. To a certain extent I am therefore glad, that eventually I found something: the definition and the use of the expression "detached observer", which appears on page 10 above of your lecture and which reappears on page 13 in connection with biology. According to my own point of view the degree of this "detachment" is gradually lessened in our theoretical explanation of nature and I am expecting further steps in this direction.

1) As you will see in the reprint on my lecture on "probability and physics", which I have sent to you, it seems to me quite appropriate to call the conceptual description of nature in classical physics, which Einstein so emphatically wishes to retain, "the ideal of the detached observer". To put it drastically the observer has according to this ideal to disappear entirely in a discrete manner as hidden spectator, never as actor, nature being left alone in a predetermined course of events, independent of the way in which the phenomena are observed. "Like the moon has a definite position" Einstein said to me last winter, "whether or not we look at the moon, the same must also hold for the atomic objects, as there is no sharp distinction possible between these and macroscopic objects. Observation cannot *create* an element of reality like a position, there must be something contained in the complete description of physical reality which corresponds to the *possibility* of observing a position, already before the observation has been actually made." I hope, that I quoted Einstein correctly; it is always difficult to quote somebody out of memory with whom one does not agree. It is precisely this kind of postulate which I call the ideal of the detached observer.

In quantum mechanics, on the contrary, an observation hic et nunc changes in general the "state" of the observed system in a way not contained in the mathematically formulated *laws*, which only apply to the automatical time dependence of the state of a *closed* system. I think here on the passage to a new phenomenon by observation which is technically taken into account by the so called "reduction of the wave packets." As it is allowed to consider the instruments of observation as a kind of prolongation of the sense organs of the observer, I consider the impredictable change of the state by a single observation—in spite of the objective character of the result of every observation and notwithstanding the statistical laws for the frequencies of repeated observation under equal conditions—to be *an abandonment of the idea of the isolation (detachment) of the observer from the course of physical events outside himself.*

To put it in nontechnical common language one can compare the role of the observer in quantum theory with that of a person, who by its freely chosen experimental arrangements and recordings brings forth a considerable "trouble" in nature, without being able to influence its unpredictable outcome and results which afterwards can be objectively checked by everyone.

Probably you mean by "our position as detached observers" something entirely different than I do, as for me this new relation of the observer to the course of physical events is entirely *identical* with the fact, that our situation as regards objective description in "this field of experience" gave rise to the demand of a renewed revision of the foundation for "the unambiguous use of our elementary concepts", logically expressed by the notion of complementarity.

2) Passing now from physics to other sciences like psychology and particularly biology I am most interested in your approach, which certainly seems to me to go in the right direction. Without entering a discussion of the dependence of such concepts as "[art?]", not only on the state of motion but

also on the psychological attitude of the observer, I am very much looking forward to your article on the organic evolution which you announced in your letter.

In discussions with biologists I met large difficulties when they apply the concept of "natural selection" in a rather wide field, without being able to estimate the probability of the occurrence *in a empirically given time* of just those events, which have been important for the biological evolution. Treating the empirical time scale of the evolution theoretically as infinity they have then an easy game, apparently to avoid the concept of purposiveness. While they pretend to stay in this way completely "scientific" and "rational", they become actually very irrational, particularly because they use the word "chance", not any longer combined with estimations of a mathematically defined probability, in its application to very rare single events more or less synonymous with the old word "miracle". I found for instance *H. J. Müller* very characteristic for this school of biologists (see also his recent article "Life" in *Science*, issue of January 7, 1955, which certainly contains very interesting material), but also our friend Max *Delbrück*. With him this is combined with vehement emotional affects and a permanent thread to run away which I interpret as obvious signs of overcompensated doubts.

You can imagine how much better than "natural selection" sounds for me "natural evolution" which I never heard before from you and which you use now on page 19 of your lecture. I hope that your announced article will tell us more about your use of the latter concept.

Concluding this letter, I add some remarks about your sentence on page 14 concerning the "medical use of psychoanalytical treatment in curing neurosis". I am quite glad about this sentence, as logic is always the weakest spot of all medical therapeuts, who never learned the rigorous logical demands of mathematics.

Historically the word "the unconscious" was used by German philosophers of the last century, particularly by *E. von Hartmann* [also E. G. Carus], developing further older allusions of Leibniz and Kant. The Psycholamarckist *A. Pauly*, on whom we spoke already, quoted von Hartmann in 1905 (Freud was not known to him), when he called processes of biological adaptation, already in plants, an "*unconscious* judgement of the psyche of the organisms". In this way however, only a new name was introduced, which did not explain anything. Freud was the first who made practical applications of the unconscious replacing hereby this word by "subconsciousness", which you also apply. With this change of the word Freud wanted to emphasize that all "contents of the subconsciousness" were earlier in the consciousness and had been suppressed ("verdrängt") afterwards. In this way Freud's subconsciousness was like a bag containing a finite number of objects. The purpose of the psychoanalytical treatment was therefore to make this bag again empty by upheaval of the suppression.

To this restricted concept of subconsciousness among others C. G. Jung is in opposition since about 1913. He reestablished the older word the unconscious of the philosophers emphasizing that every change of consciousness for instance in a medical treatment, backwards also changes the unconscious, which therefore can never be made "empty of contents", only a small part of which has ever been in consciousness. The aim of the medical treatment according to Jung and his school is therefore the establishment of a correct and sound "equilibrium between consciousness and the unconscious", like an equilibrium between two powers. This process in which this equilibrium is reached and reestablished, they also call "the assimilation or interaction of the unconscious to the consciousness".

I only refer here historically a situation without identifying myself with this kind of terminologies, which seem to me rather far from logical clarity. The Jung school is more broad minded than Freud has been, but correspondingly also less clear. Most unsatisfactory seems to me the emotional and vague use of the concept of "Psyche" by Jung, which is not even logically self consistent.

I am very glad about the prospect of a visit in Copenhagen in the autumn of this year, when also your 70th birthday will be celebrated. Francas treatment is not yet finished entirely, but she is much better and there is much hope, that she also will be able to go to Copenhagen this next time.

With all good wishes from both of us to yourself, Margrethe and the whole family,

yours old,

W. Pauli

[PS:] Where your lecture on "Unity of Knowledge" will be printed in case I would like to quote it?

N. Bohr, letter to Wolfgang Pauli, dated 2 March 1955, photocopy obtained from the Niels Bohr Institute via Henry Folse.

Dear Pauli,

On my return from the CERN meeting in Geneva, I am writing to thank you for your letter of February 15th. It was very good of you to write me so carefully about your reaction to my article and, as always, you touch upon a very central point. A phrase like "detached observer" has of course like all words different linguistic and emotional aspects, but using it in connection with the phrase "objective description", taken as theme of the discussion on Unity of Knowledge, it had to me a very definite meaning. In all unambiguous account it is indeed a primary demand that the separation between the observing subject and the objective content of communication is clearly defined and agreed upon. The aim of the article is just to stress that this condition is indispensable in all scientific knowledge, including biology and psychology, while in art as well as in religious belief one allows oneself to neglect or rather tacitly to shift such separation. In this connection, the historical information in your letter about the use of terminology by psychologists was very valuable to me, and I was glad that you on the whole sympathize with my approach. Indeed, contrary to what some of our common friends seem to believe of me, I have always sought scientific inspiration in epistemology rather than mysticism, and how horrifying it may sound, I am at present endeavoring by exactitude as regards logic to leave room for emotions.

It is on this background that it seems to me very important that we fully understand each other in questions of terminology. Of course, one may say that the trend of modern physics is the attention to the observational problem and that just in this respect a way is bridged between physics and other fields of human knowledge and interest. But it appears that what we have really learned in physics is how to eliminate subjective elements in the account of experience, and it is rather this recognition which in turn offers guidance as regards objective description in other fields of science. To my mind, this situation is well described by the phrase 'detached observer', and it seems to me that your reference to our controversy with Einstein is hardly relevant in this connection. Just as Einstein himself has shown how in relativity theory 'the ideal of the detached observer' may be retained by emphasizing that coincidences of events are common to all observers, we have in quantum physics attained the same goal by recognizing that we are always speaking of well defined observations obtained under specified experimental conditions. These conditions can be communicated to everyone who also can convince himself of the factual character of the observations by looking on the permanent marks on the photographic plates. In this respect, it makes no difference that in quantum physics the relationship between the experimental conditions and the observations are of a more general type than in classical physics. I take it for granted that, as regards the fundamental physical problems which fall within the scope of the present quantum mechanical formalism, we have the same view, but I am afraid that we sometimes use a different terminology. Thus, when speaking of the physical interpretation of the formalism, I consider such details of procedure like 'reduction of the wave packets' as integral parts of a consistent scheme conforming with the indivisibility of the phenomenon and the essential irreversibility involved in the very concept of observation. As stressed in the article, it is also in my view very essential that the formalism allows of well defined applications only to closed phenomena, and that in particular the statistical description just in this sense appears as a rational generalization of the strictly deterministic description of classical physics.

I am eager to learn your reaction to these points as I feel that it is essential, not least for the approach to the wider problems on which we are working, to be as precise as possible in terminology, and above all to avoid any vagueness as to the demands of objective description. It was a great joy to learn that we can expect a visit of you and Franca in the autumn and perhaps there is an opportunity of meeting you even earlier, since I am invited to give a talk in Basel at the end of March on the general epistemological problems.

With kindest regards and best wishes to you both from us all,

Yours, in every way, old

W. Pauli, letter to Niels Bohr, dated 11 March 1955, photocopy obtained from the Niels Bohr Institute via Henry Folse.

Dear Bohr,

I find your letter of March 2nd very youthfull, which is just the reason that it is not easy for me to answer. Although we have the same view "as regards the fundamental physical problems which fall within the scope of the present quantum mechanical formalism" and although I agree with some parts of your letter, the situation is now complicated by your use in a publication of a phrase like "detached observer" (without comment!) which I used already in some publications in a very different way. I believe that this should be better avoided to prevent a confusion of the readers[5] and I don't cling at all to particular words myself. I also felt, already before your letter arrived, that my brief characterisation of the observer in quantum theory as "non-detached" is in one important respect misleading. As is well known to both of us, it is essential in quantum mechanics that the apparatus can be described by classical concepts. Therefore the observer is always entirely detached to the *results* of his observations (marks on photographic plates etc.), just as he is in classical physics. I called him, however in quantum physics "non-detached", when he chooses his experimental arrangements.[6]

I shall try to make my point logically clear, by defining my concepts, replacing hereby the disputed phrase by other words. As I was mostly interested in the question, *how much informative reference to the observer an objective description contains*, I am emphasizing that a communication contains in general *informations on the observing subject.*

Without particularly discussing the separation between a subject and the informations about subjects (given by themself or by other persons), which can occur as elements of an "objective description", I introduced a concept "degree of detachment of the observer" in a scientific theory to be judged on the kind and measure of informative reference to the observer, which this description contains. For the objective character of this description it is of course sufficient, that every individual observer can be replaced by every other one which fullfills the same conditions and obeys the same rules. In this sense I call a referency to experimental conditions an "information on the observer" (though an impersonal one), and the establishment of an experimental arrangement fulfilling specified conditions an "action of the observer"—of course not of an individual observer but of "the observer" in general.

In physics I speak of a detached observer in a general conceptual description or explanation only then, *if it does not contain any explicit reference to the actions or the knowledge of the observer.* The ideal, that this should be so, I call now "the ideal (E)" in honor of Einstein. Historically it has its origin in celestial mechanics.

There is an important *agreement* between us that we find Einstein not consequent in this formulation of the "ideal E". Indeed, there is no a priori reason whatsoever to introduce here a difference between the *motion* of the observer on the one hand, and the realization of specified experimental conditions by the observer on the other hand. If Einstein were consequent he had to "forbid" also the word coordinate system in physics (as not being objective). That the situation in quantum mechanics has a deep similarity with the situation in relativity is already shown by the application of mathematical groups of transformation in the physical laws in both cases.

In this way I reached the conclusion to distinguish sharply between the "ideal of an objective description" (meaning science) on the one hand (which

[5] An explaining remark about it in your *new* article would be most welcome!

[6] I still believe today that this more restricted use of my terminology is very good and that it has been unhappily obscured in your article in a non-logical way!

I warmly supported just as you do) and the "ideal of the detached observer" on the other hand (which I rejected as much too narrow).

What really matters for me is not the word "detached", but the more active role of the observer in quantum physics, which is already implied in your [consideration?] of the "indivisibility of the phenomena and the essential irreversibility involved in the very concept of observation". According to quantum physics the observer has indeed a new relation to the physical events around him in comparison with the classical observer, who is merely a spectator: The experimental arrangement freely chosen by the observer lets appear *single* events *not* determined by laws, the ensembles of which are governed by *statistical* laws.[7] It is not relevant to me, if you say the same thing using *different* terminologies (but please use [essentially?] different words than I). They will only confirm my statements again as all these statements on the observer are part of an "objective description".

I confess, that very different from you, I do find sometimes scientific inspiration in mysticism[8] (if you believe that I am in danger, please let me know), but this is counterbalanced by an *immediate* sense for mathematics. The result of both seems to be my kind of physics, whilst I consider epistemology merely as a logical comment to the application of mathematics in physics.[9] Thus when I read a sentence as "how to eliminate subjective elements in the account of experience" my immediate association is "group theory" which then determines my whole reaction to your letter. Although the first step to "objectivity" is sometimes a kind of "separation", this task excites in myself the vivid picture of a superior common order to which all subjects are subjected, mathematically represented by the "laws of transformations" as the key of the "map", of which all subjects are "elements".

I hope that it will be possible to find a terminology which will turn out to be satisfactory for both of us, but it is no hurry with it. I propose to resume this discussion only when your new article will be ready, which I am eagerly awaiting. It will show me your terminologies in more general cases of objective descriptions, of which I am most interested in the application to biology, in connection with your new expression "natural evolution".

From March 16th till about 27th I am away in Germany and Holland and when I come back I hope either to see you or to hear from you (I wrote to Basel to get informations on your lecture there).[10]

Hoping that you will in the future (just as I do myself) enjoy the enrichment coming from the different kind of access to science by different scientists, expressed in different, but not contradicting terminologies, I am sending, also in the name of Franca, all good wishes to yourself, to Margrethe and to the whole family,

[7]In this way we obtain just the logical foundations of an "*objective* description" of the incidents (Ein begriffe) which the quantum mechanical observer makes within his surroundings with his experimental arrangements. Attention: there is *no* logical contradiction between a word like "trouble" and a possibility of its objective observation and description.

[8]By the way: the "Unity" of everything has always been one of the most prominent ideas of all mystics.

[9]We are here *both* in our letter in a realm of information on the writing subject, which do *not* belong to the "objective content of the communications".

[10]Meanwhile I heard from P. Huber in Basel, [Fierz is in the United States], that your lecture there is on March 30. On this date I am very glad, because I shall be back from my trip by then. Paa Gensje!

as yours complementary old

W. Pauli

N. Bohr, letter to Wolfgang Pauli, dated 25 March 1955, photocopy obtained from the Niels Bohr Institute via Henry Folse.

Dear Pauli,

Thanks for your letter of which I was glad to see that even if I am old you do not feel that I am yet so petrified that we cannot have such animated and fruitful discussions as in our younger days. You are certainly right that, as regards many personal utterances, in my letters like in yours, we are not detached onlookers, although of course we have so much in common that it is a pure discussional accident which words, like mysticism or logical systematism, the one or other of us uses for mutual educational purposes. I also read with great pleasure your beautiful Columbia radiolecture which I had not seen before. Of course, I appreciate the background for your use of the phrase "detached observer" on that occasion, but in my article I was using the phrase in a more generalized (or, if you prefer, more limited) sense suited to point to the characteristics of our position in science and art.

To characterize scientific pursuit I did not know any better word than detachment, especially in connection with psychological studies. As regards quantum mechanics and biology, I wanted to stress the difficulties which even in these fields have had to be overcome to reach the detachment required for objective description or rather for the recognition that in such field we meet with no special observational problem beyond the situations of practical life to cope with which the word "observer" has been originally introduced. To my mind, the lesson was merely that continued exploration of the regularities of nature only gradually should teach us of the necessary caution in looking for unambiguously communicable experience. As you, I am of course prepared to change terminology when it is clear that this will promote common understanding, but before any of us decides on such steps, I wish to call your attention to the pure scientific aims I have perhaps not sufficiently clearly presented in my article.

To make myself more clear, I may for a moment remind of the days of socalled "classical" physics and "critical" philosophy, when in the description of the course of events the role of the tools of observation was disregarded and spacetime coordination and causality were considered a priori categories. You are certainly right that Einstein is not consequent when speaking of the ideal of detached observer and neglecting his own wisdom of relativity, which Eddington poetically described by the picture of how long man traced a footprint in the sand until he recognized that it was his own. Seriously, I mean that you are yourself as inconsequent in stressing the difference in such respect between classical and quantum mechanics. It is true that, before the epistemological aspects of the observational problem were so widely cleared up, a certain confusion was prevalent, but after the thorough lesson which we have received, the whole situation including that of classical mechanics appears in a new light. Though in a vast field of experience one could neglect the interaction between what was regarded as separate objects of investigation and tools of observation, one often overlooked our reach of interfering with the course of events through our freedom of choosing the experimental arrangement. Indeed, in those days, relying upon the deterministic and reversible character of the mechanical description, one might rather have thought that such influencing within a large scope was possible in unlimited detail.

On the basis of the recognition of the limited divisibility of elementary phenomena we have, however, obtained a more generalized description embracing new fundamental regularities of nature, the orderly comprehension of which in principle implies statistical account even as regards reversibility, and which for the exhaustion of knowledge demands mutually exclusive experimental arrangements. The point which I especially wanted to stress in the article is that, just by avoiding any such reference to a subjective interference which would call for misleading comparison with classical approach, we have within a large scope fulfilled all requirements of an objective description of experience obtainable under specified experimental conditions. The freedom of the choice of the experimental arrangement is indeed common to classical and quantum physics and, considering all aspects of the situation, we may say that in both cases a sharp separation between the "observer" and the "phenomena" is retained. The difference is only that in quantum phenomena we have for their definition to include the description of the whole experimental arrangement and that we have less possibility of influencing the course of events.

Still, if the study of natural phenomena were exhausted by simple experience, one might not take questions of terminology too serious, at any rate within the scope in which order is already obtained. I want, however, to challenge you whether you really mean that, in the description of proper biological phenomena, we have to do with an even greater interference with events on the part of the observer than that you want to stress in quantum mechanics. To my mind, the situation is entirely opposite, since the characteristics of our position in biological studies is just the impossibility without excluding the display of life to arrange the experimental conditions required for well defined mechanistic description. It is of course true that physiological research just consists in studying the reactions of the organisms to experimental conditions open to our choice, but it appears to me to be practical as well as rational to include such reactions under varied specified conditions in an exhaustive account of organic life. A further point which in this connection is on my mind is to stress that, in the description of the characteristic properties of the organism, reversal of events is logically excluded, and just this circumstance is of course of fundamental importance for speaking of "natural evolution".

I do not know if I in any such respect was able to make the essential points sufficiently clear in my article, but I am glad that, quite apart from our present dispute, you were not unsympathetic with my striving for a unified attitude to the scientific description of that nature to which we belong and in the exploration of which we step by step have been reminded of fundamental general aspects of our position as observers, which only in limited fields may be disregarded. I shall here not go further in repeating things which are not new to any of us and leave the battle about the word "detached" to our meeting in Basel in preparation of which I only wanted to remind of our resources for defence as well as attack, irrespective of the word "old".

With kindest greetings from home to home,
and på gensyn,
Yours ever

02-04-01 *Present State of Thought* (to R. Clifton)

Good to finally meet you. (Though I think I've had some email with one of the students in your seminar.) Sorry to reply so late to your note, but I wanted to hear Mermin's thoughts first. I'll just paste in the exchange we had below: The conjunction of the two notes captures my present state of thought pretty well. Mermin, I see, does have a good point. And since I do already have promises for three papers (beside one that I could write), it might be worthwhile running in this direction. We'll let you know something at the end of June, maybe just at the start of your new tenure.

Rob's Preply

> My name is Rob Clifton and, as of July 1st, I will be the new chief editor of the journal *Studies in History and Philosophy of Modern Physics*. Jeremy Butterfield, my predecessor, has told me that you might be interested in guest editing a special issue of the journal devoted to the conceptual implications of quantum information theory, computation theory or both. To my mind, this is an excellent idea. Are you still interested? I've also mentioned the idea to David Mermin, with the thought that you and he could perhaps guest edit the issue together. Might that work?

07-04-01 *Operational Approaches* (to R. W. Spekkens)

Spekkensism 1: *I've been trying to become better acquainted with quantum information theory. For this and other reasons, I became interested in determining how the axioms of quantum mechanics appear in an* entirely *operational language. I noticed that most attempts at axiomatization (for instance those found in the standard textbooks) commit themselves to some degree of realism, assigning dynamical variables to particles, etc. Only Peres' book seems to be entirely operational in its approach. Are there other places where one can find a strictly operational axiomatization of quantum mechanics?*

You can also look at the books by Karl Kraus and Günther Ludwig. But I don't think their efforts are very convincing (though complex they certainly are). Still another source might be the book by Paul Busch and coauthors; I think it's called *Operational Quantum Mechanics*.

Spekkensism 2: *The impression I now have is that the task of deriving the axioms of quantum mechanics from a few physical principles is a task that one can hope to carry out entirely within an operational approach to the theory, the physical principles being operational principles. So it seems to me that this project can proceed pretty much independently of whether one can find a satisfactory realist interpretation. Is this in line with your thinking?*

Somewhat. You can find much more about my foundational thoughts (though in a somewhat nonorganized form) at my website: `http://www.its.caltech.edu/~cfuchs/lwl.htm`.

Spekkensism 3: *Anyhow, it's fascinating stuff. Is there any chance you'd be willing to come to Toronto to give a talk on this or other research?*

I'm pretty much tied up until the Fall, but then I should have plenty of time to travel again. So I'd love to come to Toronto possibly in that time frame. Let's keep in touch on this.

10-04-01 *Rudolph/Sanders/Teleportation* (to H. J. Kimble & H. Mabuchi)

If you look at `quant-ph` today, you'll find a new little production by Steven and me—`quant-ph/0104036`—that we think pretty much clears up the trouble brought about by Rudolph and Sanders. More than that, we think it clears up quite a few other things as well: namely, how laser light can be used most generally as a source for quantum information experiments. The point is a simple one and a basic one: There is a useful difference between thinking about the quantum state on the inside of the laser cavity and the quantum state on the outside. So, in a way, we're hoping the paper also gets attention outside of the circle of teleportation defenders—it's almost the kind of thing that ought to be in the classroom (if we say so ourselves).

In the end, Rudolph and Sanders dissolved most easily: We were still quite confused the last time we talked to you (though we didn't feel it then). Laser light can be used to generate continuous-variable entanglement after all (and you certainly did it in the Furusawa et al. experiment), it's just in the form of *distillable* entanglement. It's much better than "entanglement of assistance" in the sense that you don't even have to recover the laser cavity to recover the entanglement. And even better than that, you don't even have to distill it to do continuous-variable quantum teleportation! So everything appears to be safe with those aspects of the experiment.

I had previously mentioned to you the possibility of writing a comment to PRL, following an earlier suggestion by Hideo concerning the same but for *Physics Today*. In light of the almost trivial solution in the end, that possibility is starting to fade from my mind. But maybe I'm still open to entertaining some thoughts—I'm not ready to shut the door completely yet. Tell me what you think of our new paper, and I might revise my disposition accordingly. We'll see, but right now, like I say, I'm starting to get disinclined to the idea. (I'm not sure what Steven's thoughts are.)

Anyway, that's it. Wishing you both the best in sunny Southern Cal,

15-04-01 P_E (to A. Peres)

Asherism 4: *Please give me a reference for the formula for* P_E *in terms of* $\mathrm{tr}(\rho_1 - \rho_2)$.

You meant in terms of $\mathrm{tr}|\rho_1 - \rho_2|$, of course. The reference is:

- C. A. Fuchs, "Information Gain vs. State Disturbance in Quantum Theory," *Fortschritte der Physik* **46**(4,5), 535–565 (1998). [Reprinted in *Quantum Computation: Where Do We Want to Go Tomorrow?*, edited by S. L. Braunstein (Wiley–VCH Verlag, Weinheim, 1999), pages 229–259.]

The paper is listed on quant-ph as `9611010`. I had originally submitted it for the proceedings of PhysComp96 in *Physica D*. After waiting a ridiculously long time with no sign of movement from the editors, I finally withdrew it and submitted it to the special issue of *Fortschritte*.

Kiki, Emma and I are in Texas for the Easter holidays. I believe I had forgotten the meaning of the words humid, muggy, and sultry!! They've come back to me now with a force that won't be forgotten!

18-04-01 *Op-Ed* (to A. Peres)

I'll paste the entry from my dictionary below on the word op-ed. (It's a common term in America.)

I'm glad that all is well with your family. Kiki, Emma, and I are just back from a week in Texas. I finally submitted the old quantum de Finetti paper to `quant-ph`—it only took me two years! It will appear tomorrow morning. You may have an interest in it; it is one of my better pieces of work (conceptual *and* technical). Petra has already expressed an interest in it. You might get her to give you a report on it. van Enk and I also gave a technical application of the result in `quant-ph/0104036`, "The Quantum State of a Laser Field." (It was nice to see that it could be used in the real world of quantum information bickers!) That may interest you as well.

19-04-01 *Warsaw Wait* (to R. Jozsa)

Jozsa-ism 1: *Anyhow it'll be great to catch up in Warsaw. Have you seen the amazing papers of Koashi/Imoto recently on* `quant-ph`*? Certainly among the best papers for some years!!...*

Yes, I have ... and I agree with you completely. (I wrote Koashi telling him the same.)

I'll come to Warsaw if I can. The main issue is funding. Despite the pre-history of big bucks at Bell Labs, Lucent has fallen on hard times—you warned me!—and there are no travel funds for this year. I've been put on an "expenses-paid invitation only" alert. So I hope I can come. I'll talk about my stuff with Sasaki if I do. (Now I've just got to finished the damned paper for him! I'm a week late.)

Charlie's talk on "time travel" was less interesting than you might think. I took grave issue with it, and didn't like it. (But I bet that makes you want to hear about it even more now.)

Have fun in Japan. I'm glad you're finally getting to see the cherry blossoms.

19-04-01 *CVs and Samizdats* (to A. Peres)

Asherism 5: *Meanwhile it seems to me that I found a method for secure quantum bit commitment. It is so simple that I can't see what can go wrong.*

That is always dangerous. Have you tried very hard to apply the standard Mayers-Lo-Chau attack on it? Steven van Enk and I were able to break the "definitive" QBC protocol of Horace Yuen within about 5 minutes of discussion in just that way.

Asherism 6: *Aviva and I plan to be at ITP UCSB from Aug 13 till Sept 29. Shall we have the pleasure of seeing you?*

No, I will not be there. They didn't accept my application because I listed a time window of less than four weeks availability.

My mother-in-law leaves today to go back to Germany. She has been at our house since April 4 visiting. Also though, while we were away in Texas, she completed painting the walls downstairs and up the stairwell. I'm very thankful for all that she's done, but having extra duties of conversation have been time consuming for me. Now that she's gone, I intend to work like gangbusters to get at least one more paper on the web before April is out (a paper by Sasaki and me). Also I intend to post my big quantum samizdat on May 10; Mermin is writing a foreword for it presently. Getting it together is an exhausting and painstaking affair, making sure that there are no places where I put my foot in my mouth (where I don't want to, that is). It is 300+ pages at this stage. I will send a copy to you and a few other friends pretty soon asking for permission to quote you in a small number of places. I think no one need worry though: I have worked very hard to make it safe and tasteful.

19-04-01 *CVs and Samizdats, 2* (to A. Peres)

Asherism 7: *I have a paper by Chau and Lo on my desk, another on the table near it, and also one by Brassard et al. I have some difficulty understanding them but eventually I'll find out.*

Maybe the best place to look for an easy understanding of this idea is Jeff Bub's paper. I'll put the abstract below. He really tried to be very didactic in that paper.

I'll tell you more about the samizdat in a couple of days.

> `quant-ph/0007090`
> The quantum bit commitment theorem
> Authors: Jeffrey Bub
> Comments: LaTeX, 25 pages. Forthcoming in *Foundations of Physics*, May 2001
>
> Unconditionally secure two-party bit commitment based solely on the principles of quantum mechanics (without exploiting special relativistic signalling constraints, or principles of general relativity or thermodynamics) has been shown to be impossible, but the claim is repeatedly challenged. The quantum bit commitment theorem is reviewed here and the central conceptual point, that an 'Einstein-Podolsky-Rosen' attack or cheating strategy can always be applied, is clarified. The question of whether following such a cheating strategy can ever be disadvantageous to the cheater is considered and answered in the negative. There is, indeed, no loophole in the theorem.

19-04-01 *Quantum de Finetti* (to D. Petz and Other Friends)

Dear friends who I recall having shown some interest in quantum de Finetti issues:

My coauthors and I have finally gotten off our lazy duffs and written up our work of two years ago on a quantum analog of de Finetti's theorem on exchangeable probability assignments. The paper appeared on the Los Alamos Archive today:

`http://xxx.lanl.gov/abs/quant-ph/0104088`.

Apologies to all for the long delay,

> Title: Unknown Quantum States: The Quantum de Finetti Representation
> Authors: Carlton M. Caves, Christopher A. Fuchs, Rüdiger Schack
> Comments: 30 pages, 2 figures
>
> Abstract:
> We present an elementary proof of the quantum de Finetti representation theorem, a

quantum analogue of de Finetti's classical theorem on exchangeable probability assignments. This contrasts with the original proof of Hudson and Moody [Z. Wahrschein. verw. Geb. 33, 343 (1976)], which relies on advanced mathematics and does not share the same potential for generalization. The classical de Finetti theorem provides an operational definition of the concept of an unknown probability in Bayesian probability theory, where probabilities are taken to be degrees of belief instead of objective states of nature. The quantum de Finetti theorem, in a closely analogous fashion, deals with exchangeable density-operator assignments and provides an operational definition of the concept of an "unknown quantum state" in quantum-state tomography. This result is especially important for information-based interpretations of quantum mechanics, where quantum states, like probabilities, are taken to be states of knowledge rather than states of nature. We further demonstrate that the theorem fails for real Hilbert spaces and discuss the significance of this point.

22-04-01 *An Apple and a Commitment* (to A. Peres)

I printed out your paper and quickly skimmed it while I was eating my apple for lunch. Forget about the cryptic details of cryptology, your paper is in fundamental contradiction to the HJW result. I'm sorry, but I think that squashes your protocol. Read below, and then look up the paper, and I think you'll understand.

I am really sorry about this.

Back to my other project, the samizdat. I'll write you more details about that in a few days. And thanks for getting me an invitation to Israel Dec 17–21 (or at least I suspect you were behind it).

Review of:
Lane P. Hughston, Richard Jozsa, and William K. Wootters,
"A Complete Classification of Quantum Ensembles Having a Given Density Matrix,"
Physics Letters A, Vol. 183, pp. 14–18, 1993.

Abner Shimony likes to say that entanglement gives rise to "passion at a distance." He does this because when Alice performs a measurement on A of an entangled system AB, *something* changes for B, BUT that change cannot be used for the purpose of communication with a Bob at B. If you ask me, this is language that is just asking for trouble; it is language that is poised to confuse a generation of new physicists. Something indeed does change for B, what Alice can *say* of it. But it is nothing more than that; to think that it is truly a physical change with respect to B alone—especially one that is so contrived as to not lead to communicability—is to open up a sink hole. We are dealing here with changes of states of knowledge.

Within this context, it is quite reasonable and quite interesting to ask how many different ways Alice's knowledge can change. Depending upon which measurement Alice wishes to perform on A, there will be any of a number of different state assignments for B that follow from that. What are they, and what are their probabilities? This is the main question addressed in this little paper. It has a very clean answer: a state assignment can be created for B from a measurement on A if and only if that state falls within a pure-state decomposition of the marginal density operator of B. Moreover, the probabilities of Alice's measurement outcomes correspond precisely to the probabilities of the ultimate state assignments.

This result, it turns out, is not of purely academic interest. It has had a wide range of application in several more applied problems in quantum information: it is crucial for the proof that no quantum bit commitment schemes exist, it plays a crucial role in proving the exact expression for the entanglement of formation for two qubits, and it is crucial for defining the notion of the entanglement of assistance. This theorem is one that all quantum information theorists should have incorporated into their tool bag.

Can more be said? Actually, it turns out that this result even has some historical significance. For, unknown to the authors above, the same question was raised and even partially answered by Erwin Schrödinger in a 1936 paper! [E. Schroedinger, "Probability Relations between Separated Systems," Proc. Cam. Phil. Soc. 32, 446–452 (1936).]

24-04-01 *Endo, Exo* (to A. Peres)

Asherism 8: *You are an inexhaustible source of references. The terms "exophysical" and "endophysical" were coined by Primas (I believe). Do you know when and where? Or could you find Primas's email (he is at ETH in Zurich, I know nobody there). I'll need these terms when my current research with Danny will bear fruit. This may take a long time, at least several days.*

You flatter me. Actually I think David Finkelstein first invented the words. The story of that is written down somewhere. I think it is probably in:

- H. Primas, "Endo- and Exo-Theories of Matter," in *Inside Versus Outside: Endo- and Exo-Concepts of Observation and Knowledge in Physics, Philosophy and Cognitive Science*, edited by H. Atmanspacher and G. J. Dalenoort (Springer-Verlag, Berlin, 1994), pp. 163–193.

At times like this, I dearly miss my old paper collection. I had thousands of papers copied, and I could just turn to my file cabinet and find the answer.

My memory is awfully weak on this, but I think there is even a letter of Finkelstein's published somewhere. If it's not in the volume above, try

- O. E. Rössler, *Endophysics: The World as an Interface*, (World Scientific, Singapore, 1998).

Of this reference, I had written a note to myself: "This is a strange little book, full of crazy ideas, but still perhaps some are worth further scrutiny."

As I recall, though Primas uses the same words as Finkelstein, he had opted to do something odd with their meanings in relation to Finkelstein's. (I think he may have even *reversed* their meanings!)

I hope that helps.

26-04-01 *Short Replies* (to A. Peres)

Asherism 9: *Indeed I found in the proceedings of the 1990 Joensuu conference Primas's talk, where he refers to a 1983 paper by the Finkelsteins. I also found the talk of Schroeck, where he uses the acronym POVM.*

I procrastinated during my lunch hour the other day (lately I've been taking no lunches at all), and dug up the Primas article I told you about. He gives a full history of the terms there and their variations of usage: In particular, his is different from Finkelstein's. The references he gives are, first Rössler,

- O. E. Rössler, "Endophysics," in *Real Brains, Artificial Minds*, edited by J. L. Casti and A. Karlqvist (North-Holland, NY, 1987), pp. 25–46.

then Finkelstein,

- D. Finkelstein, "Finite Physics," in *The Universal Turing Machine. A Half-Century Survey*, edited by R. Herken (Kammerer & Unverzagt, Hamburg, 1988), pp. 349–376.
- D. Finkelstein, "The Universal Quantum," in *The World of Contemporary Physics*, edited by R. F. Kitchener (State University of New York Press, Albany, 1988), pp. 75–89.

Primas, in fact, writes: "A pertinent distinction referring to internal vs. external viewpoints has been introduced by Otto Rössler and David Finkelstein under the notions 'endophysics' and 'exophysics', respectively." And he gives the references above.

26-04-01 *Mysticism and Logic* (to N. D. Mermin)

Merminition 2: *You never replied to my comment on it: that you were getting close to Schrödingerian mysticism. I suspect the only difference (if it is a difference) is that you say the observer is in the world while Schrödinger — this is a cartoon version of what he says but is hard to resist in the current context — says that the world is in the observer. Both of you say (with Pauli) that they cannot be separated.*

But there is a big difference: Schrödinger's view was not contingent on quantum mechanics. He said the same things before and after its construction.

... Though I guess I've heard the same accused of Bohr.

26-04-01 *The Anti-Deutsch* (to R. Pike)

Remind me again of the PR guy I'm supposed to talk to before interviewing and such. I keep forgetting his name.

Discover magazine wants me to insult the many-worlds interpretation of quantum mechanics next week, and I'm more than happy to do it. (What that really means is that they'll want to make Deutsch the shining good guy, and me the bad guy. I'm sure it'll turn out that way, but somehow, someway, someone has got to say that this view is nonsense.)

26-04-01 *Your Interview Request* (to T. Folger)

Yes, I should be able to talk to you Monday or Tuesday of next week. (Tuesday is preferred, but even Wednesday is fine.) As Asher suggested, I too would suggest that you read our articles before we talk. I'll send them both to you now to make that more convenient. I would especially encourage you to read the smaller "reply to critics" article [Physics Today **53**(9), pp. 14 and 90 (2000)] (after reading the original) in preparation for the thing you say below:

Folgerism 1: *More specifically, I hope to discuss how most physicists take a very utilitarian attitude towards quantum theory, and tend to avoid considering what the theory might have to tell us about the fundamental nature of reality.*

From your choice of words in this, you hint very much that you've already talked to David Deutsch. He seems to have it in his head that there is only *one* way to extract a scientific point of view of Nature out of quantum theory: namely, his way (a many-worlds view). But I find that view essentially contentless, whether he wants to call it scientific or not.

You will not find me saying that quantum mechanics teaches us nothing about the nature of Nature. Just the opposite: I would say it has taught us a lot, and there is still a load more to learn (just by contemplating quantum mechanics alone). But we will never see the greatest things the theory has to offer if we first shut our eyes to its greatest lesson: That is, that the terms in the theory are not about a *free-standing* reality. Rather they are concerned with our interface with the world. We are part of Nature—an inextricable part when it comes to the constructions of our descriptions of it—and that has to be reckoned with. That is the great lesson of the quantum.

If I had one thing to do over again in the original article with Asher, I would have inserted the words "free-standing" in front of every instance of the word "reality" to make that absolutely clear. I would also have done it in an effort to keep people like Deutsch from taking it out of context.

26-04-01 *Tuesday Afternoon* (to T. Folger)

Tuesday afternoon will be fine.

Rereading the last note I sent to you, I realized that you wouldn't be able to have a clue what the last sentence of it was about:

> I would also have done it in an effort to keep people like Deutsch from taking it out of context.

The "it" in this refers the following exchange, which I recently inserted into a big thing I'm writing up on the quantum foundations. Let me paste that in for you:

> AUTHOR'S NOTE: In my *Physics Today* article "Quantum Theory Needs No 'Interpretation'" with Asher Peres, there is a passage that says the following:
>
>> Contrary to those desires, quantum theory does *not* describe physical reality. What it does is provide an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions. ...
>
> This passage (usually taken out of context) has been found distasteful by several readers. At least one of those readers was Alvaro Carvalho, and at least one reader of Carvalho was the iconic many-worlds activist David Deutsch.[11] I know this because Michael Nielsen forwarded to me Carvalho's posting of 12 July 2000 and Deutsch's reply of 16 July 2000 from the electronic bulletin board `Fabric-of-Reality@egroups.com`. Carvalho writes:
>
>> For those who have not read the Letter: "Quantum theory needs no 'interpretation'" by C. Fuchs and Asher Peres (Physics Today -March 2000), here are some short excerpts (with comments...): [...]
>>
>> "Contrary to those desires, quantum theory does not describe reality ..." I wonder what it can possibly describe. Is there anything else beyond reality?
>
> While Deutsch responds:

[11] It probably goes without saying, but perhaps I should have said "many-worlds activist and Star Trek fan." See `http://www.qubit.org/people/david/David.html` for details.

> No, but that's not what they think. They think it describes our observations, but that we are not entitled to regard this as telling us anything about a reality beyond our observations. Why? Just for the Bohring old reason that they don't like the look of the reality that it would describe, if it did describe reality. Why? – I have many speculations, but basically I don't know. I don't understand why.
>
> It's sad enough when cranks churn out this tawdry old excuse for refusing to contemplate the implications of science, but when highly competent physicists – quantum physicists – dust it off and proudly repeat it, it's a crying shame.

In bemusement, I forwarded these quotes to David Mermin, who responded with:

Merminition 3 : *Funny, there was this English Bayesian, Tony Garrett, who said more or less the same thing about anybody who had given up the search for hidden variables. And I suppose there's a sense in which many worlds, insofar as it can be made coherent, is the ultimate hidden variables theory.*

This comment spurred the following note on my part . . .

26-04-01 *The Itch, the Burn, the Fire* (to N. D. Mermin)

Oh, how I itch to put these in the samizdat too . . . But I won't do it! A man is nothing if he can't control his itches. (And besides, enough is enough.) [See 26-04-01 notes "Your Interview Request" and "Tuesday Afternoon" to T. Folger.]

26-04-01 *Memory Lane* (to G. Brassard)

I just took a walk down Memory Lane and I'm wondering how you're doing: I just skimmed all 214 notes I've written you since 8 May 1997! "Why?," you must be asking. I'm putting together compendium of all my q-foundational emails, and I'm leaving no stone unturned. You ought to see this book—it's really turning into something. Mermin is writing a Foreword for it, and I'll go public with it pretty soon. (But I'll be sending you a preliminary draft of it before then.)

I *do* hope you'll come to Växjö. People are really taking this idea seriously that you and I have promoted. I think you'll be quite pleased with what you get to see, hear, think about, and take part in if you come. The quantum is waiting to be explained (and extended)! And I think our ideas really are on the right track.

So, come. I'll bring a little single-malt for us.

27-04-01 *You Say Relate-a, I Say Relata* (to N. D. Mermin)

Did you ever run across anyone else using the word "correlata?" The excerpt of a note below to Howard Barnum shows that we were once worried about that.

In the mean time, I have seen old papers by Henry Folse where he essentially says that Bohr's point of view is "relation without relata" . . . and he really does use the word relata. I've been meaning to tell you about that.

I'll dig up some relevant Folse quotes and send them to you in the next email.

27-04-01 *Folse Quotes with "Relata"* **(to N. D. Mermin)**

From: H. J. Folse, "The Copenhagen Interpretation of Quantum Theory and Whitehead's Philosophy of Organism," Tulane Stud. Phil. **23**, 32–47 (1974)—

> While Whitehead and classical physics are in sympathy with regard to experience as the starting point for natural philosophy, there is a dramatic difference with respect to the manner in which the two define "nature." In the materialistic world-view of classical physics, nature is the *object* that *causes* our experiences; what we experience are observables correlated with the primary and secondary properties of material substances that exist without the aid of experience. For Whitehead, however, "nature is that which we observe in perception through the senses." Nature is not the cause of experience; it is the field of experience itself. On this point Whitehead's philosophy insists: we are not concerned with discovering the concrete object which is the objective cause of experience, the nature of which is known in abstract concepts; we are instead concerned with explicating the origins of our abstractions by reference to the concrete factors nature revealed an experience. Thus Whitehead claims that
>
> > we have nothing to do with the ultimate character of reality. It is quite possible that in the true philosophy of reality there are only individual substances with attributes, or that there are only relations with pairs of relata .... Our theme is Nature ... we confine ourselves to the factors posited in the sense-awareness of nature ....
>
> Against the traditional view of science, the Copenhagen Interpretation finds complete agreement with Whitehead's intention:
>
> > As a final consequence, the natural laws formulated mathematically in quantum theory no longer deal with the elementary particles themselves but with our knowledge of them ... we can no longer view "in themselves" the building blocks of matter which were originally thought of as the last objective reality ... basically we can only make our knowledge of these particles the object of science.
>
> It is important to note that when Heisenberg refers to "knowledge" he is not calling upon a conceptual abstraction but rather the experiences of investigators in their experiments. Since what is involved in this agreement between Whitehead and quantum theory is a redefinition of the concept of "nature" and since physics seeks to "explain" nature, it is not surprising that Bohr should call for "a reconsideration of our attitude towards the problem of physical explanation." Such a reconsideration is precisely the goal of Whitehead's philosophy.

and

> In undertaking a metaphysics which would take the professed empiricism of science seriously and yet offer a conceptual elaboration of nature, Whitehead offers us an ontology of "events" as the real termini of our experiences in sense-awareness. Each event is an "actual entity" which is a concresence of prehensions of all previous events. In a "prehension" the prehending event brings within itself with a certain determinate "subjective form" the prehended event as an objective datum entering into the process that makes up the life of the prehending event. In this process the prehending event

"becomes" while the prehended event achieves "objective immortality" and "perishes." There is no hylomorphic structure to the actual entities of this ontology; there is not "something that endures," material substance, and something which the enduring thing "has," properties, that can change. What is, is events, and events do not endure, they happen. In a prehension an event reaches out to "feel" other events; thus an actual entity "acts." Activity, not endurance, is the basic ontological status of entities in the philosophy of organism.

Since the actual entity has no hylomorphic structure, what is experienced is not properties standing for the object experienced, but rather the actual entity itself. Experience does not reveal nature mediately, but immediately. The task of science is not to explain nature through an appeal to a conceptual representation of it, but rather to explain a conceptual representation through an appeal to experience. In this way the philosophy of organism is a "critique of abstractions" and the fallacy of misplaced concreteness has been avoided.

and

If we endorse the materialistic ontology, then Bohr's position is totally unacceptable, but on the Whiteheadian view his ideas follow quite naturally. The common point, as has been stressed, is that both positions are interested in a reformulation of the doctrine of physical explanation by an appeal to direct experience. However, where Whitehead analyzes experience in general, Bohr, as a physicist, is interested in only a small set of experiences, namely those observations in which a measurement of a microsystem takes place. Whitehead protested against the idea of experience as experience of an impassive object "out there" related at each precise instance in time to a subject that in no way reached out to modify the object experienced. Since at each instance the subject is allegedly related to the object in a perfectly determinate way, there is no becoming *within* an experience; becoming can only be the succession of experiences, one following, instant after instance in a continuum, upon the other. If nothing becomes within the instant, nothing happens; there is no activity in experience. Experience, on this view, is quite literally, a point instant in time in which nothing happens. Against this notion Whitehead's process view holds that the essence of an experience is the *activity* of the prehending or experiencing entity.

This crucial role of activity in experience is the cornerstone for the compatibility between Bohr's Copenhagen position and Whitehead's view. While Whitehead appeals to the logical absurdity of the materialistic doctrine, Bohr appeals to a physically confirmed theoretical assumption, namely, the quantization of action. The introduction of the quantum of action into physical theory requires that in a measurement the system being measured and the system performing the measurement share for finite period of time at least one indivisible quantum of action; in other words, that they are in *interaction*. In speaking of "isolated systems" classical physics used an idealization or abstraction which was tolerable within the limits of accuracy relevant to large objects, but this abstraction is inapplicable and leads to ambiguities if used in microphysics. Thus complementarity gives a critique of abstractions.

The direct fit between Bohr's interpretation of physics and Whitehead's metaphysics can be seen best if we express the measurement interaction in terms of Whitehead's vocabulary. An experimental situation is a prehending in which the measuring system prehends or measures a certain other actual entity or society of such entities. The measuring apparatus itself is a society of actual entities, i.e. a nexus of mutually prehending entities each having a specific subjective form of prehending such that all share in common a defining characteristic that is determined by the function and application of the measuring instrument. The measurement itself is an event, i.e. an actual occasion in which the society forming the measuring system prehends the measured system with a certain definite subjective form determined by the whole of the experimental setup. If the experimental setup is changed, that the society of events constituting the measuring system will prehend the measured system with a different subjective form and the objective immortality achieved by the prehended entity will differ accordingly.

On the classical view the fact that microphysical entities which are all theoretically represented as being in the same state will in one observational setup appear as particles and in another appear as waves seems highly perplexing. On the Whiteheadian view, there is nothing remarkable in this fact, for what is "measured" is not an *isolated* system, but an entity that is the coming together of its relations to everything else, including the entities of the measuring system and the situation in which the two interact. Thus Bohr also emphasizes "the impossibility of separating the behavior of atomic objects from the interaction of these objects with the measuring instruments which serve to specify the conditions under which the phenomena appear."

Bohr often speaks of the above conclusion as the impossibility of separating subject from object, and he held that "but this situation in physics has so forcibly reminded us of the old truth that we're both onlookers and actors in the great drama of existence." Such a conclusion is precisely what Whitehead advocates in his account:

> The fundamental concepts are activity and process .... The notion of self-sufficient isolation is not exemplified in modern physics. There are no essentially self-contained activities within limited regions .... Nature is a theater for the interrelations of activities.

The point intended these passages is not that "subjectivity" in the usual sense has intruded into scientific explanation, but merely that like the notion of an "isolated system" the idea of an object distinctly separate from a subject is an abstraction or idealization which omits the factor of the interaction between systems.

From: H. J. Folse, "What Does Quantum Theory Tell Us About the World?," Soundings **72**, 179–205 (1989)—

> Thus we find a way to relate the *philosophers'* question about realism to the *scientists'* concerns about the systems described by quantum physics. If a neutral observer were to follow the discussion between realists and their opponents when it comes to quantum theory, and then is asked to whom to award the palm, one reasonable reply would be to say, "No decision can be made until we first know what is this 'something more' that realists want me to believe and anti-realists find so unacceptable?" Answering this question is the job of a contemporary philosophy of nature, and it is precisely here that the realist interpretation of quantum theory finds itself most embarrassed.

and

> This classical account of how we know the nature of reality behind the phenomena relies on the presupposition that knowledge requires the "truth" of theoretical statements to reside in a *correspondence* between at least some terms in these statements and the properties of independently existing entities. This correspondence account of

> truth implied that the resulting "spectator theory of knowledge" stipulates the *objective* knowledge must describe reality in terms of the properties actually *possessed* by an independent reality. (To be sure, even in a classical account, observation involves an interaction between observing and observed systems, thus what is recorded in an observation is strictly speaking a *relation* between the interactors, as even the Ancients well understood. But insofar as this interaction involves systems that can be defined as existing in separate mechanical states, such relations entirely supervene on the *possessed* properties of the relata, and thus can be "reduced" to them.) According to this outlook, the "objectivity" essential to *scientific* knowledge is guaranteed by the fact that classical mechanics makes it possible to provide a description of the object which eliminates any reference to the observer as a physical system interacting with the object to produce the "observations" on which that description is based. Thus the "subject" is "detached" from the object by treating the "observer" (*qua* physical system) as mechanically isolated from the "observed" object. In this way the observer is treated as a "ghost spectator" and any physical effect of observation is eliminated from the account in order that the description can be considered as referring to a physical world existing apart from observation.

and

> This was justified on its view because "observation" in the Cartesian framework refers to an event in the cognitive domain, *i.e.*, the human "mind," and thus even though the careers of spatio-temporal substances are the "cause" of this observation, it cannot be described as a *physical* interaction. The Cartesian ideal which pictures what the universe would look like even if no one was there to look at it is the viewpoint of a ghost spectator who pilots without physical effort his corporeal submarine through a space-time sea.
>
> In a quantum-era philosophy of nature this dualist approach to observation must be thrown out. The empirical starting point of science is the description of a phenomenon through a very specialized set of concepts in which that phenomenon is described as an observation of a neutrino or a quasar. Thus the description of observation as interaction which was exactly what had to be *left out* in the classical account now becomes exactly *what it is* that is described in the quantum description of microphysical reality.
>
> For this reason the realistic interpretation of quantum physics requires not only that we discard the spectator account of knowledge, but it also denies the presupposition that "truth" refers to a property of statements and exists in virtue of a reference relationship between terms in these statements and the properties of an independent reality to which these terms correspond. Now we learn that in physics we characterize an independent reality *not* by attributing properties to some substantial entity which is imagined to possess those properties apart from our interaction. Instead we characterize it through the phenomena which occur in our interactions with it. Truth, then, is not a property of statements but a property of the whole theoretical structure which allows us to predict those sorts of phenomena, and such a theoretical structure has that "truth" in virtue of its power to predict successfully precisely those phenomena in which we are said to observe these objects.
>
> The collapse of the hope for a hidden-variables theory which would preserve the separability of the states of mechanically isolated systems, and the dispelling of the myth that quantum physics was conceived in an anti-realist spirit now make it necessary to take seriously a philosophy of nature which represents real microentities as the seats of

objective potentials for interaction. Such a philosophy of nature will no longer characterize as ontologically fundamental those primary properties which characterized the classical body. Material objects are not vast collections of tiny extended bodies, Democritean atoms or Cartesian *rei extensae*. In breaking the presumed link between the primary properties of the classical mechanistic framework and those properties which are conceived to be ontologically fundamental, the quantum revolution point towards a philosophy of nature which "atomizes" not bits of matter, but elementary processes of interaction.

From: H. J. Folse, "Ontological Constraints and Understanding Quantum Phenomena," Dialectica **50**, 121–136 (1996)—

> The discourse of physics speaks about physical systems by attributing properties to them in two distinct ways. Insofar as physics is an empirical science it must be possible to describe physical systems as objects of *observation*. Insofar as the "explanation" of the observable properties offered by physics entails deductions from theory which are confirmed by observation, it must also be possible to attribute properties to physical systems on the basis of theory. Although the resulting distinction between "observational" and "theoretical" properties is of course deeply entrenched in the philosophical literature, the relations presumed to hold between them rest on often tacit ontological presuppositions which the quantum description shows to be idealizations acceptable at the macrolevel but not at the microlevel.

and

> The quantum theoretical description of atomic systems was born in the attempt to devise mechanical models of the chemical "atom" as a complex physical system composed of subsystems of the (then held to be) truly atomic "elementary particles." The hope that mechanistic atomism could provide an ontological basis for understanding a wide variety of phenomena traced to the behavior of the chemical "atom" seemed a rationally attainable goal because of the (then) well corroborated assumption that the classical state of an isolated system obeys strictly deterministic laws. However, the discovery that initiated the quantum revolution was the theoretical parameter of "action" could formally explain the relevant phenomena only by being theoretically represented as "quantized." This discovery in turn implied that in the obsevational interaction there is a kind of *wholeness* which precludes attributing a classical mechanical state to the observed object as an isolated system immediately (or any time) after the interaction. Consequently while the properties in terms of which the classical mechanical state is defined can be predicated as *observable*—i.e. relational—properties of the empirical object, these same properties cannot unambiguously be predicated of the isolated system as *theoretical*—i.e. possessed—properties. Thus there is a crucial difference between the ways in which classical and quantum descriptions of the phenomena allow us to attribute properties to systems in the discourse of physics.

and

> Assuming that there are a plurality of individuals in nature, an ontology must provide a means for individuating them from each other. This need for a principle of individuation entails that ontological discourse requires that at least some "fundamental" properties must be *possessed* absolutely by the entities of which they are predicated.

> To see why this is so, consider that if no properties were possessed, then all properties would be relational. But in this case there would be no way to individuate the relata as distinct entities between which the relation is said to hold. Indeed, the supposed relation could not be external between the relata, but becomes a relation "internal" to the complex whole of the supposed relata. But to say that a relation is internal to a nonreducible whole, is in effect to say that the whole, treated as an individual, possesses a certain property. So we are back to the need for possessed properties.

27-04-01 *Primrose Lane* (to G. Brassard)

Brassardisme 1: *I'm not yet saying no, but the chances of my coming are slim. Alas! Please forgive me and think of me again in the future.*

In other words, you *are* saying no. OK, I understand. But you will be sorely missed.

Brassardisme 2: *By the way, I have about 2.5 times more money to spend than in the "gobs and gobs" era! Any fresh ideas how to deal with this?*

Having gobs and gobs, and 2.5 times that(!), is a good thing. I'll think hard about a postdoc for you. Would you be willing to take someone like me? (I.e., someone with zero knowledge of computer science and cryptology?) Or do you want to stick on a more traditional basis this time?

In the mean time—you knew I would say this!—what say you about "QFILQI-2"? (It may not even be such a bad thing in the eyes of the computer scientists: Larry Schulman recently organized a small meeting like this at Caltech. Watrous gave an excellent talk there.) We could do it bigger, better, a fresh set of people (with old standbys too), if you wish. Or we could keep it small and cozy. The point is, the time is ripe, and people—good people—are starting to take these issues seriously. I could lean hard on everybody for a conference proceedings this time. Think about it.

Your old scheming friend,

30-04-01 *Rudolph/Sanders/Teleportation, 2* (to H. J. Kimble)

I'm glad you're waiting to ask your scientific questions. I'm running ragged trying to get a big symphony finished. Stay tuned to `quant-ph` May 10: I think it's likely to be the biggest submission ever. (Single spaced, 400+ pages, no figures!)

01-05-01 *Samizdat Revealed* (to A. Peres)

I finally reveal the samizdat! I'll do it mostly by tacking on a form letter that I wrote for some of the more sensitive cases of my quoting frenzy. Perhaps if you read that first, what I am about to write will make more sense.

The samizdat is a collection of some of my deepest and most forward-looking thoughts on quantum theory: it's the kind of stuff that cannot appear in a scientific journal because it is not science *yet*. Nevertheless I do not think it is all worthless—that is the main reason for my wanting to make it public. Life is short, and I want to do as much as I can in this lifetime to advance physics. Inspiring others to think about deeper things is part of what I now view as my mandate.

What I would like to ask of you is for you to follow the instructions below for getting a preview of the thing. In your case, actually, I believe I have quoted nothing sensitive: You will find no surprises. But I do very much crave your approval for the 71(!) times that I do quote you. (They are all clearly denoted as Asherisms, followed by a number.) You *by far*, had the greatest influence on this document and my letters to you alone take up about 90 pages! No other correspondent comes close. (There are even two separate chapters devoted to you.) Also I guess what I am searching for is a kind nod of tolerance for the chapter titled "Diary of a Carefully Worded Paper." Beside my point of general emphasis below about reading the Disclaimers, please do read the introduction to the DCWP chapter. I think you will find it pleasing in regard to our mission as teachers.

This document, despite its nonstandard character, is quite a serious work, I believe. And some quite serious people think that of it (Bub, Mermin, Caves, Nielsen, etc.). Worried slightly that you might not approve of my inclusion of the DCWP chapter, I had hoped that David Mermin would first share his Foreword with you—I believe he will laud the chapter there. Unfortunately he is holding on to it until the original deadline I set for him, but he did write me to tell you this:

Merminition 4: *If you want you can tell him I've been working hard on a foreword which there's no way I would have done if I didn't take your project extremely seriously and think the Samizdat well worth making public.*

So, that's about it from me. I hope I don't crash your computer. And I hope I hear some happy words from you tomorrow.

Form Letter

May 10 will be the first anniversary of the Cerro Grande fire entering the township of Los Alamos. The 400+ families that lost their homes that night will all likely commemorate the event in their own way. For myself, I plan to do what I usually do on special occasions: share my sins with the wider physics community. In this case, it will be in the form of a posting (to the Los Alamos archive) of a 400+ page book of my quantum foundational emails. I am in cahoots in this with David Mermin, who is writing a foreword for the thing.

In two or three days, I plan to start requesting proper approvals from my correspondents for the quotes of theirs I would like to use. To make it easier on everyone, I hope to have a name index in place before then (listing not only the quotes but my conversations with others about their thoughts). But in a couple of cases, I wanted to get pre-approvals even before that. Yours is one of them. The main questionable thing in connection to you is that I would like to include ___________. I hope you will say "yes" as it adds much to the theme. [In your case Asher, there is nothing really questionable; the only outstanding feature is its sheer mass.]

In either case I hope you can tell me your thoughts on this as soon as possible. I would really be very grateful. Trying to meet the anniversary date makes it such that I (and Mermin) are working around the clock.

Here's what to do:

1. Go to my website `http://netlib.bell-labs.com/who/cafuchs/index.html` and download the PostScript file titled, Wacky File #1. The file is very large (3.5 megabytes or so), so do not do this over a modem; use Ethernet only. Furthermore, don't bother to print it out; when it is complete I will send you a bound copy. Use only a screen viewer for your reading (unless you want to kill a lot of trees).
2. Read the Introduction page (so you get a deeper feel of what I'm aiming for), then read the Disclaimers and Acknowledgements on the following page. The Disclaimers are *especially* important in connection to your contribution.

3. In the Table of Contents, find your section and skip over to it. There you'll find the material I'm talking about. [In your case Asher that is Chapters 14 and 15 in the present version.]

If you say that I should not use any of the direct quotes, I will delete them before my larger approval request. But as I say, it adds much to the theme. [More importantly Asher, in your case you are the backbone of the book.]

Thanks for everything.

02-05-01 *Commitments and Burdens* (to J. Bub)

I dug up a good tidbit from '96, especially in connection to you. I'll send you a pointer once I get the new (internal) posting out. I think it hints at a new—maybe more interesting—twist to the foundational value of the bit commitment issue. ...

Well maybe I oversold it, but there is certainly a thought there that I had completely forgotten about. The places to look are the second two notes in the chapter on Michael Nielsen. [See chapter on Nielsen in *Coming of Age with Quantum Information*.] What I found interesting was that my feelings were a bit equivocal about the possibility of a no bit commitment theorem at the time. What I really seemed to think important was that quantum mechanics have the possibility of providing a provably secure digital signature. Things were muddled in my head at the time though, because I didn't seem to understand fully that the two protocols might not be equivalent.

Anyway, there it is. The further thing that I find intriguing is that Dan Gottesman slipped me a preprint by himself and Ike Chuang the other day where they *think* they have a provably secure digital signature scheme (that does not rely on bit commitment, of course). (One glitch remains in the proof.) So, putting the two together, it's just food for thought.

Enjoy London! (If you see Rob Spekkens there, tell him to look at the samizdat: all the answers to his teleportation questions are there.)

03-05-01 *Lyme Disease Newsletter* (to J. W. Nicholson)

God, even if I don't have the disease, I've got the symptom: I had *not* noticed that they used "entitled"!

Here's a good one that Mermin caught me at: I used to flaunt when I really wanted to flout. Apparently I'm not the only one: it is labeled as a "usage problem" in the *American Heritage* dictionary.

04-05-01 *Fermat* (to J. Preskill)

Preskillism 1: *If you make some copies, I'll buy one from you. You can even mark it up ...*

"I have found a truly marvelous proof that quantum mechanics is a law of thought. But this margin is too small to contain it ..."

05-05-01 *Author's Reply* (to A. Peres)

Thank you for your "referee's report".

Asherism 10: *Circa line 20200, you write "It is the theory which decides what we can observe". I remember hearing these words from von Weizsäcker in Joensuu in 1987. What is the reference? Who said that to whom?*

In the samizdat I write:

> Here's an idea attributed to Einstein by Heisenberg that I love. It captures an extremely important point (in this fuller version that is not quoted so often):
>
> > It is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe. You must appreciate that observation is a very complicated process. The phenomenon under observation produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness—we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions. When we claim that we can observe something new, we ought really to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our consciousness—function in such a way that we can rely upon them and hence speak of "observation."

The whole paragraph of thoughts is meant to be a paraphrase of Einstein. He had told this to Heisenberg on a walk, after Heisenberg's talk in Berlin. The time period was sometime strictly between Heisenberg's discovery of matrix mechanics and his discovery of the uncertainty principle. He claims that that conversation was his main motivation for searching for "something like" an uncertainty principle.

05-05-01 *Full Reference* (to A. Peres)

- W. Heisenberg, *Physics and Beyond: Encounters and Conversations*, translated by A. J. Pomerans (Harper & Row, New York, 1971), pp. 63–64.

Also quoted in:

- M. Jammer, "The Experiment in Classical and in Quantum Physics," in *Proceedings of the International Symposium, Foundations of Quantum Mechanics in the Light of New Technology*, edited by S. Kamefuchi, H. Ezawa, Y. Murayama, M. Namiki, S. Nomura, Y. Ohnuki, and T. Yajima (Physical Society of Japan, Tokyo, 1984), pp. 265–276.

This paper contains some absolutely wonderful material about the experimenter's role in varying experimental initial conditions and about how that is an essential piece in the construction of scientific theories.

08-05-01 *Depth* (to N. D. Mermin)

By the way, I find the opening sentence of this paragraph extremely deep sounding! Is that what I've been talking about all this time?!?!

Merminition 5: *The real issue is nothing less than how you and I can each construct a representation of the manifold aspects of our individual experiences (loosely known as a world), and the constraints that my representation imposes on yours, and vice-versa. By focusing explicitly on the strange information-processing capabilities inherent in the quantum mechanical description of physical reality, the new discipline of quantum information offers an opportunity to put on a sound foundation what was only hinted at in the convoluted prose of Bohr, the facile sensationalism of Heisenberg, the aphorisms of Pauli, and the poetic mysticism of Schrödinger. If it hasn't occurred to you that this is the real justification for your quantum information-theoretic pursuits, then you owe it to yourself to pause and peruse these pages.*

08-05-01 *No Irony* (to N. D. Mermin)

Merminition 6:

> [CAF wrote:] By the way, I find the opening sentence of this paragraph extremely deep sounding! Is that what I've been talking about all this time?!?!

I detect irony.

No, that's my current take on how what you're doing will ultimately bring coherence to the Ithaca Interpretation. I actually thought it might irritate you. And perhaps it has.

No irony at all. Go back and read "Fuchsian Genesis" in Comer's chapter. What the heck do you think it's about? I just thought you said it in a particularly masterful way.

10-05-01 *Leibnizian Thoughts* (to N. D. Mermin)

Merminition 7: *I know Fuchsian Genesis and its variations almost by heart at this point, but I didn't think that was what I was saying. I wasn't thinking of the constraints in terms of your information gathering requiring my information loss. I had in mind something more like a static situation. Not very well defined (or I'd be done). But maybe it's the same thing. Will think about it.*

I'm just rambling. Here's a thought from my drive in to work a minute ago.

Given what you say above, maybe you should go back and read about Leibniz's monads again (just like someone else had once suggested to you).

Thinking about that led me back to the phrase "correlata without correlation." And thinking about that lead me to a thought I had never thought before: how ridiculously Cartesian the relative state interpretation is! There is res extansa (maybe I should call it res physica to be more careful), and there is res cogitans. And the only way the two "interact" is by the happenstance—you sold me on the word—of appearing in a Schmidt decomposition of some wave function! Deutsch's multiverse boils down to about the same thing as a modern-day pineal gland.

10-05-01 *The Easy Part* (to N. D. Mermin)

I'll give you a bonus: a scotch, a beer, and comments. Unfortunately for the bulk of the comments, you will have to wait until Monday or Tuesday. I have pledged to my wife to have the computer completely turned off tomorrow for Mother's Day.

A serious problem is that the PostScript you sent me will not print. [...]

In the meantime, let me comment on footnote 5, because that's easy. Yeah, you're right—I think—that the usual textbooks don't emphasize the Born rule in the setting of bipartite systems. However, I myself have always been reluctant to take the "collapse postulate"—i.e., that after a measurement, a system is left in an eigenstate of the observable—simply because it is not true generally. The state of the system after a measurement depends in a detailed way on the precise form of the measurement interaction, and even upon whether the observer kept all the information available to him. (Remember, for me, a quantum state is a state of knowledge; and no one has the right to say that I can't throw away some of my information, etc.) There is one setting, however, where one can make something close to the collapse postulate come alive: and that is in considering measurements on subsystems of a total whole.

Below I'll place the "axioms" for quantum theory that I used in a tutorial on "Basic Quantum Mechanics" for a DIMACS meeting in 1997. Anyway, axiom 5 in particular emphasizes the point above, and comes close to the point you were trying to make. Though notice, in contrast to you, I was silent on what happens to the state on system S_1.

About an overall assessment, you're going to have to wait until I can print out the thing and view it properly.

The DIMACS Meeting Axioms

Quantum Axiom 1

A *quantum system* is any domain of physical inquiry or manipulation.

Maximal states of knowledge about such a system are in a bijective correspondence with operators

$$\Pi = |\psi\rangle\langle\psi| \quad \text{with} \quad \langle\psi|\psi\rangle = 1$$

for $|\psi\rangle$ in some Hilbert space $\mathcal{H}$.

$$\Pi = \text{“the quantum state”}$$

Quantum Axiom 2

The most precise manipulation of a quantum system that can arise without learning anything new of it corresponds to a map of the form

$$\Pi \longrightarrow U\Pi U^\dagger$$

where U is unitary.

Quantum Axiom 3

Each question that can be asked of a system corresponds to a set $\{P_1, \ldots, P_k\}$, $k \le \dim\mathcal{H}$, of operators with

$$\begin{aligned} P_i P_j &= \delta_{ij} P_j \\ \sum_j P_j &= I\ . \end{aligned}$$

The index j labels the outcomes.

When state is $\Pi = |\psi\rangle\langle\psi|$, outcome j can be expected to occur with probability

$$p(j|\Pi) = \mathrm{tr}\Pi P_j = \langle\psi|P_j|\psi\rangle \ .$$

Quantum Axiom 4

Maximal states of knowledge about a composite system $\mathcal{S}_1 \oplus \mathcal{S}_2$—the components with Hilbert spaces $\mathcal{H}_1$ and $\mathcal{H}_2$—are in bijective correspondence with all

$$\Pi = |\psi\rangle\langle\psi| \quad \text{with} \quad \langle\psi|\psi\rangle = 1$$

where

$$|\psi\rangle \in \mathcal{H}_1 \otimes \mathcal{H}_2 \ .$$

When $|\psi\rangle \neq |\alpha\rangle|\beta\rangle$, the state $|\psi\rangle$ is said to be *entangled*.

Quantum Axiom 5

Suppose $\mathcal{S}_1 \oplus \mathcal{S}_2$ consists of two noninteracting systems (e.g. spacelike separated systems) and $|\psi\rangle \in \mathcal{H}_1 \otimes \mathcal{H}_2$ is of the form

$$|\psi\rangle = \sum \sqrt{p_i}|e_i\rangle|\psi_i\rangle \ , \qquad \sum_i p_i = 1$$

for some orthonormal set $|e_i\rangle \in \mathcal{H}_1$, and $|\psi_i\rangle \in \mathcal{H}_1$ with $\langle\psi_i|\psi_i\rangle = 1$. (The $|\psi_i\rangle$ need not necessarily be orthonormal).

Then a measurement of $\{|e_i\rangle\langle e_i|\}$ on $\mathcal{S}_1$ revealing outcome k leaves the observer in a maximal state of knowledge about $\mathcal{S}_2$:

$$\mathrm{state}(\mathcal{S}_2|k) = |\psi_k\rangle\langle\psi_k| \ .$$

10-05-01 *Your Questions on de Finetti and Diffusion* (to R. W. Spekkens)

I still don't have enough time to come out of the water. But let me surface to get a breath of air: I've got NATO pounding on me for a paper, and I've got a meeting with the president of Bell Labs Monday. That combined with getting the big samizdat on `quant-ph` this morning has been killing me. Literally, only 3-4 hours of sleep each night for the past two weeks. (That kind of thing takes its toll on an old man; I'm not young like you and Terry any more.)

Spekkensism 4: *I enjoyed reading your paper on the quantum de Finetti theorem. I'm quite interested in the mathematical result, but I'm not sure I agree with the way you've interpreted it. In particular, your view that quantum states are states of knowledge doesn't make sense to me.*

Let me first see if I properly understand what your view is not. *It seems to me that it is not an operational view. This actually surprises me because I somehow got the impression that you were an operationalist (perhaps because I take Asher Peres to be an operationalist and you wrote that* Physics Today *article together). My understanding of operational approaches to quantum mechanics is that they take quantum states to represent preparation procedures (for instance, the setting on a dial on some apparatus). If an observer has only partial information about which of a set of preparation procedures was implemented, it seems to me that an operationalist should describe this observer's* *state of knowledge by a probability distribution over density operators. I get the feeling from reading your paper that you would agree with my characterization of operationalism as distinct from your view, given that you come down against interpreting the density operator as "the description of the 'true' preparation procedure". Have I got that right?*

You state that "the quantum state is a representation of the observer's knowledge of a system". What I don't quite get is what you mean by 'knowledge of a system'.

But you provide the first example of how the samizdat can serve me. (By samizdat, I mean the paper cited below.) Let me give you pointers for where to look in there. Three places: In the Brassard chapter, read the introduction to the problem set for the Montréal meeting. Also look at Proto-Problem #3. Jeffrey Bub has been big on precisely this question too. So read his chapter. Finally, there is plenty of material on your question in Chapter 23, "Diary of a Carefully Worded Paper."

Spekkensism 5 : *This actually surprises me because I somehow got the impression that you were an operationalist (perhaps because I take Asher Peres to be an operationalist and you wrote that* Physics Today *article together).*

You were perceptive to notice a difference between Asher and me. We had to fight that one out. See Chapter 23.

Spekkensism 6 : *Although I have not yet fully thought through your arguments in the "quantum state of the laser" paper, it isn't clear to me that the quantum de Finetti theorem is applicable to the state of a laser, since the finiteness of any laser's coherence time seems to imply a lack of infinite exchangeability.*

I know I still need to write Terry, but I've just going bonkers timewise. People with bigger hammers have been hitting on me. Phase diffusion has little to do with the issue: in that paper, we *ignored* phase diffusion (for the propagating beam) just as Terry and Barry do in their paper. That of course is an approximation. Our argument is about a proof of principle: namely without phase diffusion (as Terry and Barry), there is after-all entanglement in a Kimble kind of set up (in contrast to Terry and Barry): the *distillable* entanglement is equal to what was believed to be the case before, but then it was mistakenly identified as *pure* entanglement. Terry and Barry's great contribution was in getting us to recognize that. Taking into account phase diffusion will—as you say—invalidate infinite exchangeability. But I imagine (actually, that's too weak, I really should say *know*) that the flavor of our conclusion will remain intact: there will still generally be distillable entanglement (on some time scales), it will just be degrading as time goes on. An interesting, but probably hard calculation, would be to work that out explicitly. There is already a lot known about partial exchangeability classically: see the concluding remarks in our de Finetti paper. Combining that with estimates of coherence times, etc., in the present case is the path that needs to be taken.

I guess I'll CC this to Terry and also Steven van Enk: chances are strong that I'll be (completely) dropping out of the email scene for a couple of weeks again. Have a look at the beast below.

```
quant-ph/0105039 [abs, src, ps, other] :
Title:  Notes on a Paulian Idea:  Foundational, Historical, Anecdotal
   and Forward-Looking Thoughts on the Quantum
Authors:  Christopher A. Fuchs, (with foreword by) N. David Mermin
Comments:  504 pages including introduction, table of contents, and
```

```
   index of names; no figures

This document is the first installment of three in the Cerro Grande
Fire Series.  It is a collection of letters written to various
colleagues, most of whom regularly circuit this archive, including
Howard Barnum, Paul Benioff, Charles Bennett, Herbert Bernstein, Doug
Bilodeau, Gilles Brassard, Jeffrey Bub, Carlton Caves, Gregory Comer,
Robert Griffiths, Adrian Kent, Rolf Landauer, Hideo Mabuchi, David
Mermin, David Meyer, Michael Nielsen, Asher Peres, John Preskill,
Mary Beth Ruskai, Rüdiger Schack, Abner Shimony, William Wootters,
Anton Zeilinger, and many others.  The singular thread sewing all the
letters together is the quantum.  Some of the pieces are my best
efforts to date to give substance to an evanescent thought I see
rising from the field of quantum information---I call it the Paulian
idea.  To the extent I have communicated its misty shadow to my
correspondents and seen a twinkle of enthusiasm, it seemed
worthwhile to expand the jury on this anniversary occasion.  (612kb)
```

10-05-01 *The de Finetti Theorem and the Samizdat* (to A. G. Zajonc & K. Jagannathan)

(Could one of you also forward this note to Bob Romer, I don't have his email address. Could one of you give me his email address?)

I've been meaning to write you for some time to tell you that the last talk I gave at Amherst has finally made it onto paper. You can find it on `quant-ph` now; I'll put the citation below. [See "On Unknown Quantum States: The Quantum de Finetti Representation Theorem," `http://xxx.lanl.gov/abs/quant-ph/0104088`.] We think it's a fundamental and pretty nice little result, and does clear up quite a bit for those who want to take the wave function as a state of knowledge, rather than a state of nature. We tried to make the paper chock full of motivation, so even if you don't read the technical parts I think you'll still get something out of it. (In fact, for that reason, we submitted it to AJP, so maybe Bob has already seen it. I don't know whether he sees all the papers that come through there at an early stage of the game or not.)

Also, I put something else on the web today that you saw an extremely early version of (during my first interview trip, I think). I've been calling it the samizdat. I think you might enjoy browsing parts of it. (Actually Arthur makes a couple of appearances in there.) David Mermin wrote a very nice foreword for it. I have this dream (hope) that it'll somehow have an impact on the community, but it *is* probably too big for that. ["Notes on a Paulian Idea: Foundational, Historical, Anecdotal & Forward-Looking Thoughts on the Quantum (Selected Correspondence)," `http://xxx.lanl.gov/abs/quant-ph/0105039`.]

My wife and I very much want to spend a few days at the Bennetts' house this spring. I'll try to drop by the college when I'm in town and see what you fellows are up to.

10-05-01 *No Tests: I'm Beat!* (to A. Cabello)

Cabello-ism 1: *More LaTeX typos for the next edition: García, Álvaro (if it is a Spanish name). I have sent about 10 e-mails recommending your work. It is great!!!*

Thanks for the word of encouragement! And also thanks for already helping me catch some typos: I didn't realize that I should TEX your name as Adán. I will fix that in the next edition.

Anyway, the main goal here is to try to Copenhagenize people, but with a modern twist. I think accepting Copenhagen-like ideas is the first step in a great journey—not an end to the road—and that's what I'm trying to turn people on to. Thanks for showing that people are really starting to read the thing!

11-05-01 *Tot Dinsdag* (to S. J. van Enk)

van Enkism 1: *Congratulations on your submission that surely will make you even more famous!*

You made a spelling mistake: you misspelled infamous.

Robert Garisto, that assistant editor I know at PRL, wrote me that it is the "Many Letters Interpretation of Quantum Mechanics." (Actually, with hindsight now, I wish I would have written him a letter before/during our submission.)

11-05-01 *No Worry, Just No Time* (to A. S. Holevo)

I am sorry that my silence has alarmed you. I did not recognize that your request for a fax was an urgent matter. Things have just been very hectic for me in trying to get my large samizdat onto `quant-ph` in time for the anniversary of the Los Alamos fire. I have only been sleeping about 3–4 hours per night for the last two weeks, while trying to get it compiled. (It is a very big manuscript, 504 pages, so you should wait until I can give you a bound copy before attempting to read very much of it. However, you might enjoy reading David Mermin's foreword to it from your screen. I have never been so flattered. It is `quant-ph/0105039`.)

On top of that though, NATO has been putting much pressure on to complete my paper for them. (And also withholding $1,000 of my reimbursement unless I get it submitted.) So, I have had to multiplex in that way too.

So, there is nothing to worry about, I have just been very strained.

11-05-01 *Shannon Meets Bohr* (to P. Pearle)

I put a large samizdat about quantum foundation things on `quant-ph` yesterday. (Mermin wrote the foreword to it.) Parts of it, I think, you might enjoy. Other parts, though, are certainly going to irritate you. (So I apologize in advance.) In connection to your own point of view about quantum mechanics, you might have a read of the "Jeffrey Bub Chapter" and branch out from there.

11-05-01 *Sigh of Relief* (to B. W. Schumacher)

Great to hear you're definitely coming. (It seems the only definite dropout will be Brassard; Preskill is still in superposition.) And great to hear your term is coming to an end: Maybe you'll have some time for some leisure reading! My big samizdat appeared on `quant-ph` yesterday (with a foreword by Mermin). You and Mike Westmoreland might enjoy perusing some of it. I'll put the abstract below.

Ben's Reply

> Mike and I did notice your samizdat, though I haven't had time to read it. Sounds like great incindiery fun — which is perhaps appropriate, given the title …
>
> Your fellow bomb-thrower,

12-05-01 *The Observer in the Quantum Experiment* (to B. Rosenblum & F. Kuttner)

Last night I read your recent paper on `quant-ph` with the same title as above. [See `http://arxiv.org/abs/quant-ph/0011086`.] It was enjoyable reading, and I am in substantial agreement with significant parts of it.

I did however get the impression (from some of your choices of words near my citation and at the end), that you would not have guessed that I would say such a thing. Quite the opposite! That is what my paper with Asher Peres was about, albeit it in a somewhat subdued way and despite its provocative title. For, though we did write,

> Contrary to those desires, quantum theory does not describe physical reality. What it does is provide an algorithm for computing probabilities for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions.

the word "interventions" is prominent there. It is there precisely to refer to the paragraph previous to it where we write:

> To begin, let us examine the role of experiment in science. An experiment is an active intervention into the course of nature: We set up this or that experiment to see how nature reacts.

The word "nature" is a stand-in for all the "stuff" of the world. The issue is whether the theory gets at a free-standing reality rather than only our interface with nature. I opt for the latter: i.e., that the theory is indicating to us that there simply is no free-standing reality (at least now that we are on the scene).

I believe we say this more clearly in our follow-on paper C. A. Fuchs and A. Peres, "Quantum Theory – Interpretation, Formulation, Inspiration: Fuchs and Peres Reply," *Physics Today* **53**(9), pp. 14, 90 (2000).

In any case, I say these kinds of things from many, many different angles in my recently posted samizdat. Given your paper, I think you might enjoy perusing it a bit. It emphasizes many of the same points as you. Maybe the first places to look are Chapter 14, a letter to Bob Griffiths, and also the beginning of the Montréal problem set starting on page 85. I'll place the full reference below.

13-05-01 *Early Morning Sneaking* (to N. D. Mermin)

Kiki's still asleep, so I snuck in here for a while. It looks like I can view this version just fine. I presume therefore I can print it out too, but that will have to wait until tomorrow when I get it to a printer.

Merminition 8: *The way you state it would lead most readers to think that it holds only for special states of the composite system.*

Yes, you're probably right about that. But I certainly meant your version of it rather than the misreading of it.

Merminition 9: *I've never seen an argument that it's implied by 3, and am glad to see that you too state 5 as an independent assumption. Does anybody else? Or has anybody showed that it is implied by 3? Or produced a counterexample that satisfies 3 but not 5?*

It certainly cannot be derived from 3 as I have it stated. That only addresses probabilities, not state changes. 5 only addresses state changes, not probabilities. (In this version of Gleason's theorem that I'm writing up, there is a slight connection between the probability rule and the form of quantum state changes, but that's a pretty advanced matter that need not enter this discussion: in particular because it views quantum mechanical measurement as *nothing more* than a state-of-knowledge change.)

Does anybody else take 5 as an independent assumption? Now, that you mention it, I guess I never have seen anyone else state it that way. I do remember Bill Wootters in the audience at that meeting, combing his beard with his fingers, and looking intent as if the talk were actually interesting (remember it was a tutorial on basic quantum mechanics). That's the only place I've ever presented that talk where there was someone who knew quantum mechanics in the audience. But I don't remember ever having this conversation with anyone else but you: so there may well be a similar development out there, but I haven't seen it.

Kiki should be up any minute now: I'd better get going.

14-05-01 *Today's Meeting Canceled – Future of Quantum Information Processing* (to N. D. Mermin)

The easiest way to answer is below: I am so relieved! (Arun had to have his gall bladder taken out. In his own way he's probably relieved too.) BTW: do you know any of the others in the list?

Among other things, I found out last night—middle of the night—that there's been a load of work done on quantum cryptography over optical fiber networks (by Paul Townsend and the like at BT, now owned by Corning, our competitor). If I can swing it all, these are papers I need to digest (just a little at least) before meeting the big cheese.

Thanks for the developing interest in my life: it certainly helped the samizdat out.

Whose Knowledge? (I saw the title of your Växjö talk.) My answer: anyone old enough to have a driver's license and write down a density operator. (Actually, one probably doesn't need a driver's license, but it might be safer that way.)

15-05-01 *Many Letters + One More* (to R. Garisto)

Garistoism 1: *I read a few pages from your intriguing e-book and thought I'd send you a response. Although not tied to it, as you know I have some fondness for the Many-Worlds Interpretation (MWI). Although we will probably never agree about the MWI, I will say a few words in its defense anyway.*

First of all, to be glib, and to make a point, I will refer to your e-book as the "Many Letters Interpretation". It is a beautiful and complex mixture of information, but it is essentially impossible to follow without a table of contents or index. And like all compilations, superpositions if you will, it makes sense when one uses the index to apply culling conditions on it, say, all pages which mention me (I note I am equal to Aristotle ☺).

My point is that the MWI says that the world is a beautiful mosaic which makes no sense until one imposes the human viewpoint of a classical world. Once one applies the culling condition "all parts of the quantum which are consistent with me existing and having had my last thought", the mosaic falls into focus and the world is conceivable to the human mind.

Is this a slight to art, poetry, or free will? I say that it is not. There are certainly exponentially small areas of the quantum (notice how I am avoiding assuming that this quantum info is actually embodied (rather than simply described) by "the wave function"? I should surely get brownie points for that ☺) . . .

You certainly do. And thanks so much for the appellation "Many Letters Interpretation" — I like it! (I have a feeling this won't be the last I hear of it.) If there is a Chris Fuchs in another world who sways more toward MWI, I hope you meet him eventually. Then maybe the two of you could confront me in a way that might just make some headway! (My wife tells me I only listen to myself.)

With many thoughts (to match my many letters),

15-05-01 *Many Letters + One More, 2* (to R. Garisto)

Garistoism 2: *Nah, it would be no fun if you agreed with me on everything.*

In fact, it's necessary for the very notion of reality itself. Here's how I just wrote it for an article that I'm writing (in an attempt to distill the samizdat to a paper-sized statement).

> [S]urely, the existence of [the] world is the primary experimental fact of all, without which there would be no point to physics or any other science; and for which we all receive new evidence every waking minute of our lives. This direct evidence of our senses is vastly more cogent than are any of the deviously indirect experiments that are cited as evidence for the Copenhagen interpretation.
>
> — *E. T. Jaynes, 1986*

> The criticism of the Copenhagen interpretation of the quantum theory rests quite generally on the anxiety that, with this interpretation, the concept of "objective reality" which forms the basis of classical physics might be driven out of physics. . . . [T]his anxiety is groundless . . . At this point we realize the simple fact that natural science is not Nature itself but a part of the relation between Man and Nature, and therefore is dependent on Man.
>
> — *Werner Heisenberg, 1955*

There are great rewards in being a new parent. Not least of all is the opportunity to have a close-up look at a mind in formation. I have been watching my two year old daughter learn things at a fantastic rate, and though there have been untold numbers of lessons for her, there have also been a smidgen for me. For instance, I am just starting to see her come to grips with the idea that there is a world independent of her desires. What strikes me is the contrast between this and the concomitant gain in confidence I see grow in her everyday that there are aspects of existence she actually *can* control.

The two go hand in hand. She pushes on the world, and sometimes it gives in a way that she has learned to predict, and then sometimes it pushes back in a way she has not foreseen (and may never be able to). If she could manipulate the world to the complete desires of her will, I am quite sure, there would be little difference between wake and dream.

But the main point is that she learns from her forays into the world. In my more cynical moments, I find myself thinking, "How can she think that she's learned anything at all? She has no theory of measurement. She leaves measurement completely undefined. How can she have any true stake to knowledge?"

Hideo Mabuchi once told me, "The quantum measurement problem refers to a set of people." And though that is a bit harsh, maybe it also contains a bit of the truth. With the physics community making use of theories that tend to last between 100 and 300 years, we are apt to forget that scientific views of the world are built from the top down, not from the bottom up. The experiment is the basis of all that we know to be firm. But an experiment is an active intervention into the course of nature on the part of the experimenter; it is not contemplation of nature from afar. We set up this or that experiment to see how nature reacts. It is the conjunction of myriads of such interventions and their consequences that we record in our data books.

We tell ourselves that we have learned something new when we can distill from the data a compact description of all that was seen, and, more tellingly, when we can dream up further experiments to corroborate that description. This much can never change or science ceases to be science. It is the minimal requirement of what we shoot for in science. If, however, from such a description we can *further* distill a model of a free-standing "reality" independent of our interventions, then so much the better. I have no bone to pick with reality. It is the most solid thing we can expect from a theory. Classical physics is the ultimate example in this regard. It gives us a compact description, but it gives us more.

However, there is no logical necessity that this worldview always be obtainable. If the world is such that we can never identify a reality independent of our experimental interventions, then we must be prepared for that too.

16-05-01 *Reference* (to P. Busch)

Can you give me the complete reference for your paper, "Resurrection of von Neumann's No-Hidden-Variables Theorem" (including page numbers)?

I've been meaning to write you for some time to tell you that Joe Renes, Kiran Manne, Carlton Caves, and I came across essentially the same proof of a "a Gleason-like theorem for POVMs" as you did. We were getting all set to write it up as a Phys. Rev. Lett., when I leaked some word of it to David Mermin. He remembered your paper and pointed me toward it ... and sure enough! So, we never published it. (And I think it shattered poor Joe's research life.) But now I'm going to review it in a longish expository piece, so I want to get the reference right.

I'll put below the paragraph I'm just working on where the citation makes its first appearance. I hope you're OK with the wording of that. I just wanted to make sure Joe recovers even some small something for his effort.

I hope I'll see you at the Belfast meeting in September.

The structure of the remainder of the paper is as follows. In Section 3 "Why Information?," I reiterate the cleanest argument I know of that the quantum state is solely an expression of information—the information one has about a quantum system. It has no objective reality in and of itself. The argument is then refined by considering the phenomenon of quantum teleportation. In Section 4 "Information About What?," I tackle this common question head on. The answer is, "nothing more than the potential consequences of our experimental interventions into nature." Once freed from the notion that quantum measurement ought to be about revealing traces of some preexisting property (or beable, one finds no particular reason to take the standard account of measurement (in terms of complete sets of orthogonal projection operators) as a basic notion. Indeed quantum information theory, with its emphasis on the utility of generalized measurements or positive operator-valued measures (POVMs), suggests one should take those entities as the basic notion instead. The productivity of this point of view is demonstrated by the beautifully simple (Gleason-like) derivation of the quantum probability rule found recently by Paul Busch and, independently, by Joseph Renes and collaborators. In Section 5 ...

16-05-01 *Principle Theory* (to J. Bub)

Can you give me the full citation of your paper "Quantum Mechanics as a Principle Theory?" (Including starting and ending pages.)

Thanks. I'm citing that paper for the question "Information About What?" in regard to my view that the quantum state is solely subjective information. (I'm trying to distill the essence of the samizdat into about a 40 page paper: I'm about 25 pages toward it now. I'm really hoping it's finished long before Växjö. But I write so damned slowly!)

16-05-01 *Amicable Banter II* (to R. Jozsa)

Jozsa-ism 2: *Thanks for your message — I would not object to that inclusion into your volume of correspondence (but I wonder if our amicable banter would really be of value/interest to a wider readership!?...).*

More seriously, I've found that the thing (in scaled-down preliminary versions) turned out to be a surprisingly effective means for getting my (forming) point-of-view across. To see Bub, for instance, make a 180 degree turn even after writing his two books on quantum logic, was the most shocking thing. Similarly to see the dents I've made in David Mermin, John Preskill, Lucien Hardy, and a few others went a long way toward giving me some courage. Amicable banter is kind of like a sugar coating that helps the medicine go down.

I keep counting on the day that you'll see I'm not a complete fool, only a partial one (but maybe one not completely worth dismissing). It may or may not happen: but you remain my friend and sparring partner as always.

Looking forward to some beers and a little rest in Warsaw!

17-05-01 *Voracious Readers* (to J. Bub)

I found the information on the reference I needed. So, no need to reply on that. But in the process of finding it, I found a new paper of yours that I had never seen.

I'd like to see that! Can you send it to me?

J. Bub, "Indeterminacy and Entanglement: The Challenge of Quantum Mechanics," Brit. J. Phil. Sci. **51**: 597–615 (2000).

Abstract: I explore the nature of the problem generated by the transition from classical to quantum mechanics, and I survey some of the different responses to this problem. I show briefly how recent work on quantum information over the past ten years has led to a shift of focus, in which the puzzling features of quantum mechanics are seen as a resource to be developed rather than a problem to be solved.

17-05-01 *Pre-Irritation Irritation!* (to P. Pearle)

Pearley Quote 1: *I shall now get it out and get irritated! But perhaps not. I was amused by your article with Peres in* Physics Today *about quantum theory needing no interpretation—amused because I have over the years come across this point of view promulgated in one guise or another many times and I always wonder, if that is so obvious, why there is a continual necessity, starting with Bohr (so you are in a great tradition, for polemics against any other point of view).*

No, that is a very good point, and I am trying to tackle that issue head on in the present paper I'm writing. Let me send you the very much incomplete, preliminary draft right now. The first four sections are particularly important for what you say above. And I just want to show you I've thought about that!

So drop the samizdat for the moment! And go to this pre-irritation irritation. I think you'll find I'm not so flaming as you think I might be!

It's attached below as a PostScript file. If you need it in a different format, let me know.

18-05-01 *Finally!* (to P. W. Shor)

Sasha and I just put together the stuff below for Fred Roberts' request. Why don't you make any corrections/additions you deem appropriate and then forward it to Fred. I'm sure we missed some people on the potential invitee list, but maybe this is enough to get started . . . and maybe you can remember some others that ought to be on it.

Sasha and I will both be gone next week. (I'll be in Warsaw.)

Please CC us whatever you send to Fred.

Title:
Capacities and Coding for Quantum Channels

Abstract:
In 1973 the information theorist J. R. Pierce wrote for the 25th anniversary of Claude Shannon's original paper, "I think that I have never met a physicist who understood information theory. I wish that physicists would stop talking about reformulating information theory and give us a general expression for the capacity of a channel with quantum effects taken into account." Today, 28 years later, Pierce might still be saying the same thing! For though there has been an explosion of activity in understanding quantum channel capacities in recent years, Pierce had no feeling for the breadth of the question he asked: associated with a quantum channel, one can speak of its capacity for sending classical information, its one-way capacity for sending truly quantum information, its two-way quantum capacity, its entanglement-assisted classical capacity, its secrecy capacity, and the list of distinctions is still growing. This tutorial and workshop will be devoted to all aspects of

quantum channel capacity and coding, from code-construction techniques to bounds on capacities and error exponents.

Potential Invited Speakers: (NOT FOR PUBLIC VIEW YET)

Rudolf Ahlswede	(Bielefeld)
Alexei Ashikhmin	(Bell Labs)
Howard Barnum	(LANL)
V. P. Belavkin	(Nottingham U.)
Charles H. Bennett	(IBM Research)
Toby Berger	(Cornell U.)
Gilles Brassard	(U. Montreal)
Rob Calderbank	(AT&T Research)
Nicolas Cerf	(Brussels)
Thomas Cover	(Stanford)
Imre Csiszar	(Budapest)
G. David Forney	(MIT)
Michael Freedman	(Microsoft Research)
Fujiwara	(Japan)
Peter Gacs	(Boston U.)
Nicolas Gisin	(Geneva)
Daniel Gottesman	(U. California, Berkeley)
Markus Grassl	(U. Karlsruhe)
Osamu Hirota's group	(Tamagawa U.)
Alexander Holevo	(Steklov Mathematical Inst. / Bell Labs)
Richard Hughes	(LANL)
Richard Jozsa	(U. Bristol, UK)
Louis Kauffman	(U. Illinois at Chicago)
Christopher King	(Northeaster U.)
Alexei Kitaev	(Microsoft Research)
Koashi and/or Imoto	(Japan)
Emmanuel Knill	(LANL)
Lev Levitin	(Boston U.)
Hoi-Kwong Lo	(MagiQ Technologies, NY)
Peter Loeber	(Bielefeld, Germany)
Dominic Mayers	(NEC, Princeton)
Ueli Maurer	(ETH, Zurich)
Hiroshi Nagaoka	(Japan)
Prakash Narayan	(U. Maryland, College Park)
Michael A. Nielsen	(Queensland)
Denes Petz	(Budapest)
John Preskill	(Caltech)
Eric Rains	(AT&T Research)
Mary Beth Ruskai	(U. Massachusetts)

Masahide Sasaki	(Communication Research Lab., Tokyo)
Serap Savari / G. Kramer	(Bell Labs)
Benjamin W. Schumacher	(Kenyon College)
Neil Sloane	(AT&T Research)
John A. Smolin	(IBM Research)
Emina Soljanin	(Bell Labs)
Andrew Steane	(Oxford)
Yossef Steinberg	(Technion)
Barbara M. Terhal	(IBM Research)
Michael D. Westmoreland	(Dennison U., Ohio)
Reinhard Werner	(Germany)
Andreas Winter	(U. Bristol, UK)
Stefan Wolf	(Switzerland)
William K. Wootters	(Williams College)
Horace Yuen	(Northwestern U.)

19-05-01 *Golden Ages of the Quantum* (to E. Merzbacher)

I'm just off to an information theory meeting in Warsaw. But I'm so happy—and I wanted to tell you—I also just finished the fine book you gave me about the early years of the Bohr Institute. (Now I can use the flight to work rather than read.) Thanks once more for the nice book collection.

Let me also tell you about something I put on the Los Alamos archive last week. You might be interested it, as it also documents a little bit of the history of quantum mechanics (both of the old golden age and the new golden age). David Mermin wrote a foreword for it. I'll put the abstract below. [See *Notes on a Paulian Idea: Foundational, Historical, Anecdotal and Forward-Looking Thoughts on the Quantum*, `quant-ph/0105039`.]

25-05-01 *Quantum Axiomatics* (to C. H. Bennett)

Bennettism 1: *Have you read the new papers by Aerts et al. on repairing "defective" axioms of quantum mechanics? Are they worth reading?*

That's a very strange question coming from you Charles Bennett. No, I haven't looked at that: I am in Warsaw right now, with very limited email access and essentially no web access. I'll look at it when I'm back in the states next week and get back to you.

You know (or ought to know) that my only trouble with the axioms is that we have to deal with them at all. I.e., that we don't seem to have clear-cut way to pose the theory without invoking them. (As one might say that we do when it comes to special relativity.) My clearest statement yet on that point is made in the paper attached (I am in the middle of writing it, so it's not complete). But read the first two sections and tell me whether it stirs you at all. Or does it still leave you limp?

26-05-01 *The Warsaw Cafe* (to N. D. Mermin)

I'm on the last day of my trip to Poland, writing you from a little place with blue tablecloths and yellow flowers. Mostly I'm writing you again to thank you again: maybe one of these days I'll say enough is enough, but not yet. The week had its ups and downs. The biggest up was seeing an audience of classical information theorists light up to the idea of quantum cryptography. The biggest down was a collection of conversations with [Professor X] about the samizdat. It seems to eat at him that I broke the rules of academia in this way, and that I would put such rubbish on the net for "self-glorification." One night I got so depressed, I found myself opening up your foreword and reading it again. It did me a world of good, and got me through the night. I knew that without you I would not have had the courage to carry out the project in the first place. What I didn't know was that I would need you even after it was completed. Thanks again from the bottom of my heart.

The NATO paper has—of course!—gotten delayed again by this little outing.[12] I know that Gonis views me as one of the most evil men alive. But if it's not too arrogant, I think it might turn out pretty good over all. There is a fine line between trying to shock people into action and going too far, and I know I'm just playing it by ear. The manuscript just reached the 32 page mark. Once I get to the stage of having the "collapse rule argument" completely written down, I'll send the preliminary version your way: you seemed the most interested in that part of it.

My off hours here have been devoted to going through Rosenfeld with the same thoroughness that I gave Folse (and will eventually give Plotnitsky). I copied down a little passage for you. If I'm not mistaken, it's Rosenfeld's way of *trying* to say what you were *trying* to say in one of the sentences in the Foreword:

> And now comes the last great surprise: it turns out that in describing atomic phenomena one particular picture cannot suffice: according to the circumstances we are obliged to make use of several quite different pictures; one can only describe the atomic world, as it were, from one point of view at a time, and the prospects it offers from various angles are so different that they cannot be fused into one single picture in the usual sense.
>
> This situation has given atomic physicists occasion for fascinating reflections upon the very essence of human cognition, which I shall not enter upon here. I just wanted to mention it as a concluding touch in the picture of the atomic physicist that I have attempted to outline. I scarcely venture to hope that I have succeeded in giving an adequate insight into his methods of work, but perhaps I have managed to persuade you that the secret weapons that have secured him such brilliant triumphs are persevering toil and common sense.

Plotnitsky, by the way, wrote me the most wonderful letter comparing his reading of Bohr to Folse's. He also included a load of analysis on Pauli and Wheeler. You should see the size of it: it almost makes my samizdat look like small cakes. Most surprising though: it's one of his best pieces of writing I've seen yet! (I actually understand a thing or two: that used to take an actual face-to-face conversation.)

But now my coffee's gone, and I've got to start contemplating the taxi ride to the airport. I'll be in touch again soon.

26-05-01 *Your Correspondence* (to Y. J. Ng)

Thanks for the warm reception of my samizdat. I'm just flying back from a meeting on (predominantly classical) information theory in Poland. The reception of quantum cryptography was

[12] This refers to C. A. Fuchs, "Quantum Foundations in the Light of Quantum Information," in *Decoherence and its Implications in Quantum Computation and Information Transfer: Proceedings of the NATO Advanced Research Workshop, Mykonos Greece, June 25–30, 2000*, edited by A. Gonis and P. E. A. Turchi (IOS Press, Amsterdam, 2001), pp. 38–82. Also `quant-ph/0106166`.

wonderful: I could really see the enthusiasm for the idea building as the audience started to understand what it is all about.

I hope to write you some comments on your black hole paper (early) this summer. It is an important issue, and I'm glad you've turned your mind to it. Sometimes I wonder (but not nearly so strongly as in the quantum case) if one might pose a similar program for general relativity as I did in the beginning page of the samizdat (i.e., the one immediately following Mermin's foreword, with Preskill's question). That is to say, might one be able to find a natural information theoretic justification for the principle of equivalence, etc. If one could, then one might immediately find a new point of connection between GR and QM ... one that may have been passed over before.

26-05-01 *Sitting in Zurich* (to E. Merzbacher)

Thank you for your encouraging letter on the samizdat. The reactions of course have been varied, but on the whole those who are writing me seem to think they might get something out of it. I truly do want to see all this commotion about quantum mechanics "not making sense" disappear before my daughter goes to college. Life is too short to be confused for too very long.

I'm trying to condense the program of the samizdat into a single paper (of about 40 pages). It is being written for a NATO meeting I attended last summer whose hidden undercurrent was quantum foundations. With some luck I'll be able to get it on the web by the end of next week. I'll put the abstract below (to see if I can whet any more of your interest). (Beside that though, the paper will have a couple of very basic theorems that might interest you in their own right: it's time for me to get a little longer on analysis and a little shorter on poetry again.)

It sounds like you've been going to the New York theater much more than I have! That's a real shame for me, considering I'm only 32 miles away!

> In this paper, I try to cause some good-natured trouble. The issue at stake is when will we ever stop burdening the taxpayer with conferences and workshops devoted—explicitly or implicitly—to the quantum foundations? The suspicion is expressed that no end will be in sight until a means is found to reduce quantum theory to two or three statements of crisp *physical* (rather than abstract, axiomatic) significance. In this regard, no tool appears to be better calibrated for a direct assault than quantum information theory. Far from being a strained application of the latest fad to a deep-seated problem, this method holds promise precisely because a large part (but not all) of the structure of quantum theory has always concerned information. It is just that the physics community has somehow forgotten this.

26-05-01 *The Activating Observer on Hold* (to B. Rosenblum & F. Kuttner)

Thanks for your nice letter. I hope you will be patient with my reply. In fact I am putting a paper on the web probably at the end of the week (or at the end of the following week, latest) that I believe will answer your questions. If it still does not, then we can pick it up from there.

Things have been immensely busy for me lately and I've had to cut my email drastically. I'm writing to you as I'm flying back from Warsaw. (Most of my email is in fact limited to what I can do on a plane now, for the time being at least.)

Thank you for the invitation to speak at your university. I am in California from time to time (to visit Caltech etc.). I'll try to give you a heads-up when the next trip goes into the planning stage.

Bruce and Fred's Preply

Thanks for your email and the friendly comments. You're right, we would not have guessed that you would be in substantial agreement with significant parts of our argument. But in rereading your opinion piece with Peres and the other stuff you referred us to, we can see how we are not—necessarily at least—far apart.

We too might agree that "quantum theory needs no 'interpretation'." After all, the interpretations of quantum theory arise because of the measurement problem, which can be seen as the apparent intrusion of the observer. Our main thesis is that this apparent intrusion arises in the quantum experiment, logically prior to the quantum theory. If so, no mere interpretation of the theory can ever resolve this problem.

I'm puzzled though. Is the position you advocate (a version of) the Copenhagen interpretation? Your opinion piece ends with implying that all that quantum mechanics need be is a useful guide to the phenomena around us. That sounds much like Bohr's "Science is not about nature but only about what we can say about nature," which more or less seems to capture the Copenhagen spirit. In a sense, of course, the Copenhagen interpretation is not really an interpretation. (Peierls objects to the term "interpretation" because, according to him, Copenhagen is the only way to understand quantum mechanics.) Might, however, what you advocate be seen as close to Mermin's extreme rendering of Copenhagen: "Shut up and calculate!"?

You do say that you would be the last to claim that the foundations of quantum theory are not worth further scrutiny. But then the example you give (a search for minimal sets of physical assumptions) seems to advocate a rather narrow range of scrutiny. Reading other things you have written—including your comment on our piece—we'd have thought that you would welcome more far-reaching investigations. Are we wrong?

While we don't think mere interpretation can resolve the measurement problem, we do believe there likely is a real problem—that Nature may be trying to tell us something we can't yet understand. And, moreover, interpretations may have a role in addressing it.

As we see it, the role of interpretation can be two-fold: Interpretations can suggest new physical phenomena to address the measurement problem. For example: Bohm's suggested deeper levels and implicate order; interpreting collapse to be a physical phenomenon motivates the GRW and Penrose speculations; many worlds has suggested non-linear terms connecting worlds; and extending von Neumann, Stapp has a physical theory of consciousness.

Interpretations, on a yet grander scale, may suggest new world-views. The presently dominant Newtonian world-view, still the basis of much of our thinking and attitudes, is, of course, fundamentally flawed. Insignificant for all practical purposes, perhaps, but world-views go beyond practical purposes. This may all be well outside the realm of physics. But physics may be encountering something of great concern beyond physics—as it has in the past.

You say your opinion piece was motivated by a concern that the airing of various interpretations of quantum mechanics "may lead some readers to a distorted view of the validity of standard quantum mechanics." If you mean the readers of articles in *Physics Today*, AJP, and the physics research journals, that is not a concern that would worry us. Most of our colleagues are completely immunized against that problem by their courses in quantum mechanics and by their continued work with the discipline.

We'd have two different concerns. The first is that by ignoring the enigma that Nature presents us with, physics may just miss something truly important—albeit, perhaps, something beyond what is normally regarded as physics. (And we, personally, have the feeling that by concentrating solely on interpretations of the quantum theory, physics may indeed be missing something.)

A second concern is that the secret that physics has a problem is out. There is no way to honestly present the measurement problem to someone not brainwashed by a serious course in quantum mechanics in a way that doesn't leave them feeling that some mysterious spookiness is involved. To the extent that physicists don't develop these ideas among themselves and present them properly to the interested wider public, others less knowledgeable—or, worse, badly motivated—will become even more the major communicators than they now are.

After all this talk about the measurement problem, we feel like noting that when we teach quantum mechanics we spend almost zero time on the enigma. Aside from some, likely missed, offhand comments, nearly all of our own students are probably as brainwashed as most. They concentrate on what will be on the exams (and I can't blame them).

To some good extent we feel that you generally agree with much of what we say here. Reading some of the very interesting stuff you directed us to, it seems you take the issue quite seriously. (Your "daytime" publications also look very interesting.)

Which brings me (BR) to ask if you are likely to be in the California area during the next academic year. If so, would you be able to come to Santa Cruz for a colloquium? (Unfortunately, our limited colloquium budget permits few invitations from the east coast.) We'd love to hear from you.

27-05-01 *Mohrhoff* (to A. Peres)

Asherism 11: *Have a look at* `quant-ph/0105097`*. You'll probably enjoy it.*

I am home finally! And, I have now seen what you were talking about. You were right, it looks like it might be interesting. (I only read the first page quickly.) I remember that Mohrhoff impressed David Mermin in the past, but the last time I saw one of his papers I couldn't get through it. Have you read this one?

Do you think people will one day write such mean things about my samizdat? (I'm starting to get frightened that they might!) I've now, by the way, received 19 responses to the samizdat's posting. Only two of them have been overtly negative. Yesterday, on the other hand, I found a very flattering one from Eugen Merzbacher. Things like that keep my spirits high, and help build the belief that I may have done something worthwhile.

I read your paper with Danny yesterday on the flight. I will formulate a response to it tomorrow. As for the rest of today, I am going to spend some time with my family; they will be waking up soon. I think I will try to find an IHOP to take them out for a pancake breakfast.

28-05-01 *The Morristown Coffee Tank* (to N. D. Mermin)

Thanks for the cheering letter. I'm back in Morristown now, tanking up for the morning. No, I haven't seen the 't Hooft thing.

Merminition 10: *But if it happened to get two readers who appreciated its literary merits, do you think its subsequent appearance in PRL would make me a laughing stock in the quantum-info community? This worries me. Please advise.*

Yes, I can see that that might be a worry.

Here's how I opened my first talk in Warsaw the other day:

> Well I'm going to do something unexpected; I'm going to change the category of my talk from a research talk to an expository talk. I hope the organizers will forgive me; it just seemed I could make the talk more relevant that way. ... You know, I referee a lot of papers. [Someone in the audience yelled, "You're the one?!?"] And for a while, whenever I thought a paper's content was skimpy, I would describe it as a "didactic" paper in my anonymous referee reports. The implication was that the paper was merely instructive, rather than of research value. But that led me to a little trouble, because I sometimes write my colleagues directly to insult their papers. And I found that I was using the word didactic in my emails too! You can guess I was eventually caught! ... Anyway, today's talk will be a didactic one, and I have one goal in mind ...

Let me withhold judgment until I see the revamped version. PRL has been publishing so much crap lately, and turning down some rather good pieces—like Paul Busch's Gleason-like derivation—that they might do well to publish a good didactic one. And also, you might convince me that it's more than that with this new prose.

28-05-01 *Card-Carrying Greens* (to C. H. Bennett)

I'm back in Morristown now. Thanks for the (unusually long) note. I can see that I continue to leave you limp!

Bennettism 2: *Were you a Green? Did you really believe there was no difference between Gore and Bush?*

No, I wasn't a Green. And, I didn't believe that there would be no difference in the *consequence* of having Gore versus Bush in the White House.

But I do believe it of the quantum campaigns. And I do believe the reason there continues to be money in quantum foundations research is because the arguments all center around almost-invisible variations of the same thing.

Bennettism 3: *Nice introduction and motivation of the problem, especially the math vs physics versions of special relativity. However, in your table on Quantum Axioms and Imperatives, the one with the redundant right column, it seems to me you beg the question with "give an information theoretic reason".*

If I'm on the right track, the two columns really will be redundant. So, that's a good sign.

Bennettism 4: *Isn't information theory, no less than linear algebra, a branch of mathematics, rather than a branch of physics?*

But rarely is a branch of mathematics laid down without a more fuzzy kind of motivation working in the background. The fuzziness comes first, the formalization comes next. But the meaning and the value of the final construct—i.e., the final formal system—remains in the fuzziness. What I'm really asking us to do in that paper is to lay open the motivation for why we have the particular structure we do in quantum mechanics. If I had to place a bet, it would be that the answer will be found along information-theoretic or decision-theoretic lines. You undersell those fields in not realizing that they have a significantly firmer grip on their origin than quantum mechanics does. (Remember Rüdiger's nice presentation of the "dutch book" argument for the probability calculus in Montréal last year? As I recall, you appeared, at least mildly, impressed. That is the flavor of things I'm hoping for for quantum mechanics.)

Bennettism 5: *And when you ask for a physical explanation, what do you mean by "physical"? It's like pornography—you can't define it but you know it when you see it.*

I know you meant to be sarcastic with this, but, roughly, yes. Yes, we will know it when we see it: the nagging, nasty feeling that something is missing from our worldview will disappear (at least in this avenue of science). Funding for quantum foundations meetings will—by community choice—dry up. One could argue that one's pet interpretation—like the Everett program, for instance—is already virtually there ... if the rest of the community would just stop being stubborn! But I just don't think the rest of the community will stop being stubborn until the essence of the theory can be taught to a junior high student. (Your Everett interpretation is not there, except in the trivial sense of saying "all possibilities equally exist" ... which one could have just as well have said of classical physics, asserting that every initial condition is equally valid.)

Bennettism 6: *You could say that the special relativity example, despite its patent physicality, is really a geometric axiom asserting that spacetime behaves like a Minkowski space rather than a Euclidean one. So how much worse is it to have a bunch of axioms saying that states behave like rays in a Hilbert space? To me quantum mechanics is a more essentially mathematical and less physical part of physics than special relativity is. In that regard it is more like thermodynamics, which concerns the macroscopic consequences of microscopic reversibility, and would apply in any world, for example a 2 dimensional classical-mechanical world, a discrete cellular automaton world, or a 5-dimensional special-relativistic world, as long as the underlying dynamics was reversible.*

You will find little disagreement with me about part of this: i.e., that quantum theory shares a lot with thermodynamics in its range of applicability. But that is precisely why I would call quantum mechanics more a "law of thought" than a "law of nature." Just as Boole did of probability theory. And just as Jaynes did with (classical) statistical mechanics. (In fact, either of you two—i.e., you or Jaynes—might just as well have written your quote above.)

You might say, and I suspect you will say, the distinction I draw between "nature" and "thought" is only a semantic one. But I don't believe that: I think it is precisely in making that distinction clear (and operational) that we have a chance of closing the quantum foundations debate and moving on.

Physical theory is about two things: what is and what we know (or what we believe). It's the process of putting the two things together that gives a prediction in any practical setting. Quantum theory, interestingly, seems to be a nontrivial jumble of those two things. I think that is a rather deep statement about the world, and one that we have not yet come to grips with.

I hope you'll read the whole paper from beginning to end when I finally post it. You know that I crave your respect. But I know that I have to earn it.

28-05-01 *Letters: the Long and the Short* (to A. Plotnitsky)

Thank you so much for the amazing letter! I haven't read the whole thing yet, but I've read enough to know that definitely going to get something out of it. Very impressive. When I get in to the office tomorrow, the first thing I'm going to do is print it out so I can read it properly.

Let me also alert you to something I posted on quant-ph a couple of weeks ago. The citation is below. [See *Notes on a Paulian Idea: Foundational, Historical, Anecdotal and Forward-Looking Thoughts on the Quantum*, `quant-ph/0105039`.] It's the thing I've been calling the samizdat: I don't know whether I've mentioned it to you. You might enjoy parts of it.

Arkady's Preply

> First of all, I apologize for being out of touch since receiving the latest version of your "Compendium," but a busy end-of-semester schedule and too many pending (and some past-all-deadline) commitments have intervened. I have managed, however, to finish reading the expanded version of the "Compendium" in the meantime, which, a bit slow as it was (in part because I also used the occasion to visit some of the things you cite), actually provided a kind of refuge. I have by now a rather extended set of notes that nearly amounts to a small "compendium" of its own. I have, however, distilled a kind of commentary out of them over the last few days, which I thought I would pass on to you. (At your request, I also append in the end a few typos and other small glitches that I have noticed, although, as I was paying much more attention to the content, I could have easily missed others.)
>
> This commentary started as a much shorter set of remarks on some of Henry Folse's work, which your cite extensively and which especially attracted my attention, since of the works that you mention it deals with Bohr most extensively and since I am already familiar with. (By contrast, I was not familiar with D. J. Bilodeau's work, which you cite extensively as well and which I found, I take it in agreement with you, to be among the better commentaries on most subjects he addresses.) My thought, however, quickly moved to more general concerns, prompted by other things (many of them quite extraordinary) that you cite and by my own thinking about the differences among Bohr's, Pauli's, and Wheeler's views (with some of Heisenberg's ideas, my current special interest, added to the mix), and developed into the argument to follow. I shall, nevertheless, start with some comments on Folse's work, as a convenient entrance to this argument. For the sake of convenience, I shall mostly restrict myself to citing your "Compendium" (hereafter referred to as "Fuchs").
>
> Folse is among the more assiduous readers of Bohr, and his more recent readings seem to me to improve on his 1985 book, *The Philosophy of Niels Bohr*, which I read when working on *Complementarity* and on which I comment there, along with Dugald Murdoch's and Jan Faye's readings of Bohr. I must especially commend Folse on commenting (he is one of the very few who has done so) on the significance of Bohr's new conception of atomicity, specifically in his "Niels Bohr's Concept of Reality," in Pekka Lahti and Peter Mittelstaedt's *Symposium on the Foundations of Modern Physics 1987: The Copenhagen Interpretation 60 Years after the Como Lecture* (Singapore: World Scientific, 1987), which you mentioned in one of your emails and which you cite in your "Compendium" ("Fuchs," #135, p. 27). Folse's interpretation of this conception and, in part correlatively, of Bohr's epistemology as a whole in this article and elsewhere is, however, quite different from the one that I have tried to pursue in my own recent

articles. I have no quarrel with Folse's insistence on Bohr's "realism"; and he has good reasons for this argument, given some arguments against Bohr. The question is what form this "realism" takes and the areas and limits within which such concepts can apply to Bohr's view.

In my view, while there is a place for some realism in Bohr's interpretation of quantum mechanics, I cannot think of any concept of reality that could, in my interpretation of his interpretation, apply at the ultimate (quantum) level of description. Nor, accordingly, can I think of any form of realism attributable to Bohr's view of nature at this level, except of course that he believed that what we call "quantum objects" exist. How could one deny this, given that no concept of reality applies to such "objects"? To what would such concepts be inapplicable, then? And what would be responsible for the effects on the basis of which one would argue for this inapplicability?

Folse does not quite see Bohr's epistemology in terms of classical (knowable) effects, manifest in measuring instruments (or in other macro-phenomena) of nonclassical (unknowable) efficacities, defined by the interaction between quantum objects and measuring instruments, even though he (rightly) stresses the significance of this interaction, specifically for Bohr's concept of phenomenon. The lack of this or (one can think of different ways of handling the situation) a comparable epistemology sometimes prevents him from following Bohr's key concepts (such as "phenomenon" or "atomicity") and arguments to their limit. It also leads him to certain critical arguments against Bohr that are in fact, or in effect, answered by Bohr, for example, those concerning the identity of atomic systems ("Fuchs," #146, p. 31; #149, p. 33) or "properties" ("Fuchs," #149, p. 33).

On the first point, concerning the identity of atomic systems, I would argue as follows. One can have, and, in my view, Bohr does, a perfectly logically and data-wise consistent argument, whereby all individuality, identification (say, of electrons) and so forth are defined in terms of possible effects upon measuring instruments, without appealing to the properties or identity of quantum objects themselves. Bohr, incidentally, never says and, in my view, would not say, as Folse does, in reading him, that "complementary 'wave' and 'particle' phenomena are *complementary phenomenal appearances of the same atomic system*" ("Fuchs," #146, p. 31; Folse's emphasis). Both types of appearances are, of course, possible, but at least two experiments, and hence two different systems, rather than "the same system," would have to be involved in order to obtain two complementary phenomena. I leave aside, for the moment, further qualifications as concerns the possibility, or, as the case may be, impossibility, of a "wave" appearance of any given atomic system (defined, again, in terms of effects or phenomena, by a given initial detection or/as preparation). This may appear a bit too pedantic, but it may be shown that these nuances have major epistemological implications, for example, in the EPR case, implications sometimes missed by Folse.

On the second point concerning "properties," Folse is not wrong in saying that, in interpreting quantum physics, one cannot avoid speaking of "properties" at least at some level. One cannot, however, quite see this point, as Folse does, as indicating a problem in or even incompleteness of Bohr's argument. It is clear that all quantum-mechanical phenomena in Bohr's sense do possess "properties," classical-like properties, at the level of effects and indeed are defined by such properties, without—this is almost the whole point of Bohr's concept of phenomenon—the possibility of attributing any properties to quantum objects themselves or their behavior.

Let me note that David Mermin's argument concerning "correlations without correlata" may be generalized to "relations without relata" and related to Folse's remarks on properties. I might be bringing David's view closer to Bohr's than he himself has in mind, but one can see Bohr's argument in these terms in the following sense. While in classical physics observable phenomena (in the usual sense) can be properly *related* to and *correlated* with the observable properties of actual objects, in quantum mechanics, in Bohr's interpretation, observable phenomena can only be *correlated* with the behavior of quantum objects. It is not possible to establish the correlata of such correlations at the quantum level as properties of quantum objects or of their behavior, whether the quantum objects under observation or the quantum stratum of measuring instruments, through which the latter interact with quantum objects. This impossibility is intimated already in Bohr's original criticism of Heisenberg's "microscope" thought experiment in Heisenberg's original uncertainty relations paper of 1927, although it took the EPR argument and then a decade or so for this point to crystallize. It is this impossibility that eventually led Bohr to his redefinition of the term phenomenon in terms of the effects of the interactions between quantum objects and measuring instruments upon the latter (and thus, as effects, configurable in terms of classical physics properties of measuring instruments), something, again, under-appreciated by Folse.

What I find most problematic, however, is that Folse, in the essay in question and elsewhere, appears to attribute to Bohr the suspension, at least a possible suspension, of locality and to contrast Bohr's and Einstein's positions on this point, with obvious implications for Bohr's interpretation of EPR-type experiments. This argument is, I would argue, difficult to sustain, if possible at all, as opposed to the argument that Bohr and Einstein held sharply contrasting views concerning the independent (local) reality of physical description at the level of quantum objects. It is true, that, in this essay, Folse, following Don Howard's argument (which you cite as well, "Fuchs," #189, p. 41), attempts to assimilate Einstein's position on reality to his position concerning locality. This attempt is, in my view, not altogether successful, on either Howard's or Folse's part, although I do agree that the connection between reality and locality in Einstein's thought should indeed by explored further. I shall not address this subject here, since it is not required for my argument concerning Bohr's interpretation and specifically his view of locality (which derives from the overall argument outlined below). Bohr's interpretation is local and is in part designed to respond to Einstein's arguments concerning locality by offering a local interpretation of quantum mechanics. I do not think that one could even argue that Bohr's view is open as concerns the possible nonlocality of quantum physics. (I entertained such a possibility myself at some point, a few years ago, but a relatively quick "reality check" of Bohr's argument showed that even this weaker argument is difficult to sustain.) From this standpoint, I find Folse's (or Howard's) parallel assimilation of Bohr's wholeness (indivisibility) of phenomena to "nonlocality/nonseparability" problematic. This is a crucial, perhaps uniquely central point, since, in my view, it is Bohr's concept of wholeness of the phenomenon in his sense that enables the locality of his interpretation, rather than allows for even the possibility of nonlocality.

Indeed, if one reads the passage from Folse's essay that you cite, it may appear that by "separability" (of Einstein's concepts of reality) and, conversely, "nonseparability" (of Bohr's concept of reality), Folse refers, respectively, to the possibility of an unambiguous reference to the properties of quantum objects independently of—"separately" from—their interactions with measuring instruments vs. the impossibility thereof. The former is indeed desired by Einstein, while the latter is argued for by Bohr and defines his concept of the wholeness (indivisibility) of phenomenon in his sense, which is to say, his concept of phenomenon, since all phenomena are indivisible in this sense. Of course, by invoking "nonseparability" Folse has this indivisibility in mind as well. It is clear from the essay as a whole, however, that he also means by "nonseparability" more than merely "indivisibility" in this sense, in view of the two assimilations just mentioned, that of Einstein's view toward locality (which is possible, or at least is worth exploring) and Bohr's position toward a form of nonlocality, which is, I would argue, untenable. He means by it primarily (a form of) nonlocality. It is true that, in the passage you cite (and in general) Folse separates "locality" and "separability." First, however, he clearly argues that Bohr, in contrast to Einstein, does not see "locality" (rather than only "separability") as the necessary condition of "physical reality," which is incorrect in any event. Secondly, "separability" is first established by Folse as a form of nonlocality and only then is linked to Bohr's wholeness/indivisibility of phenomena. "Separability" appears to be seen by him as a physical independence of spatially separated and physically disconnected events, on which, I would argue, both Einstein and Bohr equally insist, and which Bohr also argues to be possible in view of his interpretation. Locality and nonlocality are seen in terms of, respectively, the impossibility and the possibility of instantaneous physical connections between spatially separated physical objects. It is clear, however, that nonseparability would make it, at least in principle, possible to use the resulting mutual dependence of spatially separated entities to create nonlocal connection, even if undetectable physically. Hence, it may be seen as a form of nonlocality, which makes it, again, unacceptable to both Einstein and Bohr (in the present reading).

In other words, while applied to the standard version and specifically to Bohr's interpretation of quantum mechanics, and thus making *both* nonlocal, Folse's view is ultimately that of proponents of Bohm's theory, which of course uses a different formalism. You will recall that in some versions of Bohmian mechanics, nonlocality, that is, violation of relativity, is never manifest, but is nevertheless always a strict mathematical consequence of theory. Folse's view is not unique, of course; for example, Henry Stapp argues for the nonlocality of quantum mechanics, which argument is, at least, unpersuasive, as his recent exchange with David Mermin shows as well, and of Bohr's view, which argument is simply untenable. The point is of considerable significance for the recent debates. The nonlocality of complementarity and, especially, of quantum mechanics itself, whether at the level of the data (such as the EPR correlations) or of the formalism would erase arguably the most crucial difference between the standard and Bohmian mechanics.

Folse's reading of Bohr's argument as, at least, tolerant of nonlocality (rather than his argument concerning Bohr's realism) was for me the main problem when I originally read the article (and a couple of his related pieces) a few years back. Perhaps I am becoming over-concerned with locality. A Bohmian I know (a philosopher, not a physicist) once asked me why I give so much importance to locality (leaving aside my argument that Bohr insists on it just as much as Einstein does). What if locality were not true at the ultimate level? This is of course not inconceivable in principle, even though so many things currently in place would have to go wrong (which has, however, happened in physics). I replied by repeating Einstein, when asked a similar question concerning the validity of general relativity, which of course safeguards locality as well: "Then, I would be sorry for the Lord." I guess it is all about the beauty of relativity. In any event, your email and, then, your "Compendium" prompted me to revisit Folse's argumentation and my own, ultimately opposing, argument, which I shall now sketch. It uses my argument in "Techno-Atoms" and in my Mykonos paper as background, but the main points should be clear, especially given the preceding argument. I shall comment on the question of reality first.

Here is my view of Bohr's position, which (perhaps, as I shall explain, taken a notch further) is also my own. Bohr does not and, for the reasons explained above, could not deny the existence—and in this, but only in this sense, "reality"—of quantum objects, nor (in contrast to the positivist or, to be cautious, naively positivist view) the relevance of the question. What he does deny is that any concept of reality, that is, of the properties and behavior of quantum objects, conceived on the model of classical physics is applicable to them, while such a concept is germane to classical physics, and is retained by Bohr at the level of measuring instruments. Indeed, which is equally crucial for Bohr, within certain limits, such concepts are germane in quantum physics, especially insofar as classical physics plays a role in quantum theory: the correspondence principle in its various incarnations; the classical description of measuring instruments and of the effects of their interactions with quantum objects upon them; and so forth. Thus, reality is suspended only as concerning the ultimate objects of description but is retained at the level of their effects upon what can be described in the realist way, such as the macro-world around us and, specifically, the measuring instruments involved in quantum physics. Heisenberg's "new kinematics" of his first paper introducing quantum mechanics already epistemologically reflects this argumentation and, while influenced by Bohr in turn, may well be the ultimate source of Bohr's conception.

This view appears to me to be close to Asher Peres's and your own position both in your article and your replies to letters in *Physics Today*. In truth, however, the best I can say about some of those letters would be to repeat Pauli's remarks on the commentary by the editors of *Nature* prefacing Bohr's article introducing complementarity (a much reworked version of the Como lecture): "*sancta simplicitas*." (As I was perusing your "Compendium," I have read and, in some cases, reread some of Peres's articles and his book on quantum mechanics, many of which I found quite helpful.) Some among the key aspects of Folse's argument concerning Bohr's realism are consistent with this view as well. Thus, he writes: "According to Bohr's concept of reality, real entities are entities that have power to interact, and in interacting, to produce the phenomena that comprise the natural world" ("Niels Bohr's Concept of Reality," "Fuchs," #135, p. 28). This is correct, although, again, it does not sufficiently discriminate, in the way Bohr does, between the classical and quantum levels of description (or, in the quantum case, undescribability), since this statement would apply to both, which may have been Folse's point. The differences, however, are hardly less crucial. In other words, here, too, Folse does not fully explore or take advantage of the finer structure or layering of the architecture of phenomena in Bohr's sense, which leads to a number of problems in his argumentation (such as those mentioned above).

Now, although Bohr does not present his argument in so general a way in his writings and perhaps does not take it that far (for most of his purposes he did not need to), it appears that one can take a step further here, to what may be called "the strong Copenhagen interpretation." Rather than only anything conceived of by analogy with classical physics, more radically, no conceivable concept of reality, or even any concept of "existence," or "object" and "quantum" (the latter originating in classical physics, to begin with) or, in dealing with multiple systems, possibly even "local" or "nonlocal," is applicable to "quantum objects." I would like to add that this impossibility of applying either determination, "local" or "nonlocal," at the quantum level does not mean that anything is nonlocal at the quantum level, and of course allows for the locality of all possible effects. By the same token, this view is different from those versions of hidden variables theories, in which the locality of all observable effects would be maintained as well, but which configure the underlying quantum reality as nonlocal. In accordance with this view quantum objects and behavior, including the ultimate nature of the interactions between them and the classical world (the world amenable to description in terms of classical physics) may be beyond our perceptive and conceptualizing capacities. It is through a refinement of these capacities that we constructed the conceptuality and mathematization of classical physics, even if (this is somewhat more complicated) not mathematics itself.

This last point, coupled to the strong epistemological position just outlined, "the strong Copenhagen interpretation," also suggests a better approach to Bohr's famous and often misunderstood insistence on the inevitability and necessity of using classical concepts in quantum mechanics. Bohr's argument to that effect is at the core of Schrödinger's letter to Bohr, which, as you say, "continues to haunt [you]" ("Fuchs Compendium," #200, pp. 43–44). You are, of course, not alone. This apparently excessive emphasis on the necessity of classical physical concepts appears to have been one of the most haunting aspects of Bohr's arguments for complementarity throughout the history of its reception. I think, however, that, eloquent as his letter is, Schrödinger misunderstands Bohr on this point, as do most other critics of Bohr. I shall spare you a reading of the letter itself, although such a reading would be instructive on several counts. Part of my argument concerning Bohr and quantum mechanics all along has been that the significance of reading in physics is often underestimated, and this concerns as much the works of Bohr's key critics as those of Bohr himself, notoriously demanding and inattentively read, by critics and admirers alike, as his writing is.

Indeed I am not certain whether Schrödinger sufficiently examined Bohr's overall argument, which is, I think, imperative in order to understand Bohr's point in question. In accord with his argumentation as here outlined, Bohr's argument is, more or less, as follows. Measuring instruments and their behavior, and, accordingly, the effects of the interactions between quantum objects and these instruments upon the latter, must be described in the classical manner in both physical and, correlatively, epistemological terms, while quantum objects and their behavior cannot be described at all, by any means, classical or other. This, let me reiterate, is not the same as saying that the behavior of measuring instruments under the impact of their interaction with quantum objects is described classically, while that of quantum objects themselves is described quantum-mechanically. In Bohr's view, quantum mechanics describes nothing, but only predicts the outcome of the interactions in question; and, again, most crucially quantum objects and processes, including those involved in the interaction between a quantum object under investigation and the measuring instruments involved in that investigation, are not describable by any means. Otherwise, among other things, it may not be possible to maintain locality, for example, in the case of the EPR correlations. The presence of this circumstance in quantum physics, which Bohr sees as uniquely significant to the difference between classical and quantum theory, is crucial to the rigorous description of any experiment, which hardly makes its manner classical—within Bohr's interpretation. In other words, Bohr (his reply to EPR is crucially at stake in this exchange) argues that we do have an interpretation of quantum mechanics, as complementarity, that is both complete and local, which, accordingly responds to the

EPR argument (it also specifically accounts for the specific experiment in question, or for "quantum correlations," as we say now). I shall return to the question of interpretation presently. For the moment, my main point is that Schrödinger does not quite see Bohr's point that the distinction in question is also necessary for locality, or, it appears, even completeness, and hence he missed the essence of Bohr's reply. He thought the question posed by the EPR unresolved and that new "concepts" (and perhaps even new mathematics accompanying them) were necessary. Bohr, by contrast, saw that old concepts could be used to solve the problem and to provide an interpretation of quantum mechanics as both complete, within its limits, and local. Einstein, by and large (there are some further nuances concerning locality), accepted this point, and rejected Bohr's interpretation on epistemological grounds. The epistemology rejected by Einstein was defined by Bohr's view that in quantum physics the classical description or describability in general of measuring instruments co-exists with the irreducible indescribability of quantum objects (and hence, the impossibility of classical-like reality at the quantum level). Schrödinger liked none of this epistemology either, but it appears that in his letter, or perhaps elsewhere, he does not get that far.

At this further juncture, the question becomes whether it is possible to find an interpretation or possibly a new theory (both Einstein and Schrödinger, and sometimes even Bohr, under-appreciate this crucial distinction) that is more epistemologically palatable, including by virtue of being local. Nonlocality would be epistemologically as unpalatable to Einstein or Schrödinger as the impossibility of a realist description at the quantum level. Ultimately, both appear to be nearly ready to give up on causality, but not locality and realism, which moreover they see as linked, in contrast to Bohr, who, accordingly, saw no such need, and, by and large, regarded the debate to be over. The question itself remains, I would argue, unanswered, whether by means of old or, as Einstein and Schrödinger thought possible, new physical concepts. Bohm's hidden variables offer an old-style alternative, but it is nonlocal, and Einstein did not like it on both grounds—the old conceptuality and the nonlocality of the theory. Einstein and Schrödinger looked for new (nonclassical) physical concepts to reinstate the epistemologically classical (realist and preferably causal) local theory. Bohr, by contrast, while seeking to preserve locality found a way to use classical concepts, but at the cost of a loss of classical epistemology or, it depends how one sees this philosophically, as a way, as a fringe benefit, of discovering the nonclassical epistemology. As part of this process he invents quite a few new philosophical concepts, such as complementarity. For the moment, I refer by "complementarity" to complementary physical description or experimental arrangements—mutually exclusive and hence never applicable simultaneously, yet both necessary for a comprehensive theoretical account—rather than to the overall interpretation of quantum mechanics.

The fact that, within Bohr's interpretation, quantum objects and their behavior are not, and cannot be, described by either classical or quantum-mechanical means, or, again, ultimately by any conceivably means, is crucial to Bohr's appeal to classical concepts, which link is usually missed by Bohr's opponents, Schrödinger among them, and indeed by most other commentators on Bohr, critical or sympathetic. For it follows that, whatever physical concepts we can ever conceive of could not be applicable for describing or even imagining, providing a (visualizable) intuition, *Anschaulichkeit*, concerning the properties of quantum objects and of their behavior. ("Pictorial visualization," often used, including by Bohr, in English is adequate and part of what is at stake, but is not quite accurate and strong here, as opposed to the German term, used by both Heisenberg and Bohr, while the Danish word, used by Bohr, is very close to German.) From this perspective, we may even define as *classical* whatever is conceivable and can serve for the purposes of a realist (and especially causal) *physical* description. Mathematical formalism (such as that of quantum mechanics, which cannot be seen as descriptive in this sense, already by virtue of its dependence on complex numbers, as Bohr observed on several occasions) is another question; and Bohr never speaks of any limits in this respect. Naturally, there are limits to what degree this type of mathematics, even two-dimensional complex Hilbert spaces (used in the case of spin) or indeed the field of complex numbers (the complex plane), let alone infinite-dimensional spaces of quantum mechanics, are available to our spatial intuition. (Even if they were, though, and I do not think they are, we still could not use them to describe the behavior of quantum objects, or indeed anything physical; hence Bohr's appeal to mathematical "abstractions" in quantum mechanics, in contrast to classical physics.)

Bohr does not perhaps entirely deny that new concepts could be and are being developed, but only that, no matter what new concepts or mathematical tools ("abstractions") we can and must develop, they will not bring us closer to describing or imagining the behavior of quantum objects. This argument would of course only remain valid unless the nature of the available experimental evidence drastically changes (say, uncertainty relations or locality would be no longer valid), or, I would add (Bohr might be a bit less cautious here), unless a different consistent and complete local interpretation of quantum mechanics or an alternative local theory is found. As Bohr says:

> Such argumentation does of course not imply that in atomic physics, we have no more to learn as regards experimental evidence and the mathematical tools appropriate for its comprehension. Indeed it seems likely that the introduction of still further abstractions into formalism will be required to account for the novel features revealed by the exploration of atomic processes of very high energy. The decisive point, however, is that in this connection there is no question of reverting to a mode of description which fulfills to a higher degree the accustomed demands regarding pictorial representations of the relationships between cause and effect [which obtains in classical physics]. (*The Philosophical Writings of Niels Bohr [PWNB]*, 3 vols. [Woodbridge, Conn.: Ox Bow Press, 1988), vol. 3:6)

One might indeed speak here of any pictorial representation, assuming that we could intuit anything in noncausal terms, which Wittgenstein, for example, if not already Hume and Kant, and certainly, Nietzsche questioned. In other words, there is a kind of (en)closure—enclosure and closure (not the same as "end")—of physical (or of course other) concepts, defined by our perceptual and conceptual capacities, and perhaps ultimately linked to biological and evolutionary nature. We cannot reach beyond this (en)closure, and we must depend on and (as both Bohr and Heisenberg grasped) can greatly benefit from this (en)closure even in our thinking about quantum objects, but, at the same time, this—beyond this (en)closure—is also where, in this interpretation, the nature of quantum objects and their behavior lies, forever hidden from us. The latter placement of the quantum is, again, in this interpretation, itself a consequence of the effects of the interaction between quantum objects and the classical world (including measuring instruments) upon the latter, effects whose understanding is, accordingly, within this (en)closure and is available to us. Classical physics is a refinement of these capacities, and there is of course a question of how far it reached on that road, but it would not affect the argument, here presented, concerning placing quantum objects beyond this limit in Bohr's interpretation, no matter how much further such a refinement reaches. Perhaps it defines, if not the end of this road, a certain limit, a "closure," indicating that we can only go so far and only along certain lines, perhaps incrementally infinitely, but the limit and the closure are implied, even if not expressly determined. Possibly, it is quantum physics that establishes this limit. Bohr's position on this point is not altogether clear, except, as the above statement suggests, on the point that quantum mechanics is beyond this limit, beyond the "cut." (I am of course not using this term in its strictly quantum-mechanical sense, but one can contemplate certain links between both usages.) As explained above, the overall argument just outlined remains, perhaps against Bohr's view (one can read him either way), conditioned by the particular interpretation here adopted, the strong Copenhagen interpretation, that is, such are epistemological consequences of this interpretation, on interpretation necessitated in part by the locality requirement.

Thus, Schrödinger, just as Einstein, wants new concepts to return to the epistemologically classical models in physics, which would mathematically represent, in however idealized a way, independent and, it appears, causal physical reality. By contrast, according to Bohr's interpretation (in this strong form), the appeal to classical concepts, beyond the practical role, reflects the impossibility of having such models. The abstractions (in many ways, and indeed, as Bohr anticipated, ever more radical) of the quantum-theoretical mathematical formalism, from the original quantum mechanics to the field theories of the current standard model, enable excellent, even if only statistical, predictions under these conditions. This is a kind of miracle, and an epistemological enigma, since, in contrast to classical physics, there appears to be no physical justification why they do so, and in this interpretation there cannot be, in part because the interaction between quantum objects and measuring instruments is itself quantum and subject to the same nonclassical epistemology.

From the present perspective, Bohr's interpretation, which may be called "reciprocal-interactive," appears as somewhat different from Wheeler's more or, at least, still more, "participatory" or "observer dependent" view of the quantum world, which may be ultimately closer to Pauli's, although, in my view, it is uneasily suspended between Bohr's and Pauli's views. The question is rather subtle and I should only make a few "background" points against which it could be considered, rather than addressing it as such, since to do it justice would require an engagement with Pauli and Wheeler on a much greater scale. One cannot, however, stop short of anything but admiration for the grandeur of Wheeler's vision, which is well conveyed in your selection of quotations, let alone the works themselves cited.

I would like to comment briefly on Wheeler's discussions of the delayed-choice experiment, such as that in "Law without Law" (in J. A. Wheeler and W. H. Zurek, *Quantum Theory and Measurement* and elsewhere, for example, in "'A Practical Tool,' But Puzzling Too," *New York Times*, 12 December 2000, which you cite, "Fuchs," #383, p. 74). His analysis does not appear to me (perhaps I miss something) sufficiently to take into account the architecture of phenomena in Bohr's sense. I, again, refer most especially to the wholeness-indivisibility of phenomena, in view of which we cannot attribute any properties, such as wave-like or particle-like ones, to quantum objects themselves even when a phenomenon becomes, using Bohr's term favored by Wheeler, "registered" as the result of an observation. This view would help to refine Wheeler's argument that our observation of certain quantum events that took place in the past, possibly a very distant past, in no way influences what had happened at the time of such events, even though we have a (delayed) choice in setting our equipment. This choice allows us to "observe" a different "past event" depending on a given setting. Wheeler accurately puts the same type of quotation marks throughout his discussion. Or, one can put it more accurately, and it is, I think, here that Wheeler's analysis could be refined and, I think, it would be by Bohr, as appears from his statement on or anticipating the delayed choice setup in *PWNB* (Vol. 2, p. 57), cited by Wheeler and Zurek in *Quantum Theory and Measurement* (p. 778). The choice in question allows us to create different phenomena in the sense of what will have classically manifest itself in our measuring instruments at the time of measurement (see, for example, Wheeler's design in "Law without Law," *Quantum Theory and Measurement*, p. 193), without, however, claiming anything concerning the properties of quantum objects or of their behavior. For example, we can make no claim as to how the photons in question in Wheeler's design actually traveled before leaving their traces in our measuring instruments. By the way, Wheeler's article in *The New York Times*, which you cite, contains, in my view, an inaccurate statement, unless Wheeler's whole interpretation is off, at least as against Bohr's. Wheeler says: "if you measure the departing photon in a different [and mutually exclusive] way (a complementary way), you can tell if it *took both paths at once*" ("Fuchs," #383, p. 74; emphasis added). My understanding (which follows Bohr's and most other versions of the Copenhagen Interpretation) is that in such cases we *cannot know* through which route the photon has passed. This lack of knowledge is reflected in the effects of the experiment, such as, once we repeat the experiment with a large number of photons, the interference-like pattern on a silver screen in the double-slit experiment. (In the other complementarity observation measurement case we can establish through which "route" the photon travels.) In "Law Without Law," Wheeler's puts "routes" in all cases in quotation marks (*Quantum Theory and Measurement*, p. 192), which may imply only a symbolic assignment of routes. Even so, I am not altogether happy with his usage of "both routes" in ever speaking of a single photon without qualification.

Nor could we argue, counter-factually, concerning what could or would have happened to the same photon, if we had set our equipment differently. Indeed, it appears that we cannot use counterfactual logic of that type in quantum mechanics without reinstating nonlocality, even though we can, of course, speak of different possible outcomes of future experiments, or of possible, that is, possibly obtainable, information concerning future events without introducing nonlocality. This delayed-choice experiment seems to invite the former (counterfactual) reasoning, and Wheeler needs all the help he can get from Bohr to escape its traps, nonlocality included, but, to his great credit, by and large he manages to do so.

In sum, we can never "see" what happened with quantum objects as such in the past, nor argue, it follows, always counterfactually, on the basis of what would have but did not happen. We can only predict (with certain probability) what will happen in future experiments we can perform, future effects of the interactions between quantum objects and measuring instruments upon the latter, on the basis of previously performed experiments and previously observed or measured effects of the same type.

From this viewpoint, one can speak of the "constitutively participatory measurement" in, and defining, quantum physics, or indeed of "the activating observer," as you do. I am not sure, however, in what sense one can speak, as Wheeler does of a "participatory universe," even if in quotation marks and even if designating that the sense in which the universe is "participatory" is "strange" ("Law without Law," *Quantum Theory and Measurement*, p. 194). Unless, of course, he merely means that it is the universe that participates in these interactions, but the latter would be equally true in classical physics. "The activating observer" would indeed be more accurate, since, while there is some reciprocity to these relationships, they are not exactly symmetrical.

While I am on the subject, let me point out that Pauli, in my view, also misses certain key nuances of Bohr's concept of phenomenon and his argument concerning the indivisibility of phenomena. Thus, I cannot agree (nor I think would Bohr), in any event not without further qualifications (not offered by Pauli and not apparent from the article as whole), with the following statement in "Phenomenal and Physical Reality": "[E]very act of observation is an interference is of undeterminable extent, with the instruments of observation as well as with the system observed, and interrupts the causal connection between the phenomena preceding and succeeding it" ("Fuchs," #257, p. 56). There is no such "causal connection," or there would not be in Bohr's view of the situation, and, in all rigor, there is never either a phenomena (in Bohr's sense) of either preceding or succeeding an act of measurements, but only such acts. Each such act gives rise to a phenomenon (no phenomena could appear otherwise) and such acts rarely, if ever, can present any "sequence." Instead they entail repetitions using different quantum systems. It seems to me that Pauli here thought of "phenomena" in terms of reference to the properties of quantum objects, or in any event he does not sufficiently elucidate the point. If, however, he did think in this way, he could not have properly or at least in full measure understood Bohr's counter-argument to EPR and related arguments by Einstein, since Bohr's argument does not work unless it is assumed that all observable quantities or references to physical attributes pertain to measuring instruments and only to them. The immediately following sentence in Pauli's elaboration, "The gain of knowledge by means of an observation has as a necessary and natural consequence the loss of some other knowledge," is not altogether accurate either. At the very least, one should, I think, say "the loss of other possible knowledge." The point might, again, seem minor, but everything here is in finer micrological details and nuances.

As concerns the "participatory universe," I would say, in general, that such statements as nature "responds" (the quotation marks are imperative) to the kind of questions we ask (no quotation marks are necessary), and hence is defined by the latter participation on our part, can only be used metaphorically and, even then, with much caution. We "participate" in nature only within the limits of our interaction with it, for example, by means of scientific experiments, even though this "participation" can take us far, as our knowledge, or what we learn we cannot know, is concerned. Of course this participation also defines any conceivable view of reality, or, again, the impossibility thereof, we can have, for example, the possibility of describing much (not all!) of the macroworld in terms of classical physics, or (this difference is crucial in the present context) using quantum physics to learn how the quantum micro objects affect the macro world. (Some quantum effects, such as those manifested in Josephson's devices, can of course appear at the macro level, but they would still be due to the micro constitution of nature.) The so-called "unreasonable" (we merely exclude other things as ineffective) "effectiveness of mathematics" belongs only to this level, as do certain archetypal correspondences (which attracted Pauli). Heisenberg, in his later view (although one finds anticipations of it in his earliest works), combined both of these ideas and refined their application closer to Bohr, in arguing as follows:

> If we attempt to penetrate behind this reality [the spatio-temporal reality of classical physics, or of our perception, to begin with] into the detail of atomic events, the contours of this 'objectively real' world dissolve—not in the mist of a new yet unclear idea or reality, but in transparent clarity of mathematics whose laws govern the possible [the outcome of experiments?] and not actual [what actually happens?]. It is of course not by chance that "objective reality" is limited to the realm of what Man can describe simply in terms of space and time. At this point we realize the simple fact that natural science is not Nature itself, but a part of the relation between Man and Nature, and therefore depends on Man." ("Fuchs," #176, p. 38)

This is not quite the way I (or, I think, Bohr) would put it, as my interpolating parentheses indicate, but it is essentially correct and, in its own way, admirable and philosophically appealing (Heisenberg clearly speaks here with a broader audience in mind). In any event, the ultimate "workings" of "nature" are in no way participatory in the present view, even though, and because, they make any participation on our part possible. By contrast, quantum mechanics, as a mathematical-experimental science of nature, is irreducibly participatory. It appears to me that (for perhaps different reasons) both Pauli and Wheeler blur this difference, or want to dispense with it. I prefer, however, to refrain from any definitive claim, since on a further reading this "appearance" or suspicion may yet dissolve. Some of Heisenberg's elaborations, such as the one cited above, may seem to be a form of the mathematical Platonism of nature, while in fact they are not. Plato's own view is yet another story and may even be closer to Heisenberg's and Bohr's than it appears, as Folse, incidentally, suggests by juxtaposing Democritus's and Plato's atomism, although I am, again, not in agreement with his reading of Bohr's atomism itself ("Fuchs," #129, pp. 25–26).

A (in my view) related remark by Heisenberg that you cite is also important here: "It is quite wrong to try founding theory on observable magnitude alone. In reality the very opposite happens. It is the *theory* which *decides* what we can observe. You must appreciate that observation is a very complicated process" ("Fuchs," #177, p. 38; emphasis added). One must qualify Heisenberg's point here (actually it is Einstein's). First, "the theory" ought to refer to much more than a given mathematical apparatus, but instead to the whole conceptual framework of which the latter is a part, and indeed one may need to speak of a broader "plane of mental immanence" where these concepts emerge (I am not sure one can call this plane "theory") that shapes "the theory." Second, it would be more accurate to say that the theory *shapes* what we observe and *decides* what observations are included, unless, again, we refer by the "theory" to a broader plane of immanence, just mentioned, in which case the theory might indeed be seen as *deciding* here. The remainder of the (important) elaboration that you cite might be shown to illustrate this point.

The main point that I want to make is as follows. The view here expressed by Heisenberg may appear to be in conflict with Heisenberg's approach in his first paper on quantum mechanics, to which he clearly refers here, and stressed the "magnitudes, which in principle are observable," in other words, more or less individual quantum effects in the above sense. The effects, rather than properties of quantum objects and of their behavior, that became subject to his "new kinematics." There is no contradiction, however, and, as is well known and well documented, Einstein's point concerning the theory deciding what can be observed, was indeed was guiding Heisenberg in his work. For his theory was not founded on such magnitudes alone; nor of course was Bohr's complementarity. Heisenberg's famous, but not always carefully read, opening statement (a kind of abstract, but much more than that) is worth citing: "The present paper seeks to establish a basis for theoretical quantum mechanics founded exclusively upon *relationships* between quantities [magnitudes] which are *in principle* observable" (emphasis added). "Relationships" is the key word here, and the title of the paper was, we recall, "On Quantum-Theoretical Re-Interpretation of Kinematic and Mechanical Relations." "In principle" is quite crucial too, for, no matter how theory-laden and how complicated the processes of observation, the magnitudes in question could, in principle, be observed and, as it were, "kinematized," while the classical-like physical (and ultimately any) properties of quantum objects and of their behavior could not. In other words, even leaving aside for the moment the theory-laden character of all conceivable data (equally including that of classical physics), dealing with such, "in principle observable," magnitudes is not the same as founding the theory on them, and Heisenberg's paper was not doing the latter. While working with the available data of quantum physics (such as the Rydberg-Ritz formulas and the Bohr frequency relations), his theory qua theory was founded above all on Bohr's correspondence principle. The latter was used to argue both that for large quantum numbers the data becomes the same as it would be in a classical case, at least as far as predictions are concerned (the description could, in all rigor, no longer be the same). The principle was also used to argue, in part correlatively, that the equation should be formally the same as those of classical mechanics, the Hamiltonian equations. (For large quantum numbers these of course would give correct predictions classically.)

It is this combination, this "lethal combination" (as far as classical physics is concerned), that leads to most remarkable properties of quantum mechanics, such as Bohr's probability rules, uncertainty relations, and so forth, and mathematically (in part correlatively) the irreducibility of complex numbers and replacing functions with operators as the kinematical and dynamical variables of the theory. Both Dirac's and von Neumann's schemes are more or less automatic translations of Heisenberg's matrix mechanism. Indeed, Heisenberg's stroke of genius (not altogether unprepared by, among others, Bohr, but a stroke of genius nevertheless) was itself a founding theoretical move. That is, this arrangement of the relationships between observable magnitudes, made moreover into complex, rather than real, numbers (never observable as such), into infinite matrices is already *theory*, not observation. (You may also recall that these matrices must be infinite in order to derive uncertainty relations.)

I, once again, apologize for not having enough to say on Pauli. To me, his statement, which you cite from his essay "Matter" (but the statement, with minor variations, recurs throughout his lectures): "Like an ultimate fact without any cause, the *individual* outcome of a measurement is, however, in general not comprehended by laws" ("Fuchs," #251, p. 54), has always been his greatest and most significant statement on quantum theory. Indeed, it may well have been also on much that is beyond it, perhaps ultimately on life, or life/death, the randomness of whose occurrences, including in his own life, Pauli (I could hardly doubt this), was also contemplating here. But then, Pauli appears to have been reluctant to separate life and quantum theory, anymore than Kepler wanted to separate it from his music of the cosmic spheres, either of which would indeed be difficult to do, at least in their own cases.

These are a few more or less immediate thoughts prompted by reading your "Compendium," and clearly the stakes continue to be as enormous as ever, after three quarters of a century of debates, which may never end, and the reasons (I can think of several) why they may not end are worth considering, but cannot be addressed here. I am, once again, grateful to you for allowing me to read it, and of course for putting it together. As I said, the work, while seemingly consisting of citations, has a life and argumentation of its own, not reducible to the sum of citations, if indeed we can ever sum them up. Perhaps, my sense of it is best captured by my reaction to Wheeler's remarks in his letter to Carrol Alley: "Today, the physics community is bigger and knows more that it did in 1939, but it lacks the same feeling of **desperate** puzzlement. I want to recapture that feeling for us all, even if it is my last act on Earth" ("Fuchs," #380, p. 73). Whether with Wheeler's (and Pauli's) help or not, I think your "Compendium" does bring the right sense of deep puzzlement, or perhaps better of profound complexity, whose resources are far from exhausted, and we have yet barely touched the epistemology of quantum field theories. I am not sure one needs, at least at this point (perhaps ever) to feel desperate, or even **desperately puzzled**. Perhaps one needs more resolve to move forward from where quantum mechanics has already brought us and, of course, use what it gave us, in physics and philosophy alike—*increscunt animi, virescit volnere virtus*, the spirit grows, strength is restored by the wounds it receives, as Niezsche would have it. One can hardly doubt that quantum mechanics inflicted one of the deepest wounds upon the spirit of physics; but physics survived, and more than merely survived; and, this is part of Nietzsche's meaning, what does not kill us sometimes (not always) can make us stronger. In my view (I know that not everyone shares it, but I think that Bohr would), in this case it did. Naturally, one could hardly doubt this either, this growth will bring us, it has already, new, yet deeper puzzles and puzzlement, but they will be, and some already are, new.

29-05-01 *'Typo in Note 6' and 'Worst of All'* (to N. D. Mermin)

Wootters! Wootters!

There are two t's.

29-05-01 *Bayes and Lorentz: Never the Twain Shall Meet* (to A. Peres)

I still owe you some comments on your paper. I guess in the end, I don't have much to say. I agree with its content of course. Not least of all, because you taught me long ago that the correct analogy is between quantum states and Liouville distributions, and not between quantum states and phase space points. For me, that means that both objects—quantum state and Liouville distribution—are subjective entities, and can therefore be changed by the whims and the fancies of the observer. Their valuations are not tied to physics per se. The learning of information always presents just such an opportunity: there is no law of physics that says that all observers must agree in their Liouville distributions for a given system. Measurement can lead to very nice inequities in that regard: an application of Bayes' rule is not Lorentz invariant, but then again it is not even *observer* invariant. We apply it when we gather information; we don't apply it when we don't.

But the question, I must ask is: what new point did you make in this paper? What did you say here that was not clearly in your mind when you wrote the RQM paper, for instance? (I can even find our discussion of similar points in the samizdat, pp. 270–276.) The answer to those questions don't come out clearly in your present exposition.

29-05-01 *No Miracles* (to A. Peres)

Asherism 12: *Exophysical agents are essential, and most people tend to sweep them under the rug.*

Yeah, you might say my *Notes on a Paulian Idea* is about looking under the rug.

29-05-01 *Substantiation* (to N. D. Mermin)

What particular piece in the paper directly substantiates the sentence: "It is possible to eliminate all couplings between the source and the destination because quantum qubits have a richer range of logical capabilities than do classical bits."

Have you tried explicitly walking through all your circuits with a classical Liouville distribution rather than a quantum state? That is to say, in Fig. 2 for instance, let $|\psi\rangle$ stand for a (Bayesian) probability distribution over $|0\rangle$ and $|1\rangle$, rather than a pure quantum state. Then you would have a classical state-swapping circuit: where the word state now means without doubt "state of knowledge."

What goes break and where goes break in getting from figure 2 to figure 10 in such a classical setting? Well, nothing of course, because we know that teleportation also works for mixtures. But it does probably mean that some components in the circuit are probably more "quantum" than they need to be to carry out such a classical project. (I.e., the project of teleporting a classical state of knowledge, via purely classical resources.)

Can you pinpoint where that occurs? It has now struck me that your view of teleportation may be a good laboratory for sussing that out. I'm warming up as a referee.

Today Renes arrives, and this evening Caves arrives, and ... and next Tuesday I have committed myself to lecturing on Holevo's channel capacity result, and—between all of that—somehow, someway, some pig (Charlotte's web), I must complete the NATO paper. I keep hoping for a miracle: none occurs.

You've never read it, I know, but you should read it: de Finetti's paper "Probabilismo," written in his youth while in a fascist fervor. Have the mental strength to divorce it from the fascism, and you will find it is an absolutely wonderful piece: B. de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989). [See also R. Jeffrey, "Reading *Probabilismo*," Erkenntnis **31**, 225–237 (1989).]

I've come to think of the NATO paper as my own Probabilismo. I just wish I had the youthful fervor.

31-05-01 *Quotes and Barbells* (to H. J. Folse)

No, there is never a worry in quoting me. I say right things, and I say wrong things, but I never say anything without the intention of lifting the veil of the quantum. I am more than happy to be quoted if my right/wrong thing contributes to that.

We don't have a program worked up yet: Unfortunately, both Khrennikov and I have been hugely tied up for the past weeks. But we started the process yesterday—in fact—so maybe something will come out by mid-next week. In any case, I'm fairly sure at this stage you can count on a 30 minute talk. I think we all can; there have been several additions to the conference (Bernstein, Greenberger, and several others) even though a few dropped off the list (Shimony, Brassard, Preskill).

I printed out my samizdat for the first time yesterday. It's a frightening thing, seeing it bend the table. I don't suppose you've had any luck in retrieving it. Maybe I'll just break down and send you loose-leaf copy of it. Can you send me your mailing address so I make sure I send it to the right place.

03-06-01 *Quotes* (to me)

- "Only those who will risk going too far can possibly find out how far one can go."
  – T. S. Elliot

- "Opportunity is missed by most people because it is dressed in overalls and looks like work."
  – Thomas Edison

06-06-01 *Comments and Answers* (to N. D. Mermin)

Wow. Thanks again for the very thorough reading! (And concern over my welfare.)

I reply to all your comments below. You've certainly left me hungry for more.

Merminition 11: *I didn't really get the epigraph ("imprimatur") and the apparent explanation of it on p. 2. Is that a coy reference to the bad-boy tone?*

Which meaning of coy did you have in mind?

1. Tending to avoid people and social situations; reserved.
2. Affectedly and usually flirtatiously shy or modest.
3. Annoyingly unwilling to make a commitment.

Merminition 12: *What is an Einselectionst? (p. 2.) Everything else is familiar, but, irritatingly, it's the one school you don't explain in footnote 2. Is this a (bad) joke you're playing on the reader?*

I'm letting that one stand: it was meant to be a little insulting. I can't figure out what their views are.

Merminition 13: *p. 2. You should think twice about sentences like "If I am ever going to get ... someone else's war." I know it's true, that physics would be a lot more fun if people weren't trained to hide such sentiments, etc. But it does have a certain self-indulgent quality.*

Stripped away with surgical precision ...

Merminition 14: *p. 4. Say the whole thing: 1) the speed of light IN EMPTY SPACE is [constant,] independent of the speed of its source. (Einstein did not say "is constant" — it's not necessary.) Keep "is constant" if you like, but why leave out his "in empty space" — that's crucial.*

Oh, alright!

Merminition 15 : *Bottom of p. 4. Can you explain classical E&M to a junior high-school student giving the essence, not the mathematics? Classical mechanics? Thermodynamics? I've always thought relativity was special that way.*

No. Not yet. Maybe. Me too. I.e., I consider E&M much more like the setting of a particular Hamiltonian within QM: And I wouldn't expect a high-school student to be in a position to understand why we have chosen the particular Hamiltonians that we have in any practical situation. Since the overarching belief here is that "quantum mechanics is a law of thought" (with an almost trivial side-input of honest-to-god physics), it strikes me as being in a category much more like relativity. In the words of J. Bub, it is a "framework theory."

Merminition 16 : *Also p. 5, I've been meaning to ask you: why is H a vector space if the scalars are real or complex, but a module if they are quaternions? As I remember "module" has a precise meaning — maybe having to do with the scalars lacking inverses — which is not satisfied if they're quaternions. Isn't H still an ordinary vector space?*

According to Adler's book, it is a module. Why one makes such a distinction, I don't know: I just followed convention.

Merminition 17 : *Is footnote 5 on p. 7 a little too cute? [Just asking.]*

Let me think about this one. I added the footnote first as a joke to myself. But then I decided I sort of liked it. The motivation came from Bennett having no clue about what imagery I was trying to convey with zing.

Merminition 18 : *p. 11. I find your brief knee-fortifying rejoinder to Penrose somewhat of an anticlimax. If you had added explicitly that the answer to Penrose is that it's all about the RELATION between Alice and the qubit (even without invoking correlations without correlata) I would have thrown away my Ace bandage.*

Yeah, I thought it was a bit of an anti-climax too. I continue to search for ways to strengthen my rejoinder to Penrose and Jozsa (the z comes first).

But indeed you and I differ on the climaxes we seek. As far as I can tell, you seem to seek a predominantly static worldview. I want to see true becoming ... I think.

Perhaps I said something closer to what you'd like me to say presently in the Samizdat, pages 407–408 (in a note titled Penrose Tiles). Have a read of that, and tell me whether that meshes with what you would have liked to hear. Who knows, I may well reconsider (based on the old text).

Merminition 19 : *p. 11. I'd never seen so direct a statement of correlations (between Man and Nature) without correlata in Heisenberg before. Nice quote. Where's it from?*

W. Heisenberg, "The Development of the Interpretation of the Quantum Theory," in *Niels Bohr and the Development of Physics: Essays Dedicated to Niels Bohr on the Occassion of His Seventieth Birthday*, edited by W. Pauli with the assistance of L. Rosenfeld and V. Weisskopf (McGraw-Hill, New York, 1955), pp. 12–29.

> The criticism of the Copenhagen interpretation of the quantum theory rests quite generally on the anxiety that, with this interpretation, the concept of "objective reality" which forms the basis of classical physics might be driven out of physics. As we have here exhaustively shown, this anxiety is groundless, since the "actual" plays the same decisive

part in quantum theory as it does in classical physics. The Copenhagen interpretation is indeed based upon the existence of processes which can be simply described in terms of space and time, i.e. in terms of classical concepts, and which thus compose our "reality" in the proper sense. If we attempt to penetrate behind this reality into the details of atomic events, the contours of this "objectively real" world dissolve—not in the mist of a new and yet unclear idea of reality, but in the transparent clarity of mathematics whose laws govern the possible and not the actual. It is of course not by chance that "objective reality" is limited to the realm of what Man can describe simply in terms of space and time. At this point we realize the simple fact that natural science is not Nature itself but a part of the relation between Man and Nature, and therefore dependent on Man. The idealistic argument that certain ideas are *a priori* ideas, i.e. in particular come before all natural science, is here correct. The ontology of materialism rested upon the illusion that the kind of existence, the direct "actuality" of the world around us, can be extrapolated into the atomic range. This extrapolation, however, is impossible.

Merminition 20: *p. 11. "wake and dream"? waking and dreaming?*

I left that alone. The *American Heritage Dictionary* has a huge usage note associated with "wake." I took that to give me license, plus I like the flow of it.

In general, I think that paragraph is the deepest part of the whole paper. It is the Paulian idea.

Merminition 21: *p. 13. Gleason derives the state-space structure? What do you mean? He talks about projections. What are they projecting on? What does "tr" mean? Isn't the state-space structure already there? You yourself begin by saying "Let P_d be the set of projectors associated with a . . . Hilbert space . . . "*

$$\text{state space structure (for me)} \quad = \quad \text{convex set of density operators}$$

The Hilbert space you are talking about—again for me—only has significance in that it defines the set of potential measurements. The quantum state, and with it the state-space structure, is a secondary notion.

But I will try to ward off confusion by adding the definition above to the text.

Merminition 22: *p. 16. "sum total OF ways" I wrote in the margin around here that noncontextuality for POVMS seems to be an enormously stronger assumption than noncontextuality for projections . . .*

Yes. But so what? The point is—physically—the assumption is equally deep for both notions of measurement. Noncontextuality IS Bayes' rule. (Or just about so much.) It's just that applying it to POVMs actually gets you somewhere (without having the mathematical skills required to solve one of Hilbert's problems).

06-06-01 *The Haunting* (to N. D. Mermin)

You have this habit of haunting me: even when I say I'm not going to listen to you, I end up listening to you. Demon!

Einstein was the master of clear thought. I have already expressed my reasons for thinking this in the arena of electromagnetic phenomena. Likewise, I would say he possessed the same trait when it came to analyzing the quantum. For even there, he was immaculately clear and concise in his expression. In particular, he was the first person to say in absolutely unambiguous terms why the quantum state should be viewed as information (or, to say the same thing, as a representation of one's knowledge).

06-06-01 *Reading de Finetti* (to H. M. Wiseman)

Carl Caves is visiting Bell Labs, and yesterday he shared with me some correspondence he's had with you about the quantum de Finetti paper. Thanks a million for the interest!

I just wanted to make one comment on one of your comments. You asked about the deeper reasons for my mean-spirited, off-hand remark about "for a stretch of the imagination … ".

Wisemanism 1: *But of course the Bohmian particles behave in a nonlocal way, so this means that our experience must be capable of nonlocal influence. That is, we cannot rule out faster-than-light information transfer, if information is transferred when a being becomes conscious of it. (Is this a reasonable concept of information transfer? What a tangled question! I will assume it is.) So in fact Bohmian mechanics is not even compatible with relativity, unless conscious systems are constrained somehow so as not (or at least with vanishing probability) to have nonlocal effects. This seems an ad-hoc hypothesis and even if it can be shown to work for us, I don't see how it could be proven in general.*

Actually, something like this has been one of my longer-standing pet peeves with Bohm theory. In particular, I've always thought it absolutely contrived that the nonlocal components in the theory could not be made use of. That they could not be turned into a technology, say. In that capacity, the Bohm theory seems to me to be no deeper than saying "God wills each and every event in our quantum world." It is true that the theory has a sheen of equations—which a religion normally doesn't have—but beyond that, I see no great difference. Somehow the mere possibility of writing down an equation has been deemed to be an acceptable state of affairs in the Bohmian community. But if the parameters in that equation cannot be set by the experimentalist (with a sufficient amount of effort), then, in my mind, it is no different than a burning bush proclaiming, "I am that I am."

Now, not all Bohmians, believe that the particle trajectories cannot be controlled at all. Antony Valentini is a notable exception. And he has also studied the non-uniqueness of the trajectory equation at length, hoping one day to put the various options to experimental test. (I don't have any references for his papers, but I see he has at least one paper on `quant-ph`: So you can probably find references therein.) But it has been my experience that the vast majority of the Bohmians (or the Goldstein, Duerr, Albert flavors thereof) see no need to go to the trouble of actually calculating a particle trajectory. It is enough, for them, to believe it exists.

I had not realized before that you have an interest in quantum foundations. Lately I've been trying to energize the community to think hard about "quantum foundations in the light of quantum information" and have organized two meetings to that effect. (One in Montréal last year, and one coming up in Sweden at the end of next week.) There is a good chance that Brassard and I will be doing still another in Montréal next summer. If you have an interest in coming, I'll surely make sure that you get an invitation.

06-06-01 *Bohm and the Burning Bush* (to H. M. Wiseman)

Wisemanism 2:

> [Chris said:] Somehow the mere possibility of writing down an equation has been deemed to be an acceptable state of affairs in the Bohmian community. But if the parameters in that equation cannot be set by the experimentalist (with a sufficient amount of effort), then, in my mind, it is no different than a burning bush proclaiming, "I am that I am."

But that is because you have such a strong instrumentalist philosophy (is that a fair description?). I'm in favour of giving a theory a fighting chance of producing its own interpretation. After all, wasn't gas theory criticised on your sorts of grounds a century or so ago?

No, most of the gas laws were found empirically at first. Then theory was tested quite well, by working explicitly in new regimes of pressure, temperature, etc. Quantum theory, too, has been tested empirically. We have means of preparing quantum states and checking how they evolve. We don't have the same means—so we are told by Goldstein and company—for (Bohmian) particle positions.

Wisemanism 3: *But I'd certainly like to understand an informational view of QM (and stat mech too, for example; do you think they are related?) better.*

Yes. That was a good bit of the point of the de Finetti paper.

07-06-01 *Frost* (to N. D. Mermin)

Just found this in my quote of the day:

> Poetry is a way of taking life by the throat.
>
> Robert Frost (1874-1963), U.S. poet. Quoted in: Elizabeth S. Sergeant, *Robert Frost: the Trial by Existence*, ch. 18 (1960).

I see you wrote me a note last night at 10:17. I'll have a look at it.

07-06-01 *The Mud of von Neumann* (to N. D. Mermin)

Merminition 23:

> [CAF wrote:] Forget about the larger Hilbert space. It is artifice. It is only historical accident that has confused us for so long.

Now that I've added the discovery of POVMs to my discovery of the moons of Jupiter I'm less inclined to object to this. Indeed, I'd like it to be so. But if I give you a resolution of the (2 dimensional) identity for a single qubit into 17 positive operators, can you always tell me how to set up a corresponding procedure with 17 distinct outcomes without enlarging the system to one described by a larger Hilbert space?

The point is: If I give you a resolution of the (2 dimensional) identity for a single qubit into 2 projection operators, can *YOU* always tell me how to set up a corresponding procedure with 2 distinct outcomes without enlarging the system to one described by a larger Hilbert space?

Von Neumann didn't know how. He introduced the notion of a "measurement model," enlarged the Hilbert space, and got us into the muddle most of us are still in today. Now, people like Zurek are going around trying to justify the "pointer basis" on von Neumann's ancillary Hilbert space by enlarging it still further (and calling it "the environment"). And I'm sure you'll recall that von Neumann himself did even more dastardly things.

You just have to get into a different mindset. My attitude is it's time to cut the Gordian knot. That's what Section 4 and 5 are about. To the extent that one admits a mystery to the standard (von Neumann, orthogonal projection-valued) notion of measurement, one gains *nothing* by holding tight to it. One might as well transfer the mystery to the POVMs and be done with it. The advantage of the POVMs is that they are a conceptually simpler structure: the old von Neumann notion is just a horribly contorted surface of constraint within that beautifully smooth space. What is a measurement? A refinement of one's state of knowledge, full stop. Any refinement whatsoever? Yes, any refinement.

Let me give you a homework exercise. (Now I'm going to sound like Gottfried.) Go back to a classical setting where you have a probability distribution $p(h, d)$ over two hypotheses. Marginalizing over the possibilities for d, you obtain an initial state of knowledge $p(h)$ for the hypothesis h. If you gather an explicit piece of data d, then, using Bayes' rule, you should update your knowledge about h to $p(h|d)$. The question is this: Do you not find that transition $p(h) \longrightarrow p(h|d)$ a mystery you should contend with? Does it not bother you that if someone asked you for a *physical description* of that transition, you would be at a loss for words? I mean, after all, one value for h is true and always remains true. One value for d is true and always remains true. There is no transition for those variables. The transition is in your knowledge (or belief, if you will). Should we not have a detailed theory of how the brain works before we can trust in the validity of Bayes' rule? (I ask that rhetorically of course.) Should we close all the gambling houses in Nevada, on the suspicion that they know better of Bayes' rule (and its limitations) than we do and have been using that to their (nondisclosed) advantage all along?

In my view, recognizing the ridiculousness of the rhetorical question is the *first* step to freedom. Now we've got a long haul to go, but at least we're out of the parking lot.

I think I'll CC this note to Caves (across the hall) since parts of it seem to be a point of contention between us too.

07-06-01 *Modules Over a Division Ring* (to N. D. Mermin)

Actually, I picked up the word "module" in a footnote of Adler's, where he explains that he is going to flout convention and call the object a "quaternionic Hilbert space." (Or, maybe he said vector space instead.)

Anyway, "quaternionic module" does square with the definitions in Mac Lane and Birkhoff's book *Algebra*.

p. 134: "A division ring is defined to be a non-trivial ring (not necessarily commutative) in which every non-zero element has a two-sided multiplicative inverse. A commutative division ring is thus the same thing as a field. An example of a non-commutative division ring is furnished by the quaternions ..."

p. 190: "A module is an additive abelian group whose elements can be suitably multiplied by the elements from some ring R of 'scalars.' ... This chapter will be concerned with general properties of modules over arbitrary rings; special properties of modules over fields ('vector spaces') will be studied in the next chapter."

p. 253: "We have already noted that most of the properties of vector spaces (modules over a field) are shared by modules over a division ring. As the most important example of such a division ring we now construct the ring of quaternions."

I would like to know what cherished property of vector spaces goes bust with quaternionic modules ... but I just don't have the time for that now.

07-06-01 *Two Go Bust* (to N. D. Mermin)

Actually Mac Lane and Birkhoff say that only two of their theorems in Chapter 7 "Vector Spaces" fail for modules over a division ring. But I haven't been able to locate the theorems, and I think I'm going to have to give up for now.

07-06-01 *The Mud of Mermin* (to N. D. Mermin)

Merminition 24:

> [CAF wrote:] The point is: If I give you a resolution of the (2 dimensional) identity for a single qubit into 2 projection operators, can *YOU* always tell me how to set up a corresponding procedure with 2 distinct outcomes without enlarging the system to one described by a larger Hilbert space?

Yes, I can. (That's why I picked that example.) The two orthogonal projections are necessarily of the form $(1 \pm n \cdot \sigma)/2$. *So all you do is find a Stern-Gerlach magnet and rotate it so its axis is along* n. *(I'm taking my qubit to be associated with the magnetic degree of freedom of a spin 1/2 particle that I can shoot between the poles of the magnet whenever I please. Tricky to do, of course, but not conceptually challenging.)*

Your rejoinder didn't faze me a bit. I just go to that same fine machinist who built your Stern-Gerlach device, and ask him to cut me two very good and very small mirrors, add the tippiest tip of glue to one of them, pass it through a dilute solution of just the right chemicals (so that just the right amount sticks to the glue). Et voila! In THEORY you CAN call such a thing an "atom in a cavity," that allows a $J = 0 \longrightarrow J = 8$ multipole transition (in principle) to be excited by a tenuous beam of light. We then apply a magnetic field and shine some auxiliary lasers in to "check" which of the 17 sublevels was "actually" excited.

I ask the machinist, "Did you feel particularly different when you worked for me than when you worked for Mermin?" He says, "Well you do pay better! But I promise you my prices were set by strictly objective criteria: I had to roam the earth to find that exotic chemical, and believe you me, that machining job was no easy task." I ask him, "Well, did you at least feel particularly quantum?" Just a guess, but I suspect he'll look at me with the same blank stare of the students on page 15 of my draft.

Merminition 25:

> [CAF wrote:] von Neumann didn't know how. He introduced the notion of a "measurement model," enlarged the Hilbert space, and got us into the muddle most of us are still in today. Now, people like Zurek are going around trying to justify the "pointer basis" on von Neumann's ancillary Hilbert space by enlarging it still further (and calling

> it "the environment"). And I'm sure you'll recall that von Neumann himself did even more dastardly things.

I basically agree with all of this but I don't think it has much to do with the question I asked you, which was how to describe on the down-to-earth unphilosophical laboratory (FAPP) level how to set up an experiment whose distinct outcomes correspond to the distinct positive operators. I can think of ways to do it, but if I want to describe them in the language of QM I need a larger Hilbert space to do it. I want to know how to do it in a way that refers only to the original qubit, analogous to what I told you above.

You're just not getting the point. There is no difference in principle, between the machinist's fine work for you and his even finer work for me. But von Neumann, silly von Neumann, invoked extra Hilbert space for both jobs. At least he was consistent. You're not being consistent. What do you think the Stern-Gerlach device is if it's not extra Hilbert space (in the von Neumann view)?

Merminition 26 :

> [CAF wrote:] Let me give you a homework exercise. ...

Again, very nice, but it has nothing to do with what I was asking you.

No, it has everything to do with what you are asking. You're just causing me to drag you in kicking and screaming. The point is: You don't need to invoke physics to make sense of Bayes' rule. A part of the quantum confusion has come about precisely because people have wanted to invoke "physics" to make sense of quantum collapse. It hasn't happened in 75 years, and it's not going to happen now—or at least that's my bet. In your Stern-Gerlach example you explicitly throw away the issue of "where the outcome comes from." (Von Neumann tried to answer that issue but botched it.) But your example is precisely on the right track: you don't need to ask where the outcome comes from for Bayes' rule, and you don't need to ask it for the quantum.

Now I've got to go to lunch, and then try to recover from all the work I did NOT put into the paper today. I hope though that these notes I'm writing you are clearing a little bit of the mud away.

09-06-01 *Nope* (to H. J. Folse)

Folsesm 4 : *I'm less hopeful than you that conferences such as next week's in Växjö will cease — or at least if they do cease it will be because these questions have been answered* **for once and for all**. *I suspect we suffer from the historical delusion that back in the* **old days** *of "classical physics" there was a clear cut conception of what the universe was like and how we had knowledge of it. And what we'd like is a quantum era substitute. But of course a moment of historical realism tells us that back in the old days the philosophical significance of the "***classical*** mechanical conception of the universe" was just as much controversial as is the "***quantum*** mechanical conception of the universe" today. Such questions tend more to get* **outdated** *than answered.*

Nope. I don't believe in "for once and for all" for anything. I'm just looking to clarify quantum mechanics enough so that its foundation stops being a burden to thought. Then we'll be in a position to move on to the next step in physics, with all of the wonderfully mysterious things it entails. Sort of like laying down special relativity, so we'll finally have a fighting chance of discovering general relativity.

I'm still working on the silly NATO paper after all. I've de-ego-ized the introduction quite a bit, and added more discussion on the analogy between Bayes' rule and quantum collapse than I thought I'd have to. But Mermin has been reading it carefully—that's why I made those changes—and though its slowing me down, it's certainly helping the paper. I'll bring a copy of the final to Sweden.

I'm keeping my fingers crossed that the rain will subside on your end of the world.

10-06-01 *Answers* (to N. D. Mermin)

Merminition 27: *Before I torture myself making sure that the converse really holds, I need a little more education in the formalism. [...]*

So I'm putting myself in the place of your student on p. 16 and I'm looking for a simple example where the standard postulate you've dropped doesn't hold, so I can see for myself whether it makes a difference. What could be simpler than the POVM

$$E_b = p(b)I$$

where I is the identity and the $p(b)$ are non negative probabilities that sum to 1. Now I look at your rule for $P(b)$ and it tells me that $P(b) = p(b)$, independent of the density matrix of the system. So I raise my hand and ask why you call that a measurement when it doesn't tell you a damn thing about the system.

You will not say, "Ah, that's because the POVM you picked corresponds to the case where you turn on no interaction between the system and the ancilla, so of course the measurement of the ancilla tells you nothing about the system." You will not say that because (a) the point is to eliminate the ancilla and (b) to say nothing more about "measurement" than is in your table.

My guess is you will say that some POVMS are more informative than others and I have picked a particularly bad one (just as picking I for my observable is a particularly bad choice under the standard rules).

Yes.

Merminition 28: *So then I say, "So there is some figure of merit for POVMs associated with how well their outcomes discriminate among various density matrices for the system? Could it be that the postulate you have dropped is there because it somehow maximizes this figure of merit?"*

There are two ways in which a POVM can be informative. 1) Suppose you have a density operator ρ for a system, and you know that someone else has a density operator ρ_i for that same system—where ρ_i falls within some fixed decomposition of ρ. You just don't know which value of i he happens to be using (outside of some prior probability). Therefore, you perform a measurement in an effort to obtain information about his i: this is the scenario of classical communication. 2) There is no extra player, there is simply ρ. However, you are dissatisfied with how little you can predict of a random measurement in the future, given that very mixed state ρ. So you perform a measurement now, in the hope that you can say more about a random measurement in the future. That is to say, by performing a measurement, you can reduce the mixedness of a state ... and in that sense a measurement can be informative.

Unfortunately, I do not say a lot about 1) in this paper outside of the paragraph you cite below. I do say a lot about 2) though. It sounds like you haven't read through that.

Of course some measurements will be better or worse for each of these tasks. In some examples of 1), you most certainly have cases where NON-vNM POVMs are required. As regards 2), I haven't thought about it enough: i.e. suppose you wanted to changed your mixedness from amount x to amount y. Could you always do that with a sufficiently well-chosen vNM?

Your example of $p(b)I$ is bad for both these tasks. Call it a "measurement" if you will. I keep thinking "intervention" or "act" is better, but I'm not going to change 75 years of ingrained language.

Merminition 29: *Could it be that the postulate you have dropped is there because it somehow maximizes this figure of merit?*

That would be interesting if it were the case. But I don't know of any ways in which it is true.

Merminition 30: *Is there some way to measure the ability of povms to discriminate among all the possible density matrices a system might have which shows that pvms are the most sensitive? Or, conversely, is there some natural measure of discriminatory ability for which a povm that is not a pvm does better. (I seem to remember an example in Peres — possibly even in your paper — where a three outcome povm tells you more about something or other than any possible pvm.)*

Systems don't have density operators; people ascribe density operators. But what you have in mind is 1) above.

Actually, there is at least one more interesting way to think about a POVM as being informative of a density operator. (This way is more conceptual than the two above, however.) The space of Hermitian operators is a d^2 dimensional vector space. It turns out to be possible to find POVMs with precisely d^2 linearly independent elements. For such measurements, the probabilities $P(b) = \mathrm{tr}(\rho E_b)$ uniquely specify ρ. You could of course do the same task by (conceptually) measuring $d+1$ standard observables ... like the three Pauli operators in the $d = 2$ case. But counting the total number of outcomes for this case (and lumping the whole thing into a single POVM if you will), you would have $d^2 + d$ outcomes. Thus there are "minimal informationally complete POVMs" if you move outside the von Neumann paradigm. And that too makes POVMs special over von Neumann ones. (You can read about these kinds of POVMs in our quantum de Finetti paper.)

Merminition 31: *A second question. I'm on page 20, and a student again. My povm has only one outcome (which always occurs, again, independent of the initial density matrix rho). It is just $E_1 = I$. How am I to understand the enormous range of possible final density matrices? This would appear to be a measurement from which I learn nothing. Yet it has an enormous capacity for altering the density matrix, which encapsulates my knowledge. What's going on?*

There are good interventions, and there are bad interventions. For your example $E_1 = I$ there are no ameliorating ways to do it. You can never end up with an increase in the purity of the state describing the system.

Merminition 32: *Your ground rules forbid you to tell me that the enormous range of outputs have to do with the fact that I've turned on an interaction that has entangled the poor system with an arbitrarily chosen ancilla, so of course, if I've paid attention to what I've done to it, the poor density matrix will change even if I subsequently perform no test on the ancilla. But what do you tell me?*

The word intervention really is better than measurement. (Did you read the "Penrose Tiles" section I sent you to?) Some ways I can act on the world now will help me predict the consequences of my actions in the future. Some ways will not. Quantum theory gives us the full range.

12-06-01 *Something I Wrote Once* (to R. Obajtek)

Below is something I wrote a couple of years ago. I reread it this morning. You do understand the implication of the Wheeler quote? It's solely about self-confidence.

Let's just get started on your research project this morning.

> Undergraduate Research and Quantum Information
>
> When I was an undergraduate at The University of Texas I had the opportunity to be associated briefly with the research group of John Archibald Wheeler. Two things about Wheeler's style made a great impression on me. First, he viewed research for both graduates and undergraduates in precisely the same light: It's a frying pan and you've just got to jump into it! It got him results, and it trained a generation of excellent theoretical physicists. The second thing came in a question-answer session at the end of a seminar. Someone in the audience asked, "Do you see a difference between the students at Princeton and the students at UT?" His answer was just as clean and as simple as it should have been, "Yes I do; the students at Princeton *know* they're smart. Next question."
>
> If you want to know my philosophy of how to advise research, then you need go no further than the paragraph above. Great discoveries are waiting to be made at all levels of science. And if there ever was a frying pan to jump into for the undergraduate, it is quantum information. Some of the greatest discoveries of our field have been very literally at the level of a third-year undergraduate quantum mechanics course. There is not a student who has studied Vol. III of *The Feynman Lectures on Physics* who could not have discovered quantum teleportation for himself. There is not a student at that level who could not have discovered the idea behind quantum cryptography. Wonderfully, these are not isolated incidents: there is a sense in which they define what the field is about. The field is about looking at quantum mechanics in a new way and wringing everything we can from it. The only tool a student really needs for a start in quantum information is to *know* that he's smart.
>
> The bulk of present-day research in quantum information is truly an interdisciplinary effort. Take quantum mechanics, computer science, information theory, and linear algebra, put them in a bowl and mix. Because the field is in its infancy, the use of undergraduate-level ingredients from each of those disciplines is far from exhausted. There is just so much fun work to be done; one cannot help but be thankful for the army of eager, questioning undergraduates that will teach their professors so much.

15-06-01 *New Flight, New Letter* (to D. B. L. Baker)

A new flight, a new letter to you, and still none in return since my last. Do you ever read your email anymore? I'm on my way to Växjö—pronounced Vexsher by the Danish but not the Swedes; no English speaker alive can pronounce it like the Swedes—via Stockholm via London. Of course, the flight took off an hour late and now I'm going to have a tight squeeze on my connection.

Do you ever read your email anymore? I'll ask you again: How are things going? How's the little one? Did you get the play area built that you were planning? How's your significant(?) other doing? How's your love life? How's your intellectual life? How's your health? How's the stroke recovery progressing? How's the heat in Texas? How's your mom? How's your sister? How's your niece and nephew? How's the job going? What's the general feeling in Texas since G.W.'s taking office? Does it still upset your stomach? (It does mine.)

Do you ever read your email anymore? Have you tried a new wine lately? Do you have any picks you should tell me about? Have you done any barbecuing in a while? (I wish I had.) Do you ever get to Austin? Have you heard any live music of late? (I wish I had.) Did you get your taxes in on time?

Do you ever write your email anymore? (Did I get you with that one?) Have you heard from Michael D. in a while? (I haven't.) Do you know whatever happened to Terry? Do you know if he really moved to Florida? Do you know why I'm asking you this? (I don't.) Any other gossip of Cuero? Any dreams of ever going to Turkeyfest again? (I don't suppose I have any.)

Do you ever read your email? Are you still driving the same car? (I guess not.) Are you still leasing a car? (I bet so.) Were you hit very hard with the Allison rains? (I suspect not.) Do you ever read your email anymore?

Do you remember the time I backed out of your driveway on French St. and hit a car parked across the street? Do you remember the time you and I went to Port A in your Fiat just after you got it? Do you remember the allure of the Pat Magee shirt? Do you remember the time I met Pat Magee? Do you remember the time we went to Troll Bridge and you beat your head on the roof of your car? (Or was it my car?) Do you remember the time we left Linda Henderson's trunk open in Houston and it rained? Do you remember the time we sat in your car, parked in front of my house, and listened to an interview with Rush? (Or was it my car? And was it Rush?) Do you remember the time we tried to find our way to the beach by following the signal of C-101? Do you remember the time we were going to see Simon and Garfunkel and the hurricane came? Do you remember how eerie it was in Cuero with all the refugees hanging out in the high-school parking lot? Do you remember the blonde-haired girl who kept a diary and whose father worked at Texas Eastern? (I bet you do.) Do you remember the blonde-haired girl at UT orientation that I never had the nerve to talk to? Do you remember Donna Slack? (Is my statute of limitations out yet?) Do you know whatever happened to Wendy G? Do you know whatever happened to the girl that married Warner Scott (briefly)? Or was that Scott Warner? Or Scott Hamilton? Scott Henry! (Didn't I make that mistake before?)

Do you ever read your email!?! Do you remember the wonderful taste of Malt Duck on an early morning cruise? Do you recall why we thought we were the deepest thinkers in Cuero, TX? (Besides it being a small world?) Do you remember the wonderful three day party we had at your house on French St? Do you remember who came? (Do you remember who went?) Do you remember The Nails? Do you remember Debbie D? (Or was that two D's?) Do you remember the time you helped me write a paper for Linda H? Do you still scorn me for getting so many kudos on that?

Do you remember our graduation day? Can you tell I'm going to a quantum foundations conference? Do you remember Craig Calk? (Some things never leave us.) Do you know whether Grunt ever became a dentist? Do you remember the Rudolf christmas special? Do you ever think of the girl you dated at UT? (I can't think of her name right now.) You used to. But do you anymore? And while I'm at it: Do you ever read your email anymore?

Do you miss our old days? When we hadn't grown so far apart? (Read your email every now and then.) Do you ever think of the Cuero Country Club? With Gaye K. and Sheri T? Do you think they could possibly look the same still? Do you ever get to Stubb's BBQ anymore? Do you think life is finite? Or unbounded? Do you remember a time some policeman in Cuero told me I was glassy-eyed and I told him that that was my contact lenses? Do you remember the dry hamburgers you grilled for Kiki and me once? Do you remember the time you came to Montréal? Could you forget the Vampire Lounge?

Dinner time.

Three little wines, each by my doorstep. Swinging sweet song. A melody pure and true . . . Do you ever read your email?

Do you remember our first week at UT? Do you remember the week before that? Do you remember how we drove through the middle of Gonzales the day we left Cuero? Do you remember the night we slept in the lawn of the Methodist Church? Do you remember the Hobo Pack? Do you remember Don Billings? Do you recall the time the cat shit behind your bed and you wouldn't remove the evidence? Do you remember your trip to Alaska with your uncle? Do you remember any Simon and Garfunkel songs from beginning to end? Do you remember the worn circle on the back pocket of Art Garfunkel's jeans in *Concert in the Park*?

Do you remember any Madonna videos? Do you remember what you told me about Rumble Fish? Do you ever read your email anymore? Did you ever understand why I mailed you that menu from a restaurant in Italy? Do you remember the good life? Do you ever think about the Cuero Livestock Show? Do you think Brett Wright's belly is bigger than mine? (You know, he and I share the same birthday.)

Did you know that Laura Lee had no eyebrows? Did you ever notice at the time? Did you have a look at my book on the web? Did you ever pay that outstanding ticket you owed in some county near Houston? Do you still watch Ally McBeal? Dharma and Greg? M*A*S*H? Saturday Night Live?

Do you ever think of Janet Reno's waddle? (Richard Fish does.) Do you still pull evening shifts? Or is it a 9 to 5 job now? Do you still read so much? Do you think "a mind is a terrible thing?" Do you remember the Moulin Rouge? Do you ever wish you could recover it all? All of what? Do you ever get scared when a jet starts bobbing up and down 30,000 feet over the ocean? (I do, and I am now.)

Do you ever think of the Swedish Bikini Team? (Not me.) Do you remember Mad Dog and Beans? Captain Quackenbush's? The Fajita King? Uma Thurman? Conan's Pizza? The GM Steakhouse? The Doll House? Bunk Brantley? Ken Adams? Lisa Adams? The chubby and skinny sisters that lived nearby? Vitalis hair tonic? Caskey Realty? Life on Long Island?

Do you ever download your email? Do you ever play the state lottery? Do you remember the ferry to Port A? And, who could forget Greta? (She was Swedish, wasn't she? Tell me it *is* so.) Do you think there is a god in heaven? Do you think there is a heaven? Do you ever go to church anymore? Do you remember John Hinckley Jr.?

Have you prayed for my sins? Have you ever confessed to yours? Twilight Zone. Brando. Helicopters. Napalm? Frog? American Legion? Deputy Sheriff Bobby Roberts? Charlie's Angels? The Mighty Mightor? Sealab 2020? Moby Dick? Buck Sralla? Can you forget the Thanksgiving morning I knocked on your door? (Or was it the Friday after?) Can you forget the girl from Yorktown? (I don't think you knocked on my door.) Can you remember a life before children? Can you remember Immanuel Kant? Can you remember the Square One? (I just remember Elvis.) And speaking of Elvis, can you remember an Icehouse in Houston, TX? Elvis the Pelvis. You know he had a brother, don't you? Enis.

Do you remember Octogafest? The morning after? The Butthole Surfers? Do you remember the mystical side of Coleto Creek? Do you remember a swim in Fox Crossing? Could you forget the Meyersville Country Club? The graduation party the year before ours? Could you forget Spider Man? Sandy Boehne? (I've forgotten how to spell her name.) Life on Long Island? (Again?) I must know: Do you ever read your email? Can you find the reply button? Is it spiked with needles and pins? Poison tips? Deadly fungus? Fly paper? The scent of a woman?

Do you think there is life on Mars? Do you think there is permanence? Do you think Bill Clinton's hands are big? Is there any connection between a Woody and a woody? (Some things are better left unasked.) Have you ever worn a trench coat in San Francisco? Have you ever read a Ginsberg poem? Can you remember the poutine in Montréal? Are you afraid of fires? Could you forget the Vampire Lounge? (Never!)

Yeah, I did that on purpose. Benny Williams? His mat headed, blonde headed, bicycle riding girlfriend? My crush on her? My obsessive idealism? Your cynical realism? My love of Marx? Your belief in "human nature"? Her name was Kathy—it finally came to me. Do you think the Gonzales girl ever graduated from college? The one you did the skinny dipping with? Do you think Ricky Bluntzer ever amounted to anything?

Do you ever eat chicken fried steak any more? Do you have a clear vision? Do you have clear vision?!?! Do you see the difference? (Pun intended.) Can you answer all these questions? Will you answer all these questions?

Good night old friend,

PS. Do you ever read your email anymore?

25-06-01 *Bohr and Bits* (to H. J. Folse)

It was wonderful to meet you finally. But, my deepest regret of the meeting was in not getting a chance to really talk to you! I think I should dig up a way to drop by Loyola sometime.

I'm in London right now, slowly working my way back home, taking care of some delinquent tasks, and adding some more words to my compendium. So, let me send you a little reminder. When you get back from your vacation, can you send me a more complete copy of the paper:

- H. J. Folse, "The Formal Objectivity of Quantum Mechanical Systems," Dialectica **29**, 127–136 (1975).

Also, if you could supply me with the full list of editors for the volume below (including a description of the needed diacritical marks), that'd be greatly appreciated.

- H. J. Folse, "Realism, Idealism, and Representation in the Description of Nature," in *Avartuva ajatus: Julkaisutoimikunta*, edited by U. Ketvel and ?? (Luonnonfilosofian seuran, Espoo, 1999), pp. 73–77.

I'm shooting to get a completed version of the compendium out before the end of the year.

Also I'm getting much closer to giving you some feedback on all your papers. That should arrive in the form of a longer letter sometime in the coming month. My remark after your talk was a hint of the general difficulty I'm having with your version of Bohr. If I can ask enough testing questions to prod you into trying describe "the quantum postulate" (and a few other things) from a new angle, then maybe I will be filling a useful task.

Caves and I really enjoyed the format of your "Bohr's Best Bits." In particular, the boldface to let us know what you thought was most important, while keeping the remaining text to build a context.

27-06-01 *Gleasons, and Me at AT&T* (to H. Barnum)

Barnumism 1: *I must apologize for still having only skimmed the paper you gave me to read. I really liked the Gleason's theorem for POVMs. I used to think such a thing would be superfluous except for 2D, since we have it for orthonormal frames in 3D and hence, by extension, for POVMs in 3D and higher. The lack of it for the rationals didn't bother me because I always thought the difference between the rationals and the reals is "unphysical." (I think we discussed this business a bit in Los Alamos in the fall of '99, and in fact you guys may have had the theorem by then, and it was part of the discussion.) One would need to use some kind of continuity of states (sets of probability distributions for each possible measurement) in the way we represent them. This would* *impose continuity of frame functions as a requirement, which I think is reasonable despite the fact that Gleason can do without it. Now I never tried to think about what that would do in the rational case (I guess maybe one could define continuity there?). But I think it might well require that frame functions on the rationals extend continuously when the rationals are embedded in the reals.*

Now I think the POVM version is really quite important (just how important, I am still trying to decide). It provides another, perhaps better, way of getting at this sort of issue. The thing is that the unphysicality of the difference between the rationals and the reals is closely related to the idea that we can never measure a true orthonormal frame ... we always measure a fuzzed version of it. All measurements are measurements of POVMs. So, in a sense, what should really be proved is a Gleason's theorem whose proof uses only finite strength (in the terminology of you & Kurt Jacobs) POVMs*. I.e., one which holds for the physical model in which* all *measurements are finite strength POVMs. So one should look at your proof and Busch's and see if they only use finite strength POVMs. (It seems like it ought to be possible to modify them to do so, if not.)*

Yes, it will still work there. The finite strength measurements simply corresponds to the set of POVMs but with the boundary removed.

Barnumism 2: *Here's an interesting idea to explore within the Bayesian/operationalist view of quantum theory. Okay, states are essentially (whatever else they may correspond to) summaries of our knowledge about a system insofar as it's relevant to predictions of future measurement results. Measurements (POVMs) similarly have a subjective aspect: what measurement we think we're performing depends on the "state" (subjective) of the apparatus we start out with, and* our beliefs about *the interaction dynamics, etc ... I think some of that subjectivity is irreducible, in that we won't want to say "well, we didn't know what POVM we were doing, but now we do". We can sometimes* view *some POVMs as "convex" combinations of some others, in a sense such that we learn both which of the sub-POVMs was done, and the result of the sub-POVM. But that, ex ante, is still the original POVM.*

So, the fuzz in a fuzzy measurement results from our incomplete knowledge.

Watch out: you'll start to tweak Carl's greatest fears with this.

27-06-01 *Invitation* (to A. Y. Khrennikov)

Khrennikovism 1: *I also plan to work in the direction of p-adic [Gleason] theorem, but it is not so simple as we discussed ...*

I'm really glad to hear this. More than anything, I would like to get down to the bottom of why this theorem works. It works for the reals, but not the rationals. What crucial new property comes into play there? It also seems to work when the field is the algebraic numbers. So, it is not just having a completion of the rationals that makes it fly. The more examples/counterexamples we have, the more clear the picture will become.

Khrennikovism 2: *I plan to write a paper in that I shall criticize your Bayesian approach to quantum probabilities. But I think it would be not so bad. You could reply and there could be a good exchange of viewpoints.*

In a way I'm glad to hear this too. Every opportunity for a clarification of the crucial issues is a welcome opportunity. One thing I hope you will keep in mind though in building your criticism is that: Even though Caves, Schack and I see no escape from a subjectivist account of quantum probabilities, that does not mean we strive for a subjectivistic account of Nature. The issue is, instead, that quantum mechanics seems to be a jumble of epistemic and ontological entities. The task we see is to disentangle those two aspects of the theory, with the main goal being to lay bare the ontological content. Getting straight the epistemological nature of quantum probabilities—we believe—is the first step toward a greater goal.

I will write you some notes on your context-dependent probabilities before the day is out tomorrow.

28-06-01 *Amherst-Area Visit* (to A. G. Zajonc & K. Jagannathan)

I will be in the Amherst area July 1–3. Would either of you be interested in my dropping by the college for a short visit? (July 2 would probably be the best date for that, but the morning of the third would be OK too.) I could also give an impromptu talk if there's any interest: I've recently been involved in three very foundational theorems that might titillate you. (I'll put a trouble-making abstract below that I wrote recently for a quantum foundations conference.)

> Title:
> Quantum Foundations in the Light of Quantum Information Theory
>
> Abstract:
> In this talk, I hope to cause some good-natured trouble. The issue at stake is when will we ever stop burdening the taxpayer with conferences and workshops devoted—explicitly or implicitly—to the quantum foundations? The suspicion is expressed that no end will be in sight until a means is found to reduce quantum theory to two or three statements of crisp physical (rather than abstract, axiomatic) significance. In this regard, no tool appears to be better calibrated for a direct assault than quantum information theory. Far from being a strained application of the latest fad to a deep-seated problem, this method holds promise precisely because a large part (but not all) of the structure of quantum theory has always concerned information. It is just that the physics community has somehow forgotten this.

28-06-01 *Context Dependent Probability* (to A. Y. Khrennikov)

Khrennikovism 3: *It was nice to meet you in Växjö and discuss fundamental problems of quantum theory. Unfortunately, I have the impression that my presentation on Contextual Probabilistic Interpretation of quantum theory was not so clear for participants (conversations during lunches and dinners). I try to present my views as short and clear as possible.*

Thank you for valuing my opinion on your ideas; I am flattered. So I treated the problem in a conscientious manner: I downloaded and read three of your papers (`quant-ph/0103065`, `0105059`, and `0106073`).

I am indeed quite intrigued by the possibility that quantum mechanics may be nothing more than a calculus for comparing probabilities when the experimental context cannot be deleted from the results it brings about. In vague philosophical terms, I think this is precisely the kind of idea Bohr, Heisenberg, and Pauli were bandying about in constructing their interpretation of quantum mechanics. It is certainly the kind of notion Bohr was trying to get at with his emphasis on "complementarity." So I would welcome a more precise way (a mathematical way) of expressing the essence of all this. I myself have been attracted to this sort of thing for a long time: it is a large part of the thread connecting my "Notes on a Paulian Idea"—that is, that the observer sets the context, and, in the words of Pauli, cannot be "detached" from what he finds. Also you can find discussions of it in Sections 4 and 8 of the large paper I was circulating at the conference, "Quantum Foundations in the Light of Quantum Information." I say all this to make it clear that I am more than sympathetic to your program.

However, as much as I would like to tell you otherwise (because you are my friend), I do not see that your present formulation of the problem moves very far toward quantum mechanics in a convincing way. There are problems on at least two levels.

Maybe the most devastating and immediate is your move between Eqs. (5) and (6) of `quant-ph/0106073`. (I'll focus on that paper for specificity since I did not see you make a stronger argument in either of the other two papers.) You write:

> The perturbation term $\delta(\mathcal{S},\mathcal{S}')$ depends on absolute magnitudes of probabilities. It would be natural to introduce normalized coefficient of the context transition ...

The question anyone will ask is, "Why is this natural?" What compels the precise form of the normalization other than that it forces the equation to look of a more quantum mechanical form. Why did you choose the square root rather than the third root, say? Indeed, why not divide by the absolute value of δ, or the exponential of δ, or any other combination of functions one could pull out of a hat? To put it not so gently, it looks as if you built the desired answer in at the outset, with little justification otherwise.

The second level of my problem is that, even if you do get this far, how do you make the further step to vector space representations of quantum mechanics? Why are observables POVMs and not other exotic entities? What leads us to the starting point of Gleason's theorem? Etc., etc.? I don't see that you have enough structure to do that. But more importantly, until you have done that I would have to say that your theory remains fairly empty in making a connection to quantum mechanics. Too empty.

The way I view the problem presently is that, indeed, quantum theory is a theory of contextual probabilities. This much we agree on: within each context, quantum probabilities are nothing more than standard Kolmogorovian probabilities. But the contexts are set by the structure of the Positive Operator-Valued Measures: one experimental context, one POVM. The glue that pastes the POVMs together into a unified Hilbert space is Gleason's "noncontextuality assumption": where two POVMs overlap, the probability assignments for those outcomes must not depend upon the context. Putting those two ideas together, one derives the structure of the quantum state. The quantum state (uniquely) specifies a *compendium* of probabilities, one for each context. And thus there are transformation rules for deriving probabilities in one context from another. This has the flavor of your program. But getting to that starting point from more general considerations—as you would like to do (I think)—is the challenge I haven't yet seen fulfilled.

I very much hope that I have not offended you with these comments. I greatly respect your program. But because of that I want much from it. I want it to stretch our understanding. John Wheeler used to say, "We must make as many mistakes as we can, as fast as we can, or we'll never have a hope of gaining a true understanding!" I let that philosophy rule my research life. Thus I can only commend you for your exploration, and hold the strongest hope that something firm will come from it with a little more work and contemplation.

28-06-01 *The Oblique Observer* (to N. D. Mermin)

You haunt me so that I wake up in the middle of the night just to spar with you. It's this damned noncontextuality of quantum probabilities. If one walks into the game with the *firm* belief that a quantum state is a state of knowledge, then noncontextuality is almost a given. That, in part, is what my section "Whither Bayes' Rule?" is about. Moreover, all questions about instantaneous signaling through quantum-state change just become silly: such questions spring solely from a wrong-headed view.

But, you do not walk into this game with the firm belief that a quantum state is a state of knowledge. So I am forced to work to win your approval. In the end, with clarity achieved, your demands will have been a great gift. Right now, they are just annoying.

In any case, this leads me down the path of the oblique observer. There is a way in which a von Neumann-like view of measurement may be a virtue. That is, a view of measurement where, to make it go, I first introduce an ancilla to interact with the system. Then I introduce a second ancilla to interact with the first ancilla because I didn't know how to solve the measurement problem at the new level. Then I introduce a third ancilla, and so forth. Von Neumann went to this extreme because he was chicken to let the mental update that is a measurement fall outside of physics (as Thomas Bayes would have). So, he piled up superobserver after superobserver, just so there would always be an outside view. But so be it: Let us glean what we can from this freedom of our descriptions.

The main point is this: You pick any measurement (any POVM) you wish for some system, and I can always think of a way to get at that measurement in an indirect way. That is to say, I can always delay my cutting of the Gordian knot until I get to a system with no residual causal link to the one I'm really interested in. In language I don't like, it means I can always induce the "physical collapse" somewhere else. I can always push the measurement to an arena where you would take the noncontextuality assumption (on the system of interest) as a given. My direct measurement on an ancilla serves only to refine my knowledge about the actual system: the actual system cannot care how I came to that refinement, or, indeed, if I ever pursued refining my knowledge at all.

You get my point—with that much repetition, you'd better. This gives rise to a vague idea that perhaps you can help me elicit into reality. (And save me some torture that you are the root of.) Forgetting about the precise structure of quantum mechanics, why should we not view all observations as oblique observations? Whose philosophy ever dreamt that we had *direct* access to the minutest details of the world in the first place? (I'll paste in the great Heisenberg/Einstein quote below so you'll have quick access in case you wish to remind yourself of it—especially the fourth, fifth, and sixth sentences.)

Moreover, coming back to quantum mechanics in particular, what is to keep me (in my derivation of the tensor product rule) from thinking of the two separate observations as each concerning the opposite system? The question is, can one get some quantitative mileage from this. At least that's the question on my mind as the sun is rising. Any thoughts?

It's a good thing you left the Växjö before Friday's lunch. The boiled potatoes were ridden with rocks: I lost an eighth of a molar in the process! What was your overall impression of the meeting (good and bad)?

28-06-01 *The Kettle Black* (to N. D. Mermin)

I'm just reading through a friend's comments on my NATO draft. Just after my equations 65–67, where I write, "The resemblance between the process in Eq. (66) and the classical Bayes' rule of Eq. (38) is unmistakable," he writes: "Seems to me contrived — you want to find a resemblance and then you find it."

Thinking back to one of his recent productions, I think "Boy, that's the pot calling the kettle black!"

But aside from that, I do seem to be impressing less people with this result than I would have thought. Is it my presentation, or is it really the substance? This result, in particular, is the one I'm most pleased with in the paper, but it gets the coldest reception. You've never yet told me your truest thoughts on that section. I'd like to hear them.

29-06-01 *Alex Favor* (to D. R. Terno)

I wonder if I might ask a favor of Alex. Could you ask her if she would be willing to type in the linguistic analyses she made at the Växjö meeting? She thrilled me to no end by telling me how consistent my language patterns were in my talk. (Consistency is what I'm striving for!) Most importantly, I'd like to see how some of the other attendees fared in this regard. (I'm always willing to learn by others' mistakes!)

29-06-01 *Opinion* (to S. J. van Enk)

Do you have an opinion on this as my abstract for the NATO paper on `quant-ph`? I'll drop by your office in a little bit.

> This paper reports three almost trivial theorems that nevertheless appear to have significant import for quantum foundations studies. 1) A Gleason-like derivation of the quantum probability law, but based on the positive operator-valued measures as the basic notion of measurement (see also Busch, `quant-ph/9909073`). This theorem works both for 2-dimensional vector spaces and for vector spaces over the rational numbers (where the standard Gleason theorem fails). 2) A way of rewriting the quantum collapse rule so that it looks almost precisely identical to Bayes' rule for updating probabilities in classical probability theory. And 3) a derivation of the tensor-product rule for combining quantum systems (and with it the very notion of quantum entanglement) from Gleason-like considerations for local measurements and classical communication on bipartite systems.

02-07-01 *Objective Properties* (to D. G. Chakalov)

Thank you for all the interest you've shown in the papers I have been involved with. I commend you in your efforts to get to the bottom of what's going on in our world. But I cannot believe it very likely that distinct new kinds of *physics* arise in our brain processes. Instead the road I have chosen to develop is making sense of quantum mechanics (as a theory predominantly of inference) from *within* quantum mechanics. I understand that your road is distinct: but life is short, and one has to make a cut or one will certainly never get anywhere. My own direction may turn out to be completely wrong, but I have decided to pursue it with dogged determination and not to get derailed.

I wish you luck in your own pursuits.

02-07-01 *Making Good Sense* (to J. Finkelstein)

Finkelsteinism 1: *I enjoyed reading your latest "quantum states are states of knowledge" manifesto,* `quant-ph/0106133`*. I do have sympathy for that point of view, but I would like to put my two-cents-worth in by remarking that it is not quite fair to imply that experimental results such as those of Scarini et al. which you cite furnish ADDITIONAL support for it. ...*

It is certainly important to confirm the standard quantum predictions under as wide a set of circumstances as possible, but that confirmation does not distinguish between alternative interpretations all of which agree with the prediction. For example, neither I nor (I believe) you are advocates of the many-worlds interpretation. But some folks are, and those folks would have expected Scarini, Zbinden, Gisin etc to have found exactly the results that they did find. Therefore I would say that the many-worlds interpretation has the same (small) degree of plausibility after these experiments as it did before.

Would you agree?

Thanks for the note! Yes, I guess I would (though only to the small extent that I think many-worlds is coherent in the first place). But there are two things working in the background. 1) Probably plain sloppiness on our part in our wording. And 2) the fact that Rüdiger is (presently) more conciliatory to MWI than Carl and I are. He sees Bayesian probability as holding a place even in their interpretation. (A rough cartoon is: In their interpretation, the universal wave function serves an ontologic role, while the relative states in a Schmidt decomposition with respect to an observer's mind serve the same epistemic role we ascribe to them.) We should probably either remedy 1) or make 2) more clear, or both. We'll have to huddle for that.

Finkelsteinism 2: *(And also by pointing out that the experiment with detectors in relative motion was reported in Zbinden et al. (*`quant-ph/0007009`*), rather than in your ref. 2.)*

That's probably my screw-up: I just assumed (from a search through SciSearch) that Ref. 2 was the published version of Zbinden *et al.* I presume you're telling me it's not. Is there some history here that we should be aware of? Or a different published reference?

Finkelsteinism 3: *I enjoyed reading your latest "quantum states are states of knowledge" manifesto ...*

And there's still another one coming: it should have appeared on `quant-ph` today. I hope you'll read it too. This one's a solo flight by me (titled "Quantum Foundations in the Light of Quantum Information"). (BTW: Don't let the sober sounding abstract on `quant-ph` fool you; I'm as loquacious and philosophical as usual on the inside.)

Jerry's Reply

My understanding of the history is as follows:

1. H. Zbinden, J. Brendel, W. Tittel, and N. Gisin, `quant-ph/0002031` and *Pramana-Journal of Physics* **56**, 349 (2001) [two somewhat different papers] discuss the results with moving detectors, and also claim the lower limit on the velocity of 10^7c.
2. A more detailed version of the above is H. Zbinden, J. Brendel, N. Gisin, and W. Tittel (note permuted authors), `quant-ph/0007009`, published as *Phys. Rev. A.* **63**, 022111 (2001).

3. The paper you cite (*Phys. Lett. A* **276**, 1 (2000)) is the published version of V. Scarini, W. Tittel, H. Zbinden, and N. Gisin, `quant-ph/0007008`. Here they consider the velocity in the frame of the cosmic microwave background, and claim a lower limit of (only!) $1.5\times10^4 c$.

02-07-01 *quant-ph/0106133* (to R. Schack)

I would only temper what you just said by making one addition:

Schackcosm 1: *Actually, I believe that the relative state an observer in some branch of a multiverse* [has no choice but to assign] *to, say, a qubit has a very natural interpretation as a state of knowledge.*

The lack of free choice is important there, and to that extent the whole scheme is non-Bayesian. Bayesian probabilities are never fixed by edict. In a way, this is just a fancy version of David Lewis's principal principle.

Top of the mornin',

03-07-01 *Epistemic Probabilities and Zing* (to U. Mohrhoff)

I have just started to read your new paper.

Mohrhoffism 1: *Epistemic interpretations of quantum mechanics fail to address the puzzle posed by the occurrence of probabilities in a fundamental physical theory. This is a puzzle about the physical world, not a puzzle about our relation to the physical world.*

I would appreciate hearing any thoughts you might have on my own newest: `quant-ph/0106166`. In particular, whether you think the ideas (or the research direction) supported there might temper your statement above? In a sense, it was written with precisely that purpose in mind.

04-07-01 *Carts and Horses* (to U. Mohrhoff)

Mohrhoffism 2: *I hate to dash your hopes ...*

Your pet idea that quantum information theory holds the key to the mother of all mysteries is understandable, you being a quantum information theorist, but it reminds me of something someone wrote about the father of all mysteries, consciousness. Everyone concerned with it (neuroscientists, psychologists, philosophers, AI-ers, etc) thinks that his/her particular field holds the key, so a baker would think that the secret of consciousness lies in the éclair. What would a baker think about the puzzle posed by the occurrence of probabilities in a fundamental physical theory?

You confuse the cart with the horse. I was attracted to a career in quantum information—and it can be documented—precisely because I had wanted to express quantum foundations problems in information theoretic terms all along. This was a process that started long before your publicational record and, indeed, long before Shor's factoring algorithm. But, you say what you did because it is the easy thing to say.

I will read and remark on your papers in due time, despite A) your arrogance, and B) the offensiveness of your letter. If there is something in your ideas, then it will be worthy of note regardless of its source.

[But see 16-07-01 note "Insults" to S. J. van Enk.]

04-07-01 *Invitation, 2* (to A. Y. Khrennikov)

Khrennikovism 4: *Yes, this is very well! However, for me, the only bridge between "reality" and our subjective description is given by relative frequencies . . .*

But there other ways to make the bridge: this is what gambling situations (like the Dutch-book argument that Schack spoke about) are about. They give a NON-frequency *operational* definition to probabilities. Subjective probabilities make their *objective* mark on the world by specifying how an agent should act when confronted with them.

04-07-01 *Context Dependent Probability, 2* (to A. Y. Khrennikov)

Khrennikovism 5: *P.S. But! How can you unify contextuality with subjective probability?*

I just don't see this as a problem. In choosing one experiment over another, I choose one context over another. The experiment elicits the world to do something. To say that the world is indeterministic means simply that I cannot predict with certainty what it will do in response to my action. Instead, I say what I can in the form of a probability assignment. My probability assignment comes about from the information available to me (how the system reacted in other contexts, etc., etc.). Similarly for you, even though your information may not be the same as mine. The OBJECTIVE content of the probability assignment comes from the fact that NO ONE can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation RULES from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments WITHIN each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!

04-07-01 *Gleason* (to A. Y. Khrennikov)

I still need to answer your question about Gleason. You can read about the POVM version of it in the section "Information About What?" of my new paper "Quantum Foundations in the Light of Quantum Information". But then the better places to look are the papers by Cooke, Keane, and Moran cited there, and also the paper by Pitowsky. It is those versions of the Gleason theorem that I am particularly interested in, in connection to the question of p-adic numbers.

04-07-01 *Book Reminder* (to the Los Alamos Historical Society)

Kiki and I thank the Historical Society for their kind offer. Our time in Los Alamos and our loss of everything material is something the family will have to reckon with for another generation: there will be no getting around explaining the fire to our two children.

Please send the books to our new home address below.

1) *Quads, Shoeboxes, and Sunken Livingrooms, A History of Los Alamos Housing*
   Craig Martin

2) *Robert Oppenheimer*
   Robert Bacher (Judy Gursky, editor)

3) *Tales of Los Alamos*
   Bernice Brode (Barbara Storms, editor)

4) *Los Alamos: Beginning of an Era, 1943–1945*
   Los Alamos Scientific Laboratory Staff

5) *Standing By and Making Do, Women of Wartime Los Alamos*
   Jane S. Wilson and Charlotte Serber

05-07-01 *Standing Up and Saying YES* (to J. Finkelstein)

Thanks for the comments. I welcome any that you send me!

Finkelsteinism 4: *This is not really any objection to what you have written, but the story you tell on page 10 might produce even WEAKER knees with the following modification: Suppose that Alice, instead of choosing ANY state $|\psi\rangle$ for her qubit, makes her choice from a finite and previously-agreed-upon set. She broadcasts the result of her measurement, but keeps her choice a secret, except that she reveals her choice in a sealed envelope which she sends to Chris (who initially leaves it sealed). Bob performs the appropriate Pauli rotation, then he makes a guess as to which state Alice chose, and performs a yes-no measurement with that guess; he communicates his guess, as well as the yes-no result, to Chris.*

Chris can now open the sealed envelope; if it happens that Bob's guess was in fact correct, then the result must have been "yes". So, if one wanted to be contrary (and of course I do not) one might say that, although when the yes-no measurement was performed nobody knew that the guess was correct, and although Alice did not "take the time to ... interact with it", nevertheless the qubit had "the power to stand up and say YES all by itself".

I agree, this does sound even more dramatic. And maybe I will start using it in my presentations. But the point remains the same: it is Bob's action that elicits a consequence.

You can see, I keep dreaming (modern) alchemical thoughts. Below. From: W. Heisenberg, "Wolfgang Pauli's Philosophical Outlook," in his book *Across the Frontiers*, translated by P. Heath, (Harper & Row, New York, 1974), pp. 30–38.

> The elaboration of Plato's thought had led, in neo-Platonism and Christianity, to a position where matter was characterized as void of Ideas. Hence, since the intelligible was identical with the good, matter was identified as evil. But in the new science the world-soul was finally replaced by the abstract mathematical law of nature. Against this one-sidedly spiritualizing tendency the alchemistical philosophy, championed here by Fludd, represents a certain counterpoise. In the alchemistic view "there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only 'Deo concedente.'"

> The governing symbol for this magical view of nature is the quaternary number, the so-called "tetractys" of the Pythagoreans, which is put together out of two polarities. The division is correlated with the dark side of the world (matter, the Devil), and the magical view of nature also embraces this dark region.

and

> When, in the spring of 1927, opinions on the interpretation of quantum mechanics were taking on rational shape and Bohr was forging the concept of complementarity, Pauli was one of the first physicists to decide unreservedly for the new possibility of interpretation. The characteristic feature of this interpretation—namely, that in every experiment, every incursion into nature, we have the choice of which aspect of nature we want to make visible, but that we simultaneously must sacrifice, in that we must forego other such aspects—this coupling of "choice and sacrifice," proved spontaneously congenial to Pauli's philosophical outlook. In the center of his philosophical thinking here there was always the wish for a unitary understanding of the world, a unity incorporating the tension of opposites, and he hailed the interpretation of quantum theory as a new way of thinking, in which the unity can perhaps be more easily expressed than before. In the alchemistic philosophy, he had been captivated by the attempt to speak of material and psychical processes in the same language. Pauli came to think that in the abstract territory traversed by modern atomic physics and modern psychology such a language could once more be attempted . . .

05-07-01 *Invitation, 3* (to A. Y. Khrennikov)

Khrennikovism 6 : *I think that there is some mystification in such a direct use of subjective probabilities. Do you really believe that you choose probabilities in gambling situation by your personal belief? I think you (and everybody) do in the following way: you have some experience with gambling (frequency!) and use this experience to introduce "subjective" probabilities.*

No, I think it is just the opposite: people almost never use frequency data as the determiners of their information in any common situation. Instead they use symmetry. If someone presents me with a coin that I have never seen before, then after a quick examination, I will likely *ascribe* a 50/50 probability to its coming up heads *simply* because I have no reason to believe otherwise. But if Danny Greenberger is the tosser of it, I know that he has the skill to make it look superficially as if it were being tossed in a haphazard fashion but it will still come up heads every time. The 50/50 ascription is not a property of the coin! It is simply a property of ignorance.

05-07-01 *QIC010531 (fwd)* (to S. J. van Enk)

van Enkism 2 : *I really get irritated by report I, you?*

Yes. We should probably just tackle it head on, pointing out how it is irrelevant.

Let me share some of my recent insults to cheer you up. Below is a letter I sent yesterday and for which I received profuse apologies today. What really annoyed me was that he insulted me for 24 kilobytes worth of email (in front of Peres and Mermin), and then at the end of it, had the audacity to say, "I trust you will find that we are striking consonant chords." So, I decided to strike *his* chords.

05-07-01 *Par Avion* (to H. J. Bernstein)

I just received your phone reimbursement. Thanks!

And thanks for coming by Charlie's the other day. What I really need to do the next time I come up, is just spend a day visiting you and visiting ISIS. Would you guys like to hear a talk from me? In fact, Kiki and I didn't get nearly as much (book) shopping time in as we had hoped to (mostly because of my Hirota duties): So, we might just come for another visit very soon. We'd probably just stay in a hotel in Amherst or Northampton or something to maximize our free time. What's your summer schedule like? What might be a good time for us to visit?

Charlie dismays me at times, calling my efforts to clean up quantum mechanics "theology." Strangely, it does hurt—I guess because I respect him so much. For instance, I doubt he'll even look at my latest paper just because he doesn't like the goal I have in mind. But there are some meaty theorems there that he might find useful . . . if he wouldn't just shut himself off to my trains of thought. Getting something like this from Holevo maybe makes it even harder:

> I downloaded your recent `quant-ph/0106166`. It has several interesting observations, but I like particularly the argument concerning derivation of tensor product of Hilbert spaces from the measurement statistics.

There are two great information theorists in my life, but only one of them will read my papers.

06-07-01 *Stamina!* (to S. Aaronson)

Thanks for the note! I'll give you a longer reply later. But in the mean time:

Aaronsonism 1 : *Anyway, I finished (!!!) "Notes on a Paulian Idea." Your shamelessness in mixing quantum physics, philosophy, and your personal life is an inspiration; maybe it will encourage other scientists to try something similar.*

Am I to take this to mean that you actually read the whole thing? If so, I'm shocked! I never imagined anybody would read the whole thing.

Scott's Preply, "Notes on a Preskillian Meeting"

> Remember me from Bell Labs? I'm at Caltech now, and tonight was at Preskill's group meeting where Andrew Landahl spoke on your "Quantum Foundations" paper. I thought you might want to know some highlights.
>
> - We took a vote on whether humans have free will or are mere machines. Machines won, 7-5.
> - Preskill defended the view that one can be an Everettista and still agree with most of your thesis that quantum states are states of knowledge. I think his argument was that wavefunctions are indeed mathematical constructs, but we can use them to describe those "branches of the multiverse" that interest us.
> - Andrew presented an imaginary dialogue in which he and Einstein chide you for holding that interventions in the world just tell us about the likely results of further interventions (I don't know if that's your position). Preskill said to Andrew, "you and Einstein certainly hit it off!"
> - Andrew expressed concern for your daughter, after quoting the comment that she has no theory of measurement.

Anyway, I finished (!!!) "Notes on a Paulian Idea." Your shamelessness in mixing quantum physics, philosophy, and your personal life is an inspiration; maybe it will encourage other scientists to try something similar.

I was struck by the recurrent question, why complex Hilbert spaces and not real (or quaternionic) ones? Someone ought to write a semipopular article about this issue. It seems analogous to asking why space has 3 visible dimensions and not 2 or 4, as opposed to the dozens of more nebulous questions one could ask about space.

Do you know of an argument for complex Hilbert spaces that doesn't rely on dimension-counting under tensor products (i.e. the Goldilocks Principle)? What about this: if we think time is continuous, then for all operations U, there should exist a V such that $V^2 = U$. Let U be a phase flip; then V must involve complexes.

The obvious objection is, why are phase flips possible? For example, in real quantum mechanics with arbitrary rotations but no phase flips, every operation does have a real square root. One response is that a phase flip in n dimensions can always be simulated by a rotation in $n+1$ dimensions. But since this rotation maps the n-dimensional subspace onto itself, it must (we can declare!) have a square root that also maps the subspace onto itself. That forces us to complex numbers.

It's 5AM and I'm going to sleep.

Scott's Reply

[**Chris said:**] "Am I to take this to mean that you actually read the whole thing?"

Well, I skipped some repetitive parts and extended quotations, but besides that yes. I think if condensed to, say, the best 200 pages, it would be worth publishing as a book. The jacket could say, "Just like *The Fabric of Reality* by David Deutsch ... except antithetical in its philosophical stance, and funnier!"

09-07-01 *Quantum Optics, San Feliu* (to K. Mølmer)

I just got a quote for a flight from Newark to Barcelona. It was $1182 (including taxes, etc.). That comes to about 1394 euros.

Mølmerism 1: *P.S. I started reading some of your letters in the compilation that you put on* `quant-ph`. *At first I thought: what a nut case, thinking that anybody should take an interest in such stuff, but after reading a bit, I got really impressed, and now I only regret that life is too short to read both your letters and William James. As a comment to Mermin's preface: I also had the kind of nice feeling long time after having read his paper that you describe.*

Thanks for the soothing words about the samizdat. I've gotten a lot of nice reactions (from about 25 people). My very best PRL in contrast maybe got 2! So, in the end, it has seemed worth it.

09-07-01 *The O'bleak Observer* (to N. D. Mermin)

Merminition 33: *You should not dismiss my feeling that you've not adequately justified your assumption about noncontextuality as merely a manifestation of a regretable atavistic tendency to reify the quantum state.*

The "oblique observer" note was a concession, not a dismission. It is evidence that I am taking your point very seriously (even though I'd rather being out playing with the other kids).

Merminition 34: *The question you're evading is what it means for one and the same positive operator E to appear in many different POVMS.*

No, I don't think I'm evading it. It means that those various interventions or ways of gathering data—those POVMs—physically diverse though they may be—all lead to at least one common possibility for what my knowledge can be updated to (modulo the unitary readjustment).

09-07-01 *More O'bleakness* (to N. D. Mermin)

Merminition 35:

> CAF Said: Moreover, all questions about instantaneous signaling through quantum-state change just become silly: such questions spring solely from a wrong-headed view.

No! The signalling has nothing to do with quantum-state change. (We've been through this before.) If Bob and Alice share a large number of identically prepared pairs, then a very reasonable requirement is that the statistical distribution of outcomes Bob gets from his members of the pairs cannot depend on what Alice chooses to do to her members. (If it did Alice could send useable unmediated signals to Bob.) Again, this has nothing to do with how you like to think about probabilities or quantum states. I offered this to you as an example of a situation in which you can, in fact, justify the non-contextuality of certain probabilities by appealing to an independent physical requirement (no remote signalling).

I hold firm in my opinion. It has *everything* to do with how you like to think about (quantum) probabilities. If you think the probabilities are subjective expectations for the local consequences of one's experimental interventions, then the question never arises.

However, granting you a little distrust for that, the point about oblique observations is that one might always be able to think of a quantum measurement as being enacted on a system other than the intended one. This would give your point above a natural means for being used to justify noncontextuality for *all* quantum measurements.

Again, I'm starting to feel awfully comfortable with noncontextuality as the very simplest generalization of Bayes' noncontextuality. It is the very glue that puts measurement outcomes into Hilbert space in the first place. (Otherwise we might just draw out an exhaustive list of one-outcomed, two-outcomed, three-outcomed measurements etc., etc., and never even suppose a connection between them.) But I offer the above as an effort to go in the direction you want me to.

10-07-01 $V^2 = U$ (to S. Aaronson)

Aaronsonism 2: *Do you know of an argument for complex Hilbert spaces that doesn't rely on dimension-counting under tensor products (i.e. the Goldilocks Principle)? What about this: if we think time is continuous, then for all operations U, there should exist a V such that $V^2 = U$. Let U be a phase flip; then V must involve complexes.*

I have heard some arguments not based on dimension counting. Go to Adler's book[13] and Wheeler's paper[14], both referenced on page 125 of the samizdat.

1. In the first case it is that the generators of unitary time evolution cannot then be observables.
2. In the second case it is that uniform distributions of pure states only lead to uniform distributions on the probability simplex of a fixed observable in the complex H space case.

Your argument is new to me. It might be worth writing it up properly if some reference in Adler's book hasn't yet discussed it.

Thanks again for the note on John's meeting. (And reading the samizdat!)

10-07-01 *My Recent Foundations Posting* (to J. D. Malley)

Thank you for your pleasant letter. Indeed, good hearing from you again: I trace that my last email from you was in 1996 when you were reading my PhD thesis.

I've gotten a lot of good feedback on this paper.

I checked out the Geisser book as soon as I got your letter. His attitude to parameter estimation is particularly relevant for a debate Carl Caves and I are embroiled in presently. And he raises some points similar to ones I've heard from Rüdiger Schack. So, that was a very useful reference.

Let me reply briefly to your comments.

With regard to (1), this can be posed in a purely Bayesian way through use of the "quantum de Finetti representation theorem." Maybe my description of it was too brief in the present paper. You can find a significantly longer elaboration of it in my paper with Caves and Schack, `quant-ph/0104088`.

With regard to (2), yes, true enough. But there is definitely a quantum analogue—a noncommutative analogue—to it if one only uses conditional states of knowledge (never joint). That is what my "Whither Bayes' Rule?" section is about.

With regard to (3), agreed. (This, in large part, is my present debate with Caves . . . but in the specific context of "unknown Hamiltonians.")

With regard to (4), I'm not sure I understand this. If you're suggesting that my point of view on the quantum foundations will be more interesting if it suggests some new experiments, then I most certainly agree. Unfortunately, I'm not there yet.

Jim's Preply

> Thanks for the thoroughly enjoyable and very readable account of your take on quantum foundations and its connections with information theory (e.g. `quant-ph/0106166`, 29 Jun 2001).
>
> I thought you might find it interesting to learn that your essential ideas have, it would seem to me, been the subject of much research in the statistical community for many years, though not with quantum things in mind. Specifically, there is a school of thinking which argues that as parameters in a model are generally not observable, they therefore should not be the focus of statistical inference. Instead, it is argued that making predictions should be the real purpose of inference, and to this end one should use Bayes' theorem to construct the "predictive posterior distribution." I think

[13] S. L. Adler, *Quaternionic Quantum Mechanics and Quantum Fields*, (Oxford University Press, 1995).

[14] J. A. Wheeler, "The Computer and the Universe" *International Journal of Theoretical Physics* **21**(6/7), 557–72 (1982).

this strategy accords quite well with the quantum foundations argument you're making under your imprimatur.

Prominent among those making this argument has been Seymour Geisser at the U. of Minnesota (`geisser@stat.umn.edu`). See for example, his textbook

- S. Geisser (1993), *Predictive Inference: An Introduction* (Chapman & Hall).

As background for predictive inference in a more general context you could consult

- A. H. Welsh (1996), *Aspects of Statistical Inference* (Wiley); see pp. 34–35.
- B. P. Carlin and T. A. Louis (2000), *Bayes and Empirical Bayes Methods for Data Analysis* (2nd edition) (Chapman & Hall); see pp. 20–21.

Finally I'd like to add four, small comments:

(1) given an unlimited amount of data (or, take a limit as sample size increases) the density operator is known with arbitrary accuracy, so in this sense is, finally, itself observed;

(2) use of Bayes theorem in any quantum setting is often a slippery business, since its rigorous derivation requires joint distributions for all observables: the events A and B that are outcomes in an experiment, and from which one constructs conditional probability $Pr(A|B)$, does not lead to a valid Bayes theorem unless the observables commute.

(3) it is not clear to me that the predictive posterior ever leads to any inference that is unique to its formulation—any inference obtained about parameters in a model can be considered as useful intermediates to prediction.

(4) any change in emphasis or construal of the quantum primitives has greatest interest only when new experiments are suggested, and the predictive posterior does not in itself suggest new experiments.

Again, thanks for the well-written and nicely organized discussion of this important topic.

10-07-01 *Replies on a Preskillian Meeting* (to A. J. Landahl)

Wow, what a set of notes! Thank you all for the interest in my silly efforts. This is a little unexpected.

Landahlism 1: *The talk was a smash. It went much better than I was expecting, causing much discussion. (As you know, some of the people in our group are rather reticent, so that's really saying something.)*

I am so glad to hear that. It is really very flattering.

Landahlism 2: *At the end of the talk, Sumit decided to go up to the chalkboard and take a poll, the topic and results of which I'll leave as a surprise, as I imagine John will tell you about them himself. (If he doesn't, just e-mail me back and I'll let you know.)*

I presume this is the poll Scott mentioned. I'll say more about that later.

Landahlism 3: *I also mentioned your program to establish an information-theoretic foundation for all the laws of quantum mechanics (and physics?) in this section. This proposal met with much skepticism from the audience. I'm somewhat sympathetic to your cause (certainly more so than some of our denizens!), but I don't believe that all of physics has an information theoretic description. For example, where would the (dimensionful) physical constants enter into this scheme (like Planck's constant and the speed of light)? I don't see how they could enter unless they define what physical dimensions are, which is rather peculiar.*

I really am very flattered by all this attention, but I do get dismayed when I can't seem to get the most important point across to my readers. Even sympathetic readers! Because of this, I have spent months and months trying to clarify and refine my presentation. But for some reason it is amazingly difficult to get the point across. At the very least I need people to understand what I want *before* they declare that they disagree with it. (Disagreeing with it would *then* be fair enough.) The sentences above seem to convey that you haven't gotten to the level of understanding what I want. How can the following sentences be consistent with what you say above?

1. Abstract, penultimate sentence.

   This method holds promise precisely because a large part (but not all) of the structure of quantum theory has always concerned information.

2. Section 1, last paragraph.

   Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as information—subjective information—will we be in a position to make real progress. The raw distillate that is left behind, miniscule though it may be, will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

3. Section 2, last paragraph.

   The world is sensitive to our touch. ... The whole structure of quantum mechanics—*it is speculated*—may be nothing more than the optimal method of reasoning and processing information in the light of such a fundamental (wonderful) sensitivity.

4. Section 3, penultimate paragraph, page 9.

   The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information. Put in the right mindset, this is *not* so intolerable. It is a statement about our world. There is something about the world that keeps us from ever getting more information than can be captured through the formal structure of quantum mechanics. Einstein had wanted us to look further—to find out how the incomplete information could be completed—but perhaps the real question is, "Why can it *not* be completed?"

5. Section 5, last two paragraphs.

   Perhaps the structure of the theory denotes the optimal way to reason and make decisions in light of *some* fundamental situation, waiting to be ferreted out in a more satisfactory fashion.

   This much we know: That "fundamental situation"—whatever it is—must be an ingredient Bayesian probability theory does not have. There must be something to drive a wedge between the two theories. Probability theory alone is too general of a structure. Narrowing it will require input from the world about us.

6. Section 7, last two paragraphs.

   The quantum de Finetti theorem shows that the essence of quantum-state tomography is not in revealing an "element of reality" but in deriving that various agents (who agree some minimal amount) can come to agreement in their ultimate quantum-state assignments. This is not the same thing as the stronger statement that "reality does not exist." It is simply that one need not go to the extreme of taking the "unknown quantum state" as being objectively real to make sense of the experimental practice of tomography.

   One is left with the feeling ... that perhaps this is the whole point to quantum mechanics. That is: Perhaps the missing ingredient for narrowing the structure of Bayesian probability down to the structure of quantum mechanics has been in front of us all along. It finds no better expression than in the taking account of the limitations the physical world poses to our ability to come to agreement.

I certainly believe there are some things within quantum mechanics that are beyond our subjective description. As in your example, Planck's constant could well be one of them. The dimensionality d of a Hilbert space is another one I feel fairly confident of. That number characterizes something intrinsic to a system. To that extent, it is not something that can be information-theoretic in origin.

Landahlism 4: *To press your point more forcefully in the future, you might consider rephrasing the special relativistic axioms themselves in a more information-theoretic light.*

But it doesn't seem to me that special relativity is overtly about Bayesian or information theoretic concerns in the way that quantum mechanics is. So I wouldn't want to express those axioms in a more information-theoretic light.

Landahlism 5: *Conclusion: quantum information theorists need to get out more!*

Yes! (And I speak for myself too.)

Landahlism 6: *At least I got a good groan from John Preskill when Alice flipped Bob a quarter for the "two bits" of classical communication she sent him.*

I got a similar groan from Mermin when he first read the end of Section 3. BTW, footnotes 8 and 9 are not typos; several people have asked me about that.

Landahlism 7: *In The Future section I talked about Gleason's theorem for POVMs and expressed my concerns about Emma's future psychological counseling given that you already are pressing her for a theory of measurement. None of us understood what the quote you ascribed to Hideo actually meant, which I suppose I should ask Hideo about. Do you understand it? It sure sounds amusing.*

The point is, Emma gets by without a theory of measurement, and we should all learn something from that. It is the people who think that knowledge acquisition, or better, belief acquisition, must arise from a detailed dynamical theory that are the problem.

The point of view taken here is that "detailed dynamical theories" are theories of *inference*, and therefore lie outside of the process of knowledge acquisition. This does not bar quantum theory from making contact with the REAL world—the world that was here long before man ever arose (see points about reality below)—it just means that one is not going to find it in the dynamics.

Landahlism 8 : *In the Learning section, I went over your argument for quantum collapse being a kind of Bayesian conditioning. I understand the analogy you drew here, but I hardly believe this makes quantum collapse any more gentle of a process. That's because I don't believe Bayesian conditioning is "gentle." While it's true that one can express the classical process as "plucking" a term out of a sum over conditional probabilities, the change in probabilities can be quite dramatic: the change can be from nearly zero to one in a single step!*

Fair enough. Perhaps I overplayed the imagery. The point I really wanted to emphasize is that quantum collapse can be thought of as predominantly a refinement of one's knowledge.

Landahlism 9 : *I also don't understand the meaning of the "mental readjustment" step in the quantum process. Is this just a change-of-basis for the description of the state? Is it something more or less than this? If that's all it is, then I really don't like this phrase "mental readjustment" at all.*

The track I'm on is that quantum state change is essentially Bayesian updating of knowledge, but with the proviso that the things we have to do to update our knowledge are (generally) not without effect on the world. This must be taken into account in some way. That the updating is Bayesian-like has a trace in quantum mechanics through Eqs. (57) and (58). That we still have to take into account our knowledge of our invasiveness, this has a trace in Eq. (59). That is the "mental readjustment"—i.e., taking into account what we know about our own invasiveness. (I agree, I should have found a better word for it.) When we know that our knowledge acquisition could not have physically affected the system it was concerned with, then we need do nothing whatsoever beyond Bayesian updating. Eq. (64) is an example of that.

Landahlism 10 : *I finished the Learning section by sketching how "typical von Neumann entropy" as an uncertainty measure increases after every measurement. A question I had in your argument was why the integration is done over only von Neumann measurements. I looked over your original paper on this subject and didn't find an answer there either. Some mention is made of projective measurements being "maximally predictive," but I'm not totally convinced by this argument. I suspect that the true reason for restricting attention to these measurements is technical. A measure over POVMs doesn't exist, so one can't integrate over them. Wouldn't life be so much nicer if there were one! Is this the true reason for the von Neumann measurement restriction?*

Your suspicion is correct. My choice was no deeper than that.

Landahlism 11 : *I spent most of my time discussing the Correlations section. I went in detail over your proof of what I called "Gleason's theorem for Classically Semilocalizable Operations (CSOs)" in deference to the terminology introduced in a recent paper by Beckman et al. Personally, I thought it was cool that the tensor product arose out of noncontextuality and the measurement model. John Preskill wasn't so impressed — he believes that the tensor product will arise out of any reasonable model of measurement which has the property of locality. (I.e where neither Alice's nor Bob's local actions can meaningfully impact the other.) He may be right, but for me that isn't the point. The point for me is that the proper way to view Gleason's theorem is as a machine. The input to the machine is the measurement model and the output of the machine is the state space structure and the probability law.*

Indeed you did get the point. Thanks. The point is, how much of the structure of quantum mechanics can we shove into the simple choice: "measurements = POVMs." How much of quantum mechanics is really independent of that choice? There has been a hell of a lot of work trying to reduce all of quantum mechanics to the assumption of unitarity. I'm trying to go the other way.

Landahlism 12 : *What especially excites me about this point-of-view its potential impact on quantum field theory. The main point of the Beckman et al. paper is that causal measurements and localizable measurements are not one in the same. Wouldn't it be interesting to see what happens when we impose only causality on our measurement model and send it through the "Gleason machine?" What do you suppose the resultant structure of the state space would be?*

If I understand you correctly, Mermin in his talk in Montréal and Sweden has been wondering something very similar. In fact, he would like to see the quantum probability rule AND the tensor product rule arise out of the idea that measurement cannot give instantaneous signaling. He doesn't yet feel comfortable with my (Gleason's) noncontextuality assumption. Yours is a good question; I'll try to have a look at that paper.

Landahlism 13 : *On the whole I portrayed your "party platform" as the statement that "Quantum states are states of knowledge about the consequences of future interventions."*

That statement, as it stands, is true.

Landahlism 14 : *In particular, those consequences aren't consequences to reality, but rather consequences to states of knowledge about even further future interventions.*

That statement, as it stands, is not. (Do you not see the difference?!?!)

Landahlism 15 : *In this worldview Bayesian agents don't work to align their predictions with an underlying reality.*

They would if they could, but they don't because they can't. Realizing this—it seems to me—is the first step to understanding what the quantum world is about.

Landahlism 16 : *Instead they work to align their predictions with each other. It is as if reality in this picture is solely the agreement of predictions!*

I'd be interested to hear if you believe that this is a fair characterization of your party's platform. After reading this paper, I came to the conclusion that you didn't believe in reality at all. (Or at best I thought you believed reality = knowledge.) John Preskill tells me you believe otherwise, namely that there is a reality, which surprised me.

Yeah, you botch it pretty badly there. John is right. See my diatribe under C) above. But, let me also add to that:

1. Section 4, first paragraph.

   I have been watching my two year old daughter learn things at a fantastic rate, and though there have been untold numbers of lessons for her, there have also been a sprinkling for me. For instance, I am just starting to see her come to grips with the idea that there is a world independent of her desires. What strikes me is the contrast between this and the concomitant gain in confidence I see grow in her everyday that there are aspects of existence she actually *can* control. The two go hand in hand. She pushes on the world, and sometimes it gives in a way that she has learned to predict, and sometimes it pushes back in a way she has not foreseen (and may never be able to). If she could manipulate the world to the complete desires of her will, I am quite sure, there would be little difference between wake and dream.

This wispy little piece is the closest I've been able to come to giving substance what I call "the Paulian idea." See my "Notes on a ...", page vii. The world must have some impredictability about it, otherwise we would never be able to say we have seen any trustworthy trace of a reality.

Landahlism 17: *I'm curious to hear what you believe reality is.*

Me too. The idea is not well formed yet. Perhaps this accounts somewhat for people not getting my point that the first part of attempting to identify what is real in the quantum world is to identify what is subjective and governed by "laws of thought." We should do that because that's the easier part of the program. Contemplating what's left behind is when the real fun will begin.

People are too used to seeing gurus (like Deutsch or Mohrhoff) sit on high and declare what reality *is*. My goals are more modest, even if my method of advertisement is not. I don't have an answer yet; I just feel a direction. One should not confuse my method of attack with my answer.

If you were to push me real hard on this "what-you-believe-reality-is" business, I might be inclined to say, "Read Schopenhauer's *The World as Will and Representation*." But since I haven't read it myself, I can hardly expect you to do that! Anyway, as a very *provisional* answer, I might say it's something like the "will" (the quotes around that word are very important) that Schopenhauer attributes to every piece of the world, animate and inanimate alike. For want of a better term, I call it zing.

Landahlism 18: *As for the mechanical details of the paper itself, I enjoyed your refreshingly casual writing style. I found one of your section titles to be either exceedingly clever or merely a typographical error. Either way, I'm the only one out of a dozen people who noticed it, even after I pointed it out. I'm hoping that you intended the convey the clever interpretation.*

Caves, Bilodeau, and Schumacher also asked me if it is a typo. It is not: you can be relieved.

Landahlism 19: *If so, I suggest you correct the grammar on the section title to "Wither Entanglement!" to make the homonym less subtle without sacrificing any wit.*

Too late. But in any case, I wanted all the section headings to be questions, except the beginning and end ones.

Landahlism 20: *The only other typo that jumped out at me was on page 13: "shear difficulty" should read "sheer difficulty."*

Thanks, I hadn't noticed the difference before.

Landahlism 21: *Once again, great paper. I'm psyched that it stirred up so much discussion in our group meeting.*

Me too!

Now, I said I would make some comments on Sumit's poll. But I'm too tired for that after all this writing. So I won't tell you what I think in any great detail at the present. I'll just cut and paste what Hans Primas thinks. It's below. I will say, however, that I don't see that there should be a qualitative distinction between my description of you (Andrew Landahl) and my description of the coffee maker sitting to my right. You are both physical systems embedded in this thing we call the world.

From: H. Primas, "Beyond Baconian Quantum Physics," in *Kohti uutta todellisuuskäsitystä. Juhlakirja professori Laurikaisen 75-vuotispäivänä* (Towards a New Conception of Reality. Anniversary Publication to Professor Laurikainen's 75th Birthday), edited by U. Ketvel (Yliopistopaino, Helsinki, 1990), pp. 100–112.

The methodology of experimental scientific research and engineering science is to a large extent characterized by the regulative principles emphasized by Francis Bacon. It is a tacit assumption of all engineering sciences that nature can be *manipulated* and that the initial conditions required by experiments can be brought about by interventions of the world external to the object under investigation. That is, *we assume that the experimenter has a certain freedom of action which is not accounted for by first principles of physics.* Without this freedom of choice, experiments would be impossible. Man's free will implies the ability to carry out actions, it constitutes his essence as an actor. We act under the idea of freedom, but the topic under discussion is neither man's sense of personal freedom as a subjective experience, nor the question whether this idea could be an illusion or not, nor any questions of moral philosophy, but that *the framework of experimental science requires the freedom of action as a constitutive though tacit presupposition.*

The metaphysics of Baconian science is based on the confidence that only the past is factual, that we are able to change the present state of nature, and that nothing can be known about nature except what can be proved by *experiments.* Francis Bacon's motto *dissecare naturam* led to a preferred way of dividing the world into object and observing systems. An experiment is an *intervention* in nature, it requires artificially produced and deliberately controlled, reproducible conditions. In *experiments* in contradistinction to *observations* – one *prepares* systems in initial states, *controls* some of the variables, and finally *measures* a particular variable. The regulative principles of Baconian science require *power to create initial conditions*, stress *the facticity of the past* and *the probabilistic predictability of the future*, and reject *teleological considerations.*

10-07-01 *Old McBleak's Ale House* (to N. D. Mermin)

Merminition 36: *I can't believe we're talking past each other on something this basic! [. . .]*

> [CAF wrote:] I hold firm in my opinion. It has *everything* to do with how you like to think about (quantum) probabilities. If you think the probabilities are subjective expectations for the local consequences of one's experimental interventions, then the question never arises.

To say that the question never arises is to say that probabilities can never have any bearing on frequencies of experimental outcomes.

Try it in the language you prefer: if Alice's subjective expectations for the local consequences of her experimental interventions differ, depending on what kind of an experimental intervention Bob chooses to make over in the next county — not, I stress, on what Bob learns from his intervention but just on how (or whether) he decides to intervene — then Alice will be wiped out by any competent Dutch bookie, unless unmediated action at a distance is an objective feature of the world.

We are talking past each other.

But, my wording was careful enough to cover your reply (modulo the confusing parenthesis I put around the word quantum, for which I apologize). If a physical action associated with a POVM—by definition—only affects the system associated with the POVM's Hilbert space, then by definition that is all it affects. Standard quantum mechanics has that feature.

The issue is whether we should question the reasonableness of that. Or, indeed, as you would like, turn the tables and check whether the physical requirement of no-signaling gives rise to the standard probability rule full stop. Your question is a well-posed question, I do not deny that. But, as I view it, its motivation is a throw-back to the days when entanglement was thought to have some connection to the spooky ghosts of nonlocality.

I am torn. 75% of the time, I think your question is a regressive turn to the Popescu-Rohrlich-Aharonov-Shimony-Gisin "passion-at-a-distance" mentality. It seems to me acknowledging that as an interesting paradigm (even one to be ultimately shot down) is a wrong turn. But 25% of the time, I think, "Why not? It is a valid question, so answer it if you can."

Still no sympathy for me? (Probably not.) But, am I at least coherent?

11-07-01 *Quantum Information / Evolutionary Universes* (to L. Smolin)

We've met before (when I was at Caltech), but you probably don't remember me. My name is Chris Fuchs; I'm on the research staff at Bell Labs, and my specialization is quantum information theory.

I'm writing you because lately I've taken a shine to evolutionary-universe ideas, because I've enquired with the Perimeter Institute about the opportunities they might hold for my quantum foundations program, and because I've heard that you will be joining them. (The first part of the sentence—the part about the shine—is the cause, not the effect!) Thus I would like to get to know you better.

For one thing, please allow me to share with you two of my writings on the subject:

- C. A. Fuchs, "Notes on a Paulian Idea: Foundational, Historical, Anecdotal & Forward-Looking Thoughts on the Quantum (Selected Correspondence)." 504 pages. Foreword by N. David Mermin. See `quant-ph/0105039`.

- C. A. Fuchs, "Quantum Foundations in the Light of Quantum Information," to appear in *Proceedings of the NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer*, edited by A. Gonis (Plenum Press, NY, 2001). (Until then, see `quant-ph/0106166`.)

In the case of the book (the samizdat), a good place to start within it is the chapter on Rolf Landauer. But also look up the references to John Wheeler and furthermore the discussion on Poincaré (and Boutroux) on page 195. Both manuscripts, by the way, are also posted at my website listed below. (At the website, you can find my CV in case you worry that I may not be legitimate.)

Of a more important nature, I wonder if you might help me with a project I am currently working on. It is in putting together a large compendium of quotes from and citations to materials concerned with the idea that the universe may be a "malleable" entity. It will eventually be submitted to *Studies in History and Philosophy of Modern Physics* (probably this Fall), and presently stands at 96 pages, with 422 citations. The working title is, "The Activating Observer: Resource Material for a Paulian–Wheelerish Conception of Nature."

If you wouldn't mind, I'd like you to look over it and point out any citations I missed. Especially as regards your own work, I know that I must have missed several things. But also, you may be able to help me with some obscure materials that I've never seen before. I'm shooting to make the document as complete as I can.

16-07-01 *Insults* (to S. J. van Enk)

> "The only gracious way to accept an insult is to ignore it; if you can't ignore it, top it; if you can't top it, laugh at it; if you can't laugh at it, it's probably deserved."
>
> — Russell Lynes (b. 1910), U.S. editor, critic. *Reader's Digest* (Brit. ed., Dec. 1961)

[The recording of this was prompted by my 04-07-01 note "Carts and Horses" to U. Mohrhoff. It hardly needs to be said, but on that day I was not laughing.]

18-07-01 *Horizons* (to J. Bub)

That is awful news about your visual problem and its possible causes. Please do keep me up-to-date on your health. For my own part I will cross my fingers and think of you often.

Don't worry at all about leaving me up in the air concerning a visit to Provence. Because of certain of our own medical issues, I've been lobbying my wife to postpone her European vacation until September or early October anyway. (So, you see, I would have to leave you up in the air right now too.) If it happens, it happens. The main thing is that it sounded like a good opportunity to pound out the similarities and distinctions between our points of view on quantum mechanics without being interrupted every three minutes.

I know I suggested I would write a longer letter soon, but I'm going to wimp out of it again for now. It would concern the main point of distinction I see between us (and also between myself and Pitowsky). Namely, A) that I view a large part of quantum mechanics as merely classical probability theory (which on my view may be an a priori "law of thought") PLUS an extra assumption narrowing down the characteristics of the phenomena to which we happen to be applying it to at the moment, while B) you are more tempted to view quantum mechanics as a *generalization* of classical probability theory (and with it information theory). I know that my view is not fully consistent yet, especially as I have always distrusted mathematical Platonism—which you pointed out to me I am getting oh so close to—but it still feels more right (to me, of course). Ben Schumacher, Rüdiger Schack, and I had a long discussion on this (on a long walk) the day after the round table, and I'd like to record that too. Ben took a stance quite similar to yours, and maybe even Rüdiger did too (despite his overwhelming Bayesianism). So, I may be the lonely guy out on this. And my view may be subject to change.

What I probably really need right now is more conversation than writing. So, I do hope I get to see you in an uninterrupted way soon. (By the way, would it be possible for me to get a copy of the talk you gave in Växjö? Could you copy that and mail it to me?) For now, let me post below parts of two notes I wrote Andrei Khrennikov. They touch on the discussion above, even if they are somewhat out of context here. The second note, in particular, struck me as a clean way of stating my position (in a way that I hadn't explored before). Maybe that'll help to zoom us in on the relevant issues.

There is some good news on the horizon. Gilles and I will be holding another foundations meeting in 2002. (Purely quantum info people, much like the original.) All the details aren't clear yet, but we think we may be able to have desk space for people, it may be for an extended period—maybe a month—with a revolving set of participants, etc., etc. I hope you'll be able to join us (once I get the details to you). The main difficulty is that it may have to be in the fall (after the school semester starts), which will cause participation trouble for those with a teaching load.

18-07-01 *Letters: the Long and the Short, 2* (to A. Plotnitsky)

I agree. We ought to get together (for lunch?) sometime while you're in NYC! I'll probably be tied up until at least July 25, though. Are you in email contact all the time? So that we can try to set something up more spur-of-the-moment if need be? I'd probably just take a train in and meet you somewhere convenient in Manhattan.

Do you know anything about Schopenhauer's "will"? Could you give me a small lecture on it when we meet. Would you say it's anything at all like you "efficacity"?

Arkady's Reply

> I can easily explain to you Schopenhauer's "will" and its relation to "efficacity." The short answer is no, they are not the same, but one could interestingly qualify why not and thus illuminate better what I mean by efficacity or how quantum objects should be seen according to informational, rather than ontological, view. If we do not manage to meet this time around, I could still explain this to you by email, or we might arrange for a phone date one day.

18-07-01 *Page 270!!* (to L. Smolin)

I'm excited to hear that you're reading some of my things. Especially since I've come across pages 270–272 of your book (paperback edition)! Indeed, there appears to be a significant overlap between some of our toy ideas. The one I'm speaking about is (in a technical way) the undercurrent of my paper "Quantum Foundations in the Light of Quantum Information." But, you can find broader-view statements of it on pages 156 and 190 of my samizdat. Also, you can see a trace of it in Mermin's foreword, page iii, last paragraph. Do you see the overlap that I do?

Anyway, I find this quite intriguing: Somehow, I had gotten the impression that you were a staunch many-worlder, and that our views of quantum mechanics might be diametrically opposite. I really apologize for my previous misreading (based on reading your book for an hour in a bookstore one day). I will read your book more carefully, and also look at the papers you recommended.

I also apologize for writing you back so late. Late last week I had to give a presentation to the president and vice-president of Bell Labs, and that really had me on edge until it was over with. (The VP, Bill Brinkman, by the way, will become the president of the APS in January.) After that, my wife and I took a long weekend in the country, and then I returned to find a hundred fires that needed putting out at the office.

I had contacted Perimeter (both Laflamme and Burton) previous to my contacting you, and I talked to Howard Burton for a long time on the phone Monday. So, they are aware of my existence. You may have been confused: I'm not looking for a postdoc position, but for (possibly) a long-term one. I already have a permanent research position at Bell Labs. The issue I'm toying with in my mind is in turning a more directed focus on the quantum foundations. (I've recently organized two large meetings with that theme, one in Montréal and one in Sweden—and am now working on a third bigger one in Montréal—and it's given me a taste for the hope that the quantum information community may be the key to this long-annoying puzzle.) If Perimeter can help me in making these dreams come true, then I may be all for it. What are your thoughts on Perimeter?

You asked me, "What is the Paulian idea?" I wish I knew! More seriously, the best summaries I can give you are 1) the *conjunction* of two Pauli quotes on page vii of the samizdat, and 2) the wispy little piece I wrote in the first paragraph of Section 4 of my paper "Quantum Foundations in the Light of Quantum Information". This is a very deep idea I think, and I don't know that I've ever seen it expressed anywhere except (very sketchily) in Pauli's writings. It is that, in a world where the experimental context cannot be deleted from the consequences it brings about, there must be a kind of randomness or impredictability. Else there would be no way to distinguish between wake and dream for any observer who makes use of such contexts. It is the ultimate impredictability of the consequences of our interactions with the world that gives us firm evidence that there is something beyond us. By this view, the world is not real because it can be mathematized completely, but because it cannot.

20-07-01 *The Matchmaker* (to A. J. Landahl & N. D. Mermin)

Please allow me to introduce you two to each other:

> David meet Andrew: Andrew Landahl is a promising young grad student in quantum information at Caltech, with a little bit of a taste for fundamental questions.
>
> Andrew meet David: David Mermin is a promising recent retiree from Cornell with a long history in fundamental questions, and a little bit of a taste for quantum information.

I woke up this morning and said to myself, "If you can't do good physics yourself, be a matchmaker!" So, I introduce you to each other because you've both recently written me about—I think—the same question.

Andrew said,

Landahlism 22: *I spent most of my time discussing the Correlations section. I went in detail over your proof of what I called "Gleason's theorem for Classically Semilocalizable Operations (CSOs)" in deference to the terminology introduced in a recent paper by Beckman et al. [i.e.,* `quant-ph/0102043`*]. Personally, I thought it was cool that the tensor product arose out of noncontextuality and the measurement model. John Preskill wasn't so impressed — he believes that the tensor product will arise out of any reasonable model of measurement which has the property of locality. (I.e., where neither Alice's nor Bob's local actions can meaningfully impact the other.) He may be right, but for me that isn't the point. The point for me is that the proper way to view Gleason's theorem is as a machine. The input to the machine is the measurement model and the output of the machine is the state space structure and the probability law. I suppose I should have realized this a long time ago (especially once the POVM Gleason's theorem was proven), but it took this proof to open my eyes.*

What especially excites me about this point-of-view is its potential impact on quantum field theory. The main point of the Beckman et al. paper is that causal measurements and localizable measurements are not one in the same. Wouldn't it be interesting to see what happens when we impose only causality on our measurement model and send it through the "Gleason machine?" What do you suppose the resultant structure of the state space would be?

David said,

Merminition 37: *The question you're evading is what it means for one and the same positive operator E to appear in many different POVMS. A POVM is defined by the the entire collection of E's that sum to the identity. What does it mean to associate a probability with a single one of them without reference to any of the others? Isn't this a question that simply has to be addressed, independent of whether you think a quantum state is a state of knowledge or a state of the world, or whether you think a probability is a degree of belief or an objective propensity? Different POVMs (with one E in common) are associated with entirely different interventions. But somehow you're thinking of that one E these utterly different procedures have in common as an independent outcome with a likelihood all its own. It sounds to me as if you're the one who's reifying things. [...]*

No! The signalling has nothing to do with quantum-state change. (We've been through this before.) If Bob and Alice share a large number of identically prepared pairs, then a very reasonable requirement is that the statistical distribution of outcomes Bob gets from his members of the pairs cannot depend on what Alice chooses to do to her members. (If it did Alice could send useable unmediated signals to Bob.) Again, this has nothing to do with how you like to think about probabilities or quantum states. I offered this to you as an example of a situation in which you can, in fact, justify the non-contextuality of certain probabilities by appealing to an independent physical requirement (no remote signalling).

1) Am I indeed on the right track that you two are thinking about the same things? 2) Am I to take it from Andrew's note that there is some evidence that no-signalling (in a Gleason-like theorem) may not be enough to specify the standard quantum probability rule?

If any sparks fly, I hope you'll let me be a voyeur.

21-07-01 *The Reality of Wives* (to A. J. Landahl & J. Preskill)

This morning one of the local hospitals had a fund-raising flea market, and I picked up a copy of Martin Gardner's *The Whys of a Philosophical Scrivener* for $0.50. I haven't been able to put the thing down all day; it's quite good, and the beginning parts are especially relevant to my recent discussion with you.

This evening while sitting outside enjoying the end of the day, I couldn't help but read Kiki a cute little story from it. Gardner writes:

> When I was an undergraduate philosophy student at the University of Chicago I attended a seminar given by Bertrand Russell. Carnap, then a professor at Chicago, went to these sessions and often engaged Russell in spirited debates which I only partly comprehended. On one occasion they got into a tangled argument over whether science should assert, as an ontological thesis, the reality of a world behind the phaneron. [Phaneron was Peirce's term for the world of our experience, the phenomenal world.] Carnap struggled to keep the argument technical, but Russell slyly turned it into a discussion of whether their respective wives (Russell's new wife was knitting and smiling in a back-row seat) existed in some ontologically real sense or should be regarded as mere logical fictions based on regularities in their husbands' phaneron.
>
> The next day I happened to be in the campus post office, where faculty members came to pick up mail. Professor Charles Hartshorne, a whimsical philosopher from whom I was then taking a stimulating course, walked in, recognized me, and stopped to chat.
>
> "Did you attend the Russell seminar yesterday?" he asked. "I was unable to go."
>
> "Yes," I said. "It was exciting. Russell tried to persuade Carnap that his wife existed, but Carnap wouldn't admit it."

> Hartshorne laughed. Then, by a quirk of fate, in walked Carnap to get his mail. Hartshorne introduced us (it was the first time I had met Carnap; years later we would collaborate on a book); then, to my profound embarrassment, Hartshorne said: "Mr. Gardner tells me that yesterday Russell tried to convince you your wife existed, but you wouldn't admit it."
>
> Carnap did not smile. He glowered down at me and said, "But that was not the point at all."

I followed that by saying, "You know some of my friends are afraid that I don't believe in reality. So there, you're just a figment of my imagination!" She reacted in shock. "Well, I know that can't be true," she said. "Clearly you'd make some changes!"

22-07-01 *Noncontextual Sundays* (to N. D. Mermin)

I know you're busy, but I'm going to try again. (Don't feel the need to write back until you get some time.) The issue is still noncontextuality in the Gleason-like theorems: Is it a natural assumption or not?

Here was the best answer I gave you before, but now I'm going to try to improve on it.

> **Merminition 38:** *The question you're evading is what it means for one and the same positive operator E to appear in many different POVMS.*
>
> No, I don't think I'm evading it. It means that those various interventions or ways of gathering data—those POVMs—physically diverse though they may be—all lead to at least one common possibility for what my knowledge can be updated to (modulo the unitary readjustment).

The point I'm going to try to make is that not only am I finding noncontextuality a natural assumption, but actually it may be the most *basic* assumption of the whole game. (I.e., it may even be prior to the notion that measurements correspond to POVMs.) The idea is captured above, but—I can see now—it is in too idiosyncratic of a language to convince you easily.

Here's the new shot at it (emphasizing a slightly different aspect than previously).

1) Here's the scenario. Forget about quantum mechanics for the moment. Let me take a system S and imagine acting on it with one of two machines, M and N. For the case of machine M, let us label the possible consequences of that action $\{m_1, m_2, \ldots\}$. For the case of machine N, let us label them $\{n_1, n_2, \ldots\}$.

2) If we are good Bayesians, nothing will stop us from using all the information available to us to ascribe probabilities to the consequences of these two potential actions. Thus we naturally have lying around two probability distributions, $p_M(m_k)$ and $p_N(n_k)$.

That's well and good, but it's hardly a physical theory yet. We need more. So, let us suppose the labels m_k and n_k are at least drawn from the same master set (possibly even a set with further structure, like a vector space or something). But then we must ask, under what conditions should we identify two particular labels m_i and n_j with the same element in the master set?

There's really only one thing lying around to do it with, and that's the probability assignments. If $p_M(m_i) \neq p_N(n_j)$, then surely we would not imagine identifying m_i with n_j. If, on the other hand, $p_M(m_i) = p_N(n_j)$ *regardless* of the initial state of knowledge about S, then we might think there's some warrant for it.

And that's the whole story of noncontextuality. It is nothing more than: The consequences (m_i and n_j) of our disparate actions (M and N) should be labeled the same when we would bet the same on them in all possible circumstances (i.e., regardless of our initial knowledge of S).

By this point of view, noncontextuality is a tautology—it is built in from the start. Asking why we have it is a waste of time. Where we do have a freedom is in asking why we make one particular choice of a master set over another. Why should the m_i's be drawn from the set of "effects" (i.e., the positive operators smaller than the identity on some Hilbert space)? Recall the problem on page 86 of the samizdat. Not all choices of the master set are equally interesting once we've settled on noncontextuality for the probability assignments.

You see, I really never do dismiss anything you say! Now I'm off to buy a new BBQ grill. (My family is tugging on me, and refused to let me try to hone this letter. But I really hope by this point it does make some sense.)

23-07-01 *Law without Law* (to J. Summhammer)

I very much enjoyed reading your letter to Carl Caves titled "promoting the Bayesian view." It was quite thoughtful, and makes me regret not having talked to you more while we were in Sweden.

Please allow me to ask a couple of questions based on what you wrote.

Summhammerism 1: *Here I think that, even if there is absolutely no order in the physical world, it will exhibit statistical order to a rational observer. The existence of rationality is to be taken as outside the physical world. It is a transcendental fact. (The term "law of thought" in your paper circles around the same thing.) Analysis of brain functions and molecules explains nothing, because that analysis is done by means of rationality.*

There have been times in my life when I have been very attracted to ideas like this. In particular, right now might be one of them (though I have a history of going up and going down). My first exposure to the idea came from John Wheeler's writings on what he termed "law without law." In fact presently, I'm putting together a large compendium of quotes and citations titled "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature." At the moment, it consists of 423 annotated citations, taking up 96 pages of print. The manuscript is far from complete, but will eventually be submitted to *Studies in History and Philosophy of Modern Physics*.

I wonder if you have any suggestions for things I should include in it (based on your passage above). If so, please give me as complete of references as possible. If I'm not mistaken, I detect a Kantian tinge in your thought: that's an area I haven't explored too deeply in my compendium.

Summhammerism 2: *p.21: ... the Bayesian interpretation places actualization outside its provenance ...*

I agree. Defenders of interpretations which claim to handle actualization should read selected articles on the mind-body problem, written over the last three thousand years.

Would you mind expanding on this, and also what are some of those selected articles? Can you provide references?

Anyway, it was very nice meeting you for the first time.

Johann's Reply

Now I come to your mail.

As to references on what is to be accepted as transcendentally given I would first think of Kant, in the *Critique of Pure Reason*. Then Hume should have something on it, but I read only passages of Hume. Then there is C. F. von Weizsäcker's *Aufbau der Physik* (1985), (perhaps *Reconstruction of Physics*) and his more philosophical book *Zeit und Wissen* (1992), (*Time and Knowledge*), both of which, unfortunately, are not translated into English, as far as I know. But he has a number of articles in English. Lyre has put a number of papers onto the `quant-ph` server in recent years. He is expounding on von Weizsäcker's ideas. (There is one paper of Lyre which I like quite much. It dissects Bell's "Against Measurement". Perhaps it fits into your compendium. Exposing the naive parts of Bell's thinking might be wholesome to the community.)

I have scanned through *Aufbau der Physik* and a little through *Critique of Pure Reason* over the weekend to find something that would support the following passage of my mail that you wanted me to find references to:

Summhammerism 3: *Here I think that, even if there is absolutely no order in the physical world, it will exhibit statistical order to a rational observer. The existence of rationality is to be taken as outside the physical word. It is a transcendental fact. (The term "law of thought" in your paper circles around the same thing.) Analysis of brain functions and molecules explains nothing, because that analysis is done by means of rationality.*

I found a compact one (but certainly not the best one) in *Aufbau der Physik*, where v. Weizsäcker freely outlines Kant's thinking and then says in which way his way of thinking about QM has gone beyond Kant.

It is in a section on Geist und Form (Mind and Form, pages 580–587). First he says there is no reason why abstract quantum theory should not also be applicable to knowledge which consciousness can obtain about itself. Then he remarks that one might have objections against this, and, following Kant, one objection might go like this: [I translate a section from pages 582–583]

> "... Loosely leaning on Kant we could make the following injection: Science is knowledge, and therefore — insofar as we may use the notion of consciousness — it is the contents of consciousness. Matter is the object of knowledge. In scientific usage forms are notions. Therefore it appears meaningful to explain the matter of physics by means of forms, thus notions; as such physics is what we can know of matter. Consciousness is a precondition of knowledge; consequently it would be circular to explain it by the means with which knowledge operates, the forms. Following Kant, it is for this reason that the knowing subject cannot be described as substance; for substance is itself a category, a notion.
>
> This injection is based on the argument of circularity, thus on the hierarchical approach of traditional philosophy (also see *Zeit und Wissen* 5.2.3). [But] we are moving on the circular walk around and through nature. Physics describes that which is lawful in nature, which in classical physics was called matter. Abstract quantum theory can attempt with the same right to describe that which is lawful in consciousness. ... The claim to achieve a complete description of reality in this manner is presumably unattainable; but it is legitimate to expect a consistent description. ..."

So much the quotation from von Weizsäcker. An important idea of his is the "Kreisgang der Natur" (= walking through nature in a circle). This does not mean circularity in reasoning, but recognizing that no matter with which notions you start, you must start with some, and you can try to see how far they can be illuminated when applying this whole set of notions to analyze each of them. In this von Weizsäcker seems to have been influenced by Bohr (as he himself often admits, his other influences being Plato and Kant). For instance, you could start with what we know of molecules and see how much it explains of the workings of the brain and the formation of concepts the brain arrives at and why these should in turn lead to the idea of molecules. What I like about this is that the question of what exists primarily is tacitly shown as ill-conceived. (If I am not wrong Bohr liked to ask "what do you mean by this?", and this outraged so many people, because they could not accept that you can think penetratingly and live happily without believing that all is based on material points that interact with each other. Here, von Weizsäcker has a poignant statement: Druck und Stoss sind keine Grundbegriffe der Physik. (Hitting and tossing are not basic notions of physics.) You couldn't dismiss classical physics more thoroughly. Personally I have a less dignified view of classical physics: It is the ape's way of projecting his muscular self-experience onto the world.)

Perhaps you should get in contact with von Weizsäcker directly, before he dies. The man is 89, but from what I hear still very much active in seminars. I have met him only once at a conference. He was a close friend of Heisenberg. Lyre would be able to give you his coordinates. If I am not mistaken, there was a common effort of Wheeler, von Weizsacker, D. Finkelstein and others, in the late sixties, early seventies, of really getting on with quantum theory. von Weizsäcker's input was "Ur-theory" and that the distinction of past and future is fundamental (one is fact, the other is possibility, like data and wavefunction). In Ur-theory knowledge is built up from empirically decidable alternatives, but that these decisions must have a material embodiment. That's what struck me most, when I first encountered this. That the distinction of knowing, and that what you know about need not be considered so fundamental. Knowing constitutes the thing, but not in the sense of idealism. Very quantum mechanical. A nice idea that shows how this can permit you to at least try to break out of the old realism-idealism prison is that, also human consciousness, as something that can be talked about by means of decidable alternatives, must have a physical manifestation. I translate a passage from page 581 of *Aufbau der Physik*: "... When we do the step towards concrete quantum mechanics, we will build up the alternatives encountered in obtaining the knowledge consciousness can have of itself by means of ur-alternatives. From this we must conclude that it must be possible to describe human consciousness as a body in three-dimensional space. We might expect that this should be the human body. ..." It is perhaps necessary to say that for von Weizsäcker three-dimensional space follows almost trivially from yes-no alternatives. (To me it is not that trivial.)

I think the recent effort of Anton Zeilinger and Časlav Brukner, of deriving quantum theory by saying an elementary system carries one bit is a revisit to ur-theory. (But I don't think Anton would agree, and Časlav might be hesitant. They are reading it off from their EPR and teleportation experiments, where you can posit something and then ask yourself how many bits of information do you have about it, rather than saying, I know so many bits, what something does this constitute?)

As to actualization as essentially the mind-body problem, I touched on this with Kant and Schopenhauer. My occupation with the latter was 20 years ago, and with Kant 15 years ago. But from works of von Weizsäcker I learnt that already Aristotle and Plato were aware of the problem, and the modern distinction of mind and body as two substances stems from Descartes. That is why I said the problem of actualization has three thousand years of history. To me it boils down to why there is something rather than nothing. To say there is something implies two concepts: The sensor and the sensed, or The observer and the observed, or The contemplator and the contemplated (ordered with increasing levels of complexity). Anyway, the question has a long tradition. In `quant-ph/0107005` on p.8 Ulrich Mohrhoff has just cited it in the following form: "Accounting for the existence of facts is the same as explaining why there is anything at all, rather than nothing — an impossible task." Perhaps he knows who pondered this first.

Finally, a quote from Kant, which I happened to open accidentally just now. It deals with empirical information (*Critique of Pure Reason*, in the second analogy of transcendental analysis; A210, B255, my translation from the German edition):

> "... All increase of empirical knowledge, and every progress in perception is nothing than an extension of the determinations of the inner sense, that is, a progression in time, the objects may be what they please, appearances [to the senses], or pure intuition. ..."

(My addition in rectangular brackets.)

It must have been this or a similar statement that lead me to the idea of the robot observer whose information about the world can only increase, which I mentioned in the mail to Carl Caves.

In my next mail I hope to get down to asking you a few questions on your paper "Quantum Foundations in the Light of Quantum Information". And the prior probability distribution of probabilities in the classical de Finetti theorem is causing me some trouble. Also, I would appreciate a copy of your paper (PS, PDF, or address for downloading preferred) "How much information in a state vector?" Unfortunately I had not read any of your papers before Växjö. There would have been a number of interesting points to discuss.

23-07-01 *A Nonbayesian Bayesian?* (to C. M. Caves)

I enjoyed reading your dialogue with Summhammer.

I have one question of my own.

Cavesism 1: *I do believe that natural selection only works in a world with at least a statistical order, which leads to the quasi-determinism of the macroscopic world. That we and other creatures are exquisitely attuned to this order, to the point of often finding it where it's not really there, is not surprising. The costs of finding order where there is only chaos must be less than the cost of failing to notice and take advantage of order when it is there.*

What on earth do you mean by this? Similarly when you write, "statistical order is the first element of the Bayesian reality," in Section 7 of your *Resource Material*?

Your phrase "world with statistical order" seems to teeter awfully close to an objectivist notion of probability. And it frightens me, of course, having fully made a conversion now.

23-07-01 *The Principal PrincipleS* (to C. M. Caves)

It dawned on me that I should chide you on your discussion on page 21, starting with "Two further comments on Hamiltonians ...." You left out the all-important part about how to connect the two notions of Hamiltonian, the subjective—or effective, as you call it—with the objective. It must be a Cavesian version of the principal principle: When the objective determinant of the time evolution of one's subjective states of knowledge is known, then the subjective determinant of one's subjective states of knowledge should coincide with it.

Another typo btw (I think). Page 19: "If you have maximal information about a quantum system and you want to retain it, you must know the system is Hamiltonian." Don't you mean "system's Hamiltonian"?

24-07-01 *Feynman Quote* (to me)

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis (or the atomic fact) that all things are made of atoms—little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into one another.
>
> Everything is made of atoms. That is the key hypothesis.

26-07-01 *BZZ* (to N. D. Mermin)

Renes and I are meeting Plotnitsky for lunch in NYC tomorrow. The agenda is Schopenhauer's "will" and Plotnitsky's "efficacity" (not to be confused with "effervescence").

26-07-01 *In the Middle* (to Y. J. Ng)

As you can tell from the note I just cc'd you, I'm finally taking to your paper(s) in a serious way. I'm not sure if you're familiar with all the terms I used in the letter (effects, operations, etc.), but my main worry is that your Eq. (1) numerically coincides with what has come to be known as the "standard quantum limit" (albeit in a different context), which we in the quantum measurement community know can be broken. Maybe I told you about some of those troubles during my visit to NC. Your Eq. (3) seems to be on firmer ground, but once again when we get into the quantum regime, it is not clear to me that troubles don't creep in.

Also I have some lesser—perhaps semantic—worries. Namely, identifying a single-component system with a computational device. That does not fall within the standard model of computing (either quantum or classical), the Turing machine. So, one might call the black hole a limit point of a sequence of computers, but I think I would be hard pressed to identify it with a computer itself.

Also I worry about the general nature of your "simple computers", and why these bounds should only apply to such things. This does strike me as a weak point. You really should build a stronger argument for it (i.e., one better than "this is the only case in which the derivation seems to work").

But I certainly like the general flow of the paper. It has the right feel about how to start to build up quantum considerations about spacetime—these are things that I have maybe pushed to the back of my mind for too long.

Anyway, once I hear from Ozawa, I will probably react again.

27-07-01 *In the Night* (to Y. J. Ng)

Ng-ism 1: *But the first thing that I am interested in is what replaces my Eq. (1) according to you.*

My fear is that there is simply no limit at all. (But it really is a fear, and I hope I'm wrong for your sake.) While we await Ozawa's reply, have a look at the article below. As I recall, it is written in a style that you would appreciate. It gives a nice physical motivation for the SQL that is very similar in flavor to the derivations in your article. Tell me whether you find its arguments believable. The problem is, they turned out to be wrong. You'll also note the significant overlap between that problem and yours. So, even if it is otherwise irrelevant—which I am afraid it's not—we're all bound to learn something from tabulating the distinctions. I really need to look up the old Wigner article, but I haven't had a chance to get to the library and I'm traveling this weekend.

> Phys. Rev. Lett. **54**, 2465–2468 (1985)
>
> Defense of the standard quantum limit for free-mass position
>
> Carlton M. Caves
> Theoretical Astrophysics 130-33, California Institute of Technology, Pasadena, California 91125
>
> Measurements of the position x of a free mass m are thought to be governed by the standard quantum limit (SQL): In two successive measurements of x spaced a time τ apart, the result of the second measurement cannot be predicted with uncertainty smaller than $(\hbar\tau/m)^{1/2}$. Yuen has suggested there might be ways to beat the SQL. Here I give an improved formulation of the SQL, and I argue for, but do not prove, its validity.

29-07-01 *Sunday Morning, Thinking of You* (to H. J. Bernstein)

Hey old philosophy friend,

I haven't heard from you in a while. And you never even replied to my last message. So, I wonder whether you got it.

Lately I've taken to trying to mix Arthur Schopenhauer, John Dewey and William James into a good quantum stew. I'm certainly getting the feeling that some of their ideas were before their time (and, indeed, a better fit to the quantum world than the classical world of their focus).

But do write me. I am lonely for your company.

30-07-01 *The Morning After* (to S. Henry)

Henryism 1: *I remember you dude! I guess the most vivid memory is being at a party at Marie Clark's house. If I remember correctly you called me Mr. Magician because I did a little trick with a quarter or something. Or maybe it was the way I could persuade Lori to do what I wanted while you had a harder time getting her to bend to your will! (hehe)*

It seems you've really excelled in science. And yes I'm totally interested in "quantum teleportation" and physics in general. I haven't read any of your writings yet but will do so. I've read several books on physics/science over the years. Some of my favorites are Hyperspace *by Michio Kaku, and* A Brief History of Time *by Stephen Hawking. And, of course, I did read* The Tao Of Physics *by Fritjof Capra that explores the parallels between physics and Eastern mysticism. It sounded so much like The Tao of Pooh I just had to read it. If you know of any other books/authors that you think I'd be interested in please let me know!*

I remember deciding I would devote most of my life to quantum mechanics my second week in college after reading Heinz Pagel's *The Cosmic Code*. It made a big splash in my mind at the time: I don't know what it'd do now, maybe the same. For my present mentality, a very good book for the layman is David Mermin's *Boojums All the Way Through*. (Most importantly, read the chapters on "quantum mysteries for anyone.") Both these books are out of print, I think. But you can find them easily enough in a library, or by going to the used book section at `BarnesAndNoble.com` (there's hardly a book one can't find there). Things getting closer to what I do in the field—quantum information theory and quantum computing—can be found in Tom Siegfried's *The Bit and the Pendulum*. I've heard it's pretty good, but I haven't read it myself. Also, tangentially, Simon Singh's *The Code Book*—in connection to quantum cryptography—but, again, I haven't read it either. I've just heard good things about it.

Henryism 2: *So how'd you find my webpage? Were you looking for the DJ Scott Henry and found my page instead?*

No, it was 4:00 AM, and I was going through the search engines looking for things on Arthur Schopenhauer and his ideas about "the world as will and representation." An advertisement for `classmates.com` kept flickering at me, and I finally had a weak moment. So, I went to have a look (like I did a few months earlier when I put my own name there). This time I saw your name and went to your website.

30-07-01 *Britannica* (to J. M. Renes)

This was the only passage in the *Encyclopedia Britannica* that I could find about Nietzsche that even remotely resembled quantum mechanics. In general, he looks like a lot to wade through for little return.

> Perspectivism is a concept which holds that knowledge is always perspectival, that there are no immaculate perceptions, and that knowledge from no point of view is as incoherent a notion as seeing from no particular vantage point. Perspectivism also denies the possibility of an all-inclusive perspective, which could contain all others and, hence, make reality available as it is in itself. The concept of such an all-inclusive perspective is as incoherent as the concept of seeing an object from every possible vantage point simultaneously.
>
> Nietzsche's perspectivism has sometimes been mistakenly identified with relativism and skepticism. Nonetheless, it raises the question of how one is to understand Nietzsche's own theses, for example, that the dominant values of the common heritage have been underwritten by an ascetic ideal. Is this thesis true absolutely or only from a certain perspective? It may also be asked whether perspectivism can be asserted consistently without self-contradiction, since perspectivism must presumably be true in an absolute, that is a nonperspectival sense. Concerns such as these have generated much fruitful Nietzsche commentary as well as useful work in the theory of knowledge.

31-07-01 *Parachutes* (to C. H. Bennett)

Bennettism 7: *I'd be glad to recommend you to the Perimeter Institute, and I think it a prudent preparation given what I've heard about Lucent. But before you place too much faith in a place called perimeter, I would remind you of a famous saying in general relativity (a field I understand hardly at all) to the effect that the boundary of a boundary is nothing.*

John Wheeler would say "the boundary of a boundary is zero," so you were pretty close. But, yes, you pinpoint a worry of mine.

Today, the bosses are gonna give another big general announcement. I'm sitting on needles and pins. I just want to do physics, and stop worrying about jobs!

01-08-01 *The Montréal Commune* (to W. K. Wootters)

Gilles and I are once again putting together plans for a quantum information/foundations party in Montréal, though this one may be a little more like a commune than a party. The Montréal Commune. (Probably more officially, "Workshop on the Impact of Quantum Information Theory on Quantum Foundations," or some such thing.) We hope you will join us as a communard. [...]

On another subject, the meeting in Sweden went quite well, but we did miss you. Maybe some of the most interesting discussions centered around Andy Steane's paper "Quantum Computation Only Needs One World" (which Richard Jozsa presented). Doug Bilodeau had the wonderful idea that perhaps some combination of it and your old Ph.D. thesis could give us a deeper insight into where quantum computing derives its power from: Quantum computers are not powerful because they perform so many calculations in parallel (as the many-worlds pundits imagine), but rather because they do so FEW calculations! I.e., Their power derives from not doing anything they don't have to do for the final result. (Much like in your thesis, the photon—which can only express its preparation through a probabilistic law—does better by explicitly NOT carrying around the baggage of a local hidden variable theory.) So, your input at these meetings really would be very valuable.

Let us hear from you as soon as you can.

01-08-01 *Missing Anyone?* (to L. Hardy)

Good to hear back from you so quickly. Let me ask your advice: would you mind looking over my list of potential attendees and telling me whether I missed anyone really interesting? The criteria for getting there are:

1. Have to be practicing quantum information or something sufficiently close,

2. Should have a healthy attitude that there are still some mysteries in QM. (By this criterion, Bennett, Deutsch, and Griffiths probably should be taken off the list.)

02-08-01 *Schopenhauer* (to U. Mohrhoff)

Schopenhauer-like, not Kant-like.

02-08-01 *Quantum Philosophy* (to C. H. Bennett)

That guy Mohrhoff. By his classification, you are indeed more Platonic and I more Kantian. (Even though I don't think I'm overly Kantian.) But more importantly, also according to him ... HAH! ... I am merely "unilluminating" whereas you are "inconsistent."

02-08-01 *The Montréal Commune – Ditto* (to B. W. Schumacher)

I've marked you down in my spreadsheet, and we'll let you know what's up in a couple of weeks.

Schumacherism 1: *An interesting result that I happened on, based on two propositions: (1) "Information" resides in the relation between systems, and (2) "classical" information is exactly that information which may be copied. So we have two systems, Q and R, in a joint state ρ_{RQ}. Think of R as a "record" of Q. Suppose we require that there exists an operation on R only such that, at the end of the day, there are two systems $R1$ and $R2$ so that $\rho_{R1Q} = \rho_{R2Q} = \rho_{RQ}$. (So both $R1$ and $R2$ can have just the same relation with Q that R had.) This is possible if and only if the state is of the form*

$$\rho_{RQ} = \sum_k p_k |k\rangle\langle k| \otimes \rho_k$$

where $|k\rangle$ is an orthonormal set of R-states. The result is a pleasant application of the no-broadcasting theorem.

I like your result (which can't be questioned)!

No subject without an object. No object without a subject. No information without both. I like that—it seems like a good track for ontologizing information, to the extent that it can be. The only thing that scares me is your secret desire to reify the quantum state—namely by translating (1) into a statement about bipartite quantum states (which seem to me to have no other good interpretation than information to begin with)! It takes information to get information off the ground?

03-08-01 *Making Quantum Look Like Bayes* (to M. C. Galavotti)

I was reading Brian Skyrms' book *Pragmatism and Empiricism* this morning, and I thought of you. Perhaps you remember me from the old note below? It dawned on me that I haven't told you about two of my latest papers (listed below). They can be found on the Los Alamos archive `http://xxx.lanl.gov/find/quant-ph` by doing a search on my name, or, if you wish, I can mail them to you (provide an address). I think you may enjoy them, as they go the greatest distance yet in my efforts to draw out the analogies between quantum mechanics and Bayesian probability theory.

I hope things are going well for you and that you are continuing your own Bayesian efforts.

- C. A. Fuchs, "Quantum Foundations in the Light of Quantum Information," to appear in *Proceedings of the NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer* edited by A. Gonis (Plenum Press, NY, 2001). (Until then, see `quant-ph/0106166`.)
- C. M. Caves, C. A. Fuchs and R. Schack, "Making Good Sense of Quantum Probabilities," submitted to *Physical Review A*. (See `quant-ph/0106133`.)

Letter to M. C. Galavotti dated 5 May 1998, "If It's Not Too Late ..."

> I have just read your paper "Probabilism and Beyond" and am now even more excited than I was several days ago! It looks as if there is much that I have missed out on by not having studied Ramsey and de Finetti before! Unfortunately, I am also finding that there is even more of your work that I am not able to get my hands on here at this small library. (Though Caltech has had literally 26 Nobel Prizes distributed to its faculty and alumni, it has a library about the size of a pea.) If it is not too late—if you have not already sent the last one—can you also send me your article "F. P. Ramsey and the Notion of Chance"? I am now hoping that something along the lines of Ramsey's "objective chance"—to the extent that I understand the notion from your last article—is just the sort of thing that I have been looking for.
>
> My collection of your papers presently consists of:
>
> 1. M. C. Galavotti, "Comments on Patrick Suppes 'Propensity Interpretations of Probability'" Erkenntnis **26**, 359–368 (1987).
> 2. M. C. Galavotti, "Anti-Realism in the Philosophy of Probability: Bruno de Finetti's Subjectivism," Erkenntnis **31**, 239–261 (1989).
> 3. M. C. Galavotti, "The Notion of Subjective Probability in the Work of Ramsey and de Finetti," Theoria **62**, 239–259 (1991).
> 4. M. C. Galavotti, "Probabilism and Beyond," Erkenntnis **45**, 113–125 (1996).
>
> If there is anything else that you've written that you think I should be aware of, please send that on also!

05-08-01 *Sunday Morning, Thinking of You, 2* (to H. J. Bernstein)

Bernsteinism 1: *ISIS's fall series is supposed to be about scientific and medical ethics: Can you connect your proposed talk and simplify its level to be appropriate?*

Does that mean there's an honorarium that comes with giving the talk?

I might be induced to give a William James kind of lecture ... with a title something like, "Ethics in an Ultimately Lawless World." Or, "Ethics in a Law-Without-Law World." Or, "Timeless Ethics in an Evolving Universe?" Use your imagination.

07-08-01 *Knowledge, Only Knowledge* (to T. A. Brun, J. Finkelstein & N. D. Mermin)

Below is a note I started composing last Friday—but then had to leave for a long weekend for my wife's birthday—and only finished up today. In the mean time, Todd and Jerry have skirted very close to the point I wanted to make. So, the note is not quite as relevant as it might have been, but maybe some of it is still worth contemplating.

Allow me to start off in a fanciful way (like usual) with a couple of quotes:

> The subjectivist, operationalist viewpoint has led us to the conclusion that, if we aspire to quantitative coherence, individual degrees of belief, expressed as probabilities, are inescapably the starting point for descriptions of uncertainty. There can be

no theories without theoreticians; no learning without learners; in general, no science without scientists. It follows that learning processes, whatever their particular concerns and fashions at any given point in time, are necessarily reasoning processes which take place in the minds of individuals. To be sure, the object of attention and interest may well be an assumed external, objective reality: but the actuality of the learning process consists in the evolution of individual, subjective beliefs about that reality. However, it is important to emphasize, as in our earlier discussion in Section 2.8, that the primitive and fundamental notions of *individual* preference and belief will typically provide the starting point for *interpersonal* communication and reporting processes. In what follows, both here, and more particularly in Chapter 5, we shall therefore often be concerned to identify and examine features of the individual learning process which relate to interpersonal issues, such as the conditions under which an approximate consensus of beliefs might occur in a population of individuals. — pp. 165–166, Bernardo and Smith, *Bayesian Theory*

What is the nature and scope of Bayesian Statistics within this spectrum of activity? Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way." — p. 4, Bernardo and Smith, *Bayesian Theory*

Thanks again to all of you for letting me look in on your interesting emails! I've learned a lot from this exchange. Last night I was up between 1:30 and 5:00 reading them all one more time (and David's original paper too), and thinking much harder than I had before on these issues. So, now I hope to say some things in that regard, but not inane things (as I had done a couple of days ago).

The main thing that started striking me more deeply last night is that now it is very obvious that you *have* an answer, but—much more than ever before—I don't really understand what the question ought to be. In a sense, I'm only coming across the same troubles (Samizdat, p. 236) I've had ever since David first wrote me on this issue, and it relates to the comment I made after his talk in Växjö (in case he remembers). Let me try to explain.

David, in his paper, quotes Peierls as saying,

Merminition 39: *In my view the most fundamental statement of quantum mechanics is that the wavefunction, or, more generally the density matrix, represents our knowledge of the system we are trying to describe. ... [Yet, density matrices] may differ, as the nature and amount of knowledge may differ. People may have observed the system by different methods, with more or less accuracy; they may have seen part of the results of another physicist. However, there are limitations to the extent to which their knowledge may differ.*

And David himself says,

Merminition 40: *I have the feeling that if quantum mechanics is really about knowledge and only knowledge, then there ought to be further elementary constraints on the possible density matrices describing one and the same physical system that are stronger than the very weak second condition of Peierls, but not as strong as his overly restrictive first condition.*

What is being called for here—perhaps unintentionally—is a way to think about quantum mechanics from the bottom up (as Caves, Schack, and I might like it, in a Bayesian way), rather than from the top down (as Everett, Deutsch, and Bennett might like it). That is, one should view quantum mechanics as a conduit for stitching our individual pictures/thoughts/beliefs into a pastiche we ultimately call "the world." This contrasts with imagining that we have miraculously grasped the ultimate reality (the universal wavefunction, say), and can somehow see our individual points of view as being derived back out of that.

But if this is the case, then I cannot understand Peierls' command, David's quest, or the *answer* all three of you ended up coming up with:

> Two density operators ρ_a and ρ_b can describe the same system if and only if the support of ρ_a has a nontrivial intersection with the support of ρ_b.

This theorem is certainly consistent with a top-down view of the Everett sort—that, to be explained below, is really is what it seems to me to demonstrate—but it is not consistent with the bottom-up view. For, from the bottom-up view, there should not—and more importantly, there cannot—be any constraints whatsoever on what an agent can believe. I have every right to be as wrong-headed as I want to be with respect to you: The density operator belongs to me, not to you, and not to the system. I have every right to say inane things and make inane predictions—I do it all the time. What I should not do, however, if I want to remain rational, is refuse to listen to you when you point out my inanities or refuse to listen to the detector clicks that contradict my previous predictions. From the Bayesian view, it is the process of updating and the general structure of beliefs that is constrained by rationality—i.e., by the physical world, or the Platonic ideal, depending upon your orientation. It is not the actual beliefs themselves.

This point, perhaps more than any other, is why I (and Caves and Schack) should adopt the word "belief"—rather than "knowledge" or "information"—for describing the operational significance of quantum states. (I will try to be more consistent in the future, but that is really an aside as far as this note is concerned.)

Of course, your theorem is a theorem—or at least I can see nothing wrong with it—the issue here is how I, with my little Bayesian mind, can put it into a context I am more happy with. At first, I was the most pleased with Jerry's way of motivating it: From that point of view, what the theorem seems to express is simply the conditions under which a third agent Carol can consistently incorporate Alice and Bob's disparate beliefs into her own belief system.

But from the Bayesian view, why should we care about a Carol at all? What if there's no Carol to be found? What if neither Alice nor Bob ever intend to share their thoughts about this poor physical system with anyone else? To say that there is always a Carol about, or that there ought to be one, is to come dangerously close to endorsing the Everettian (or, for that matter, Bishop Berkeleyian) program. This, of course, may not bother Jerry or Todd—I'm not completely sure about their foundational dispositions—but it does bother me, and I suspect it might bother David, with his newfound deconstructionist tendencies.

Thus, it now seems to me that Todd's original way of posing the issue may be the safer way after all. BUT that is not because it gives us *the* answer, but instead *an* answer. (I.e., there should *only* be sufficient conditions, rather than necessary ones.) Let me let Bernardo and Smith speak again (and again):

> [T]here is an interesting sense, even from our standpoint, in which the parametric model and the prior can be seen as having different roles. Instead of viewing these roles as corresponding to an objective/subjective dichotomy, we view them in terms of an intersubjective/subjective dichotomy. To this end, consider a *group* of Bayesians, all concerned with their belief distributions for the same sequence of observables. In the absence of any general agreement over assumptions of symmetry, invariance or sufficiency, the individuals are each simply left with their own subjective assessments. However, given some set of common assumptions, the results of this chapter imply that the entire group will structure their beliefs using some common form of mixture representation. Within the mixture, the parametric forms adopted will be the same (the *intersubjective* component), while the priors for the parameter will differ from individual to individual (the *subjective* component). Such intersubjective agreement clearly facilitates communication within the group and reduces areas of potential disagreement to just that of different prior judgements for the parameter. As we shall see in Chapter 5, judgements about the parameter will tend more towards a consensus as more data are acquired, so that such a group of Bayesians may eventually come to share very similar beliefs, even if their initial judgements about the parameter were markedly different. We emphasize again, however, that the key element here is intersubjective agreement or consensus. We can find no real role for the idea of objectivity except, perhaps, as a possibly convenient, but potentially misleading, "shorthand" for intersubjective communality of beliefs. — pp. 236–237, Bernardo and Smith, *Bayesian Theory*

> In the approach we have adopted, the fundamental notion of a model is that of a predictive probability specification of observables. However, the forms of representation theorems we have been discussing provide, in typical cases, a basis for separating out, if required, two components; the parametric model, and the belief model for the parameters. Indeed, we have drawn attention in Section 4.8.2 to the fact that shared structural belief assumptions among a group of individuals can imply the adoption of a common form of parametric model, while allowing the belief models for the parameters to vary from individual to individual. One might go further and argue that without some element of agreement of this kind there would be great difficulty in obtaining any meaningful form of scientific discussion or possible consensus. — p. 237, Bernardo and Smith, *Bayesian Theory*

You'll find something similar to this theme infused throughout my paper `quant-ph/0106166`, "Quantum Foundations in the Light of Quantum Information." In the present context the question is, under what conditions (on the density operators themselves), can Alice and Bob move toward consensus in their future density operator assignments for a system? You guys have answered this for the *case* when there are some extra systems beside the one of interest (all noninteracting) lying around for Alice and Bob to make measurements on. If there is no overlap between the initial supports, then no measurement-at-a-distance, which can only have the effect of refining a density operator, can get Alice and Bob any closer to agreement.

What about necessity? As I've tried to say, there can be no requirement of necessity from the Bayesian view I'd like to see prevail. Instead the question is *always* to identify those situations where agents (with disparate beliefs) can move toward consensus be it by indirect measurements, direct ones, or even by communication with further members of the community.

A good and indicative example comes from the quantum de Finetti theorem along with the points made by `quant-ph/0008113` (Schack, Brun, and Caves). From it, we have a natural case where two agents can start out with distinct density operator assignments on a large collection of systems, but through updating via some commonly viewed measurement outcomes, they can move toward complete agreement in their estimates of the outcomes of all future measurements. The only thing the agents need to walk into the room with is this much INITIAL AGREEMENT: (1) that the systems in the collection are exchangeable, and (2) that the parametric form given by the de Finetti theorem has full support (in the sense defined there). Without that initial agreement, the techniques of quantum-state tomography would lead to no final agreement at all. No one can require that two observers in Mike Raymer's lab must walk into it with such an initial agreement, but if they happen to, there will be a reward at the end of the day.

That's my spiel. How would I modify David's quest in light of all that I've just said? Here's my shot:

> I have the feeling that if quantum mechanics is really about knowledge and only knowledge—or better, belief convergence and *only* belief convergence—then FOR ANY GIVEN METHOD OF GATHERING INFORMATION, there *should* be a way to ferret out of quantum mechanics the necessary and sufficient conditions on two observers' initial state assignments, so that the gathered information leaves them in a better agreement than they started out with.

Let me give you an example of where such an exercise can go. This is a question I posed to Rüdiger while we were in Växjö, but also it is a generalization of Todd's considerations (now letting Alice and Bob's measurements being disturbing ones).

Let us agree on a distance measure on density operators. A convenient one (and my favorite) is

$$d(\rho, \sigma) = \mathrm{tr}|\rho - \sigma|,$$

but that's not so important for the considerations here. (Well, it may be in the long-run, but I don't want to let such a technicality detract from posing the question.)

Suppose Alice walks into a room and says to herself that a system is described by ρ, while Bob says to himself that it is described by σ. We can gauge their amount of initial consensus by $d(\rho, \sigma)$. Now, suppose Carol (or Alice or Bob for that matter) performs a measurement on the given system whose action—i.e., whose associated completely positive map—is to take any initial state τ to

$$\tau \longrightarrow \tau_b = \frac{1}{\mathrm{tr}(\tau A_b^\dagger A_b)} A_b \tau A_b^\dagger$$

depending upon the particular outcome b. Here, of course,

$$\sum_b A_b^\dagger A_b = I.$$

(This is nothing more than the general form of an "efficient" measurement as defined in my QFILQI paper cited above.) Let us allow Alice and Bob to be privy to this map, and indeed to the actual outcome b.

Thus, the final consensus of Alice and Bob will be gauged by $d(\rho_b, \sigma_b)$. The question is: When can Alice and Bob expect to be in better agreement after the measurement than before? That is, as far as Alice is concerned, she will *expect* their final distance to be

$$D_A = \sum \mathrm{tr}(\rho A_b^\dagger A_b) d(\rho_b, \sigma_b).$$

As far as Bob is concerned, he will *expect* their final distance to be

$$D_B = \sum \mathrm{tr}(\sigma A_b^\dagger A_b) d(\rho_b, \sigma_b).$$

For a given set of A_b's, what are the necessary and sufficient conditions on ρ and σ so that

$$D_A \le d(\rho, \sigma)$$

and

$$D_B \le d(\rho, \sigma) \text{ ?}$$

Suppose we can answer these questions. Then we will be able to identify the minimal fact Alice and Bob must reveal to each other (even without explicitly revealing their full beliefs captured by ρ and σ) so that they can expect to walk out of the room in better agreement ... even if they still can't say with certainty what each other now believes.

Beliefs, only beliefs. But sometimes we can say something about their convergence. And you guys have provided an example.

07-08-01 *Knowledge, Only Knowledge: Amendment* (to T. A. Brun, J. Finkelstein & N. D. Mermin)

I just reread the thing after sending it. Let me be more careful before one of you accuses me of being a flaming positivist again. (I suspect Todd, in particular, will be inclined to do so, and I want to fend that off before it happens.) In my closing sentence, I wrote:

> Beliefs, only beliefs. But sometimes we can say something about their convergence. And you guys have provided an example.

Let me temper that to:

> Beliefs, only beliefs. But sometimes we can say something about their convergence, as they are steered by our interactions with the world external to us. And you guys have provided an example.

Now, I can sleep safely ...
Good wishes to all!

07-08-01 *The First Amendment* (to J. Finkelstein)

Thanks for the note. But nothing changes for me (yet, at least). Sorry for my overemphasis on Carol.

Finkelsteinism 5: *If that can be agreed to, then the rest is essentially just algebra. $S[\rho_a]$ is the orthogonal complement of the zero-eigensubspace of ρ_a, which is the set of $|\phi\rangle$ such that Alice knows with certainty that a measurement of $|\phi\rangle\langle\phi|$ must yield the result zero. So the support of the updated version of ρ_a must be a subset of the support of the original ρ_a. Likewise, Alice can see that the support of the updated ρ must be a subset of the support of ρ_b. Etc.*

The issue for me is not whether Alice *might* be willing to incorporate Bob's beliefs/knowledge into her own knowledge base—that, I'll grant her, in which case I agree that everything you say is true. Instead it is whether she *must* be willing to do it. I think you are (tacitly) trying to get me to agree that she *must* be willing to accept Bob's quantum state as extra, valid information. That's something I can't do. I reserve the right for Alice to think that Bob's quantum state is complete nonsense, something that she would never want to incorporate into her own knowledge base. (It's part of being an American.)

To say that two observers *must* be willing to incorporate their separate states of knowledge into a single state is—I think—to tacitly (there's that word again) accept an Everettian kind of view. For it never allows that quantum states are states of belief in the normal sense. Instead it makes them more like "objective points of view" (relative states) that *must* be derivable from a larger, more encompassing picture. Why else would the states have to be "consistent" with each other (for instance, in your and Todd's sense)?

Is that helping any to get my wacky point of view across?

07-08-01 *Kiki, James, and Dewey* (to J. A. Waskan)

The last month or so, I've been logging quite a few hours in the philosophical world. I read about 100 pages from this little book: A. Schopenhauer, *The Philosophy of Schopenhauer*, edited, with an introduction by I. Edman, (Modern Library, New York, 1928).

It's a small collection of pieces from Schopenhauer's big masterwork. I read about 50 pages on the world as representation, and about 50 on the world as will. Even that little was not easy for me. It's fanciful stuff, but maybe I extracted an idea or two that I like. The main one is simply the idea of a dichotomy between what things look like from the inside of any phenomenon (when there is a view from that perspective)—Schopenhauer called it will—and what things look like from the outside of the phenomenon (when there is a view from that perspective)—Schopenhauer called it idea. Or, maybe I should have more safely said "is" rather than "look like." But, in any case, that distinction (and a strict separation between the concepts) strikes me as useful or at least worth contemplating. Just about all the rest, though, I probably wouldn't be able to accept: the strict Kantian categories, the principle of sufficient reason, etc., etc.

On the other hand, I have gotten *completely* carried away with William James and John Dewey. Here's how I put it to my friend Carl Caves the other day:

> Today I focused on rounding up some more William James, John Dewey, Percy Bridgman material. I think James is taking me over like a new lover. I had read a little bit of him before, but I think I was more impressed with his writing style than anything. But I was drawn back to him by accident, after reading Martin Gardner's *Whys of a Philosophical Scrivener*. Gardner devoted a lot of time knocking down James' theory of truth, because it is just so much easier to accept an underlying reality that signifies whether a proposition is true or false, rather than saying that the knowing agent is involved in eliciting the very proposition itself (along with its truth value). And something clicked! I could see that what James was talking about might as well have been a debate about quantum mechanics. He was saying everything in just the right way. (Let me translate that: he was saying things in a way similar to the way I did in my NATO "appassionato.") And things have only gotten better since.

And indeed, they have only gotten better since! Since coming to Munich, I have not been able to put James and Dewey down (when I'm not writing emails trying to translate their ideas into the quantum mechanical context, in particular for a technical problem Caves, Schack and I are disagreeing violently on). I read James' *Pragmatism, and four essays from the Meaning of Truth*, and now I'm about halfway through *John Dewey: The Essential Writings*. I'm moved by this stuff like nothing else I've ever read.

You can't tell me philosophers don't have the good life!

07-08-01 *Parachutes* (to J. Preskill)

Preskillism 2: *[T]hanks for teaching me the word "communard". . . . [I] guess I have a conflict in October 2002. I have agreed to go to Leiden for that month to be the "Lorentz chair," teaching a short course on quantum computing. It is an odd thing to do for someone who travels as little as I do, but they convinced me that it is too big an honor to turn down. The list of past recipients includes 10 Nobel Prize winners.*

Congratulations on the Lorentz chair thing. That is quite an honor. I looked at the list: Steam started rising from my computer screen! Here's my challenge: Use the fanaticism of Wheeler, the thoroughness of Wigner, the religion of Rosenfeld, the science fiction of Klein, the common sense of Peierls, the intuition of Yang, and the poetry of Mermin to tell your audience in a convincing way that quantum computing and information is just the beginning of quantum mechanics, not the end.

08-08-01 *Cross Entropy Min* (to J. Finkelstein)

Finkelsteinism 6: *Which brings up a slightly different question. Suppose, again, that Alice describes a system by ρ_a, but this time let's say for simplicity that she considers what would happen if Bob were to tell her that his ρ_b corresponded to a pure state $|\phi\rangle$; then, given her knowledge of the system (ie, given ρ_a) what could Alice say about what $|\phi\rangle$ might be?*

It follows from what we have been saying that Alice knows that for $|\phi\rangle$ to be possible, it must be in $S[\rho_a]$, and that (in finite dimensions, at least) she cannot, based on her own knowledge, rule out any $|\phi\rangle$ that is in $S[\rho_a]$. But can Alice say any more than that? Would it make sense for Alice to put a probability distribution on the possible $|\phi\rangle$ that Bob might announce to her? (It would have to be a probability density; eg, if ρ_a were a multiple of the identity, then Alice would surely judge all states to be equally-likely.) If that did make sense, it would mean that Alice would be constructing a particular (continuous) ensemble representation of ρ_a. What could that be?

As I recall, this is quite similar to the classical problem ("principle of minimum cross-entropy") explored in the two references below. But I'm not going to have a chance to refresh my memory for a while. If you've got the time, you might see if it's relevant.

1. John E. Shore and Rodney W. Johnson, "Axiomatic Derivation of the Principle of Maximum Entropy and the Principle of Minimum Cross-Entropy," IEEE Transactions on Information Theory **IT-26**(1), 26–37 (1980).

2. John E. Shore and Rodney W. Johnson, "Properties of Cross-Entropy Minimization," IEEE Transactions on Information Theory **IT-27**(4), 472–482 (1981).

08-08-01 *The First Eye* (to C. M. Caves)

I am just about to get down to some serious (political) scheming to do with our Bayesian program: I'll let you know what I'm talking about if it turns out to be successful. First though, I want to take a moment to tell you about a point of similarity between our (both far-from-completely worked out) flavors of quantum ontology. This one just struck me a few days ago.

Some time ago, I tried to explain to you what I was hoping for for [sic] an ontology behind quantum mechanics. (See Samizdat, pp. 127–129.) I said it would have something to do with the information-disturbance tradeoff in quantum eavesdropping. You replied that, try as you might, you could see no ontological content in such a statement. I think what was troubling you was that the information-disturbance relations (as I am thinking of them), by their very nature, require explicit reference to the *subjective* points-of-view/opinions/beliefs (i.e., quantum states) of various *agents*.

I, on the other hand, have no problem with that. For the way to think about it is that the world (independent of our existence) has latent within it a property that simply has no way of being properly expressed without inserting information-manipulating agents into the picture. (Or, at the very least, that this anthropocentric way of stating things may be our first firm handle for getting at a better, more objective-sounding, formulation of the latent property.) Taking this tack does not mean, of course, that there is no world independent of human existence, and that is my point. It just means that we may sometimes have to take into account our (presumably contingent) existence for expressing some of the world's properties.

Here is something, however, that you should think about in connection to your "world = Hamiltonian" hopes. Let me put the ball back into your court. Can you explain to me the role of Hamiltonians in your ontology in a way that does not make use—even tacitly—of the concept of a (subjective) quantum state? What is it that Hamiltonians do if their primary role is not in evolving (subjective, agent-required) quantum states? In the classical world, one could give an answer to this question by saying, "They evolve the positions of phase-space points." Such a statement makes no use of the concept of information-bearing agents for its formulation. (I view it as just nitpicking to argue whether the points or their trajectories (from which we can derive the Hamiltonian) are the more primary of the entities. Or whether they are equally primary.) But in the quantum case, I haven't yet seen what you can say if I take away the linguistic tool of "the quantum state" from your explanatory repertoire. What can you say?

I'll put a few passages of Schopenhauer below to inspire you. [From: A. Schopenhauer, *The Philosophy of Schopenhauer*, edited, with an introduction by I. Edman, (Modern Library, New York, 1928).]

> "No object without a subject," is the principle which renders all materialism for ever impossible. Suns and planets without an eye that sees them, and an understanding that knows them, may indeed be spoken of in words, but for the idea, these words are absolutely meaningless. On the other hand, the law of causality and the treatment and investigation of nature which is based upon it, lead us necessarily to the conclusion that, in time, each more highly organised state of matter has succeeded a cruder state: so that the lower animals existed before men, fishes before land animals, plants before fishes, and the unorganised before all that is organised; that, consequently, the original mass had to pass through a long series of changes before the first eye could be opened. And yet, the existence of this whole world remains ever dependent upon the first eye that opened, even if it were that of an insect. For such an eye is necessary condition of the possibility of knowledge, and the whole world exists only in and for knowledge,

and without it is not even thinkable. The world is entirely idea, and as such demands the knowing subject as the supporter of its existence. This long course of time itself, filled with innumerable changes, through which matter rose from form to form till at last the first percipient creature appeared—this whole time itself is only thinkable in the identity of a consciousness whose succession of ideas, whose form of knowing it is, and apart from which, it loses all meaning and is nothing at all. Thus we see, on the one hand, the existence of the whole world necessarily dependent upon the first conscious being, however undeveloped it may be; on the other hand, this conscious being just as necessarily entirely dependent upon a long chain of causes and effects which have preceded it, and in which it itself appears as a small link. These two contradictory points of view, to each of which we are led with the same necessity, we might again call an *antinomy* in our faculty of knowledge, and set it up as the counterpart of that which we found in the first extreme of natural science. The objective world, the world as idea, is not the only side of the world, but merely its outward side; and it has an entirely different side—the side of its inmost nature—its kernel—the thing-in-itself. This we shall consider in the second book, calling it after the most immediate of its objective manifestations—will. But the world as idea, with which alone we are here concerned, only appears with the opening of the first eye. Without this medium of knowledge it cannot be, and therefore it was not before it. But without that eye, that is to say, outside of knowledge, there was also no before, no time. Thus time has no beginning, but all beginning is in time. Since, however, it is the most universal form of the knowable, in which all phenomena are united together through causality, time, with its infinity of past and future, is present in the beginning of knowledge. The phenomenon which fills the first present must at once be known as causally bound up with and dependent upon a sequence of phenomena which stretches infinitely into the past, and this past itself is just as truly conditioned by this first present, as conversely the present is by the past. Accordingly the past out of which the first present arises, is, like it, dependent upon the knowing subject, without which it is nothing. It necessarily happens, however, that this first present does not manifest itself as the first, that is, as having no past for its parent, but as being the beginning of time. It manifests itself rather as the consequence of the past, according to the principle of existence in time. In the same way, the phenomena which fill this first present appear as the effects of earlier phenomena which filled the past, in accordance with the law of causality. Those who like mythological interpretations may take the birth of Kronos ($\chi\varrho o\nu o\varsigma$), the youngest of the Titans, as a symbol of the moment here referred to at which time appears, though indeed it has no beginning; for with him, since he ate his father, the crude productions of heaven and earth cease, and the races of gods and men appear upon the scene.

09-08-01 *Lock Box Reference* (to J. A. Smolin)

Page 101 in:

C. A. Fuchs, *Notes on a Paulian Idea: Foundational, Historical, Anecdotal & Forward-Looking Thoughts on the Quantum (Selected Correspondence).* 504 pages. Foreword by N. David Mermin. See `quant-ph/0105039`.

10-08-01 *The Fifth Amendment* (to T. A. Brun)

Thanks for the note, which I thoroughly enjoyed. You hit a lot of nails on the head with it. Let me try to expand on some of the points that—I believe—show that at least you and I are coming to a little consensus. (As for Jerry and David, I will put them in a superposition for the time being, and see how this this interaction shakes things up.)

Brunism 1 : *That doesn't mean that I completely disagree with you, Chris, but I think you are making a point which is pretty far from the spirit of this problem.*

This is the point. What I am trying to get straight is: *What* is the spirit of this problem?

You write,

Brunism 2 : *We have been describing a consistency criterion.* If *one wishes to combine two state descriptions of a single system into a* single *state description, the criterion tells one* when *it is consistent to do so (i.e., when the two descriptions are not actually contradictory).*

I agree that nobody is holding a gun to Alice's head and forcing her to incorporate Bob's information.

Putting it like that, I can certainly accept the proposition. I want to emphasize that. The attractive feature for me is that it is built on a conditional at the outset.

But you speak of *the* spirit of *this* problem. How is your statement to be reconciled with the tone of David's `quant-ph`? In particular, say, David's quote of Peierls (which he takes as his guiding light):

Merminition 41 : *In my view the most fundamental statement of quantum mechanics is that the wavefunction, or, more generally the density matrix, represents our knowledge of the system we are trying to describe. ... [Yet, density matrices] may differ, as the nature and amount of knowledge may differ. People may have observed the system by different methods, with more or less accuracy; they may have seen part of the results of another physicist. However, there are limitations to the extent to which their knowledge may differ.*

Peierls does not use the qualification that you did—nor do any of you three in many of the emails I have seen—and that is what bothers me.

What I see in Peierls's version of the spirit is that two quantum states cannot co-exist (even in a Platonic sense) unless they are consistent (in one manner or another, yet to be fleshed out). It is as if the universe has these little properties floating about, called quantum states, that MUST be consistent in the BFM sense (or some other sense). I will agree that that might be fine from an Everett-kind of point of view. But if one insists on consistency (as one should with ontological, physical properties), then—it seems to me—one breaks away from the desire to give the quantum state a *purely* epistemological role ... which, as I understood it, was the goal of Peierls and Mermin (though I am willing to accept that it may not be the goal for you and Jerry).

Brunism 3 : *The point of consistency is to determine* if *two points of view can be combined into a single description, not to require that they must be.*

This I also agree with: It was meant to be the whole point of my note "Knowledge, Only Knowledge." My only point of disagreement, was that it was seeming to me that the tendency in David (and Jerry?) was the assumption that the "if" must be satisfied—in some sense—in the "real" physical world.

I just don't know how to say this more clearly. I think it is a valid worry about the intent, the very definition of what the problem is about. I don't think I'm being subtle: that is certainly the last thing I want to be.

Brunism 4 : *I will make one additional comment. I think that in science there is usually a* tacit *(to use your word) assumption that the separate states of knowledge of different observers can be combined, provided that they make no errors and reason logically. This then implies that there* is *a kind of "global state," in the limited sense of a state including all available knowledge. If two observers' beliefs are so inconsistent that they cannot be combined together with any amount of communication and experimental data, they might as well be living in different worlds. This is why we say that insane people are "out of touch with reality."*

I can see why one would say that, especially if one believes that the process of science has an end, and that there is a sense in which the universe is pre-formed. (So maybe I will admit to *some* postmodernist tendencies.) But then . . .

Brunism 5 : *Also for this reason, a rational person may very well never assign a perfectly pure state to a system, but always a mixed state of the form*

$$\rho = (1-\epsilon)|\psi\rangle\langle\psi| + \epsilon\rho'$$

where ϵ contains the unspoken acknowledgement "But I might be wrong."

giving an agent the right to set his own density operator seems to me to be a concession to the quantum state's (purely?) epistemological content.

A note on the "The Second Amendment" to be sent to David soon after lunch.

12-08-01 *A Silver Lining in the Deutsch Cloud* (to R. Pike)

I like to think that there are silver linings to the clouds of my many diatribes! (E.g. the note I just sent Patrick.) Anyway, thinking more about Deutsch this morning (and his silliness that quantum computing only makes sense from a many-worlds view), see the two papers below.

Brooding on the Raussendorf-Briegel one especially (since David DiVincenzo brought it to Steven's and my attention Thursday), I think these are really important ones. And they may give us a much easier, nicer, more insightful way to describe what quantum computing is about.

`quant-ph/0010033`
Title: Quantum computing via measurements only
Authors: Robert Raussendorf, Hans J. Briegel

`quant-ph/0108020`
Title: Universal quantum computation using only projective measurement, quantum memory, and preparation of the 0 state
Authors: Michael A. Nielsen

13-08-01 *Out of the Frying Pan . . .* (to R. Garisto)

My thoughts are with you, and thanks for including me in the list of updatees.

I don't know if it'll work for you, but in times when I'm hanging on the edge, listening to John Coltrane and contemplating quantum mechanics seem to help more than anything. The two activities are not unrelated.

13-08-01 *Reference You Wanted* (to D. P. DiVincenzo)

The formula can be found on pages 26 and 27 of:

> C. A. Fuchs, "Quantum Foundations in the Light of Quantum Information," to appear in *Proceedings of the NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer*, edited by A. Gonis (Plenum Press, NY, 2001). (Until then, see `quant-ph/0106166`.)

See you tomorrow probably.

13-08-01 *A Silver Lining in the Deutsch Cloud, 2* (to N. D. Mermin)

By the way, you might enjoy these too if you haven't already run across them. I think there is some parallel here to thinking of quantum teleportation in the two ways: i.e., the original 1993 way, and the quantum circuit way (like you wrote about recently). The dichotomy goes much deeper and may pervade all of quantum computation, or least that's what is starting to strike me.

"Pinging the sensitive substrate, that's what quantum computing is about." Or, at least, that's what I think would build a pretty picture. The two papers below may give us the right language to view it that way.

14-08-01 *Law without Law, 2* (to J. Summhammer)

Thanks for your note. Give me a few days to digest it. In the meantime,

Summhammerism 4: *In my next mail I hope to get down to asking you a few questions on your paper "Quantum Foundations in the Light of Quantum Information". And the prior probability distribution of probabilities in the classical de Finetti theorem is causing me some trouble. Also, I would appreciate a copy of your paper (PS, PDF, or address for downloading preferred) "How much information in a state vector?"*

let me give you the reference you requested. It can be found on the Los Alamos archive (as can about 75% of my publications). Here's the abstract:

> `quant-ph/9601025`
> Date: Wed, 24 Jan 96 21:21:01 MST (31kb)
>
> Quantum information: How much information in a state vector?
> Authors: Carlton M. Caves, Christopher A. Fuchs
> Comments: 32 pages in plain TEX, to appear in *Sixty Years of EPR*, edited by A. Mann and M. Revzen, Ann. Phys. Soc., Israel, 1996
>
> Quantum information refers to the distinctive information-processing properties of quantum systems, which arise when information is stored in or retrieved from nonorthogonal quantum states. More information is required to prepare an ensemble of nonorthogonal quantum states than can be recovered from the ensemble by measurements. Nonorthogonal quantum states cannot be distinguished reliably, cannot be copied or cloned, and do not lead to exact predictions for the results of measurements. These properties contrast sharply with those of information stored in the microstates of a classical system.

15-08-01 *Knowledge, Only Knowledge – Reprise* (to T. A. Brun)

Brunism 6: *Frankly, I find your negativity about this problem puzzling.*

On the contrary, I'm quite taken with it: otherwise, I wouldn't have given it the time of day. Reading the collective emails has been a great learning experience for me.

15-08-01 *Compatible States* (to R. Schack)

Schackcosm 2: *I got a number of rather incoherent messages on compatible states from you. Is there anything more recent on this?*

Yes. About three thousand more notes. The issue refers to David Mermin's recent `quant-ph` paper "Whose Knowledge?"

Where it stands right now is with Brun, Finkelstein, and Mermin all ganged up against me (as being the unreasonable one). The conclusion they've come to is something like this:

> Two density operators ρ_a and ρ_b can describe the same system iff the support of ρ_a has a non-trivial intersection with the support of ρ_b.

I, on the other hand, think such a statement is far too dictatorial for a Bayesian's taste. Try as I might, they just don't think my points are relevant.

I'll paste (what I deem to be) my most lucid notes below. You can judge for yourself. I'm soon going to drop out of the debate, I believe: it's now at a point of diminishing returns. But, I would like to hear your thoughts!

16-08-01 *Subject-Object* (to P. Grangier)

Thanks for your note! I'm always amazed when anyone reads or skims my papers: You have a friend for life!

Last night I read your paper `quant-ph/0012122`, which I had never seen before. Thanks for bringing it to my attention.

Grangierisme 1: *You are probably aware that statements such as : "The quantum state is information. Subjective, incomplete information." ... "Quantum states are states of knowledge, not states of nature. That statement is the cornerstone of this paper." are unwarranted, since just opposite statements can be made without changing any physical predictions or even any technical development. My personal view is that these statements are even wrong, as soon as "quantum state" is understood as "pure quantum state" (see eg* `quant-ph/0012122`*, missing in your list on p.1).*

In your words,

> [C]ontrary to the copenhagian dogma, a central point in our approach will be to give an "objective reality" to the quantum state of a physical system, in a sense which is developed below. ... The quantum state of a physical system is defined by the values of a set of physical quantities, which can be predicted with certainty and measured repeatedly without perturbing in any way the system.

Here is a problem I have with this conception. (It is a problem I am quite sure you are aware of, but for some reason you did not address it directly in your paper.) Consider two electrons originally prepared in a spin-singlet state—one electron in the possession of Alice, one in the possession of Bob. Let us imagine now two alternative scenarios. In one, Alice measures σ_x on her particle; in the other scenario, she measures σ_z. By your criterion, Bob's particle does not start out with a quantum state (since the two electrons are in an entangled state)—which is fair enough (I have no qualm with that)—but *immediately upon* the measurement it will go into a definite quantum state, either an eigenstate of σ_x or an eigenstate of σ_z, depending upon the scenario. We know this because if Bob were to thereafter repeatedly send his particle through a Stern-Gerlach device with the proper orientation (for the given of the two scenarios), Alice would be able predict with complete certainty which way Bob's particle would go. Moreover, if Bob is careful enough, these further measurements on his part will not perturb his system (in the sense of changing the spin quantum state of the electron). So, we have just what you had wanted: complete certainty and no necessary perturbation.

But if the quantum state is an objective feature of the electron, then we see that it can be toggled one way or the other instantaneously from a distance. (Alice's measurements causes Bob's electron to go into one or another quantum state instantaneously.) Thus, if you accept the objectivity of the quantum state, then you must also accept the objectivity of instantaneous action at a distance (that in no way diminishes with distance or the particulars of the medium filling the space between Alice and Bob).

This is something I'm not willing to do. Not out of dogma, but because it strikes me that the world would have to be horribly contrived to have this property: a little private (instantaneous) telephone line between each and every physical system that will not accept outside calls. I.e., I can never make use of this instantaneous action for real, live communication even though it really, really, really is there. It stretches my imagination too far.

You, of course, may accept it as you wish; but the reasons above are mine for not doing so. Let me address your other point that I quoted above:

> [Your statements] are unwarranted, since just opposite statements can be made without changing any physical predictions or even any technical development.

I certainly disagree with the latter part of this sentence: that was the whole point of my paper (i.e., that it can change the technical development of the theory). Taking one or another point of view about the objectivity of the quantum state motivates different directions of theoretical exploration. In my case, it motivated trying to find the four theorems I presented in that paper. One who believed in the objectivity of the quantum state—I am quite sure—would not have sought those theorems in the first place.

Grangierisme 2: *By the way I also disagree with your point of view that "Quantum Theory Needs No 'Interpretation'," Phys. Today* **53***(3), 70 (2000). The fact that a physical theory ALWAYS needs an interpretation is in my opinion a central difference between physics and mathematics.*

You won't find a disagreement with me here. The title and closing sentence of that paper were meant to be tongue-in-cheek plays on something Rudolf Peierls once said: "The Copenhagen interpretation *is* quantum mechanics." The whole paper is very definitely about an interpretation, and why one does not need to go any further than it to make sense of quantum mechanics as it stands. My paper `quant-ph/0106166` and the large (more personal) collection `quant-ph/0105039` is about going the next step, i.e., what to do once we have established the belief that quantum states are states of knowledge.

When we do finally dig up an ontology underneath quantum mechanics, I'm quite sure it will be an interesting one!

17-08-01 *Your Computational Model* (to H. J. Briegel)

A couple of weeks ago, David DiVincenzo brought my attention to your paper with Raussendorf on models of quantum computation that make use of measurement only (once the initial state is set). I must say, this model has me absolutely enamored! (Besides being intrinsically interesting for our field, it seems to mesh well with the quantum foundational direction I have been trying to develop.)

I wonder if you (or Raussendorf) might have some time to let me hear the word from the horse's mouth, so to speak? I tried reading the first small paper the other night, but it was a bit tough going for me. So, I'd really like more to be eased into the subject with a presentation or two. I will be in Munich (Zorneding actually) visiting my parents-in-law from August 30 through September 9. If you've got some free time in the middle of that, I'd surely like to drop by your university for a discussion.

17-08-01 *Unloading Bayes* (to C. M. Caves & R. Schack)

Let me unload a couple more Bayesian thoughts on you—i.e., some things we will probably want to address in the RMP article.

1) Attached below is a note I wrote to Mermin giving what I think is the cleanest justification for noncontextuality in any Gleason theorem. In fact, it shows that noncontextuality is more basic in the hierarchy of theories than anything else we've dealt with yet. I.e., it comes far before the particular details of quantum mechanics. (Maybe this is what Carl has been saying all along, but I had to work through it for myself before it stuck.)

2) Let me bring your attention to a cluster of papers that I think are really important.

> `quant-ph/0010033`:
> Title: Quantum computing via measurements only
> Authors: Robert Raussendorf, Hans J. Briegel (LMU Munich)
>
> `quant-ph/0108020`:
> Title: Universal quantum computation using only projective measurement, quantum memory, and preparation of the 0 state
> Authors: Michael A. Nielsen
>
> `quant-ph/0108067`:
> Title: Computational model underlying the one-way quantum computer
> Authors: Robert Raussendorf, Hans Briegel

I am especially taken with the Raussendorf-Briegel development. On one level, I think it might be the simplest avenue for addressing quantum computation from a Bayesian point of view: One just builds up the proper initial (universally valid) entangled state, and then does a measurement site by site, doing proper (Bayesian) updating of the quantum state of the remaining sites at each step. At the end of the day, one's knowledge is updated to the sought after answer. On another level (not for RMP), I think it starts to capture what I have been hoping for as an explanation of the power of quantum computation: it is not quantum parallelism that is doing the trick, but the "zing" of quantum systems that makes them sensitive to our interventions.

3) Let me put further below my replies to Khrennikov on his "contextual probabilities" business. I've already shared this with Carl, but not Rüdiger yet. What I like about my statement there is that it starts to put Gleason's theorem in a more Dutch-book kind of light. Just as in classical theory, the setting of initial probabilities is completely free (and therefore subjective). What is set by coherence/rationality is the transformation rules. To that extent, this is the objective content of probability theory. Similarly with quantum mechanics in the light of Gleason's theorem: The objective content of quantum mechanics (or at least part of it) is that if we subjectively set our probabilities for the outcomes of any informationally complete POVM, we are no longer free to set them arbitrarily for any other observable. The probabilities are now fixed and can be calculated uniquely from our previous subjective judgment. (I hate the American way of spelling "judgement.")

That's about it for now. I've certainly written loads more (Bayesian oriented stuff) since we've last seen each other. But nothing else that might have been striking is coming to mind right now. I'll probably unload more as I think of it.

As I'm hearing about more and more of you arriving in Santa Barbara, I'm starting to get a little envious that I'm missing all the fun. At least Kiki and Emma will be off to Munich Monday evening. I won't be joining them until the following Wednesday. I have this great dream of idling away my hours in the mean time with Arthur Schopenhauer, William James, and the Reverend Bayes. But I'm sure some reality will set in to knock me off my course of purity.

20-08-01 *Unloading Bayes, 2* (to C. M. Caves)

Cavesism 2: *I'm not sure anyone is going to be convinced by the above, but I think it is a start. So here's the scenario. There is a big set of things that can be true or false. The big set is determined by the (physical) theory you are dealing with. There are further rules that say how the elements in this big set can be gathered into subsets that correspond to questions whose outcomes are exclusive and exhaustive. Now, if you make noncontextual probability assignments to the questions, you have just ignored the structure that led to your original set. That being the input from your basic theory, if you make noncontextual assignments, you are deciding not to pay attention to your own theory. Not too bright. So you should make noncontextual assignments.*

I know this won't come as a surprise to you, but (really) I found my (operational) explanation much more convincing. So, I guess I really didn't take my cue from you after all. Probably more from Pitowsky/Renes/Hardy.

Can you pinpoint what you didn't find convincing about my argument?

My trouble with your argument is that I still don't find the statement, "if you make noncontextual probability assignments to the questions, you have just ignored the structure that led to your original set," all that compelling. I guess I still don't quite understand what you are saying.

21-08-01 *Contextual Reality = Information??* (to P. Grangier)

Thanks for the longer explanation. It has indeed clarified things for me.

What I think is funny though, is that for precisely everything you say below (which I quote), I would call the quantum state "information" rather than an "objective reality."

Let me ask you this: Would you reread Section 3, "Why Information?" in my `quant-ph/0106166` and comment why and how you would use a different language in rewriting that? It seems to me that to say "a (pure) quantum state is objective, but it is contextual, i.e., defined relatively to a particular set of measurements results" (as you do), can only be to teeter very close to admitting that the state is information (and only information). If the state is only defined relative to Alice's measurement results, why not call those measurement results the actual reality and be done with it? What does having the quantum state being objectively real add to the story?

Philippe's Preply

Alice may obviously choose between several measurements, but she must eventually decide for one and perform it. My definition of the "objective quantum state of a system" *requires* that Alice's measurement is completed, which does not "toggle" the state, but simply define it (at Alice's location).

Once Alice's measurement is completed, and given her measurement result, she will be able to make predictions about Bob's state. But obviously the corresponding information (orientation of Alice's polarizer and measurement result) will have to travel (non-instantaneously) from Alice to Bob. Thus there is nothing here like "objective instantaneous action at a distance", that I dislike as much as you do. On the other hand, there is an objective quantum state, in the sense that once Alice's results have reached Bob (and not before !), Alice and Bob will know "the values of a set of physical quantities, which can be predicted with certainty and measured repeatedly without perturbing in any way the system". In this view BI are violated as a consequence of the lack of "separate reality" of the two particles, rather than as a consequence of non-locality.

To put it another way: a (pure) quantum state is objective, but it is contextual, i.e., defined relatively to a particular set of measurements results. If new measurements are done that are not in the initial set, the definition of the new physical state *must* include the results of the new measurements. In some sense, this is simply a restatement of Bohr's 1935 answer to the EPR article (and maybe this can be related to your "Bayesian" approach?).

21-08-01 *Noncontextuality* (to C. M. Caves)

Cavesism 3: *This is a weird notion of operational, since you rightly note immediately afterward that it is really tautological. How does it justify assigning the same probability to an element in the big set, no matter what the context, to note that you have a rule that assigns the same probability to an element in the big set, no matter what the context? It seems to me the real point is why you would ever identify two elements from different contexts, and the reason is that that they are the SAME element in the underlying set, which is handed to you by your underlying theory. Moreover, you only have one thing to go on, and that is the fact that the theory tells you that two elements are actually the same element in the underlying set. This means that the theory wants them to have the same probability in all contexts.*

I think what is creeping in here (anew) is a fundamental philosophical difference that is starting to come between us. Maybe I can characterize it in the following way. You want the theory to come first, and then to (somehow) recover our activity as scientific agents back out of that grander picture. I, on the other hand, am becoming more and more content to start with the scientific agents and thereafter pluck out those terms in their discourse with various common features to call a theory.

Cavesism 4: *How does it justify assigning the same probability to an element in the big set, no matter what the context, to note that you have a rule that assigns the same probability to an element in the big set, no matter what the context?*

It doesn't justify assigning the same probability. Assigning the same probability is the very reason for assigning a common element in our theoretical descriptions of two very distinct devices.

22-08-01 *Contextual and Absolute Realities* (to P. Grangier)

Grangierisme 3: *This may sound like rhetoric, but if a theory explains nothing less than the stability of matter, is able to calculate $g = 2$ (and many other things) with an incredible accuracy, and nevertheless does not speak about "physical reality", what is physics? I thus consider much more useful to put forward a reasonable definition of reality, that allows me to say to a journalist : "QM speaks about the reality of micro-objects. It is a weird reality, ... but it is REAL."*

You ought to know that I could not agree more with this (the part that I quoted, not the part that I did not quote). The issue on my mind is whether it is productive to view the quantum state in particular as THE term in the theory that corresponds to the objectively real. The way I view the issue is this: Quantum theory is a mixture of objective and subjective elements and we will only make progress in quantum foundations when we have had the intellectual strength to cleanly separate those two ingredients. With this point of view, I can answer the journalist just as soberly as you (i.e., without carrying him off to a postmodern fuzz fest).

There are certainly elements in quantum theory that I am immediately willing to identify as objectively real. A good example comes from our Alice and Bob example, generalized ever so slightly. Suppose Alice's system has a Hilbert space of dimension E and Bob's has a Hilbert space of dimension D, and that again Alice and Bob start off with an entangled state for the bipartite system. I say that the quantum state must be (freely) subjective information because, depending upon what measurement Alice chooses to perform on her side, she will ascribe one or another quantum state to the system in Bob's possession. However, there is one obvious thing that Alice cannot change by the free choices she makes on her side of the world: It is the dimensionality of Bob's system's Hilbert space. Thus, I would say the number D is the objective reality in this situation. The number D remains constant regardless of what Alice does.

I could put this in language more to your liking, saying something like:

> The quantum state that arises for Bob's system after Alice's measurement is a "contextual reality," whereas the Hilbert-space dimension of Bob's system is an "absolute reality."

but I personally don't see that as a road to further progress. I.e., it distracts from what I view as the main point of our task in clearing up the foundations: namely, working hard to separate the subjective from the objective.

What I want to know in the most physical of terms is what does that number D signify? I want to find a way of describing its meaning that never once refers to a quantum state. When I can do that, then I will say that some progress has been made in identifying the objective part of quantum theory. But that is just an example.

Grangierisme 4: *Remember my quotation about the 2 electrons in He: in their singlet state is "subjective information", how do the electrons know it?*

It seems to me, this is just a varied form of the Penrose argument I wrote at the end of my Section 3. I dismiss it in the same way that I did there. From my point of view, to say that the electrons are in a singlet state is to give nothing more than a compendium of things we can say about how they will react to our experimental interventions into their nonhuman bliss. It is not they who can predict the consequences of my invasions into their territory, it is me.

22-08-01 *Identity Crisis* (to C. M. Caves & R. Schack)

I enter this note with trepidation, because I know that what I am about to say will not be taken lightly by either of you, and chances are you will just view me as a troublemaker (again). I don't want to be a troublemaker, but I do have some concerns that are starting to eat more and more at me.

The problem is, I am starting to have serious misgivings about our Sections II and IV of "Making Good Sense" (PRA Version). Most of this new thinking has come about through my taking David Mermin's quest in his paper "Whose Knowledge?" to task, and my ensuing debate with Todd Brun, Jerry Finkelstein, and David himself. But some of it, I think, flows directly from the spirit of Bruno de Finetti, which I now believe I had shut my eyes to for too long.

The issue is no less than whether we really believe probabilities are subjective or not. I think a failure on our part to take their subjective character completely seriously is causing us to go down a path I'd rather not take.

Let me try to explain as best I can. The trouble is localized in our claim:

> ... if two scientists have maximal information about a quantum system, Dutch-book consistency forces them to assign the same pure state.

In the mildest version of my troubles, I am starting to think this statement is contentless. In the stronger versions, however, I find it misleading, and I almost want to say "wrong" (though maybe I won't go that far).

To make sense of what I mean by this, let me start by taking a cue from Bernardo and Smith:

> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. ... The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way." — p. 4, Bernardo and Smith, *Bayesian Theory*

If we accept this, then I think there is a much better way to word our "$p = 1$ when certainty" addition to the Dutch book argument in Section II. It seems to me it should more properly be viewed as a normalization condition, subordinate *only* to internal consistency/rationality as all the rest of the Dutch-book argument is. I say the latter to contrast it with how we presently have the argument worded in our paper—namely, by making the $p = 1$ condition subordinate to some objective feature of the world. We write:

> The only case in which consistency alone leads to a particular numerical probability is the case of certainty, or *maximal information*. If the outcome E is certain to occur, the probability assignment $p < 1$ means the bettor is willing to take the side of the bookie in

> a bet on E, receiving an amount px up front and paying out x if E occurs, leading to a certain loss of $x(1-p) > 0$. Consistency thus requires that the bettor assign probability $p = 1$. More generally, consistency requires a particular probability assignment only in the case of maximal information, which classically always means $p = 1$ or 0.

What does it mean for an outcome E to be "certain to occur"? I think that phrase is much more loaded than we have previously treated it. In the Dutch book argument there are three players, two of them animate and conscious (the bettor and the bookie) and one of them presumably inanimate and unconscious (the world). To which player does the certainty get attached? I don't think we make this clear in the way we ought to.

If the certainty is to be attached to the world, then what business does it have to do with my subjective judgments (which by definition cannot be in a bijective correspondence with the world's states)? Instead, I would say the "certainty" can only be a subjective judgment in and of itself. The Dutch-book argument for requiring $p = 1$ in the case of certainty should then be more accurately advertised as a call to be "true to our hearts." I.e., the argument is really that, *when* we believe an event will happen with certainty (a nonnumerical judgment), then we should ascribe $p = 1$ (a numerical judgment) for booking purposes. That is, Dutch-book coherence gives us a way to translate a nonnumeric belief into a numeric one.

The thing that is really at issue here is that I think we should remind ourselves always that "certainty" itself is nothing more than a belief. It may be a belief that can ultimately be tested against the world in a single shot, but nonetheless it is a belief. I believe with all my heart that my mother loves me; Schopenhauer believes with all his heart that she hates me. The only thing Dutch-book consistency can give us is that "if we wish to avoid the possibility of undesirable consequences" (Bernardo/Smith), then I should ascribe $p = 1$ and Schopenhauer should ascribe $p = 0$ to the proposition "love." The Dutch-book argument prescribes that we each should be true to our hearts—that we should both act in accordance with our beliefs. But it does not have within it the power to make us believe the same thing ... EVEN in the case of "certainty."

Now, we whitewash all that by introducing the phrase "maximal information," which somehow makes "certainty" seem more objective, but now I'm starting to think that that phrase is pretty impotent in this context. What role does it really play in our argument? I can't find any, other than that it is a euphemism for declaring that we *believe* we have nothing left to learn (in the sense that we *believe* there is nothing left to learn from the remainder of the world that will help us refine our predictions for the system at hand). That belief may be wrong in the sense that rationality AND the world will not allow us to perpetuate the belief AFTER the experimental trial, but until the trial, "certainty"—from the Bayesian view—can mean nothing more than a metered belief.

You should be able to tell where I'm going with this by now. In Section IV, we write:

> Maximal information in quantum theory instead corresponds to knowing the answer to questions that share one particular projector.

I suppose what I am saying is that I just cannot accept this anymore. At least not in its present form. Instead, if I were to modify it to bring it into alignment with everything I said above, I would have to write something like this:

> Maximal information in quantum theory instead corresponds to believing adamantly that one knows the answer to all the questions that share one particular projector.

You might think this is nitpicking, but it completely takes all the steam out of Section IV. For it gives Dutch-book consistency no grounds for enforcing that two agents "with certainty" should believe the same thing. And consequently it gives no grounds for enforcing that two agents with "maximal information" should make the same quantum-state assignment.

The only way I see to reinstate our original role for maximal information is to say that two observers can only have maximal information *when* they are both right (in the sense that the world MUST CONFORM to their probability one predictions). But then, using our argument for Gleason's theorem, we would have DERIVED that quantum probabilities are objective probabilities! (This will be my only exclamation point in the whole note, so you should take it in seriousness.) That is, we would be saying that we have maximal information only when we *know* an objective reality, and by our derivation, that objective reality would then be equivalent to a compendium of probabilities.

Instead I think the best we can say is: If Alice and Bob both believe adamantly that they know the answer to some potential measurement AND that measurement happens to be the same for both of them, then Dutch-book consistency and Gleason's theorem will enforce that they make the same probability assignments for all other measurements (i.e., that they assign the same quantum state). But said that way, I don't think any non-Bayesian will be particularly impressed: For they would say that all we have shown is "if two people know the same things, then they will know the same things." Woop-ti-do.

The Bayesians among us will still have some room to be impressed: For it will not be a priori obvious to them that beliefs about one observable should have anything to do with beliefs about another. In particular, it might even surprise them that a common belief in certainty (for two observers) for any fixed observable should lead to equal probability assignments for all other observables. But even then, I think our the shock-value of our paper will be diminished. For I think in no way have we shown that when two observers make two pure-state assignments for a system, those pure states MUST be identical.

For me, this is a liberating thing to understand, i.e., that there are no dictatorial constraints on quantum state assignments. But I suspect you will feel otherwise, at least on the first reading of this note. So, let me beg your forgiveness in advance.

As I alluded to in the beginning, these thoughts of mine don't live in a vacuum. They have been spurred by my debates with Brun, Finkelstein, and Mermin. Thus let me give you some more material to chew upon: I'll attach it below, in the form of a composite note that I've already sent to Rüdiger. Perhaps it will help clarify the things that have brought me to this position. Some of it, of course, will require that you try to imagine the context, but I think the notes are self-contained enough that you will be able to fall into the line of thought and see its relevance.

Best wishes (in spite of my predictable trouble),

23-08-01 *My Own Version of a Short Note* (to C. M. Caves)

I'm just back from a very long day in NY City (bookshopping), and a very long night before that (reading). So I won't reply to your notes until I'm a little more refreshed. But one quick comment:

Cavesism 5: *I get the feeling, strengthened by your own confession that it is true, that my e-mail doesn't make much of a dent, so why bother with it.*

That's absolutely not true. I read everything you send me many, many times over. When they are reasoned well, I accept your arguments. And you know I much prefer this method of communication, just so I can have the opportunity to fully understand what my correspondent hopes me to absorb—I've never been a quick thinker, and this helps me fill in for that inadequacy.

Today I focused on rounding up some more William James, John Dewey, Percy Bridgman material. I think James is taking me over like a new lover. I had read a little bit of him before, but I think I was more impressed with his writing style than anything. But I was drawn back to him by accident, after reading Martin Gardner's *Whys of a Philosophical Scrivener*. Gardner devoted a lot of time knocking down James' theory of truth, because it is just so much easier to accept an underlying reality that signifies whether a proposition is true or false, rather than saying that the knowing agent is involved in eliciting the very proposition itself (along with its truth value). And something clicked! I could see that what James was talking about might as well have been a debate about quantum mechanics. He was saying everything in just the right way. (Let me translate that: he was saying things in a way similar to the way I did in my NATO "appassionata.") And things have only gotten better since.

Have safe trips to everywhere you need to go. Kiki is due December 23, but I'll see what can be done about ITP in November.

26-08-01 *Quantum Commune* (to the invitees)

As many of you know, Gilles Brassard and I have become enamored with the idea that the field of quantum information and computing stands to tell us something deep about quantum mechanics itself. To that end, we organized a small party in May of 2000 to flesh out the issue; we titled it "Quantum Foundations in the Light of Quantum Information." This coming year we will do it again, but on a grander scale—something more like a commune than a party. We hope you will join us as a communard.

Quantum Foundations in the Light of Quantum Information II
Montréal, Canada
October 13, 2002 – November 2, 2002

The purpose of the meeting is to gather a group from the quantum information circle who think there are some aspects of quantum foundations that are more mysterious than they ought to be and, importantly, are intrigued by the idea of applying quantum information to the task of cleaning things up. The atmosphere of the meeting should be quite relaxed with plenty of time for discussion and/or private brooding. The autumn leaves in Montréal will likely be quite beautiful. Keeping with the tradition of the last meeting, we only ask that all attendees compile and share a list of concrete problems whose solution, they believe, will tell us something novel about quantum foundations.

We can accommodate up to 15 attendees at any one time, with all attendees having a desk and a computer in an office of one or two mates. (Of course, the longer you can stay, the better, subject only to our head-count constraint.) At this time, we can promise to cover all local and living expenses. There may also be some money for travel costs.

What we would like to know from you for the present are three things:

1) Would you like to attend?

2) If so, will your ability to attend be contingent upon travel reimbursement?

3) What is your desired length of stay?

It is fairly important that we get this information before September 1, so that we can put the attendees' names on a poster the Centre de Recherche Mathématiques (our sponsors) will use to advertise their theme year.

We look forward to hearing from you. (Since Gilles and I will both be traveling this week, please carbon copy your reply to both of us.)

With warm wishes,

Chris (and Gilles)

PS. If you can attend, your companions may be any of the colleagues listed below. (A * beside a name denotes attendance at the first QFILQI meeting.)

Howard Barnum
Charles Bennett (*)
Gilles Brassard (*)
Jeffrey Bub (*)
Adán Cabello
Carlton Caves
Ignacio Cirac
Richard Cleve
Claude Crepeau
Christopher Fuchs (*)
Nicolas Gisin
Daniel Greenberger
Lucien Hardy
Patrick Hayden (*)
Alexander Holevo
Richard Jozsa (*)
Adrian Kent
Hideo Mabuchi
Dominic Mayers
David Mermin (*)
Tal Mor
Michael Nielsen
Asher Peres
Itamar Pitowsky
Sandu Popescu
Rüdiger Schack (*)
Ben Schumacher (*)
Abner Shimony
John Smolin
Robert Spekkens
Andrew Steane
David Wallace
John Watrous
William Wootters (*)
Arthur Zajonc
Anton Zeilinger

26-08-01 *Lock Box* (to R. W. Spekkens)

Spekkensism 7: *Onto my question. It seems to be implied in the above quote that the statements "none of us can ever completely hide the effects of our interactions with the world" and "There are no lock boxes for information" are roughly synonymous. So it seems that your conception of a lock box refers only to the property of concealment. As far as I can tell, in the sense you use the term here, a lock box need not be binding. Later you state "Item (1) sounds a lot like the nonexistence of bit commitment." However, it doesn't sound that way to me because being able to "hide the effects of my interactions with the world" requires only a BC protocol that is concealing. Indeed, it seems to me that item (1), as I am understanding it, is in fact false for QM. QM* does *allow me to hide the effects of my interactions with the world. Simply use a BC protocol that is perfectly concealing but not at all binding. Indeed, I would argue that if one only seeks lock boxes that are concealing, then one need only look as far as Wiesner's proposal for quantum money to see that such lock boxes* are *provided by QM (note that Wiesner's scheme can easily be modified so as not to rely upon the ability to prepare 'absolutely' pure states, so the finite strength of preparatory measurements is not an issue). So my question, in a nutshell, is where does bindingness enter the picture?*

You are certainly right about this; I was not being careful enough in my formulation. Thinking back on it, I guess I was tacitly wanting to reject concealingness and while retaining bindingness, but the thoughts were still pretty vague at the time ... and *are* still pretty vague today.

Let me record a few random thoughts:

1. I certainly have no problem with an information-writing agent being able to reverse his symbol, so long as the physical system on which he writes it can be thought of as an extension of his body. But there should come a time when the agent becomes detached from the system, and at that point he should lose such privileges.

2. In the quantum mechanical case, when Alice makes it so that the system is perfectly concealing, then it has zero binding power. In that case, one might say that she has not "really" recorded any symbol at all.

3. What is the meaning of the existence of intermediate cases? Do they have anything to do with the motif I was trying to imagine? Or do they lean on a completely different motif?

I'll be sending you an invitation to a quantum foundations workshop Gilles and I are organizing in Montréal for next October, in a very short while. I hope you can come.

27-08-01 *Bayesian Pill Taking* (to N. D. Mermin)

I am finally writing to reply to your long note on "knowledge." I apologize for keeping you waiting so long, but even now—since Rüdiger is still in the woods—I feel a little like I am writing you prematurely. Nonetheless, I have this overpowering desire to get this issue out of my mailbox and be done with it. So here, I am. I would ask you, however, to please keep these thoughts private, at least until Rüdiger finally emerges. For, what I am about to say involves him (and Caves) directly.

I know that you think your note is ultimately conciliatory, writing:

Merminition 42: *It seems to me that these are all valid statements about the formal structure of quantum mechanics, independent of what interpretation you favor.*

But after much soul-searching, I still cannot agree with your language. The soul-searching was required because this position of mine flies in the face of some of my very own published words (`quant-ph/0106133`).

The difficulty hinges on your Proposition 1:

Merminition 43: *1. A system that is known (by somebody) to be in a state psi cannot be found (by anybody) to be in a state orthogonal to psi.*

In my new view, to say this is to throw away all that we have been striving so hard for in establishing that quantum states are subjective entities (and purely subjective entities). You admit so much yourself at the end of your Comment (c). [You can take this to mean that I also disagree with the beginning part of Comment (c).]

In 1880, I suspect there was not a single educated physicist who doubted one iota that the speed of light was set with respect to the stationary ether. Regardless of that, in 1881 the first evidence came out against the common belief. The *spirit* of your Proposition 1 would outlaw the very happening of that wonderful historical event.

I see no way (nor even want to anymore) to get around this. If the quantum state is a subjective entity, without rigid connection to the world in itself,—as I think Einstein's original argument for its subjectivity indisputably shows us—then this is something we simply have to live with. We must allow that experimental data can speak against our predictions, no matter how set we are to believe that they will not. Yet, on top of this, we must also accept that there is no objective sense in which a (pure) quantum state is simply "wrong" before the experiment is performed.

I put all my heart and soul into presenting this point of view in the note below to my coauthors, and I will share it with you. It took me a whole day to write the thing; I would like to think it is worth reading. (Also I have slight evidence that it makes sense in that Caves ultimately concurred himself.) I believe the note is just as relevant to you and your note on "knowledge" as it is to Caves and Schack. I will also send you the PRA version of `quant-ph/0106133`, so you can track the sections.

There. Now I will clean out my mailbox.

27-08-01 *A Little Contextuality on Noncontextuality* (to C. M. Caves)

I've finally got enough time to write a small reply to your old note.

Cavesism 6: *There are two distinct approaches here, and I don't know which is to be preferred.*

1. The first point of view, which I have been pushing (as a way to justify noncontextuality), is that there is an underlying theory that sets up the structure of questions. This theory is primary. Noncontextuality emerges as the natural assumption that probability assignments should recognize the structure provided by the underlying theory.

2. The second point of view, which you have been pushing, starts with the role of scientific agents, as you put it, and uses the fact that probabilities are the same in different contexts to say that the elements with the same probabilities in all contexts explains why they are actually the same element in different contexts.

I do want you to understand my position, which is that I appreciate both these points of view. I'm not sure which will be the most fruitful in the long run. But they are trying to do quite different things. Here are two points:

1. The second point of view doesn't provide a justification for assuming noncontextuality, as you understand. Coming at things from the back door, as it were, it uses the fact of noncontextual quantum probability assignments to conclude that apparently different things are, in fact, the same.

I would not use language quite like that. I would say it IS a justification for noncontextuality. And it relies on quantum mechanics not one iota. Noncontextuality should be a property of any instrumentalistic theory (where Bayesian probability has been grafted onto to the world as the best way for us to steer our actions within it). By an instrumentalistic theory, I mean one where we explicitly have to talk about our various possibilities for experimental intervention into nature—a theory where we cannot detach the experiment from the phenomenon.

Cavesism 7: *2. The first point of view appeals to me presently because it manages to make a long straight run to the state-space structure and the quantum probability rule given only the Hilbert-space structure of questions and probabilities faithful to that structure (i.e., noncontextuality). I think the second point of view needs to address the following question: given a set of elements to which noncontextual probabilities are assigned, what structure is forced onto the set by the existence of these noncontextual probabilities? This question seems hopelessly underconstrained to me, but Howard described to Joe and me some math research on this sort of question.*

What you say "needs" to be done, seems hopelessly underconstrained to me too. But I think you shouldn't view the problem that way. The point is, one simply has a way of clearing the air of the noncontextuality issue BEFORE getting down to the nitty-gritty of quantum mechanics. Noncontextuality is the base assumption upon which one plays a new game: What *physical* assumption makes it so that our instruments should correspond to POVMs and not some other mathematical structure?

By the way, you know I really dislike your phrase "given only the Hilbert-space structure of questions." I've probably said this to you before, but let me try to articulate why in more detail so that maybe you'll remember it a little better. In your own words, the phrase is "hopelessly unconstrained." What does it mean? It seems to me there are all kinds of possibilities one could imagine if one didn't know quantum mechanics beforehand. Here's a simple example: An even more basic feature of Hilbert-space before orthogonality is linear independence. When God came down and said, "You will use Hilbert-space structure for the questions you can ask of nature," why did he not mean that any set of linearly-independent vectors corresponds to a valid question? Presumably there are good reasons. But those good reasons need to be spelled out, and are not at all implied by the simple phrase "Hilbert-space structure."

Let me send you to pages 86–88 and pages 361–362 of the samizdat. There it is shown that linear independence does not mesh so well noncontextuality. It is a dumb exercise, I know, but it does indicate that ones need to be careful with one's phraseology.

Oh, let me tell you another thing, of historical note. I talked to Howard Friday, and he tells me that this kind of justification for noncontextuality goes all the way back to Mackey (but then he settles on the setting of ODOPs thereafter).

Cavesism 8: *I'm also quite interested in another question: How general are the theories where maximal information does not lead [to] certainty, yet does lead to unique noncontextual probability assignments? In other words, for what classes of theories is there a Gleason theorem?*

I think this is a really good question. I was talking to Eric Rains the other day and he thinks that the appropriate generalized setting might be the Jordan algebras. This is because this is the largest structure he knows where there is a notion of positive operator. (I had shown him the trivial POVM way of proving Gleason.)

If Gleason can be proved in such a wildly general setting, I think it would be quite interesting. For it would tell us that the quantum probability rule is not very closely connected to physics at all. Dreaming of the process of deriving quantum mechanics as successively tucking up the more general structure of Bayesian probability theory, one might say that the real physical assumptions don't come until much later in the game. That would be worthy knowledge!

Waiting for a stupid doctor's appointment,

28-08-01 *Introduction* (to A. Wilce)

Please allow me to introduce myself: We have a mutual friend in Howard Barnum. Howard brought my attention to some of your work, and he has also told me that you are reading my paper "Quantum Foundations in the Light of Quantum Information."

As you surely know by now, my section "Whither Entanglement?" is just plain wrong. Positivity alone on local measurements does not narrow down the bi-linear map to one derived from a density operator. I am so ashamed of this mistake. And I am already planning how I can make amends for my insult to nature.

I think what I would like to do rather than amend the original paper, is instead write a short comment and post it on `quant-ph`, with a title something like "Wither 'Wither Entanglement?'?" I could use this as another opportunity to inspire people to this issue, but also (importantly) to draw attention and advertise your and your colleagues' work to an audience not previously aware of it.

I can see at least two routes by which to stitch up my failed attempt of a theorem. The first is to simply say that local measurements (with one-way classical communication) alone are enough to get us that "states of knowledge" must correspond to linear operators on the tensor product of the Hilbert spaces. But then to completely nail down the state-space structure we contemplate putting the systems back together and imagine performing completely general measurements subject to this weaker property we have just derived. Positivity *then* forces us to the density operators on the tensor product space. Another way to say this is that local measurements alone tell us how to derive a dimensionality for the composite system—they just don't get us to positive semi-definiteness.

In a way, though, that is a dull way to patch the problem. Another way that comes to mind is to ask what further conditions should we add upon the local-measurement statistics so that we nail down the density operators solely. What I mean by this is that, since local measurements will get us to a *unique* linear map, it must be the case that some of can be ruled out by the local-measurement statistics they give rise to.

So, let me ask you these questions:

1. Can you give me pointers to all the relevant literature to do with deriving the tensor product structure along our kinds of lines?

2. Do you know of any conditions like the ones I'm contemplating in the second strategy above that will do the trick? Where are they published?

I hope you will be patient with me in that I am not so familiar with the language quantum logicians use.

I'm glad to get to know you.

28-08-01 *Some Questions Regarding Your Comments* (to N. D. Mermin)

Merminition 44: *The trouble with our exchanges is that I'm always trying to zoom in on the issues under dispute and you're always trying to zoom out.*

Thanks for the note. Your point is a good one. (It's just that I've had to write so much email lately, and always, that it does take a toll. For instance, the present debate has been particularly taxing in that regard. Yet the issues have been important enough to not give up. So certainly I was hoping to recycle some material.)

I will try to write you a (focused) reply tomorrow, after recovering from today. Tomorrow evening, I fly out to Munich for a week and a half. Then I go to the quantum foundations meeting in Ireland to tote the wares.

Still tonight I've got to work on packing, etc.

31-08-01 *Tina* (to A. S. Holevo)

Thanks for the good wishes for my travel. They worked! The flights went without a hitch, and I got a significant amount of reading done. (I've been focusing on the writings of William James, John Dewey, and the other American pragmatists lately. I now believe they had a set of thoughts significantly in sync with the quantum foundational program I sketched in my paper.)

31-08-01 *MSRI Q. Info Workshop Dates?* (to R. Jozsa)

I hope Umesh can easily changes his dates: that certainly would be the easiest option for the Montreal thing. I will keep my fingers crossed. If I hear anything from Umesh I'll let you know as soon as I do. Likewise, if you hear anything before I do, please let me know. It'll give me a sigh of relief.

I'm in dreary Munich for the week. But I like dreary weather like that: I find it especially conducive to philosophical thought. I'm going to visit Hans Briegel next week, by the way. I'm quite enamored by this "measurement-only" computational model of his and coworkers'.

01-09-01 *Left Wing, Right Wing, Not a Wing to Fly With* (to N. D. Mermin)

I was going to use my day today to write you a long, thoughtful (but focused) note on all the recent issues you raised with me on "Whose Knowledge?", but now you've gone and angered me. I mean that.

Merminition 45: *I took my left-right terminology from the Science Wars. It seems to me that in arguing against anything objective other than knowledge Chris is taking a decidedly post-modernist position and therefore allying himself with the "Academic Left" attacked by Gross and Levitt.*

Is my point of view so very subtle that even my most sympathetic patrons cannot decipher it? Or have I finally caught on that you're really not listening to me after all?

You can't stand this, but you deserve it: I will excerpt part of a note I recently wrote to Andrew Landahl. What set me off in his case was when he wrote me the following after having read my paper "Quantum Foundations in the Light of Quantum Information" and given a Caltech journal club talk on it.

> On the whole I portrayed your "party platform" as the statement that "Quantum states are states of knowledge about the consequences of future interventions." In particular, those consequences aren't consequences to reality, but rather consequences to

states of knowledge about even further future interventions. In this worldview Bayesian agents don't work to align their predictions with an underlying reality. Instead they work to align their predictions with *each other*. It is as if reality in this picture is solely the agreement of predictions!

I'd be interested to hear if you believe that this is a fair characterization of your party's platform. After reading this paper, I came to the conclusion that you didn't believe in reality at all. (Or at best I thought you believed reality = knowledge.) John Preskill tells me you believe otherwise, namely that there *is* a reality, which surprised me. I'm curious to hear what you believe reality is.

The scheme below is that everything marked with a ">" is a direct quote from Andrew Landahl's letter. Everything else is either me, or a quote from my paper. I will put only the very most relevant part of my reply.

Please do read it before you—*yes*, you, the most trusted of my academic friends—slander me again. It is nothing if not *exactly* relevant to what you wrote about me above.

With surprisingly kind regards,

02-09-01 *Left and Right* (to N. D. Mermin)

Merminition 46: *I thought you were safely away in Ireland or pre-Ireland.*

Yes, I am in pre-Ireland mode (in Munich), but keep in mind that I am never safely away.

Merminition 47: *I'll read what you sent in a little while. But note that I would not have made such a remark (even in jest) before I got your comments on my summary (and the cc of your letter to your own coauthors) which struck me (and I thought you too) as going beyond your earlier position.*

I will take this remark into account for the longer reply I am presently constructing for your earlier query. (It is sitting at 6K in length now, and will likely be finished tomorrow. Right now, I'm having my first beer of the evening.) But preliminarily, let me say that the only thing that letter to my coauthors did was clarify my position on the subjective character of the state vector. In that I went further than before, having gotten weak in the knees briefly about my position on two agents sharing differing quantum states. But, I do believe that I have long held fast in my opinion that there is something in the universe beyond human ken: It has always been a problem of finding the right language and right ideas for expressing what that something is, AND how it is PARTIALLY reflected in the structure of quantum mechanics.

If this does not make sense yet, I hope it will make more sense after my long note to you tomorrow.

02-09-01 *Intersubjective Agreement* (to N. D. Mermin)

Let me finally throw myself into the ring of intersubjective-agreement to see if I can wrestle you down a little. I will try to be every bit as focused for you as the issue will let me be.

Merminition 48: *Well maybe you're more radical than I thought. It was to avoid correlations floating in the void, unattached to anything whatever, that I've been interested in trying to follow you down the path of knowledge. If all it led to were knowledge, floating in the void, unattached* *to anything whatever — even to other knowledge — then I'd be no better off [than] when I started down the trail.*

I think you misunderstand something very deeply here. The point of separating the categories "knowledge" and "reality" (or "subject" and "object" for that matter) is not to make knowledge an objective reality in its own right or, even worse, to make it the sole reality. Rather it is to say that there is a distinction and that that distinction should be recognized. I see nothing wrong with allowing a physical theory (such as quantum mechanics) to contain formal elements that correspond to *both* categories. The issue in my mind is which elements should be thrown into which category? The answer is not completely clear to me, but I am fairly convinced of one thing: The state vector should not be thrown into the reality side of the line.

What I have ultimately NOT been able to stomach about your wording of the whose-knowledge "answer", and Jerry's wording of the whose-knowledge "answer"—some of Todd's versions would actually survive—is that you say, under certain circumstances, two scientific agents (observers, or what have you) MUST assign "consistent" quantum states to a given system. In the case of pure states, the two agents MUST assign the *same* pure state to the system.

Let me get that through your head: What I object to is the word MUST. Todd once wrote it this way,

Brunism 7: *We have been describing a consistency criterion.* If *one wishes to combine two state descriptions of a single system into a* single *state description, the criterion tells one* when *it is consistent to do so (i.e., when the two descriptions are not actually contradictory).*

I agree that nobody is holding a gun to Alice's head and forcing her to incorporate Bob's information.

and to this way of speaking I can agree. But if you take away Todd's "*If*", then everything collapses in my mind. Enforcing that two agents MUST make the same state assignment if they are going to be "right" at all reinstates the very objectivity, the very agent-independence of the quantum state that the Mechanica-Quantica-Lex-Cogitationis-Est program has been working so hard to exorcise. [As you know, we made a serious misstep in our `quant-ph/0106133`, but that will be rectified in the next edition.]

It is much like the old debate. Is materialism right? Or is it Berkeley's idealism that is right? Who cares, I say. Both philosophies are just simple samples of realism: They only disagree on the precise concept which ought to be taken as real, mundane matter or sublime consciousness. The way you characterize it above, one would think that the only fruit of the Mechanica Quantica program would be the RENAMING of a material reality into an ideal one—a shift more of emphasis, rather than anything of grit.

Merminition 49: *Are you also unable to agree with the statement that a photon that is known (by somebody) to have just passed through a horizontal polaroid cannot immediately thereafter be found (by anybody) to pass through a vertical polaroid.*

I'm asking you about this concrete example of the general proposition because I can't tell whether you're objecting to the language in which I generalized it or whether you object to the statement about photon polarizations too. If it's only the former I'm happy to use less provocative phrasing. All I meant by "be in a state psi" was "has been found to be" in the sense I specified prior to making the objectionable statement. But I worry that you object to both statements. In that case you are walking a dangerous path, denying that one of the most elementary applications of quantum mechanics has a legitimate meaning.

Here is what you are losing sight of. In the Bayesian world, two agents must agree a little before they can agree a lot. Agreeing a lot is the currency they are seeking, but agreeing a little to begin with is not the limitation of their existence. I'll come back to this from a more positive angle in a minute, but let me tackle your particular question before that.

What does it mean for "a photon that is known (by somebody) to have just passed through a horizontal polaroid?" Presumably it means that a particular quantum mechanical test, a POVM, $\{E_b\}$ has been performed and one of the outcomes of that test has obtained—in this case, the label b is "photon passed through horizontal polaroid." Now, you ask, "immediately thereafter [can it] be found (by anybody) to pass through a vertical polaroid?" Implicit in that, you are thinking that the state transformation, subject to the measurement outcome, is

$$\rho \longrightarrow E_b^{1/2} \rho E_b^{1/2}$$

up to normalization.

Suppose you are the somebody spoken of above; and let me be part of the anybody. Now, let us say that I stubbornly insist that the state transformation is

$$\rho \longrightarrow U_b E_b^{1/2} \rho E_b^{1/2} U_b^\dagger,$$

where the U_b are some unitary operators, and in particular, when

$$b = \text{photon passed through horizontal polaroid}$$

we have

$$U_b = \text{horizontal} \rightarrow \text{vertical}.$$

There is nothing in quantum mechanics (as a theory) that can keep me from believing that, so long as the ONLY thing specified is the "measurement" $\{E_b\}$. The point is, let us suppose we disagree on how our beliefs should get updated upon the incorporation of a measurement outcome into our knowledge bank. [As an aside, notice the distinction here: b is given the lofty title of knowledge, whereas ρ is subjected to being a belief. I allowed myself to do that because I am assuming we agree on the $\{E_b\}$, even if not the state-change rule. You might say we need at least this much to get the problem off the ground.]

So, it boils down to this in more common language,

Merminition 50: *Are you also unable to agree with the statement that a photon that is known (by somebody) to have just passed through a horizontal polaroid cannot immediately thereafter be found (by anybody) to pass through a vertical polaroid?*

And I say no, I cannot agree: I saw Hideo Mabuchi in his lab yesterday, and I saw that he inserted a really fancy polarizer into his lab bench, one with an intriguing optical coating that allows horizontal photons in, but has them come out as vertical ones. I insist that I saw him do that: There's not a doubt in my mind. You insist that he is an honest upright boy, and he would never do such a thing to confuse us. With equal tenacity, there is not a doubt in your mind. We disagree, and in the strongest of ways.

How do we put our disagreement to test in the context of photo-detector clicks? WE insert a vertical polaroid—this one, I assume, we both do agree on—behind the "horizontal" one and see what happens. Aha! You were right after all! Mabuchi really did use an honest-to-god von Neumann polaroid; the input photon never made it to the final detector. It wasn't the fancy-coated polaroid after all, but I could swear I saw him put it in.

By now, I know that you are thinking I have gone through a ridiculously long-winded and pedantic way of describing a triviality: that one of us made a FALSE assumption. Implicit in your question was the reasonable starting point—indeed, the one we use in all common discourse—that all the agents involved start from a TRUE state of affairs.

But what can TRUE and FALSE mean in a world where our only handle for getting at things are SUBJECTIVE quantum states? We get at the world through our beliefs and belief updates—that's the fundamental tenet for me. And in that light, the only thing a FALSE belief can mean—as I put it to Caves and Schack in the infamous email—is that rationality (i.e., the Bayesian laws of thought) PLUS the world (i.e., the detector clicks, whose meanings in the end are also set by subjective considerations) will not allow the believer to perpetuate any remnant of his initial belief after the experimental trial. Let me say that sentence again for extra emphasis:

> The only thing a FALSE belief can mean is that rationality (i.e., the Bayesian laws of thought) PLUS the world (i.e., the detector clicks, whose meanings in the end are also set by subjective considerations) will not allow the believer to perpetuate any remnant of his initial belief after the experimental trial.

But if that is the case, what is so overpoweringly evil about having a "false" belief? Why must two valid quantum scientists necessarily be aligned in their beliefs, even in the case of "true" and "false"? I remain hardened: I see no compelling reason for asserting that necessity. Indeed, such an assertion is antithetical to the idea that a quantum state is a compendium of subjective degrees of belief.

If you think TRUE and FALSE mean something more substantial than I just described, then you tell me what role they play in my life other than as a kind of shorthand for some characteristics of my belief updates.

In pointing out these deficiencies, I am not "denying that one of the most elementary applications of quantum mechanics has a legitimate meaning." I am coming nowhere near that. I am merely asserting each scientific agent's constitutional right to believe what he will—i.e., to carry about whatever quantum state he wishes—SO LONG AS those beliefs do not contradict the constitution itself.

It is the latter that, from my view, is the most essential point you have been missing. So, let me get to that directly. With this I can finally start to define a positive program.

Merminition 51: *I don't see that limiting "objectivity" to mean "complete and necessary intersubjective agreement" is abandoning your quest. Indeed, your Bayesian authorities say as much,*

> We can find no real role for the idea of objectivity except, perhaps, as a possibly convenient, but potentially misleading, "shorthand" for intersubjective communality of beliefs.

It must be the "necessary" that raises your hackles [...]

If anyone cannot see by now that it is almost solely the word "necessary" that raises my hackles, then they are not listening. You wrote at the beginning of your note that:

Merminition 52: *If all it led to were knowledge, floating in the void, unattached to anything whatever — even to other knowledge — then I'd be no better off [than] when I started down the trail.*

But that is just not true. We have gained a serious amount of positive knowledge from this exercise. It has allowed us to see much more clearly what is firm and what is squishy in quantum mechanics. The state assignments I would say are always squishy; the rules for updating them are not. To the extent that these rules fulfill an edict in the spirit of Bernardo and Smith,

> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. ... The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."

I would say we have identified an objective piece of quantum mechanics.

It is not that "physics as intersubjective agreement" requires that agents always agree, or, at least, that they must agree in certain limiting circumstances. Instead it is that there is a procedure in any given case for deciding whether two agents will move closer to agreement than not after looking at the world. Quantum mechanics gives us such a framework. It might have been otherwise: One can imagine a world so chaotic that any percipient beings which happen to arise in it would forever be in their own individual dreamlike states, never realizing that it is even possible to come to agreement with their fellow quixoticoids.

Maybe a good (but limited) analogy is this. Think of an electric potential function from which, by taking a gradient, we can derive the electric field. The potential itself cannot be the real stuff because of its ridiculous freedom that can be set freely from observer to observer. Instead it is the way the potential changes spatially that is what is of interest. That spatial change which is the common denominator of all the disparate potential assignments is the real, real stuff. Now think of the quantum state in the role of the potential, and the quantum structures of POVMs, the Gleason theorem, and the state-change rule in the role of the potential's gradient.

As long as you and I play according to the quantum rules for updating our beliefs in your example—you with your

$$\rho \longrightarrow E_b^{1/2} \rho E_b^{1/2},$$

me with my

$$\rho \longrightarrow U_b E_b^{1/2} \rho E_b^{1/2} U_b^\dagger$$

—who is to fault one of us for being irrational? We just have different beliefs about how a state ought to get updated in the particular situation. Neither one of us is taking the constitution to task; neither one of us are using a state-updating method that does not fall into the quantum mold.

The analogy of this with classical probability theory is that we both might agree on the probability for some hypothesis $p(h)$, but disagree on the joint distribution $p(h, d)$ for hypothesis and data. Learning the data and using Bayes' rule, we will generally then come to two distinct posterior assignments $p(h|d)$. That nevertheless gives us no warrant for backtracking our opinion that $p(h, d)$ is just a subjective belief (as are all probability assignments). Instead it helps us see that the objectivity working in the background is Bayes' rule; it is our common denominator.

Quantum states—or through Gleason's theorem, nothing more than compendia of quantum probabilities—do not float in a void. They are tied together more tightly than any other probabilities hitherto ever found. I cannot assign probabilities for σ_x outcomes, σ_y outcomes, and σ_z outcomes at the same time as *independently* assigning them to the outcomes of any more exotic POVMs. In changing my probabilities for the outcomes of some potential new measurements (just after a previous measurement), I had better tie all those probabilities together along the lines of the general form of the quantum state-change rule.

In this, we see something like a much greater deepening of the Dutch book argument. In the classical case, we find that we will bring havoc upon ourselves if we allow ourselves to freely assign $P(A)$, $P(B)$, $P(A \wedge B)$, and $P(A \vee B)$ all independently. All compendia of quantum probability assignments must be tied to the particular structure of quantum states and the quantum state change rules. You should be thinking of the firmament rather than the void.

I think that's all the really general remarks I had wanted to say. Let me now *briefly* address the remaining specific points in your notes.

Merminition 53: *If you do indeed object to both, then the only reason I can see for it (and I agree that this does raises non-trivial issues) is that probability 1 and probability 0 statements are idealizations — that nothing in the actual world we inhabit can be said to be certain or impossible. In that case, of course, the support of any acceptable density matrix is the whole Hilbert space and there is no content left to the criterion. But since the theory does allow you to talk about certain or impossible measurement outcomes, I'm reluctant to declare that its illegitimate to consider them in trying to develop a better understanding of the theory.*

I hope that you can see by now that "probability 1 and 0 statements being idealizations" (i.e., states of belief that we none of us, even Job, are really ever in possession of) has nothing to do with my considerations. A belief is a belief. Rationality itself cannot infringe on what numerical value a belief ought to be. It is therefore perfectly legitimate to think about these idealized situations.

Merminition 54:

> CAF Said: on top of this, we must also accept that there is no objective sense in which a (pure) quantum state is simply "wrong" before the experiment is performed.

[Do] you also require me to accept there is no objective sense in which a pure quantum state is simply correct, after the experiment is performed?

Yes. The ghost of Bruno de Finetti haunts us:

QUANTUM STATES DO NOT EXIST

And I understand that oh so much better now than I did two months ago.

Merminition 55: *P.S. I also asked for clarification of your views on objectivity as nothing more than intersubjective agreement, which on the one hand you seemed to reject in accusing me of going objective in comment (c) but on the other hand you seemed to endorse in quoting approvingly your Bayesian gurus.*

Bernardo and Smith would have never held fast to a "necessity clause" like you seem to be doing. That puts a gulf of distance between your two separate uses of the phrase "intersubjective agreement."

Merminition 56: *P.P.S. Just got a blast from the Eastern Front (Mohrhoff — cc'd to you, I believe). I have the funny feeling that you two, who are so far apart in opposite directions (knowledge-without-facts vs facts-without-knowledge), may yet turn out to be strangely similar in some respects.*

To the extent that I understand him, I myself don't believe this is likely. The direction I see for physics, and quantum mechanics in particular, was perhaps no better put than by William James:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself. 'God,' 'Matter,' 'Reason,' 'the Absolute,' 'Energy,' are so many solving names. You can rest when you have them. You are at the end of your metaphysical quest.
>
> But if you follow the pragmatic method, you cannot look on any such word as closing your quest. You must bring out of each word its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

Mohrhoff, from what I can tell, sees a "block universe" (to use another piece of Jamesian terminology). It is a completed thought in the cosmic consciousness.

Good wishes, and I hope this document answers more questions for you than it raises. Now I've got to run to the biergarten again for a little oompah-pah.

02-09-01 *Truth and Beauty* (to N. D. Mermin)

Here's another passage from William James's *Pragmatism* that may help reveal a little more of my mindset.

> The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication*. Its validity is the process of its valid-*ation*.

03-09-01 *Subject/Object* (to M. A. Nielsen)

Nielsenism 1: *You may also be interested to hear that I'm engaged to be married :-)*

Excellent! This is only a joke partially, but lately I've been so taken with the idea that unions can give rise to things greater than those contained in the parts—thinking of quantum measurement, in particular, from this angle—I've thought about calling my view on QM "the sexual interpretation of quantum mechanics."

Many congratulations!

04-09-01 *Note on Terminology* (to C. M. Caves & R. Schack)

Thinking about it more, I would like to emphasize a point that was buried away as an "aside" in my recent note to Mermin titled "Intersubjective Agreement."

I am becoming more and more dissatisfied with the slogan "A quantum state is a state of knowledge, not a state of nature." The reason for this is that people tend to view the word "knowledge" as something that can be right or wrong, depending upon whether it is in direct correspondence or not with something in the external world. For this reason—as brought out clearly in my debate with Mermin, Brun, and Finkelstein—I think we should get more into the habit of calling a quantum state a state of BELIEF. This is more in line with the language both de Finetti and Bernardo and Smith use for probabilities anyway, and therefore gets us into a quicker connection with the personalistic Bayesians.

I now think it is much better to reserve the word KNOWLEDGE solely for the outcomes of quantum measurements once they become part of the mental makeup of an agent interested in them. I walk into Mabuchi's lab, and to the extent that he and I agree that he is performing some POVM (denoted by a set of positive operators $\{E_b\}$), it seems to me valid to call the outcome b we both witness to be an addition to our knowledge. Now, what either of us may do with that knowledge is a different story. One thing is for sure, it ought to cause both of us to update our beliefs.

Thus knowledge (and information) bear on how we change our beliefs, and in that way—in a sense—become incorporated into our beliefs, but there is no rigid connection between the two concepts. Knowledge/information, as it is encoded in measurement outcomes, is a bridge to the external world that the quantum state has no right to be.

You may also recall another strange phrase I used in my note to Mermin: "the world (i.e., the detector clicks, whose meanings in the end are also set by subjective considerations)." This oddity is reflected in my definition of knowledge above: that is, I make a distinction between the raw stuff of the world that the measurement intervention brings about and the registration b in our noggins (as a flag for further actions in our role as agents). What I am thinking here is something roughly like the following. Take the famous white-on-black or black-on-white visual illusion that can be viewed either as a vase or as two faces facing each other. The raw stuff of the world may be compared to the ink and the paper giving the image. In order to say, however, that Mabuchi and I gain the same knowledge in viewing this we need the deeper cultural agreement that we will *both* call it a face or instead a vase when we see it.

Below I will put a glossary that tries to summarize where I have come so far in my attempts to make sense of quantum mechanics. Essentially, I'm expecting only the two terms above to be relevant to the fights we'll be having in writing our RMP article. But maybe it is nevertheless useful for me to lay my full set of language oddities on the table.

Lately, I've been jokingly calling my view (as it stands) the "sexual interpretation of quantum mechanics." (Most people turn red and become uncomfortable when I do that and explain why. I suspect the same will be true even in your reading of this note. So, brace yourselves.) The essential idea is that something new really does come into the world when two of its pieces are united. We capture the idea that something new really arises by saying that physical law cannot go there—that the individual outcome of a quantum measurement is random and lawless. The very fact that the consequence of the union is random signifies that there is more to the sum than is contained in the parts. But I promise you I won't reflect the licentious details of this view in the glossary below. I'll leave the missing terms to your imagination.

- ACT – The actual carrying out of a quantum measurement/INTERVENTION, after a DE-

CISION has been made by an AGENT to do so.

- AGENT – Any participant in the construction of a scientific theory. In older language, the observer.
- BELIEF – In the context of quantum discussions, a quantum state. Or one might say the quantum state is a compendium of BELIEFS.
- CONDITIONALIZING BELIEF – The rule one uses to update one's BELIEF consequent to the completion of a measurement INTERVENTION. In the language of Kraus and Preskill this would be called the "quantum operation" or "superoperator", respectively.
- CONSEQUENCE – Whatever it is that a measurement INTERVENTION elicits out of the world.
- DECISION – An AGENT, within his power can decide to perform one ACT or another upon the world. Just as physical law cannot impinge on what determines the random outcome of a quantum measurement, neither can it impinge on the mechanism behind an AGENT's decision.
- FACT – This is a word I do not like. One might have said that the outcomes of quantum measurement could be called facts just as well as CONSEQUENCES: But the word fact, to me, contains the connotation of a kind of permanence that I do not see in the quantum world. Facts are irreversible additions to the furniture of the world. But measurement INTERVENTIONS (and their CONSEQUENCES) can be reversed through the agency of a further outside intervener.
- INTERVENTION – The physical act that we call in older language the measurement of a POVM.
- KNOWLEDGE – One's mental representation of the obtained CONSEQUENCE of a given INTERVENTION into the world. Implicit in the use of this word, is that all communicating parties agree to the meaning of the given INTERVENTION, i.e., that it is this POVM rather than another.
- PROPERTY – A property is something possessed by a FACT. I don't like the word FACT.

04-09-01 *Brilliance* (to N. D. Mermin)

Merminition 57:

> CAF Said: I am becoming more and more dissatisfied with the slogan "A quantum state is a state of knowledge, not a state of nature." ... I think we should get more into the habit of calling a quantum state a state of BELIEF.

Brilliant! All kinds of trouble would have been avoided.

You know, I'm not one to turn down a "Brilliant!" But your second sentence does clash a little with what you wrote on August 8:

Merminition 58: *It seems to me Chris is getting much too subtle about this. I would talk about knowledge, not belief.*

All kinds of trouble WOULD HAVE BEEN avoided?

Speaking of brilliant—real brilliance this time—today I'm going into Munich to talk to Hans Briegel about his papers with Raussendorf. (I told you I would be in Munich, but I'm actually in the little village of Zorneding outside of Munich.) I think there's something very deep in them, if they hold up. You may recall I recommended them to you once.

I've got more things of a philosophic nature to write you, but I've just got to find some time to do it. I'll try to be back to the waves tomorrow.

04-09-01 *Objective Probability* (to C. M. Caves & R. Schack)

Cavesism 9: *I expect you to have a really hard time with this—please skip the lectures on my not being sufficiently Bayesian—but it is, in my present view, a necessary feature that expresses the tension that exists in the notion that maximal information is not complete. The state assignment can't be verified by examining the system, but it can be verified by examining the trail of evidence from which I acquired maximal information. If someone else finding that trail of evidence could say that he didn't have maximal information or that he had different maximal information, the notion that the information is maximal would be untenable, since apparently something further would be required to make it so. This seems like a natural for someone who takes seriously those quotes about the process of intersubjective coming to agreement. It grants to maximal information in quantum mechanics some, but not all of the properties of maximal information in a realistic world.*

It is hard for me to understand what that "trail of evidence" is a stand-in for if it is not a compendium of OBJECTIVE probabilities. You follow that trail, and you have NO CHOICE but to assign all the probabilities that the Gleason theorem gives (presumably if you are rational). So, pure quantum states give rise to "propensities" ... when those pure states are "right"? Is that what you are saying? (I said all of this, of course, in my original longer note, but it seems good to isolate it here.) Can you give me an operational definition of this notion of "propensity"?

And why can we toggle these propensities from a distance? Are you giving up on spacetime after all? Or is this a new way of applying the principal principle?

Now I really do have to join the family.

04-09-01 *Fourth and Fifth Reading* (to C. M. Caves & R. Schack)

Cavesism 10: *The point of our conclusion is that the Dutch-book argument leads to a unique probability or density-operator assignment in the case and only in the case of maximal information. This is just an entirely different thing from using frequency data—or something else—to specify every component of a density operator.*

I still don't get it (though I've had a lot of wine by now). By hook or crook, I use the information available to me to assign a probability distribution over the outcomes of some informationally complete measurement. That assignment gives rise to a unique density operator.

I'm still having trouble seeing what is special about a "maximal information" assignment. I'm not lying; I'm not trying to cause trouble; I'm just not seeing it. (Think of me as the second referee of the paper. Would that be ethical?)

Good night!

05-09-01 *Noncontextuality Again (and Again)* (to C. M. Caves)

Cavesism 11 : *The underlying structure is a specification of alternatives that can be grouped in various ways—these are the contexts—to make exhaustive sets. We are required to make noncontextual assignments; otherwise we are ignoring the fact that this specification doesn't distinguish an alternative in two different contexts. If it did, we would be dealing with a different specification. This is the perspective of my first point of view, which justifies noncontextual probability assignments in quantum mechanics from the Hilbert-space specification of alternatives.*

I still don't entirely get this. You say we are required to make noncontextual assignments, otherwise we would be ignoring the fact that the original groupings do not distinguish an alternative in two different contexts. But why could we not ignore it? Perhaps the underlying structure is there for an entirely different reason than something to do with probabilities? For some reason, this point of view is just not clicking for me.

Cavesism 12 : *Your perspective is different. As I understood it, you think of an alternative in different contexts as a single alternative because it has the same probability in all contexts. But where did you get this equal probabilities? Surely they're not measured or determined or anything like that, since they are states of knowledge. In quantum mechanics you get that they're the same because the standard quantum rule says so, but this is using noncontextuality, not justifying it.*

I don't know what more to say on this. It means that identifying this consequence of this intervention with that consequence of that intervention is a SUBJECTIVE judgment. (I.e., that identifying this outcome of this measurement with that outcome of that measurement is a subjective judgment.)

Cavesism 13 :

> CAF Said: What you say "needs" to be done, seems hopelessly underconstrained to me too. But I think you shouldn't view the problem that way. The point is, one simply has a way of clearing the air of the noncontextuality issue BEFORE getting down to the nitty-gritty of quantum mechanics. Noncontextuality is the base assumption upon which one plays a new game: What *physical* assumption makes it so that our instruments should correspond to POVMs and not some other mathematical structure?

I'm going to adopt your strategy, and just flatly say I don't get this.

Let me try again. Here is the game we should be playing. In the most general terms, a measurement is defined to be a group of elements (satisfying some given property) drawn from a set with a given structure. The individual elements correspond to the outcomes of the measurement. The question is, what should that structure be? What should that property be? What are the reasons for those choices? This much we will safely assume (for the reasons given above): The probabilities of the outcomes should depend only upon the individual elements, not the group.

That's all I'm saying. Here is an example of dumb theory.

> UNDERLYING STRUCTURE = one-d projectors onto a complex vector space.
> GROUPING PROPERTY = choose any set of projectors that project onto a complete set of linearly independent vectors.

Then the only probability assignment that can be given to the outcomes of such a notion of "measurement" is the uniform distribution.

So, we start over and say, "Maybe the grouping property ought to be that the projectors add up to the identity." Aha, that gives us quantum mechanics. But you see, there are any number of other combinations of structures and properties one might have played with. The question is, what is essential about the structure and grouping properties that we do use? By saying that we have cleared the air of noncontextuality, I simply mean that the existence of noncontextuality in the probability assignments should not be a question. It was settled before we ever started the game.

Cavesism 14: *Well, when I say "Hilbert-space structure of questions," I clearly don't mean only that there is a Hilbert space, but that the questions correspond to one-d projectors. That's why I add "of questions" to the phrase.*

No, what you mean precisely is: A "question" corresponds to a set of one-d projectors that sum up to the identity. So why don't you just say it in a precise way rather than a vague way? If I were uninitiated to quantum mechanics, I might have thought that you meant the dumb theory above. I'm serious about this.

05-09-01 *Unique Assignment* (to C. M. Caves & R. Schack)

Cavesism 15: *The point of our conclusion is that the Dutch-book argument leads to a unique probability or density-operator assignment in the case and only in the case of maximal information. This is just an entirely different thing from using frequency data—or something else—to specify every component of a density operator.*

I guess my trouble stems from one of the things I said in the long note announcing my worries about "Making Good Sense." There I said:

> I think the best we can say is: If Alice and Bob both believe adamantly that they know the answer to some potential measurement AND that measurement happens to be the same for both of them, then Dutch-book consistency and Gleason's theorem will enforce that they make the same probability assignments for all other measurements (i.e., that they assign the same quantum state). But said that way, I don't think any non-Bayesian will be particularly impressed: For they would say that all we have shown is "if two people know the same things, then they will know the same things." Woop-ti-do.
>
> The Bayesians among us will still have some room to be impressed: For it will not be a priori obvious to them that beliefs about one observable should have anything to do with beliefs about another. In particular, it might even surprise them that a common belief in certainty (for two observers) for any fixed observable should lead to equal probability assignments for all other observables.

What I am wondering is: What would impress a devout Bayesian (who is just learning quantum mechanics) about our argument? Thus, given what I said above, I wonder whether he would not be equally impressed by the following. By hook or crook, Alice and Bob individually come to their own subjective probability assignments for the various outcomes of a single informationally complete POVM. Then, because of Gleason, they will have to match in their subjective beliefs about the outcomes of all other measurements they might perform. That matching made no use of the concept of maximal information. What does the maximal information case give us in shock value?

I think what you're going to say is that in the case of nonmaximal information, Alice and Bob may have come to different probability assignments for that informationally complete observable. And that they couldn't have done that if they had had "maximal information" in the first place. But as all this debate has already shown, I think I reject that position.

As my glossary from yesterday attests, I think what is going on with me is that I am becoming ever more uncomfortable with identifying quantum states with information, maximal or otherwise. Thus, instead of calling a pure state maximal information, I am becoming more inclined to something like:

$$\text{pure state} \;=\; \text{maximally tight belief (or judgment)}$$

or

$$\text{pure state} \;=\; \text{a nonrefinable belief (or judgment)}$$

or anything else more along those lines.

Leave the word information for what we gain when we see the outcomes of measurements. This entity we know two disparate observers should agree upon—by definition—if they are in free communication with each other. But the quantum state, on the inside of one's head, is a more personal state of affairs.

I'm going into Munich for much of the day (to visit with Briegel), so you may not hear from me again until tomorrow.

05-09-01 *Identity Crisis, 2* (to C. M. Caves & R. Schack)

I've now given your long note the fourth and fifth readings it deserves. (I'm holed away in an office near Briegel's.) And I'm not sure how to respond yet. I think I will await your responses to my other notes first. I think it is clear in the time that has elapsed since our first communication that we have moved further away from each other's position. Or I should say I've moved further away, from our original position.

Briegel just came; I'll be back.

05-09-01 *Malleable World* (to G. L. Comer)

Just a little quote I liked.

> Once you bring life into the world, you must protect it. We must protect it by changing the world.

From: Elie Wiesel (b. 1928), Rumanian-born U.S. writer. Interview in Writers at Work (Eighth Series, ed. by George Plimpton, 1988).

06-09-01 *The Underappreciated Point* (to C. M. Caves & R. Schack)

My fit of insomnia is running out, so I'm going to have to go back to bed soon.

Schackcosm 3: *Please give me some feedback on these thoughts. I know that you wrote A LOT more, but I find it easier to go through your emails paragraph by paragraph.*

You know, of that LOT, most of it was written very carefully and very purposefully, so I do hope you will try to read it all with that in mind before trying to get me to readdress too much. (You can ignore the stuff on noncontextuality for now; that's not important for present issues.) Next week I won't have the leisure of writing too many notes, as I'll be out on the election trail trying to stump this Bayesian point of view … and trying to make it LOOK consistent. (I.e., I'll be at the quantum foundations meeting in Ireland.)

But, before crawling back into bed, let me address your greatest FEAR:

Schackcosm 4: *The most important thing to remember is the limited scope of the paper. It tries to show that Bayesian probabilities do have a place in qm, not more. Remember that most physicists would reject this. We show that, contrary to conventional wisdom, subjective quantum probabilities are not arbitrary. Let me remind you that you agreed to a paper of this limited scope last year in Montréal. I do not want a paper that is significantly expanded.*

This must be addressing my single remark of September 3,

> Of course, as you know, I view this as a good opportunity (with page limitations no longer of great concern) to expand some points and give some more references. I think we can read the referee as agreeing with that.

because, in all my voluminous letters, I don't think I ever mentioned modifying the paper otherwise. Of course I think it is unhealthy that your first trip-up would be that point … but we are all moved by different things. (I don't fathom your pressures, and you don't fathom mine.) In any case, let me say this for the record to try to clear the air:

> If given free reign—which I do not actually want—I think I could modify the paper to my own tolerance without changing its length at all, or, at most by a paragraph. I would be more than happy if you and Carl would just find a language I could agree to, and modify the paper accordingly. The phrase *more references* was a euphemism for citing more of Chris's papers.

Deep in my heart, I believe you guys fool yourselves in thinking that this paper will be more widely read simply by being short, but that is not the issue (and it has never been the issue). My passion is to get quantum mechanics straight: So, let's get it straight.

On the whole, in reading your two notes, I found your method of expression better fit to my present mentality than Carl's. Maybe I'll give more specific examples tomorrow. For the present, let me just mention two things.

Schackcosm 5: *It does indeed not follow from our Dutch book argument alone that two agents must agree on the maximal info they have. But suppose agent A has maximal information and agent B insists on assigning a pure state that is not consistent with A's information. A can then extract money from B. I don't think the symmetry of the situation is a problem here. From A's perspective, B is wrong, in the same sense of wrong as if A had a piece of classical knowledge that B chooses to ignore in a bet.*

Perhaps the greatest life change I have had is that I no longer like the phrase "maximal information" in this context. That little phrase carries with it an entire philosophy, and it is one that, to me, does not seem consistent with its roots and, more importantly, does not seem right.

The most one can say on Bayesian principles is that:

> From A's perspective, B is wrong. And from B's perspective, A is wrong.

If A and B can have two pure-state assignments, and the most one can say is the item above, then pure states should not be called "maximal information." They are maximal "something else", but it is not information. (In another note, I have outlined what I think that "something else" is.)

Carl thinks he can fix this by invoking a "trail of evidence" that uniquely fixes which of two pure states is actually the case. But let me juxtapose two of his paragraphs and then try to reemphasize the underappreciated point.

> Paragraph 1:
> In quantum theory maximal information also constitutes a belief, but we resist the notion that it corresponds to some objective reality out there. Why this resistance? Ultimately it's because the maximal information leads to a pure-state assignment that gives probabilities whose only reasonable interpretation is subjective. It is very important to remember that this is the primary motivation for much of what we do. Probabilities are the subjective language used to deal with situations of uncertainty, so wherever we find them, they must be subjective. The subjective view of pure quantum states gains additional support from the fact that a pure-state assignment can't be verified by consulting the system—the same can be said for a probability assignment—and the fact that a state assignment for a distant system changes when we obtain information about it without ever getting close to it—this also holds for correlated probability assignments.

and

> Paragraph 2:
> The state assignment can't be verified by examining the system, but it can be verified by examining the trail of evidence from which I acquired maximal information. If someone else finding that trail of evidence could say that he didn't have maximal information or that he had different maximal information, the notion that the information is maximal would be untenable, since apparently something further would be required to make it so. This seems like a natural for someone who takes seriously those quotes about the process of intersubjective coming to agreement. It grants to maximal information in quantum mechanics some, but not all of the properties of maximal information in a realistic world.

If you hold fast to the view that that trail of evidence must *exist*, then you hold fast to the view that quantum probabilities (in some cases) must be objective after all ... *independently* of the issue of intersubjective agreement. And that negates Paragraph 1.

I am now of the opinion that if we can just clear the air [I'm fond of that phrase] of this nonBayesian trapping from bygone times, we will finally be in a position for real progress. It is in Dutch-bookian type coherence (as a general principle) that one finds an objective statement in quantum mechanics; it is never in the quantum state itself, even when that state is pure. The OBJECTIVE statement is: All of you, each and every one of you, should manipulate your compendia of beliefs according to the rules of quantum mechanics if you wish to maximally avoid undesirable consequences in your gambles. The particular quantum states at any one time are just thin films of subjectivity floating on that wider sea of objectivity.

But, please, please do read the other notes carefully. I can only write a finite amount. I'll comment more particularly on your present notes tomorrow (i.e., today, after I get back up).

06-09-01 *Another Way* (to C. M. Caves & R. Schack)

> If you hold fast to the view that that trail of evidence must *exist*, then you hold fast to the view that quantum probabilities (in some cases) must be objective after all … *independently* of the issue of intersubjective agreement. And that negates Paragraph 1.

Let me put it another way. By Carl's view, if trails of evidence MUST exist, then quantum states MUST exist, and the ghost of Bruno de Finetti should have stayed in the netherworld. For the probabilities derived from the quantum state will exist after all.

06-09-01 *Some Comments* (to C. M. Caves & R. Schack)

Now I return from an unrestful morning in bed.

Schackcosm 6: *I guess you are right that we should be more explicit about "whose certainty". It is the bettor's certainty.*

The deeper issue is not that we *should* be more explicit about "whose certainty," but *why* we should.

Schackcosm 7: *You should leave Schopenhauer and your mother out of this discussion. The distinction between the cases of certainty (classical logic) and reasoning in the face of uncertainty (probability theory) is useful.*

I didn't understand this comment.

Schackcosm 8: *As I said in my previous message, two agents having conflicting certainties is a completely classical situation. If you accept classically that in this case, one of them must misread or ignore some of the available information, then the point of the paper is that the same classical argument gives you unique state assignment, even though states are Bayesian. This is a forceful conclusion.*

It is safer to have the wrong metaphysics in the classical case. This is because certainty (i.e., overpowering belief in the outcome) for one question means certainty for all questions. And that certainty can be verified or falsified in a single shot. So, one gets in the habit of thinking that the proposition (or its material counterpart, as instantiated in the world) was already there before the verification. One can accept that metaphysics or leave it, but it is usually more convenient to accept it. In the quantum case, however, if you assert that the proposition was already there (say, as uniquely specified by Carl's "trail of evidence") then you have to assert that all the rest of the quantum probabilities were already there too. That sounds an awful lot like objective, agent independent probabilities to me.

You can retreat to objective probabilities if you wish. But I say it is better to be creative with our metaphysics. JAW said it like this, "No question, no answer." And that distinction is rearing its head in this very problem.

Schackcosm 9: *Making a pure-state assignment is an extreme statement. It entails the conviction that assigning a different state is equivalent to handing over money. It entails the conviction that the agent assigning the different state is wrong in this sense, in the sense of irrational behavior, not in the sense of not conforming to reality.*

It "entails the conviction." That is language I can accept. It is language I like. Trying to instate that way of saying things has been the whole point of my writing such detailed notes, especially the point about "not in the sense of not conforming to reality." But though you use it so nonchalantly now, it had no representation in our previous discussion, and it has no representation in our paper. At least looking at myself personally, I feel as if I have come through a phase transition.

Schackcosm 10 :

> CAF Said: But said that way, I don't think any non-Bayesian will be particularly impressed: For they would say that all we have shown is "if two people know the same things, then they will know the same things." Woop-ti-do.

Still quite impressive. A and B know the same certain thing. Hence they must assign the same subjective probabilities to all questions. Even subjective probabilities $0 < p < 1$ *are prescribed by this knowledge. The non-bayesian should be quite surprised and impressed by this.*

I said non-Bayesian. Non-Bayesians do not accept subjective probabilities.

Schackcosm 11 :

> CAF Said: For I think in no way have we shown that when two observers make two pure-state assignments for a system, those pure states MUST be identical.

If they are not identical, each agent has perfect reason to assume that the other one is unreasonable.

I accept that. But the point has been, and remains, that that is the ONLY conclusion we can draw.

Schackcosm 12 : *There are dictatorial constraints only in the limiting case of maximal information.*

Unless all of this email has been a grave mistake on my part, I continue to not be able to accept this. The only argument we have at our beck and call is that Dutch Book + Gleason dictates what *I* must do in my head and what you must do in your head. It tells us each how to translate a *nonnumeric* belief (certainty) about the outcomes of a single question, to a *numeric* belief about the outcomes of all possible questions. Indeed, I will lighten up: For a raw BAYESIAN that must be quite an impressive conclusion. There must a good way to say that in the paper. [For the non-Bayesian however—one with no qualms about objective probabilities, one with no qualms about the objectivity of quantum states—I remain in my belief: it will strike him as little more than a tautology.]

But all of this does not lessen my debate with Brun, Finkelstein, and Mermin which started this whole affair. There is no a priori principle in the universe that will tell us that two quantum states OUGHT to have overlapping supports. The best one can say is that IF Alice and Bob have overlapping support, then (if they wish) they may be able to communicate the reasons for their beliefs and come to a more refined consensus. If they do not have overlap in their supports, then the only they can do to lessen their strife is consult the world.

The objectivity is not in the states, but in the state-space STRUCTURE and in the answers the world gives us upon our consultations. When one has gained the latter, one has gained information. But the quantum state before and after remains belief, pure state or not.

06-09-01 *Weak Point* (to C. M. Caves & R. Schack)

Cavesism 16: *I think we all agree that if states are Bayesian , then anyone can assign any state he pleases, including any pure state. He can be misled or tricked, or he can just be crazy, but this sort of freedom to assign any state is not of much interest for our paper. An objectivist will have no trouble agreeing that someone who is misled or irrational will use the "wrong" quantum state.*

Let me try to say it again. The main point is, in the quantum mechanical world, these "trails of evidence" you are thinking of in the back of your mind are NEVER enough to uniquely specify a quantum state. It has NOTHING to do with being misled or being irrational. Even the purest of states is thoroughly infused with belief from the get-go. That is what my note titled "Fw: Intersubjective Agreement" from September 3 is essentially about. So, this is not a case of measure zero, where the players are irrational or dumb to begin with ... so long as we take our own arguments about subjectivity seriously.

Cavesism 17: *What he wants to know is whether scientists acting like scientists, sharing all information in a spirit of genuine co-operation, mutual respect, and dedication to truth, can assign different pure states. And we show that scientists acting like scientists can't: sharing maximal information, they must make a unique quantum state assignment.*

If that is what he wants to know, then he is not going to find it from anyone's Dutch-book argument: our last one, or our slightly modified new one. Your point is a weak one. The Dutch-book game is an adversarial game. Anyone whose intention is to make his opponent go bankrupt is NOT going to share everything he knows with him. He will be silent and bet his money.

Cavesism 18: *What rescues this conclusion from trivia? First, it answers the question of why science doesn't go down the drain: subjective state assignments are constrained in the case of common maximal information.*

I don't believe the conclusion is trivial; I said this to Rüdiger yesterday. But I also don't believe it has anything to do with rescuing science. Playing by the quantum rules ought to be enough.

Cavesism 19: *Second, it answers the question without referring to real, verifiable properties of the system in question. In a realistic world one might justify the agreement in the case of maximal information by saying that any disagreement can be resolved simply by looking at the system and seeing who's wrong.*

Disagreements in the quantum world are resolved also simply by looking at the system. Suppose you and I agree to everything in the world *except* the quantum state for a given system. How do we resolve our dispute? We perform a maximally refined quantum measurement (a POVM with rank-one elements). We agree on the system's state thereafter. That is all that has ever been important in science anyway—that the world provides us with a way to *come into* agreement for all *future* predictions. For god's sake, Albert Michelson did not believe that the speed of light could be a constant. But his tenure was not stripped away when he found a negative result. He revised his "impossible" belief and got over it.

All that one need to demand from a theory is that it provide a way for two agents to come to agreement for all FUTURE predictions. Quantum mechanics (surely) satisfies that. It has nothing to do with re-objectifying quantum probabilities, and I can't see that it has anything to do with this stuff we got in the habit of calling "maximal information."

06-09-01 *The Well Appreciated Point* (to C. M. Caves & R. Schack)

Your notes are well-appreciated themselves; I am finding reading them productive. Unfortunately, I cannot reply in detail tonight, but I hope more will be waiting for me tomorrow morning.

Let me do say though that I think I addressed some of your points in the note I just sent to Carl (and CC'd to you). The main thing was this:

Schackcosm 13: *What the Bayesian can say is: If A assigns a pure state, he knows with certainty that any other pure-state assignment is foolish (handing over lots of money).*

It is not a situation that can be resolved within science, by discussion or experiment or comparison of notes. Both A and B are certain there is nothing that could change their belief. For A, B could just as well reject all of quantum mechanics.

It is not a sin for A and B to disagree about the present. What would be a sin is if they could not come to agreement in the future. And quantum mechanics provides just such a mechanism. It is not true that experiment cannot change (absolutely firm) beliefs in the quantum world: quantum measurements are invasive, and thankfully so. Each measurement gives us the opportunity to throw away the past and start afresh.

07-09-01 *Email Not Received* (to C. M. Caves & R. Schack)

Schackcosm 14: *But, as I argued in my last email, I think that a modified betting argument, now having A and B as adversaries (Carl thinks that this modified argument should not be called a Dutch book argument), shows that starting from two different pure states to come to a later agreement is not what science is about. A must dismiss B as a crackpot. This argument would be useful in the Peierls debate. But maybe not for our paper.*

I didn't receive such an email; can either of you send it again?

Indeed I would bet that science cannot be made in a (purely) adversarial environment. Science is about cooperation, trying our best to come to a consensus. (That is why I have not lost heart in writing all these ridiculous emails!) But, nevertheless, from time to time I do talk and try to come to consensus with people I deem crackpots. The point is, though someone may be adamantly wrong about ONE thing (say, a pure-state assignment from my perspective), it does not mean that he is adamantly wrong about ALL things. And therein lies a backdoor for a discussion with such a person.

The only thing that one has to trust in the making of science is that one's colleague is internally consistent. It is OK if he got SOME of the *facts* (*The Well Appreciated Point*) wrong (from my perspective), and that I got SOME of the "facts" wrong (from his perspective). It is enough that he is willing to join in with me in letting the world pull us together. I.e., that each of us is willing to participate in trying to convince the other that he is wrong by consulting the ultimate arbiter.

But, I'll write more later (in the context of your last two notes).

07-09-01 *(Backbreaking) Analysis* (to C. M. Caves & R. Schack)

Schackcosm 15: *We need to find some common ground.*

Yes, that is true. And I think we already have some, maybe even a lot.

But, as I see it, there still remains a significant amount of trouble in the language we chose to use in the past ... and that is what is putting stoppers on our progress in the present.

Of course, I feel like I am repeating myself over and over, but let me go to your explicit "common ground" paragraph and try to lay out what I like and what I don't like about it. The thing that keeps me going is the hope that maybe this whole debate is a lot like beer: On their very first taste of it, most kids think it is a foul stuff. But after more and more of their friends offer it to them over time, it starts to become a pleasant diversion.

Schackcosm 16: *Assume A has information of the kind we call maximal, i.e., A knows that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be A's inconsistency with her own belief. The beliefs of the bookie or of Nature do not matter. A knows that assigning any other state would make her accept a bet in which she (not Nature or the bookie) knows that she will lose for any outcome she believes is possible.*

Now assume B has access to the same piece of maximal information. B then knows that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be B's inconsistency with his own belief.

Hence: Two agents having access to the same maximal information MUST assign the same state.

Here is how I would reword it to suit my present tastes.

> Assume A is absolutely sure that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be A's inconsistency with her own belief. The beliefs of the bookie or of Nature do not matter. A knows that assigning any other state would make her accept a bet in which she (not Nature or the bookie) is absolutely sure she will lose for any outcome.
>
> Now assume that B is absolutely sure of the same thing, i.e., that a measurement of a POVM containing the 1D projector P will give the outcome corresponding to P. Then assigning any state but P will be Dutch-book inconsistent. This will be B's inconsistency with his own belief.
>
> Hence:
>
>> Two agents having the same absolutistic belief about the outcome of a measurement containing the projector P *MUST* assign the same quantum state.
>
> Or equivalently (but, to me, more forcefully):
>
>> Two agents having the same absolutistic belief about the outcome of a measurement contain the projector P *MUST* assign the same (subjective) probabilities to the outcomes of all measurements that can be contemplated.

What were the main substitutions? Essentially, they were simply:

$$\text{"maximal information"} \longrightarrow \text{"absolutely sure"}$$
$$\text{and}$$
$$\text{"maximal information"} \longrightarrow \text{"absolutistic belief"}$$

And the same substitutions count for the word "know."

To me, those simple substitutions completely change the metaphysical complexion of the statement. The statement goes no further than it has to go to make the quantum Dutch-book theorem stand its ground. Why go further?

Carl gave his reasons: To save science. But I do not see that is necessary in any way, and I do not personally believe that that method is on the right track.

What is wrong with the word "know"? To my Western-trained mind, it conveys the idea that there is something in the external world (the world outside of my head and beyond my control) and that my mind contains a mirror image of it. It conveys the idea that the outcome to the contemplated measurement already exists "out there" in some deterministic or fatalistic sense. It conveys the idea that I really need never have a look to see if the outcome is produced by my measurement: It's already there, and I know it; why waste time on a measurement? Notice that I let the word "knows" stand when it came to describing the very logic of the Dutch-book argument.

What is right about "absolutely sure" and "absolutistic belief" for me? They convey the feeling that what I have in hand is a belief, an extreme belief to be sure, but nonetheless a belief. That phrase never reaches out to the external world for its justification—or, at least it seems so to me.

What is wrong with "maximal information"? I think it screams out no more clearly than in your concluding statement above. In words that Gary Herling might use: The very phrase "SAME maximal information" is an abomination of the English language. In my mind, information is much like the word "know" (though a little looser in constitution). It too conveys the idea of a mind or a newspaper mirroring aspects of a preexisting reality. Besides that, the very fact that we have to go to the trouble to use the word "same" in conjunction with "maximal" conveys the feeling that the word "maximal information" was never appropriate in the first place. If information is some stuff we have gathered from the world AND it is maximal—the very most one can get—AND two agents really should be gathering up the same stuff, else one of them is wrong, THEN why do we have to go to the trouble of using the word "same"?

Well, we use it (i.e., "same") to keep ourselves from contradicting the belief that probabilities are subjective after all. Fine, so that is a good reason to keep the word "same". BUT, it is a bad reason to keep the phrase "maximal information" to merely convey the concept that one is "absolutely sure" of the consequence of some action that one might take (i.e., the measurement being contemplated here).

Schackcosm 17:

> CAF Said: Suppose you and I agree to everything in the world *except* the quantum state for a given system.

It follows from what I just wrote that this situation cannot arise in the case of maximal information.

To me, that statement is a *non sequitur*. I cannot find any *logic* to bring it about. And I say that especially if you can agree to the validity of my attempt at expressing a "common ground" for us in the highlighted paragraphs above ... no matter how pedantic you think my actual phrasing is.

If the quantum state is not uniquely declared by some reality, then there is nothing to stop us from agreeing on some aspects of the world and disagreeing on others.

Please read the note I wrote Mermin titled "Intersubjective Agreement" again. If you and I (in the presence of each other) perform a given POVM consisting of rank-one projectors on a system, then you might say that we will agree on the system's state thereafter. But that requires the assumption that we BELIEVE the same quantum operation (for updating our states) will be associated with that measurement. If we don't agree on that at the outset, then we will come to conclusion of two different pure states for the system after the measurement is completed.

You say, well a quantum operation is surely an element of reality: It is either right or it is wrong. (Carl would say that in any case; I wouldn't.) But suppose it is so—I will relax my debate with Carl for the moment. How would we know which quantum operation we had? We would have to have prepared a load of quantum states beforehand to map which quantum operation is "really" there. But then we would have had to agree on our cache of exploratory quantum states in the first place. How did we get to that stage of agreement, I ask you? And, on I will do the same, ad infinitum.

The point is, in a world where our only exploratory tools are quantum states and quantum measurement outcomes, we can never terminate the chain. This is one aspect of what I meant yesterday when I said that quantum states are infused with beliefs from the get-go.

Quantum measurement outcomes alone will never, ever be enough to uniquely determine a quantum state. One has to have some further a priori information or beliefs to do that. You can play the game—as Carl wants to—that that a priori information is the world's Hamiltonian. But then you will be about as stuck as Kant was with his transcendental idealism: you will still have to start off with agents of some initial common belief before they will ever be able to come to agreement about the Hamiltonian's form. And how are we poor finite beings to ever get to such a starting stage?

I say simply: throw out any trappings that a quantum state can ever be objective.

Schackcosm 18 : *Is there ever maximal information? Yes. You give us an example where C and F both obtain the same maximal information about the system:*

> CAF Said: We perform a maximally refined quantum measurement (a POVM with rank-one elements). We agree on the system's state thereafter.

In the sense that you want maximal information, i.e., something beyond absolutistic belief, I would therefore say that there is never any. My example required that the two agents share an almost strangulating amount of common belief. See discussion in previous section.

Schackcosm 19 : *As Carl writes, if C and F have the same maximal information, they must assign the same state. This is an important situation, as scientists share the information they have.*

If we can get past the language, I will (clearly) agree that this is an important situation. Scientists share the data AND the beliefs (interpretations, machine designs, etc.) they have.

Schackcosm 20 :

> CAF Said: The Dutch-book game is an adversarial game. Anyone whose intention is to make his opponent go bankrupt is NOT going to share everything he knows with him. He will be silent and bet his money.

No. The Dutch book game is about ONE agent's consistency.

Yes, you are absolutely right. And I apologize for throwing in extra junk that is not relevant. But the only point I really meant was that there is nothing in the Dutch-book set-up that forces the bettor and the bookie to share their information. That is an extra requirement if you want it. But it is a requirement that seems to me almost to give up the whole spirit of the Dutch-book situation: it involves no communication beyond the numbers p and x.

Schackcosm 21 : *That agents having access to the same maximal information must assign the same state is all we need for our paper.*

It seems to me, the only thing we need for our paper is the "common ground" statement I made above. I would not think that I need to say it again, but I'm getting pretty fearful of the phrase "maximal information." At the very least, I would like to start using it in a more limited sense or in a more limited way. Or perhaps in a vague enough way (for the present project) that I can worm out of it when I want to write future papers of my own without you two. (But this issue is likely to haunt us all the way through to the end of the RMP deal.)

All this email is starting to exhaust me, and it has certainly kept me from making this trip to Munich even resemble a vacation. I would like to draw it to a reasonable end soon. (But I do understand that that will require flexibility on all our parts, even me.) This may help my samizdat production, but it no longer feels like it is helping the rest of my life. I feel like I have hold of some important points that we were just too much in the "classical" tradition to recognize before. If we ultimately disagree, then we'll just have to do that, but I would rather not end up in that state of affairs.

If you have to make choices on what to do with your own time, please comment on my note titled "Note on Terminology" sooner rather than later. I fly out for Ireland Sunday morning.

08-09-01 *Negotiation and Compromise* (to C. M. Caves & R. Schack)

Let me tell you a little story I dreamed up while driving through the Austrian countryside today. It is based on one of the most annoying realities of my life: there are times when Kiki and I just cannot come to agreement. If I can use Rüdiger's words, there are times when I just think:

Schackcosm 22: *A is a physicist who would bet his career on his state assignment. If he says B is wrong he means this in an absolute, very strong sense. He has examined all the evidence, and there is no doubt left. He is certain that B is missing some evidence. The Dutch book argument shows that A is certain that B's position is equivalent to handing over B's entire fortune. Wrong implies foolish, deluded. For A, B is a crackpot, and the circumstance that B is certain that A is wrong reinforces this position.*

But there are realities: Kiki and I are married; we share a bank account. And here and there, Kiki consorts with the Dutch.

What I am leading up to is that I think there is a place for Ben Schumacher's observation about a three-person Dutch book in our ongoing debate.

You two want to believe that there are god-given constraints on how much two people can disagree. I say there aren't. It'll be a miracle if we ever come to some consensus on this. But I have never said that there ought not to be reasons that two people might want to come to agreement ... EVEN when they have differing but, nevertheless, "maximal information." (I use the phrase "maximal information" despite my dislike for it in order to be sarcastic and to underline a further difficulty with the term in a moment.)

I am internally consistent; there's no Dutch bookie who can take me to the cleaners. And despite my feelings for Kiki's complete foolishness, I feel that she is internally consistent; there's no Dutch bookie who can take her to the cleaners (as far as she is concerned). But we will be in deep trouble if that Dutch bookie approaches us separately. (Being married, we report all our beliefs to each other.)

I can see two outs to this problem. The first one—which is less interesting—is that we make an effort to come to agreement by consulting the world. We make a measurement, and thereby, through its invasiveness, force the quantum system into a state we can agree upon. (Assuming, as

I keep harping on, we can agree on the quantum operation associated with our measuring device.) But what if we have no access to the system of interest? What are we to do then?

I think we would have no choice but to, each of us, back off in the firmness of our beliefs. That is, we should agree upon a density operator that contains in its support both of our earlier ascriptions. We both give up some of our certainty is this process, but the upshot is that we no longer have probability one of becoming bankrupt.

So think about this: Two agents start out saying that they are absolute in their convictions about a some quantum measurement. But then the reality of their partner's stubbornness hits them, and the only thing they can do is back off.

You continue to want to call a quantum state information. But for the present case, again, it seems the term "information" is stretched beyond common usage by the factors people must sometimes take into account in coming to their assignments. From my point of view, Kiki's foolish quantum state assignment is not information about the physical world at all. It is completely wrong, with no reflection in the world as far as I am concerned. Nevertheless, I had better take it into account in making my bets if I don't want to lose our whole joint bank account.

The point I take home is that is sometimes better to negotiate and compromise even when one has "maximal information."

10-09-01 *Short Reply* (to C. M. Caves & R. Schack)

Just a very short reply to your latest posting. But first let me say something about this:

Schackcosm 23: *I hope this email establishes a little more common ground. I would like to start, next week, on revising the paper in the view of this discussion and the referee's comments.*

Yes, do it. With the draft in hand, I'll be better able to see which statements make me feel like a liar and which do not. (Hopefully most of them won't.) And then we can be done with this, and then you can finally stop saying to me, "But not for this paper."

Schackcosm 24:

> CAF Said: What is wrong with the word "know"? To my Western-trained mind, it conveys the idea that there is something in the external world (the world outside of my head and beyond my control) and that my mind contains a mirror image of it. It conveys the idea that the outcome to the contemplated measurement already exists "out there" in some deterministic or fatalistic sense. It conveys the idea that I really need never have a look to see if the outcome is produced by my measurement: It's already there, and I know it; why waste time on a measurement? Notice that I let the word "knows" stand when it came to describing the very logic of the Dutch-book argument.

I do not think any of this is clear. I "know" something means I have a firm belief in it. I don't think more is implied. The question of whether something corresponding to the knowledge (or the belief) exists out there is entirely separate from these wording issues.

Mermin once wrote me this:

> It seems to me Chris is getting much too subtle about this. I would talk about knowledge, not belief. I take "knowledge" to mean simply "true belief", a definition that as I remember goes all the way back to Plato and can be made unproblematic even in the quantum context. [Only a postmodernist would sneer at my saying this.]

And then this:

> C'mon, don't drag your heels. QM is sometimes capable of assigning probability 0 to certain outcomes. For those one doesn't have to argue about whether probability has to do with ensembles or degrees of belief or anything else. "True belief" seemed a good term to describe such outcomes, and then I remembered that Plato (I think) had used the same term (in contrast, as I remember, to "opinion".)

William James writes it like this:

> The popular notion is that a true idea must copy its reality.

The *Encyclopedia Britannica* says this (mainly the last sentences are relevant):

> In general, the philosophical tradition from the Greeks to the present has focused on the kind of knowledge expressed when it is said that someone knows that such and such is the case, e.g., that A knows that snow is white. This sort of knowledge, called propositional knowledge, raises the classical epistemological questions about the truth or falsity of the asserted claim, the evidence for it, and a host of other problems. Among them is the much debated issue of what kind of thing is known when one knows that p, i.e., what counts as a substitution instance of p. The list of such candidates includes beliefs, propositions, statements, sentences, and utterances of sentences. Each has or has had its proponents, and the arguments pro and con are too subtle to be explored here. Two things should, however, be noted in this connection: first, that the issue is closely related to the problem of universals (i.e., whether what is known to be true is an abstract entity, such as a proposition, or whether it is a linguistic expression, such as a sentence or a sentence-token) and, *second, that it is agreed by all sides that one cannot have knowledge, in this sense of "knowledge," of that which is not true. One of the necessary conditions for saying that A knows that p is that p must be true, and this condition can therefore be regarded as one of the main elements in any accurate characterization of knowledge.*

I put stars beside the hot stuff. [It is italicized in this samizdat version.] And I'd send you more quotes if I could, but I'm a gazillion miles away from home in a piss-poor dormitory room (with no library).

Schackcosm 25: *There must be a misunderstanding. If you and I agree about everything in the world, we also agree on the certainty of a particular measurement outcome. We can not then disagree on the state to assign, because at least one of us would be internally inconsistent.*

You must mean that we agree on everything except the state AND the measurement.

Yes.

Schackcosm 26:

> CAF Said: I now think it is much better to reserve the word KNOWLEDGE solely for the outcomes of quantum measurements once they become part of the mental makeup of an agent interested in them. I walk into Mabuchi's lab, and to the extent that he and I agree that he is performing some POVM (denoted by a set of positive operators $\{E_b\}$), it seems to me valid to call the outcome b we both witness to be an addition to our knowledge. Now, what either of us may do with that knowledge is a different story. One thing is for sure, it ought to cause both of us to update our beliefs.

This is a very difficult debate. I am not sure I understand why you draw the line between knowledge and belief exactly where you do. Why give belief in b a special role? I need to think much longer about this.

Because I had assumed that Mabuchi and I had agreed to a fixed "random variable." With respect to that prior assumption the thing we gain is "information" in the standard Shannon sense. I'm willing to call that knowledge that we did not have before. There is nothing personalistic about it; we both have gained the same thing.

I went to dinner with Jon Barrett, Harvey Brown, Matthew Donald, and David Wallace. Getting Bayesian ideas across to them is going to be a hard sell. But the most amazing thing is that each and every one of them was familiar with the Dutch book argument! (Now, how could anyone know it, and yet it not change their life?)

12-09-01 *Ireland* (to S. L. Braunstein)

Thanks for the offer. If it looks like I'm stuck here, I'll surely take you up on it. (Mostly I want to get home, safe and sound, and then *not* get on a plane again for some time.)

It's amazing here, at this meeting: It has practically stopped. People just can't get the news off their minds.[15]

13-09-01 *Re: Hope You Are Fine* (to H. J. Briegel)

Thanks for the concern. They have indeed been horrible events. But my family and I are both safe: Kiki and Emma are in the States, and I am still in Ireland (I may be here for a while, depending upon the airlines).

I certainly enjoyed getting the little private lecture. The word will be spread: You've had a great set of papers.

I'll write you with information about Montréal as soon as Gilles and I get our heads together (maybe next week or the week after).

15-09-01 *Question on the Manuscript* (to R. Schack)

Just send me the draft when you're pleased with it. And I'll tell you then what I can tolerate.

Schackcosm 27: *But as your (ugly) term "absolutistic belief" shows, they should be extremely careful before making pure-state assignments.*

I am not committed to that term, like you guys are to the (inaccurate) term "maximal information." It was just the best I could come with at the time. Change it if you wish. The most important thing is that whatever you substitute for it should carry no flavor of a "reflection theory" of knowledge.

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[15]The conference in question was the "10th UK Conference on Foundations of Physics" in Belfast, Northern Ireland, September 10–14, 2001. It is the meeting where I met Marcus Appleby for the first time, at a dinner with Matthew Donald. Matthew said, "Would you mind if Marcus Appleby comes along? He's done some interesting work on Bohmian mechanics."

You won't buy it, and you'll think I'm just saying this out of less than pure reasons, but you would be surprised at how many people have now encouraged me to call the quantum state a "belief" rather than "knowledge." (Four.)

I'd like to come to SB, but we'll have to see how things play out with Kiki.

15-09-01 *Melancholy Molly* (to N. D. Mermin)

I'm sorry to be writing you back so late: this week has taken a big toll on me.

Merminition 59: *Well for better or for worse Todd, Jerry, and I came up with something we could all agree on.*

I suspect for the worse (if Renes's report to me on it is accurate). I haven't been able to download the paper myself yet, and won't until I get back into the states. My personal opinion is that it is likely to muddy the already troubled waters even more.

Merminition 60: *Am still enchanted by the knowledge-belief breakthrough. How do your coauthors like it?*

The world has become a little lonely for me lately. I'll show you the lengths I've had to go to with all of you below. You are a troublemaker, getting me started down this route! (The document is in the format of the samizdat.)

Merminition 61: *Are you now in Ireland? What part? Be careful.*

To think, I was once scared to be in Ireland. I'll instead take "be careful" to mean "get back into the US safely." I am in London as I'm writing this to you. My original return flight was cancelled; now I'm on the backlog with a million other people.

> Blow up your TV,
> Throw away your paper.
> Move to the country,
> Build you a home.
> Plant a little garden,
> Eat a lot of peaches,
> Try to find Jesus on your own.

16-09-01 *For Worse* (to N. D. Mermin)

Merminition 62: *Well for better or for worse Todd, Jerry, and I came up with something we could all agree on.*

Well, I managed to download your paper this morning after all. I ended up in the airport for over four hours. Now, I'm somewhere over the Atlantic.

I hate it, of course. It's built around the same stubborn disregard for the issues involved in trying to give the quantum state a NONontological status that all the BFM emails were.

To use a term I picked up from a philosopher this week in Ireland, the proposed outcome of a single quantum measurement simply cannot be a "candidate for knowledge." Its source must be considered ineffable, else the wrath of Gleason's theorem would strip away the nonontological status that was being sought for the quantum state in the first place.

I can guess from the format of the paper that it is being submitted to PRL. If I were being mean, I might say, "Good. That'll give it a higher chance of being rejected." But I'm not mean. If I were the referee I might even accept it. But, ONLY if the authors use the extra column at their disposal to explain why their statement of the problem is not a deep endorsement of the Everettista manifesto. (Or, if it is, then just come out and say it.) What else is Zeno but a baroque name for the quantum state of the universe (mixed or not) or a candidate for the one that really gets it "right"?

On a personal note: If you're going to cite a personal communication that appears in the samizdat, why don't you cite the samizdat? (I'm always looking for further ways to draw people into it.)

Best regards (in continuing disagreement),

16-09-01 *Persnickety Business* (to N. D. Mermin)

Since my last note, I've looked over your paper again. I must say I found the sentence,

> It is surely a significant feature of the theory that consideration of impossible outcomes and very little else leads, without any invocation of "the uncertainty principle" or "maximal information", to the fact that pure state assignments must be unique, as well as the more general constraint on mixed-state assignments.

a bit snobbish. Especially since you gave zero devotion to the issue of whether a quantum state could be "true" or not. If they can be "true," then why don't you just take them to be a bit of material reality and forget all this crap about knowledge? If they can't be, then what do you mean by the word "impossible"?

I still hate the paper, probably more so now.

A little further over the Atlantic (and wishing I were closer to Greenland),

16-09-01 *Arlo and Arlo* (to D. B. L. Baker)

As I just wrote another David—i.e., Mermin instead of Baker—right now I'm somewhere over the Atlantic, wishing I were a little closer to Greenland. I'm on my way back home from Northern Ireland. What a week it has been. I've never been so homesick for America before.

> Good mornin' America, how are you?
> Don't you know me, I'm your native son.

I think I might just kiss the ground if I really do land at JFK.

Once upon a time, we could actually see the twin towers from Morristown on a clear day. They pierced the horizon like nothing else in Manhattan. Hearing Emma's voice on the phone the other night was one of the toughest things. She said, "Hi Daddy-o," and then told me all about her first day in playschool. It's not easy to juxtapose that with the death of 5000 people and the many more deaths that may come in the near future.

Life is an essence in this universe. Creative, productive, reproductive life: I'm not one of those scientists who think it is just an illusion, an epiphenomena rolling on top of a clockwork or a dice-rolling world. It is something in itself. It was latent there all along, long before the trilobites had found their place, and it is our task to see that it does not return back to that latency.

Very sad times.

16-09-01 *Registered Complaints* (to J. M. Renes)

Good to hear from you; thanks for the note. I'm flying back from Ireland as I write this to you. I think the conference was successful in many ways. While there, I got to know Marcus Appleby, and he is a really good guy. He's very Jamesian in his perspective, very clear in thought, and a real seeker of the truth. I think I'm going to work to get him to Montréal next year (even though he does not practice quantum information).

I agree with your assessment of the BFM paper. I've already registered my complaint with Mermin. But you know, it's not like there are no pansies even in the midst of our own little group: Carl and Rüdiger have had no great fortitude when it came to this issue, no courage of their convictions.

I'll place my small samizdat on the subject below in case you're curious.

I hope your transition back to New Mexico has gone well. We'll miss you at Bell Labs.

16-09-01 *Nerve Therapy* (to R. Schack)

I'm in the last 1.75 hours of my flight to New York, and I'm doing everything I can to keep from getting too nervous. Let me finally turn my sights to your note from a week ago.

Schackcosm 28 : *I was referring to the argument that, from A's perspective, if B assigned a different pure state, that would force B to accept a bet offered by A that amounts to handing over money to A. Therefore B is a crackpot from A's perspective (but not from his own, of course). This is interesting because it is the very fact that B has maximal information that forces him to accept A's bet: B "knows" A cannot know anything that would give her an unfair advantage in the bet. I still think this is a strong argument. Scientists would try to overcome this critical situation by questioning the world, i.e., by making measurements, but NOT on the system in question, but on everything else, e.g., Hideo's optical table. After all they both are convinced that the world is disentangled from the system in question.*

Personally, I only find this restriction to not touching the system of interest (by you and BFM) ad hoc. Since when in science do we restrict the experimentalists to never touch the objects of their interest? Besides, I think you may be missing one of the deepest facts of the quantum world: it has the power to cause agents to agree *henceforth*, no matter how adamant their previous beliefs ... AND EVEN without the underlying reality of preexisting measurement outcomes that the classical world had. With each quantum measurement, a part of the world starts afresh.

Schackcosm 29 :

> CAF Said: But there are realities: Kiki and I are married; we share a bank account. And here and there, Kiki consorts with the Dutch.
>
> What I am leading up to is that I think there is a place for Ben Schumacher's observation about a three-person Dutch book in our ongoing debate.

I cannot quite see where this situation offers any specifically quantum insight. In any case, it is completely different from the "adversarial game" I am talking about.

I'm not sure that it does lead to any specific quantum insight. But then again, I had never seen you and Carl be so adamant about taking probabilities to be objective features of nature before. (Don't do it! I know you're going to say "NO, NO, NO, we have never wanted objective probabilities!" But all the pieces of evidence—to me—point to the contrary.)

What it does show—and that is maybe where quantum theory comes in a little bit—is that there are times when no matter how adamantly I believe something, I should bet according to odds different from my belief. I.e., there are internal beliefs, and external "beliefs."

To that extent, I am starting to wonder if "belief" is even an appropriate word for capturing the essence of a quantum state. Instead it is starting to seem to me that it may more appropriately be described as a negotiated signifier to external action. I.e., in some cases, it signifies the betting strategies that a community of scientists can agree upon, even in the case of more refined and divergent beliefs.

Whose knowledge, Mermin asked? Sometimes, mine. Sometimes, yours. Sometimes it's more a matter of the policy we have been able to negotiate.

I do get a little worried in saying all of the above, in that, maybe I have not taken Savage's and Bernardo and Smith's views of probability into adequate account: namely, that probabilities and utilities spring into existence at the same time, and are a little inseparable. But presently I don't see how that fits this problem. Nor does it extinguish the problem that sometimes it really is in people's best interest to lie about what they believe.

Schackcosm 30:

> CAF Said: I am internally consistent; there's no Dutch bookie who can take me to the cleaners. And despite my feelings for Kiki's complete foolishness, I feel that she is internally consistent; there's no Dutch bookie who can take her to the cleaners (as far as she is concerned).

I strongly believe that here marriage is fundamentally different from science.

Perhaps. But the point is, sometimes we bet according to situations that have nothing to do with our beliefs. We bet so as to obtain the best common good. And with that we do return to some aspects of science (and politics).

Schackcosm 31:

> CAF Said: But we will be in deep trouble if that Dutch bookie approaches us separately. (Being married, we report all our beliefs to each other.)

This deep trouble arises even if both you and Kiki assign mixed states with the same support, I believe (but I need to think more about this). I am trying to make the point that pure-state assignments are different.

That is true. I chose the particular example of pure states to be as dramatic as possible ... in an attempt to hit you and Carl in a point where you were being the most hardheaded.

Schackcosm 32:

> CAF Said: I can see two outs to this problem. The first one—which is less interesting—is that we make an effort to come to agreement by consulting the world. We make a measurement, and thereby, through its invasiveness, force the quantum system into a state we can agree upon. (Assuming, as I keep harping on, we can agree on the quantum operation associated with our measuring device.) But what if we have no access to the system of interest? What are we to do then?

Well, I gave my answer above. We question everything in the world BUT the system of interest. Using quantum measurements, of course.

Ad hoc.

Schackcosm 33 :

> CAF Said: I think we would have no choice but to, each of us, back off in the firmness of our beliefs. That is, we should agree upon a density operator that contains in its support both of our earlier ascriptions.

Yes, the same is true for two reasonable scientists. But as your (ugly) term "absolutistic belief" shows, they should be extremely careful before making pure-state assignments. Your way out is similar to having second thoughts after placing the bet.

No. My way out is to point out that there is, after all, a distinction between an (internal) belief and its manifestation as an outward bet. The belief comes before the bet.

OK, I've just passed Boston, and in the mean time, got another meal in my stomach. For the obvious reason, getting past Boston carries some symbolic significance. Let us believe in symbols.

18-09-01 *Hi Back* (to A. Peres)

Good to hear from. Yes, I am home. I was delayed by a day, spent a night in London, had to return to JFK rather than Newark, and had the most tense flight of my life, but I am home. I held and held and held on to Emma when I saw her waiting for me.

I have been in a huge email debate with David Mermin about his two latest papers. Consequently, I also got into a huge debate with Caves and Schack, though they are (slightly) more sensible. (The whole thing has been quite taxing.) John Wheeler once told us a story of a condemned man who, while waiting for the firing squad, calmed his nerves by contemplating Hamilton's beautiful equations. I learned a lesson from this: When I was worried about the safety of my flight, I calmed my nerves by continuing my email debate with Mermin, Caves, and Schack!

I hope you and Aviva are able to get home on schedule. I suspect your grandchildren are waiting for your return with excited eyes.

18-09-01 *Goodbye* (to N. D. Mermin)

I just don't know what more to say. Clearly there is something that is keeping us from communicating. I write more and more, and you just don't get my points. It's probably better if I just write less and less.

I'll record the way Caves put the best part of your paper the other day and just leave it at that. I've never said anything different, but maybe if you hear it from another voice something will click.

> It seems to me that there are various kinds of coming to agreement or inability to do so. Much of this is a recapitulation of what we have already discussed. It is phrased in a way that is supposed to avoid the notion that beliefs incorporated in a state assignment require the world to do things. [. . .]
>
> 1. BFM consistency (a multi-party condition): If the parties share their information, it is not ruled out that they can come to agreement on a common state assignment.

If they share information, they must rule out all vectors in the subspace generated by the union of their null subspaces, and they must assign nonzero probability to all vectors in the intersection of their supports. They have the possibility of making a joint state assignment if and only if the intersection of their supports is of dimension 1 or more.

Notice that we don't have to say here, as BFM do, that an outcome assigned probability zero by any party definitely cannot occur.

But let me say one last thing before I give up on this conversation. In your last note you wrote:

Merminition 63: *We've sent the hateful paper to Phys Rev A as an ordinary low-grade submission. My concern is that a referee will condemn it as trivial or well-known, not outrageous or Everettistan.*

You have probably summed up the referee's response correctly, but it doesn't make it so. Nor does it make you paper non-Everettistan after all. The whole point can be summed up with the following part of your paper.

A necessary condition for compatibility

Suppose Alice, Bob, Carol, … describe a system with density matrices $\rho_a, \rho_b, \rho_c, \ldots$. Each of their different density matrix assignments incorporates some subset of a valid body of currently relevant information about the system, all of which could, in principle, be known by a particularly well-informed Zeno.

This is an edict you are placing ON TOP OF quantum mechanics. It is a construction of your own doing. I find it in no axiom system I have ever looked at. It IS a good half of the Everettista starting point.

You might say the other half of Everett is that he takes the quantum state explicitly to be an ontological entity. But you've got that too. By imagining that facts of the world (measurement outcomes) uniquely determine a quantum state, what else could a quantum state be but a stand-in for those real things, those FACTS (the stuff that one absorbs into one's mind and calls information)? You simply cannot say that quantum states MUST be consistent, if you want them to retain a nonontological status. The second you say they MUST be, then you have given up the game: states are then properties of the world, and this question of consistency was a waste of time from the beginning.

With this, I end the conversation: I give up.

19-09-01 *Morning Coffee* (to C. M. Caves & R. Schack)

Schackcosm 34: *Yes, and this is great progress!*

Well, the note really is some progress. Let me just accept the gifts the Lord has given me.

I have two questions.

1) What does Peierls' consistency have to do with coming to agreement? I don't see it yet. It seems to me to be more a technical definition for the word "wrong" than anything else. Using it, I can say when you are "wrong" with respect to my beliefs, but that seems about it.

2) This thing you call fuchsian consistency. You bring to the surface, a point I've always been a little worried about. Why refuse to consider outcomes lying in the null subspace of a density operator? Let me give an example:

Chris: I say the state is spin-up in the z-direction.
Carl: I say it is spin-down.
Chris: Spin-up!
Carl: Spin-down!!
Chris: OK, let's test it. Here is an ideal z-spin-measuring device. Its Kraus operation is just a von Neumann collapse. Do you agree?
Carl: Yes, I agree.
Chris: Are you sure you agree? We need some agreement or this won't be a meaningful game.
Carl: Yes, I agree and am growing impatient.
Hideo: Spin measurement done. It's spin-down.
Chris: Damn, I honestly believed with all my heart that it'd be spin-up!
Carl: Well, I wouldn't have said it if I didn't know it.
Chris: OK, from here out, I say the spin is spin-down. I was a fool before, but at least we agree now.

Your worry seems to be that the formal apparatus of quantum mechanics will not allow me to propagate my belief. According to the Kraus rules, my new state should be the zero operator renormalized by a probability zero: i.e., it is an undefined object. Nevertheless, life goes on, just as the story above indicates. This, at least, does have a formal counterpart in that, if I back off in my belief even the slightest amount to any mixed state (not on the boundary), then the Kraus operation will take us both to the same place, to the same one-d projector. Consequently, the limit exists even if the actual point is undefined.

So, in saying this, I don't think one has to do anything fancy with a measurement to get this thing you call fuchsian consistency off the ground. No intricate constructions of states seem to be needed. Choose ANY (mutually agreed upon) von Neumann measurement you like, and Carl and Chris will have to agree at the end of the day. One of them will feel foolish for having been so wrong, but as long as he is rational, agreement can still be had. As Rüdiger said, the idea is that this is a property of quantum mechanics (in its capacity as a law of thought), not of the initial states.

Alternatively, one can turn the problem around, as I tried to do in my August 7 note to BFM: suppose Carl and Chris agree to a *particular* Krausian state change rule (one not even closely resembling a von Neumann collapse). Now one can ask, under what conditions on the initial states will C and C be left with more agreement at the end of the day than they started with? But that is a different problem than this f-consistency condition you speak of.

19-09-01 *2001 Japanese-American Frontiers – Background Information Needed* **(to E. R. Patte)**

Title: Quantum Information, Quantum Channels

Abstract: Most physics students, with their first lesson on the Heisenberg uncertainty principle, are given a subliminal message: quantum mechanics is a limitation. The attitude is, "Quantum mechanics is something we deal with because we have to, but wouldn't the world have been so much better if we could just measure a particle's position and momentum simultaneously?" This talk is about the counterpoint to that attitude. Recent advances in the fields of quantum computation, quantum cryptography, and quantum information theory show that the physical resources supplied by the quantum world are anything but a limitation. With these new resources we can do things almost undreamt of before, from the secure distribution of one-time pads for use in cryptography to the factoring of large numbers with a polynomial number of steps. The magic ingredient in all this is something called quantum information. I will illustrate the subtle strangeness of this new kind of information and the nice effects it buys with several concrete examples drawn from my own work in quantum cryptography and quantum channel-capacity theory.

Review Articles:

1. D. Gottesman and H.-K. Lo, "From Quantum Cheating to Quantum Security," *Physics Today*, November 2000, p. 22.

2. J. Preskill, "Battling Decoherence: The Fault-Tolerant Quantum Computer," *Physics Today*, June 1999, p. 24.

3. C. H. Bennett, "Quantum Information and Computation," *Physics Today*, October 1995, p. 24.

19-09-01 *Tentative Hello* (to N. D. Mermin)

Merminition 64: *I may take a shot at rewriting our argument, since I'd like to produce something for the Växjö proceedings. At that point I may try it out on you, but if you think it's likely to induce nausea, you don't have to respond.*

I owe you many things. If you want me to look at it, I will look at it, and I will try to respond (i.e., give you constructive criticism) in a civil tone.

19-09-01 *Practical Art* (to C. M. Caves, N. D. Mermin & R. Schack)

I suspect all of you have seen, in one form or another, the optical illusion where, looking at the drawing in one way, it appears to be a beautiful young woman. But looking at it in another way, it looks to be an old hag. Here's a true story about that. The first time I ever saw such a thing was in Roger Penrose's book *The Emperor's New Mind* in 1989 or so. Below the picture was a caption, explaining just exactly what I ought to see: alternatively, a beautiful woman and a old hag. But the oddity was, I could only see the beautiful woman. As much as I tried to find a hag in the picture, I couldn't do it. That fascinated me—so much so that, from time to time throughout the next year, I would pull the book off the shelf and try to find the old hag. I never did see her until one day I was searching through the book for something completely different and happened to come across the page. In fact, that may have even been two years after my initial encounter with the picture.

I think it is safe to assume from this that if Penrose had never pointed out to me that there ought to be an old hag there, I simply may have never seen her.

Let me apply this piece of art to a question on my mind. Let us imagine that some perpetrator has committed a dastardly deed to my family and left a note promising that before the end of her life she would do it again. Beyond this I have no clue of the identity of the perpetrator (or of her cause) except a sketch made of her by an eyewitness, who has now also disappeared from the scene.

For intentional completeness, let us suppose there is simply no further evidence that I can lay my hands on—there is no more "currently relevant information," no deeper "trail of evidence." The carbon atoms laid out on the two pieces of paper—the note and the drawing—are the only links I have to the cause of the crime. The trouble is, in the case of the drawing, the carbon atoms sketch out the shape of Penrose's beauty-hag.

Now suppose an insurance salesman appears on the scene and is willing to sell me insurance against a recurrence of the crime precisely 30 years hence at such and such a premium. Do I buy it? That, of course, will depend upon the probability I ascribe to the crime's recurrence in precisely 30 years. What probability do I ascribe?

It will depend upon what I see in the picture. If I "see" the old hag, it will be one thing. If I "see" the beautiful woman it will be another. If I "see" there is an ambiguity, it will be still a third. But one thing is sure from this example, what I "see" is not completely dependent upon the pattern laid out by the carbon atoms. Part of what I "see" is dependent upon psychological factors that I *myself* may simply never have access to—things deep within my head, things determined when I was first toilet trained, things to do with the self-referential nature of consciousness, things that physics just ought to (and maybe even has to) leave alone. My beliefs (my probability assignments) after seeing the sketch are determined in part by something objective in the world external to me, but also in part by my previous, purely subjective beliefs. This example ought to make it clear that I cannot put all the weight of my posterior beliefs on the external world.

Here's my question: Where do you draw the line? More importantly, how do you draw the line? What is it in your view that gives the clicks of a quantum mechanical measurement a status that goes deeper than the example above? What makes the phrase "currently relevant information" more decisive in the quantum mechanical case? How does that "trail of evidence" have more power to eradicate my subjective pre-beliefs than any other? So much so, in fact, that one gets the feeling that we might even view the quantum state as determined solely by such a trail? You say, "Well, there the information conforms to a physical theory." But in what way does that change things? Can you pinpoint it? Can you make that declaration meaningful? Indeed, can you pinpoint what about the example above is not quantum mechanical? It had atoms, it had systems, it had probabilities.

I said I would not talk to David anymore, and here I am talking to him. I guess that just goes to show that that Mermin can steal your heart ... as can all of you.

20-09-01 *The Stopgap* (to C. M. Caves & R. Schack)

Attached is the latest draft. You will find two (and only two) changes.

1. I added the word "the" in front of "records."

2. I added two citations to myself. One at the beginning, one at the end. If you don't like where I put them, that's fine—feel free to change their positions—but I would like to have them somewhere in the paper.

Of course I am still troubled by the use of the phrases "maximal information" and "with certainty". I continue to think that they convey an image that is better left unconveyed, but I'm the odd man out here so I'll shut up. The main thing is that you left me enough wiggle room that I can defend my view of the quantum state when I need to. E.g., "Oh, I'm sorry, that was just a bad choice of language. The thing to keep in mind is ..." Rather than my having to say, for instance, "Yes, I believe we were wrong about that, but I couldn't convince my coauthors."

Another thing I guess I really didn't like—and this is only a new one for me, it's not something that had eaten away at me before—is the slogan "Gleason's theorem can be regarded as the greatest triumph of Bayesian reasoning." To say that is to imply that Bayesian reasoning IMPLIES Gleason's theorem. I don't think you mean that. I think what you mean is that it is one of the most valuable additions to Bayesian reasoning ever. But I won't cause trouble here: I know you both like the saying.

I think I read the paper very carefully again—more carefully than ever actually. I will live with its consequences, and I apologize for dragging my feet for three or four years.

Now, I just wish we three could come to agreement in our views of quantum mechanics!!!!!! So that I could look back on that day and say, "We few, we lucky few, we band of brothers ..."

20-09-01 *Praise, Folly, Enthusiasm* (to W. K. Wootters)

A million thanks for your encouraging letter of August 30! But also, please accept my apologies for not writing you back for more than 20 days. I had wanted to write you a rather long, contemplative note in response, and I kept waiting for the right mood. Somehow it just never came—first with my travels to Munich and Belfast getting in the way, and then with all the horrible events in the world in the last 10 days.

I certainly like aspects of this speculation of yours. Indeed, I wish you would write it down so we could all have a chance to think about it a more deeply. (Would you be willing to do that? You do have tenure now, and our individual lives are finite: we should never lose sight of that.) But of what little bit I understand of your ideas presently, they don't seem to have as much reciprocality or as much dynamism as I would like to think the world has. What I mean by this is that your ideas seem to carry a significant flavor of the Cartesian cut: the graphs take the place of res extensa, and the identifications take the place of res cogitans. The two realms—as I understood your explanation—don't really interact: The graphs are timeless and independent of the identifications we might make between them.

But, as you say, we are both speculating. And we both realize that. Here's the way William James put it:

> The history of philosophy is to a great extent that of a certain clash of human temperaments. Undignified as such a treatment may seem to some of my colleagues, I shall have to take account of this clash and explain a good many of the divergencies of philosophies by it. Of whatever temperament a professional philosopher is, he tries, when philosophizing, to sink the fact of his temperament. Temperament is no conventionally recognized reason, so he urges impersonal reasons only for his conclusions. Yet his temperament really gives him a stronger bias than any of his more strictly objective premises. It loads the evidence for him one way or the other, making a more sentimental or more hard-hearted view of the universe, just as this fact or that principle would. He *trusts* his temperament. Wanting a universe that suits it, he believes in any representation of the universe that does suit it. He feels men of opposite temper to be out of key with the world's character, and in his heart considers them incompetent and 'not in it,' in the philosophic business, even though they may far excel him in dialectical ability.
>
> Yet in the forum he can make no claim, on the bare ground of his temperament, to superior discernment or authority. There arises thus a certain insincerity in our philosophic discussions: the potentest of all our premises is never mentioned. I am sure it would contribute to clearness if in these lectures we should break this rule and mention it, and I accordingly feel free to do so.

Since reading this a month ago, I've been wondering what my temperament really is. What is the "potentest of all my premises"? I haven't completely tied it down yet, but I think it has to do with the idea that the world can be moved (from within), that it is malleable, that it is still under construction. That the future, for better or for worse, is not yet determined. And that this malleability—like the turtles—goes all the way down: there is no ultimate level that grounds it.

This, if it is, my "potentest of premises" has so far taken manifestation in the speculation that the components of the world are "sensitive" or "irritable." But I suppose that is part of the "impersonal reasons" that James speaks of—i.e., that they form not quite the whole truth of my motivation. The better truth is perhaps that the components of the world—the things that come out of the ways we carve the world up—are movable in a deep sense. They reach out and affect us in ways that we cannot foresee, and we reach out and affect them in ways that they cannot. And through that intercourse, birth arises in the world in a sense every bit as real as biological birth.

But I don't know how to make any of that more precise. It stands at just a program and a direction, but one I take seriously.

Your letter gave me courage, and with such speculations, believe me, one needs courage! How I wish we could get together more often to hash these things out, letting our disparate speculations refine each other. I feel deep in my heart that there is progress to be made—technical progress—it's just a question of building a community with a critical mass of ideas, constructive opinions and techniques.

What are your plans for your sabbatical this year? In which direction do you plan to use your time? Will you be visiting IBM often? If I could get a chance to talk to you more often about the sublime side of physics, I'd surely take it.

By the way, let me draw your attention to a mistake I made in the paper you read. Howard Barnum brought it to my attention last week, and next week I plan to write a short comment on it and post it on `quant-ph` (before someone else does). It is in the derivation of the tensor product rule. For the most part the derivation holds, but I got sloppy in the very last step. I.e., by requiring the existence of a noncontextual probability assignment to the outcomes of local measurements (with one-way classical communication), one does indeed get that the probabilities are controlled by a linear operator on the tensor product of the two spaces. But, these assumptions alone don't get you that the linear operator ought to be a positive semidefinite operator. That requires more assumptions. In principle, one should be able to state those assumptions as a restriction on the correlations one can obtain by local measurements, but I don't quite see how to do it yet. (An easy way out would be to require that the linear operator give rise to positive probabilities for all measurements, local and nonlocal. But, that's kind of a dull answer after relying on locality for so much. I'm sure one can find a more interesting answer.) For instance, I'm not quite sure how to tackle the question of ping-ponging measurements in this framework. Or even whether that's the sort of thing that should be looked at for a natural restriction.

There's so much work to be done! But, it's our place to do it . . .

20-09-01 *Pots, Kettles, and Frying Pans* (to N. D. Mermin)

More seriously. I think the line you are talking about is not the line I was talking about.

Caves has been writing me things like this:

Cavesism 20: *I do want to conclude with my obligatory diatribe against wholly e-mail exchanges. You think all your messages were perfectly clear, I think all mine were perfectly clear, Rüdiger probably thinks the same, but the evidence is that they were not. It's just not possible to come to agreement by e-mail, the reasons among others being that, first, questions arise in reading something* *after which further comments get devalued and, second, peripheral and main points often have their roles exchanged when a message is read.*

And ever more I am having to come over to his view on this subject. But, it is just so hard to give up my old email habits, especially since I've seen them become so ineffective of late. It's like a captain who just can't tear himself away from a sinking ship.

So let me say a few words in response to you. You should know I've got too much invested in the phrase "knowledge about the consequences of our interventions" to back out: it's part of my whole being. If you think that is what is going on with me, you are mistaken.

I do not deny: (1) that trails of evidence exist, and (2) that trails of evidence are created in part by our interventions into the world. What I deny is that those trails of evidence can ever uniquely determine a quantum state, even a pure one. That is what is at issue (with me at least), and that is what yesterday's note was referring to. (Somehow I get the impression that you saw something completely different in the note.)

A detector goes click. You write down that it went click; I write down that it went click. To that extent, the click is part of the objective world independent of us; it is part of the trail of evidence that you and Caves speak of. (Now, maybe the measurement apparatus itself isn't completely part of the agent-independent world—somebody built it to begin with—but that is a different story.) The formal structure of quantum mechanics says that we must identify the click with an element in some POVM. Fine. I agree with that. (Presumably you do too.)

The issue is, which POVM? And which state-change rule associated with that POVM? Show me a place in quantum mechanics where you are told how to do that. I say it is a subjective judgment, just like the quantum state is. Or, more precisely, it is exactly *that* subjectivity that keeps our view of the quantum state as being subjective from being an inconsistent notion. In any given experiment, if there is a single POVM (and state-change rule) that is correct and objective, then so must be the quantum state.

Do you just not see the mapping problem here? If a POVM (and state-change rule) is an objective property of the interaction of two systems, then so will be the post-measurement quantum state ascribed to one of those systems. Now, if the quantum state is an objective state of affairs, why quibble about calling it "knowledge" or "information"? That would be just using two words for it that never needed to be invoked in the first place. If the quantum state is objective, then call it "quantum state" and be done with it.

If you can stomach it, try to read my note "Practical Art" again. It was meant to be a call to do some soul-searching. It was meant to try to persuade you that the subjective element can never be eliminated in a theory that makes fundamental use of probabilities. It was not meant to convey the idea that one cannot draw a line between systems and apparatuses. It was not meant to be a call to join the ranks of the Everettista.

Chris (on a rainy day—it always happens on a rainy day)

20-09-01 *Comment on Practical Art* (to C. M. Caves)

You still don't recognize that the difficulty is a logical one, do you? There is nothing squishy, postmodern, deconstructionist, or new age about the issue: if POVMs (von Neumann ones in particular) are nonsubjective, then so are quantum states. Period. You can't get around that. If you want to claim that quantum states are subjective judgments, then you have to accept that POVMs (and their associated state-change rules) are subjective judgments too. Else the post-measurement states that they give rise to would be objective.

I say this slightly better in my reply to Mermin. It is not that I am leaving the realm of science: it is the strict practice of the art that put me here. Assumptions $\longrightarrow$ conclusions.

Don't worry though: I'm not offended by your note. I'm just (continuingly) surprised by your immense rigidity.

25-09-01 *Further Comment on Practical Art* (to C. M. Caves)

Cavesism 21: *I was initially mightily annoyed by the tone and content of your message. My comment said nothing about and intended nothing about being "squishy, postmodern, deconstructionist, or new age." Moreover, your reply didn't address any of the points I raised, apparently because they were all just further examples of "rigidity."*

Let me reply to your last point first. Here's what happened: I thought I'd reply with a short and to-the-point comment first (about what I saw as the overriding issue), and then follow that up with a detailed reply to your particular points. But then time ran out before having to leave for a weekend trip to the beach. I certainly did not mean to ignore your points, and I certainly won't (as time permits this week). It was just that I was banned from email for the weekend: I'm sorry I didn't warn you of that.

Now, as far as the postmodern business, I apologize for being oversensitive on that point. It had to do with this remark:

Cavesism 22: *It suggests to me that agonizing over the sorts of questions in your hag-beauty-insurance story is not going to help us understand what we're doing as scientists and, moreover, that it will lead our program into a place where no other scientists wants to go.*

This is (what I recall to be) the third time you have made such an allusion, i.e., that I am going places where no scientist ought to want to go. The only conclusion I could draw from this was that you were implying that the subject of this whole discussion is nonscientific. I hope you will at least understand that I might find that insulting. And my reply was an attempt to put a quick end to that train of thought. Over and over, I feel that I have been trying to make a simple point, and most importantly, a logical point—it just happened to be an unpalatable point. So, in the end I guess it seemed to boil down to both of us accusing the other of being unscientific.

I will write a (calm) set of emails replying to all the new issues you and Rüdiger have brought up in the next day or two.

26-09-01 *Form 1* (to R. Pike)

Below is what I wrote up this afternoon. You'll probably find it too long, but you can tell me what you'd like to see trimmed out.

My First Year at Bell Labs

> If I had to pinpoint the thing I discovered liking most at Bell Labs, it would be the incredible freedom it afforded me to pursue pure research, unchained from the bureaucratic duties of my previous institutions. This year has been a good one for science. My focus (as promised last year) has been in mapping out the very foundations of quantum information. The idea is a simple one: Where the foundations are strongest, the tallest,

most sweeping structures will be built. The best example of this in my own work came from trying to make sense of the concept of an "unknown quantum state" from the point of view of Bayesian probability theory. The solution to the conundrum required the proof of a representation theorem for the most general form a density operator can take for sequences of quantum systems with permutation symmetry. As abstract as this problem sounds, the same representation theorem allowed Steven van Enk and I to give a (surprisingly) never-explored analysis of the quantum state of a propagating laser field. This analysis assured the validity of all recent and future quantum-information experiments to do with laser light, from Caltech's quantum teleportation experiments to Richart Slusher's squeezed-light experiments here at Bell Labs—both of which had come under recent attack in the literature.

Along the same foundational lines, I deem my deepest physical insight of the year to be in a reexpression of the content of quantum entanglement, long considered the main ingredient of all quantum information processing. Carried along by the same current of thought that led to the representation theorem above, I was able to show that quantum entanglement is a secondary effect within quantum mechanics: Entanglement, it turns out, is subordinate to the structure of quantum mechanical measurements. This lays open a whole new point of view on quantum information processing: Its power may actually come from the invasiveness of quantum measurements, not from the smooth flow of unitary evolution. Indeed, a concrete expression of this can be found in the new quantum computational model of Raussendorf and Briegel that makes use solely of quantum measurement as its computational primitive. This is now a field of research that is wide open for rapid progress.

In all, I submitted six papers for publication in professional journals, and one very large paper as an invited contribution to a NATO Advanced Research Workshop. In step with this activity, and in perceiving one of my main roles in the physics community as an enthusiastic promoter of the view that the power of quantum information arises from something deep in the quantum measurement process, I also posted a 504 page (edited) collection of correspondence on this subject on the Los Alamos archive. I believe—and joking only slightly—this work has received more attention than all of my more technical publications combined. Also in the same role, I accepted 10 invitations to present my work at international meetings and colloquia around the world, including Hungary, Northern Ireland, Japan, Poland, and Sweden. I turned down three other ones because of time constraints. Furthermore, I co-organized two meetings of my own, and am in the process of co-organizing two major meetings for next year: "Workshop on Quantum Foundations in the Light of Quantum Information" to be held in Montréal, and "DIMACS-CCR Tutorial and Workshop on Capacities and Coding for Quantum Channels" to be held in Princeton. As far as media relations are concerned, I garnered a little attention for Bell Labs in two small appearances: (1) in an episode of *History's Mysteries* on the History Channel, and (2) the September 2001 issue of *Discover Magazine*. Finally, in the capacity of general service to the physics community, I served as an Associate Editor for the new journal *Quantum Information and Computation* and accepted a position on the Advisory Board of the International Center for Mathematical Modeling, Växjö University, Sweden.

As concerns internal service for Bell Labs, here are some items I recall. (1) I gave a presentation to Arun Netravali and Bill Brinkman in a private session on "The Future of Quantum Information Processing." With Brinkman mentioning quantum cryptography and computing as long-term research areas for Bell Labs in his July 31 address, I like to think the presentation was somewhat effective. (2) I played an active part in the recruitment of Alexander Holevo to our group. (Holevo is to quantum channel capacities, what Peter Shor is to quantum computing.) (3) I gave six tutorial lectures to the Mathematical Sciences Research Center on quantum information theory. (4) I mentored an undergraduate student for the SRP program. (5) I presented a talk in Arnold Auditorium for the Lucent Global Science Scholars Program. And (6) I have spent a lot of time in general trying to get people throughout the company practicing research in quantum information. Indicative examples can be found in the recent work of Lorenz Huelsbergen, Gerhard Kramer, and Serap Savari, but also in my role in co-organizing the Wednesday quantum talks.

26-09-01 *How Close Is He?* (to G. Brassard)

Not as close as I want him to be, but there are a couple of good theorems in the paper. Charlie Bennett once asked me, "How will you know when you've got a clean, crisp, PHYSICAL statement of the content of quantum theory? Is it like pornography: you'll know it when you see it?" I replied, "Yeah, actually." Hardy's porn is good for a starter, but it's not as hot as I want it to be.

01-10-01 *Difficulties* (to A. Furusawa)

I wonder if you might consider allowing me to lay a huge burden upon you?

I am an invited speaker at the 4th Annual Japanese-American Frontiers of Science Symposium, sponsored by the Japan Society for the Promotion of Science and the U. S. National Academy of Sciences, Wednesday, October 10 through Friday, October 12 in Tokyo at the Tokyo International Exchange Center. I think it is a fairly prestigious talk. However, because of the recent terrorist threats to Americans, and because our "state department" issued an announcement Friday warning particularly of potential trouble in Korea and Japan, I have decided that I will not come to it. My wife and I will be having a new baby in December and this would have been my fifth trip overseas this year: In all, it seemed like a good time to curtail my travels, before my "number" came up. Tomorrow I will announce this to the conference organizers.

But, all that said, I am plagued by a great sense of guilt in turning down my invitation on such short notice: This is why I am writing you. Would you consider giving a talk in my place? The format of the conference is to have two talks in each of eight subjects: one speaker in each subject is Japanese, one speaker is American. In my session, the Japanese organizer is Hiroshi Imai (from your university), and the other speaker is Tetsuro Nishino. My plan had been to give an introductory talk on quantum cryptography and quantum teleportation. Because the audience is mixed, from all areas of science, the talk should be a very introductory one.

I fully realize that you are not an American; however, your quantum teleportation experiment was performed in America, and I am hoping to use that as a bargaining chip. I think it would be a good talk for you to give, to get you and quantum information recognized by a wider audience.

If you are willing to do this, then I will send your name to the organizers tomorrow and write a strong letter of recommendation. Thanks for any help you can give.

03-10-01 *Kid Sheleen* (to N. D. Mermin)

What a busy couple of weeks it's been for me! I am sorry it has taken me so long to reply to you. But cars needed buying, lots of newspapers needed reading, lots of soul-searching needed doing (before canceling what would have been my fifth trip abroad this year), and Bell Labs needed some tolerance (with the transition from Brinkman to Jaffe as our Research VP).

Merminition 65 : *Sorry. Didn't mean to go on for so long. Caves is right; this is a rotten way to have a conversation. No need to reply.*

Yeah, Caves is right in many ways. I've never had more frustration in an email run before. But then, I don't think I've ever made such a headlong transition in open view before either. Regardless, however, I have certainly benefited from this: I've never had to strive so hard to make a simple point clear, and I think that gave me a load more perspective on the issue than I would have come to by myself. Strangely, in a way, the whole affair has hardened me and made me confident in this new direction of thought.

Merminition 66 : *so I'm sympathetic to what you're saying, but I worry that you're giving up "objectivity" on too many fronts.*

You think you worry! That reminds me of a dialogue in the old movie *Cat Ballou* (with Jane Fonda and Lee Marvin):

> Jackson Two-Bears: Kid, Kid, what a time to fall off the wagon. Look at your eyes.
> Kid Sheleen: What's wrong with my eyes?
> Jackson Two-Bears: Well they're red, bloodshot.
> Kid Sheleen: You ought to see 'em from my side.

But I don't think I've given up objectivity on too many fronts: If there's one thing for sure, it's that I don't want to go too far. The tenet I hold fast to is that there is something happening in the world, something was happening before scientists appeared on the scene, and something will continue to happen (in one form or another) should we wipe ourselves off the planet next year.

The issue is, how does that something interface with us, to what extent can we grasp it, and how do we modify it by our very attempt to grasp it?

Let me try to reply to some of the points in your letter.

Merminition 67 : *On the other hand you did seem to be undermining the impact of the trails of evidence when you gave a central role to the question of whether your Lucent colleague really had the polaroid oriented the way you had been led to believe it was oriented.*

Hideo Mabuchi is a young professor at Caltech (probably the youngest); he won a MacArthur Fellow (i.e., $500K) last year too. He started grad school at Caltech at about the same time that I started up with Caves. You can see he's the smart one. We've been friends ever since we first met at the Torino meeting of 1994. He keeps the napkin where I first explained the Holevo bound to him; I mention him whenever I can (like in the NATO paper) because of all the pearls of wisdom he gave me.

Merminition 68 : *I'll grant you that that's something to worry about, but it seems to me on a different level from the characteristic quantum ambiguities,*

Three months ago, I would have thought it was on a different level too. But now it is clear to me that the two NECESSARILY feed in to each other. (Though the whole issue has been building in me for two years: See Samizdat, page 127, in a note to Caves titled "The Dangers of Probabilismo." I'll talk about that more in a minute.)

Merminition 69 : *unless you want to deal with your colleague and his polaroid on the same footing as the photons, whence my joke about your closet Everretism. (Please read on before concluding that I still haven't got the point.)*

I did read on—many times—and I think you've got some of the point, but not the whole thing. In particular, I still contend that Everettism is the antithesis to my point of view.

There is a sense in which Hideo and his polaroid are on the same footing as the photons (and I have always thought this). It is that in reasoning about them—i.e., in reasoning about what kinds of traces they will leave from my interactions with them—I am obliged to use the formal structure of quantum mechanics if I want to do the best job I can in that reasoning. But I don't think your remark in particular has anything to do with this.

Your remark seems to be more of the flavor: Chris says POVMs (even simple von Neumann measurements) are "subjective judgments," so he must mean that their outcomes are every bit as dreamlike and subjective too. Measurements that have no concrete outcomes? What else could this be but Everettism in disguise?

But there is a non sequitur there (if indeed that is the flavor of your reasoning). It is that you (or my caricature of you) think I am identifying—in a supremely steadfast way—the "click" that makes its way to our senses when we perform a measurement and the very being of a POVM.

Instead, the idea is that the "click" is REAL, as real as you could want anything to be (for those concerned, for those who know of it). But, the index b (from some POVM $\{E_b\}$ that we associate with it) is not the thing-in-itself. It just gives the "click" a NAME and a *context* through which we can draw inferences and through which we can start to contemplate further reasoning. It is that identification which is an ultimately subjective element; it is not the "click" itself.

I had hoped to draw your attention to this distinction with the Penrose beauty-hag example, but I see I completely failed on that count (not only with you, but also with Caves and Schack). There is the stuff of the world—the "click" (in part). And then there is our description of it. The two things are not the same things; one lives in my head, and one lives partially outside it. To accept quantum mechanics, is to accept a template for that description and to accept a method of manipulating one's judgments thereafter.

Does any of this make sense? I think it is the main point I have been wanting to make to you, but as far as I can tell, you've ignored it.

Merminition 70 : *I agree that figuring out what measurements are associated with what operators (even at the von Neumann level) is something you have to bring in from the outside. So is knowing what the Hamiltonian is. I'm not so sure these are the same kinds of subjective judgments as a state assignment, as you maintain next. . . .*

The new thing I've been saying since August is that the identification of a particular POVM (and one element therein) with a measurement "click" is a subjective judgment. I had not clearly appreciated that before. However your mention of the Hamiltonian is apropos, because that one, at least, I had caught before. (That is what Samizdat, p. 127 is about.) There are several ways to see that "the Hamiltonian" must be on the same subjective footing as the quantum state. Here's one; I'll just quote you:

Merminition 71 : *I worry that "objective" is taking on too many different meanings here. For example, EPR makes it clear (at least to sensible people) that the polarization state of a photon is not an objective property of that photon.*

Will you accept that the existence of quantum teleportation carries as much force as the EPR argument that the quantum state is not an objective property of a photon? If you will, then can you tell me what the import of all the recent papers on "teleporting a unitary operator" is? (See for example `quant-ph/0005061`, but there are a plethora more.) But even if you don't, the argument is simple: Hamiltonians can be toggled from afar by our measurements on entangled states. (Carl gets himself out of this by saying that the only thing being toggled is the "effective" Hamiltonian, not the underlying quantum circuit, but I say where there is a tear in the fabric there is a rip.)

Merminition 72 : *I think you're saying that if the vertical alignment of the polaroid is an objective fact, then the state — "vertically polarized" — of the emerging photon is also an objective fact. But that's not the same as saying it's an objective property of that photon.*

That is right, there is a difference. And you have gathered what I was saying (almost) correctly. (I wrote "almost" to help remind you of the points above about identifying "clicks" with POVM elements.) The point is, the quantum state had better not be an objective fact, or the point of view that Caves and Schack and I have been trying to build up will be in deep trouble. What is wrong with taking a quantum state to be an objective fact, as long as one drops the insistence that that fact be localized with the photon? (I.e., as long as one does not make it a property of the photon where it stands.) At first sight, maybe nothing: I think that is probably the point of view you are trying to build; it is also the point of view Philippe Grangier has been trying to build in his recent `quant-ph`'s. But, on second sight, one cannot forget that the quantum state uniquely specifies a set of probabilities. If the quantum state is an objective fact, then so are those probabilities. And now it is on your shoulders to tell me what objective probability can possibly be. I won't stand for anything short of an operational definition.

Merminition 73 : *I worry that "objective" is taking on too many different meanings here. For example, EPR makes it clear (at least to sensible people) that the polarization state of a photon is not an objective property of that photon. It appears from the above that you believe you can only consistently take this position if you deny objective status to the outcome of the actual polarization measurement which enables you to predict the outcome of the measurement that has not yet been made.*

No. See the point above, where I used a little TeX notation. I accede to the objective status of something happening in a measurement intervention. I just don't accede to an objective status for what we decide to call it, i.e., for which POVM we decide to associate it with.

Merminition 74 : *(Forgive me, but this smells like a many-worlds strategy again. Your answer to the EPR paradox would seem to be to deny that the first measurement had an objective outcome. Recall Henry Stapp who has been saying for decades that only for the Everretista is nonlocality not a consequence of EPR.)*

I forgive you. But I hope you'll tell me that, with your newfound enlightenment, it doesn't smell so much like Everett anymore. (Your imagery conjures up my own imagery of walking on a warm day near a trash can full of lobster parts behind some coastal New England restaurant.)

Merminition 75 : *I'd prefer a middle ground which allows me to talk about objective facts but not objective properties. (It just now strikes me that one might call this correlations without correlata.) I've never been sure I can do this coherently, so I'm sympathetic to what you're saying, but I worry that you're giving up "objectivity" on too many fronts.*

I am in partial agreement with your first sentence, and I would like to think I have hit a sweet spot for that part. I'll give you objective "clicks" (though I might not call them "facts" ... but that's a story I probably shouldn't get into right now); I just stand fast against the objectivity of the quantum state. What is more middle ground than that? However, I do not share your aversion for objective properties.

It seems to me, quantum systems do have some properties that we can get our hands on. I usually preach the bundle of information-disturbance curves associated with a system, but let me try from a different angle to convince you of at least one property. I say that the quantum state cannot be an objective property because we can toggle it from a distance. I say that the Hamiltonian cannot be an objective property because we can toggle it from a distance. But what about a quantum system's Hilbert-space dimension d? Can you think of a way to toggle that number from a distance? I can't. And so, to that extent, I'm willing to call the raw number d an objective property of a part of the world. Now, what is the physical meaning of d? Well, that's why I struggle with all this information-disturbance stuff, but that's not the issue at hand. The issue is that one need not give up on all objective properties.

There are things in the world beyond our control: One them is the outcome of a quantum measurement, and one of them may be the dimensionality of a Hilbert space. Objectivity means nothing to me if it doesn't mean that some things are beyond my control, are beyond my whim and fancy. To the extent that I'm willing to say this, I don't think I'm giving up on objectivity on too many fronts.

Does this strike any chords in you?

PS. By now you should have received our modified version of "Making Good Sense." I won some good battles there: We no longer claim that two observers must be compelled to the same unique state via a Dutch-book argument. But I lost some too. I continue to think the paper is misleading as hell, always talking about a "unique" state assignment and using the word "certainty" in a way that still troubles me. We were able to compromise only in that I thought things were now worded in a sufficiently vague way that I could worm out of them in my future talks and publications. I don't think we say anything factually against my beliefs, but the reader will have to be on his toes to not get fooled about where I really stand.

PPS. Here's another thing I ought to tell you. PRA made the mistake of asking me to referee the BFM paper. Despite what I wrote you earlier about probably accepting the challenge if it came up, I decided to decline the opportunity. I like the math of the paper, but I just could not agree with what you make of it. It seemed more appropriate to let some less tainted souls than mine tell you what they think of it.

03-10-01 *Replies on Practical Art* (to C. M. Caves & R. Schack)

Cavesism 23: *The philosophers tend to proceed by telling a story—reasoning by analogy, they call it. The actual problem is too hard for them to formulate, so they immediately introduce a simple analogy, reach a conclusion they like within the analogy, and then transfer the conclusion back to the actual problem, without ever justifying why the analogy has anything to do with the actual problem.*

Except for omitting the final justification—which is more than important—is this so different from what you teach in your physics classes? I.e., That one ought to try one's ideas out on a simple example first? One that may already contain the essence of the problem, before embellishing it with too many details?

Cavesism 24 : *I enjoy reading your stories, but perhaps you're falling into the same sort of trap in a less obvious way. The difficult and very personalistic questions about assigning probabilities in your hag-beauty-insurance story are important in thinking about Bayesian probabilities. These personalistic factors are well known to be present in a subjective interpretation of probabilities, but do we really have to worry so much about them in the context of interpreting quantum probabilities?*

My point was to remind you guys and Mermin that these personalistic factors always must exist, else we would have no need to take such pains to talk about a "subjective interpretation of probabilities." If they are well known (as you say), then they should not be forgotten and replaced with purely objective "trails of evidence."

The point is, yes we must worry about them in the context of interpreting quantum probabilities. We must recognize that that is part of the very problem. Once we have recognized it, then we can move on and almost forget that the issue was ever there—just as one can do in whole textbooks on orthodox probability theory—but that first step is a supremely important step.

Cavesism 25 : *You and others write papers every day where this party assigns this state and that party assigns that state, and I don't see any of these papers agonizing over difficult, personalistic questions of what state to assign. You're right to keep badgering us to pinpoint why this is true, but the fact that it is true—we don't worry about this kind of stuff when making quantum state assignments—leads me to believe that there is an answer.*

Nor do you see any sophomore-level textbooks on probability theory agonize over these personalistic questions when posing its exercises at the end of each chapter. On the one hand, you completely missed the point I was trying to make, but on the other you also completely answered it.

The point is one does not have to worry about these personalistic questions to get quantum mechanics as a *calculational* tool off the ground. In that regard, the present issue is no different than with classical probability theory. Indeed, this is probably why in both theories a large sect of the practitioners have turned to "objective" interpretations of their main terms (alternatively, probabilities and quantum states) in such a misguided way. It is in the very recognition that personalistic questions exist, that one is compelled to finally get the foundations of the two theories straight.

In practice, what almost always happens? In the case of classical probabilities, when given a specific problem, one reduces and reduces the problem until one has transformed it into an equivalent problem for which one feels confident in making the uniform probability assignment. Thereafter one *derives* the probabilities for the problem of real interest by transforming and grouping, etc., until one rearrives back at the starting point. (This is a point I probably first learned from Rüdiger.) Think for instance, if I were to ask you what is the probability of obtaining a 7 or an 11 in a roll of two dice. Your mind would probably first jump to the judgment that all six sides of each die are equally likely, and then let the calculations flow.

Now of course, being a Bayesian, you would leave open the possibility for something else than a uniform assignment in that step above. But in practice, there are some things that most of us can usually agree upon . . . and those are usually the starting points for textbook problems.

The issue is little different in quantum mechanics. When presented with a problem of calculating a quantum state for a given physical system, what do we usually do but reduce and reduce (or expand and expand) the problem until we come across an equivalent one for which we are confident we can predict the outcome of some measurement with certainty? Thereafter we work our way back just as before. Just think of Scully and Lamb's derivation of "the" quantum state of a laser.

Alternatively, think about Mølmer's justification of the same state. [Indeed one might say that this is what the whole (worthless) decoherence program amounts to: deriving one subjective state from another and then thinking there is something deep about it. But that's an aside.]

Now just as before, being a Bayesian, one ought to leave open the possibility for something else than the particular pure state in the basic step of this derivation. But in practice, there are some things that most of us can usually agree upon ... and these are usually the starting points for textbook problems.

I think the similarity is overpowering. It is enough in both cases to recognize that ultimate personalistic issues exist, but then the homework assignment goes on—the student reduces the problem to a judgment few people would dissent on.

Cavesism 26 : *It suggests to me that agonizing over the sorts of questions in your hag-beauty-insurance story is not going to help us understand what we're doing as scientists and, moreover, that it will lead our program into a place where no other scientists wants to go.*

Looking back over this note again, your language really was very scolding throughout—"trap," "badgering," "agonizing," "a place where no other scientists wants to go"—in spite of the fact that you warned me it would be "a constructive and gentle criticism." I understand that I am guilty of no less: There is no doubt that I can be arrogant and abrupt (and paranoid) at times. But in all this massive email, I feel that I have been providing a service, sharing ideas that I might not have if I didn't feel we should be brothers in arms. It became a little hard to gulp that all these notes might be viewed as little more than a nuisance.

Cavesism 27 : *The answer might be as simple as this: we can only do science in situations where we scientists have agreed that such personalistic factors can be essentially eliminated, and quantum mechanics is the very pinnacle of this kind of situation. I think that's the content of our statement that "Gleason's theorem is the greatest triumph of Bayesian reasoning" and of our "principle of quantum indeterminism."*

I think the answer might just be as simple as that, but at a level higher than the one you are contemplating. The agreement we need is in accepting quantum mechanics as a method and a restriction for shuffling about our more mundane, everyday beliefs. What that entails is accepting POVMs as the structure of the questions we can ask a system and the Kraus state-change rule as our method for updating our beliefs.

Everyone keeps asking, what is the objective piece of quantum mechanics? I answered some of my beliefs on that issue in the letter I just sent off to Mermin (and then forwarded to you). But I think there is also quite a bit to be learned on the issue by first turning the question toward Bayesian probability theory. What is the objective piece of Bayesian probability theory? I think all three of us are in agreement that it is not in the particular probability assignments that one might make. But is there *any* objective piece at all? I think there is. Take Bayes' rule as an example. I would say that it is something objective in the theory: it is the ideal of behavior. If one doesn't use it, one can be taken advantage of. You agree that Bayes' rule is the ideal of behavior, and I agree that Bayes' rule is the ideal of behavior: it would remain the ideal of behavior if all of us were wiped off the planet.

Likewise, it seems to me, Gleason's theorem plays a similar role. There must be a sense in which accepting that the structure of our questions to the world (or, alternatively, our interventions upon it) conforms to the structure of the POVMs must be the ideal of behavior—something not so far removed from Bayes' rule itself. It is the ideal of behavior in the light of some crisp, physical fact.

I don't know what that fact is yet (in any precise sense), but that does not stop me from seeing the outline of how the various structures in quantum theory should be classified:

measurements = POVMs	objective feature (physical fact)
Born RULE (via Gleason)	objective feature (an ideal of behavior)
Kraus state-change RULE	objective feature (an ideal of behavior)
quantum state	subjective judgment (always)
time-evolution map	subjective judgment (always)
Hilbert-space dimension	objective feature (physical fact)
particular POVM assignment	subjective judgment (always)
particular Krausian assignment	subjective judgment (always)

The list is not exhaustive; but I think these are the ones I see clearly at the moment.

The point is: Agreement required for science? Yes. Compelling interpersonal agreement as a (potential) statement about the agent-independent world? Yes. Agreement necessary at the level of quantum states? No.

Cavesism 28: *I'm not sending this to Rüdiger and Mermin, but you can send it to them if you think it's worthwhile to do so.*

Well, clearly I thought it was worthwhile to share my answers ... but there's that arrogance again. ;-)

With a smile and a conciliatory tone,

03-10-01 *Further Replies on Practical Art* (to C. M. Caves & R. Schack)

Cavesism 29: *I think we agree that there are things that are effectively facts in the effective reality of ordinary experience.*

In the words of Bennett's father (in such a context), "These are very deep waters." Since becoming enamored with James, Dewey, and Schiller—and having read copious (by my standards) amounts of them—I'm not completely sure how I should answer you. The issue is, I'm not completely sure in which sense you are using the word "fact." I have a feeling it is a more loaded sense than you would guess. But I don't want to get into that now: You suggested some simmer time, so I will leave you some until it becomes absolutely necessary.

There is, however, one thing I dislike about this sentence, and that is the phrase "the effective reality of ordinary experience." But you touched upon that very point in your note "More on Pots and Kettles"; so I'll say more to the issue in detail when I reply to that note. The main thing, though, is that I would say our ordinary experience is the rawest stuff around: It's the very stuff from which we build these super-smooth pictures by way of which we derive our further expectations. There is nothing effective about it: It is the stuff, it is the starting point. To use the word "effective" makes it feel secondary and derived (which is what you have been striving to get at, not me).

Cavesism 30 : *The questions arise in what those facts tell us or, perhaps, in whether and what they compel us to believe. The argument is about pure-state assignments, not about mixed-state assignments. You believe that the subjectivity of pure states requires that it be possible for different agents to assign different pure states.*

Yes.

Cavesism 31 : *To say something is subjective is to say that it exists only because of us and does not have an independent existence out there in the world. It also implies that different agents can disagree, the degree of possible disagreement being just the flip side of the degree of intersubjective agreement.*

Yes.

Cavesism 32 : *Suppose we had the idea that facts in the effective reality force one to a particular pure-state assignment. The resulting pure state is then based on a trail of evidence in the effective reality and is embedded in each agent's mind. Is the pure state then out there in the world, independent of us?*

I would say, yes it is. The agent's state of belief is then an unneeded complication in everything under discussion. The fact is that there is a one-to-one correspondence between (sets of) facts and quantum states. You can say the agent's mind is nevertheless needed to "house" that state, but then, to me that looks to be nothing deeper than invoking the luminiferous ether to support the electromagnetic field.

Cavesism 33 : *Do I have it right that this is the issue, or at least an issue?*

Yes. I have always perceived this to be the main issue. To the extent that I have said words all around this, it has been—I believe—to attempt to give different angles for viewing the same thing. Every time I saw you, Rüdiger, Mermin, Brun, etc., be reluctant to accept the point, I tried to present it from a different angle so as to be more convincing. I take it now that everyone only found that to be confusing. But what else could I do? And I can't complain too much, because I think the whole process has sharpened my presentation of the point (which I maybe only dimly perceived at the beginning).

Cavesism 34 : *I don't necessarily see where the pure state is if it's thought to be out there in the world. The trail of evidence is not a pure state; we construct the pure state from the trail of evidence, but dogs don't and dinosaurs didn't.*

A one-to-one mapping is a one-to-one mapping. I do not see how YOU cannot see that making these statements is not a tacit acceptance that the quantum state is an objective entity after all. Maybe you have thought this all along. Namely, that when you said a quantum state is not a state of nature, what you really meant was that it was simply not localized on the physical system it is meant to describe. It is a state of nature, i.e., it is a collection of facts within nature, it is just not living on the system it's intended for. From this point of view, it's clear why dogs don't use them: Dogs aren't clever enough or advanced enough technologically to discover the true states of nature.

But I surely never thought this when I used the slogan, "a quantum state is a state of knowledge, not a state of nature." If facts can uniquely determine a quantum state, and facts live in nature, then a quantum state is a state of nature after all.

Cavesism 35 : *The pure state is not out there in the system for the reasons we have long discussed: the system can't report its pure state, and a system's state can be changed to any pure state drawn from incompatible sets without ever getting close to the system. It looks to me like the pure state is purely in our minds.*

I'm not sure how this remark fits in. You might be making a call for me to consider putting the "trails of evidence" into the mind, but I'm not sure.

Cavesism 36 : *You are insisting, I believe, that in order for a pure state to be subjective, it must be possible for different agents to disagree on a pure-state assignment. You say, I believe, that if we are forced to a particular pure-state assignment by the facts in the effective reality, then the pure state becomes objective.*

Yes.

Cavesism 37 : *I don't know where to come down on this. It is one aspect of the question I always ask of which aspects of maximal belief get translated from realism to quantum mechanics. It also has to do with the nature of the "facts" in the effective reality and thus how the effective reality arises out of quantum mechanics (this is the content of Mermin's Pots and Kettles). My own take at present is that the effective reality is a form of intersubjective agreement.*

Fair enough that you don't know where to come down on this: I will try not to lose my patience any more.

Cavesism 38 : *You are right in principle that nothing compels us to a particular pure-state assignment, but clearly wrong in practice. All our experience with quantum mechanics suggests that we have no trouble agreeing about pure-state assignments, so no matter how the facts in the effective reality arise, there is nearly total intersubjective agreement on what they imply for pure-state assignments (this is the content of my Comment on Practical Art and further comments on Pots and Kettles).*

I hope my previous note addressed this adequately. In contrast to what you say, I believe that I am right in principle and right in practice. You might have said the "all our experience" sentence about classical probability theory if your name were Richard von Mises. He would have said that all our experience with dice shows that we have no trouble agreeing that its outcomes are all equally probable. But you're not von Mises, and you've had the advantage of having had 75 years of good Bayesians clear the air for you. The issue you bring up has little to do with the structure of the physical world, and little to do with the structure of the Hamiltonians we feel compelled to describe it with.

Cavesism 39 : *Let me know if I have got your position straight.*

I think you did.

Cavesism 40 : *If I have, then it seems to me that we are not far apart, the only gap being how much we are willing to ascribe to the apparent agreement that exists in assigning pure states. You prefer to emphasize that nobody can be coerced into this agreement, and I prefer to emphasize that in practice nobody has to be coerced into it.*

A point of emphasis can make a huge difference in a philosophy. And a difference in a philosophy can make a huge difference in the practical and applied questions one might ask of quantum mechanics.

And I'm off to Lupé's for the best Mexican food in New Jersey. (You know that's not saying much.)

More tomorrow!

03-10-01 *Next Week* (to J. N. Butterfield)

Thank you for reminding me that you'll be in town soon! It was a hard decision, but I decided not to go to Japan after all. (It would have been my fifth trip overseas this year, in any case, which is maybe too much.)

I would love to meet with you. Probably the most fun thing for me would be to meet you in the city somewhere, if you're game to that. I could spend the morning book shopping, meet you for lunch and afternoon discussions somewhere in the Village or East Village, and then return to book shopping. How does that sound? Monday would probably be best for me; but Wednesday (second most favorite) and Tuesday (least preferred) would be doable too.

I would especially like to hear more about the deeper desires that underlie the program you outlined in your talk in Belfast.

I'll put all my phone numbers (work and home) below. Let me hear from you before your departure and we can start to work on a more precise plan.

04-10-01 *Champagne and Roses* (to J. N. Butterfield)

Butterfieldism 1: *Thanks for yours. Im very Glad theres a chance well meet; but I don't want you to come to NY unlesss (i.e. and only unless! A Halmos-ism) youd like to come for book-shopping etc anyway.*

Please realize, you are the imperfection on the smooth surface of my champagne flute. You are the catalyst in my converter. No, I wouldn't normally go to NY City on a Wednesday, no matter the strength of my constant obsession to hoard books. But all it ever takes to seed the bubble is the promise of a good conversation! Let's meet in NY City.

I know of a good Afghani restaurant ("Khyber Pass") in the East Village. Would you like to meet there if it's still open?

04-10-01 *Finicky Sins* (to C. M. Caves)

By the way, in saying this yesterday,

> There is, however, one thing I dislike about this sentence, and that is the phrase "the effective reality of ordinary experience." But you touched upon that very point in your note "More on Pots and Kettles"; so I'll say more to the issue in detail when I reply to that note. The main thing, though, is that I would say our ordinary experience is the rawest stuff around: It's the very stuff from which we build these super-smooth pictures by way of which we derive our further expectations. There is nothing effective about it: It is the stuff, it is the starting point. To use the word "effective" makes it feel secondary and derived (which is what you have been striving to get at, not me).

it dawned on me afterward (on my drive home) that I was committing a sin: There was a time when I liked the phrase. As you know, I commandeered it when writing my *Physics Today* articles with Asher.

So, maybe I was being overharsh, or maybe just finicky. In any case, even when I used it unreservedly, I know that I had a distinct flavor of the phrase in mind from the way you had been using is. [See our discussion starting on page 133 of the Samizdat.]

Maybe I'll say more about this later today.

04-10-01 *Replies on Pots and Kettles* (to C. M. Caves & R. Schack)

This letter is going to be a hard one to reply to, because I don't quite see how much of what I have said in the past led to the points you make here. So, let me just plunge into the thing and see what comes out.

Cavesism 41: *I thought you were in the camp that holds that the our experiences—our actions or interventions and our perceptions of the world's response—are primary*

I thought I was too.

Cavesism 42: *and that the function of science is to account for them.*

It's this part of the sentence that I'm not so sure of (though it's not clear to me exactly what you have in mind). If our experiences are primary, then it does not seem to me to be within science's purview to account for them. I believe, instead, the view I have had for quantum mechanics for some time is best mimicked by these words I picked up from William James last month:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself. 'God,' 'Matter,' 'Reason,' 'the Absolute,' 'Energy,' are so many solving names. You can rest when you have them. You are at the end of your metaphysical quest.
>
> But if you follow the pragmatic method, you cannot look on any such word as closing your quest. You must bring out of each word its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

Science does not account for our experiences. Science builds on them and gives us a structure by which to imagine pushing them to a new extreme. This is why I have laid such emphasis on calling the world "malleable" (for instance in my "Activating Observer" document that I shared with Rüdiger … and maybe you too, I can't remember). It seems to me, science does not say so much about what is, but what can be (subject to the limitations to our actions captured in the very structure of the given theory).

Cavesism 43: *Trouble is that when our interventions proceed to too fine a level, the world's response is not deterministic and, furthermore, cannot be described within the realistic language of ordinary experience. Surprisingly we find that we can use the strange, unrealistic formalism of quantum mechanics to describe the intrinsic indeterminism that intervenes between our actions and our perception of the world's response.*

What do you mean by "too fine a level?"

Cavesism 44: *I thought you were ascribing some sort of objective or intersubjective reality to our primary experiences.*

Pretty much. Or, at least that's what I thought too.

Cavesism 45: *I thought the difference between you and me was that I think that we must somehow derive from quantum mechanics—or, at least, make consistent with quantum mechanics—the apparently realistic features of the emergent "effective reality" of ordinary experience, whereas you think this is unimportant, thus accounting for our different reactions to the decoherence program. But you now seem to be demanding much more than I do,*

I've always thought that I've demanded much less than you, and I don't think I've changed my tune on this account for several years. For the view I dream of constructing, what is the classical world? It is a world for which the agents describing it are full of ignorance and the best to which they can muster is a lot of imprecise control.

I look out at one of the trees outside my window, and I ask how I might capture everything I'm willing to say about it into a single density operator. I can't say much about that, but I'm willing to bet that if I would carry the project through, what I would end up with will be so mixed, so thermal, that it'll be just about commuting with anything else I might have come up with, even if I had stared at the tree a little longer. This little fact—it seems to me—has nothing to do with the Hamiltonians of the world (as if they were objective things). It is a function of my pure ignorance and my unwillingness to tear the tree apart and refine my beliefs.

The idea toyed with here is that it is just ignorance, no matter how we each walk into the room with it, that leads to the classical world. If I am so ignorant as to use an almost commuting set of density operators for a given object, then any (gentle) attempt you—as another scientist—may make to refine those beliefs will not cause my beliefs to be any less valid: Your information gathering, will not cause a disturbance to my description. And therein—the speculation goes—lies the essence of classical mechanics.

Cavesism 46: *that before we even start, we be able to explain exactly how the effective reality works and at what point it emerges. As David points out, this is exactly what the Everettistas demand.*

As best I can tell, this remark can only come from viewing my program (more accurately, my dream) from your philosophical predispositions. I would never demand that we "explain exactly how the effective reality works and at what point it emerges." The classical world comes first.

Quantum mechanics (as a theory of inference) extends beyond it, by taking into account new phenomena that simply can only be seen when working in a regime of less ignorance.

A relevant ditty to read might be my essay "Always One Theory Behind" on page 464 of the Samizdat.

Cavesism 47: *Of course, after making the demand and finding present responses unsatisfactory, you and the Everettistas go in quite different directions. They, out of an anal need for naive realism, simply concoct a naive realism to go with the state vector. You certainly aren't going in that direction, but being risk averse and already burned, I'm not going to risk a description of your direction here.*

I loved the phrase "anal need."

Cavesism 48: *To your credit I think you won't claim to have gotten your ideas worked out entirely (perhaps the rest of us can be allowed some access to that defense). Still you might want to think about the road you're traveling on and how it relates to this question of taking as given the apparently objective experiences of our daily lives.*

Thanks for the credit. It's refreshing to be complemented for not acting like a guru with all the answers. People at foundations meetings will have none of that. It's been my experience that they demand you tell them exactly what reality *is* ... before they quickly tell you you're wrong. (Matthew Donald told me he couldn't take me seriously as a foundations researcher because I keep evading the question of what reality *is*.)

Anyway, in conclusion, give me some feedback: Did I answer anything that you wanted answered? (I sure hope I did: I'm trying.)

04-10-01 *Replies to Morning Coffee* (to C. M. Caves & R. Schack)

Schackcosm 35: *Oh no, I thought we had reached some agreement. One problem is that Kraus operations (unless they are 1D projectors) will not bring you to a unique place. Which means that qm does NOT provide a universal rule of coming to agreement. You need some ad hoc assignment.*

Yes, I had always understood that. I am sorry if I was sloppy about expressing it, but I thought I had always emphasized that there are two things that one can contemplate: 1) if Alice and Bob have complete freedom to choose what measurement they might perform, and 2) if instead they have at their disposal some fixed measurement (perhaps not of their choosing). In the later case, only certain initial states for Alice and Bob will lead to further agreement after the measurement. (See, for instance, my note to Mermin and company dated August 7.)

Schackcosm 36: *The other problem is that your conversation is far too playful. State assignments are compilations of betting odds. They are COMMITMENTS. Chris in your dialogue should have been deeply shaken. He would have betted his house in New Jersey on this outcome to be impossible.*

Yes, perhaps. But, on the other hand, there is a counter trend in you that troubles me. And that is the basic philosophy that comes across as the message, "Once a quack, always a quack." What I mean by this is, suppose I ascribe a pure state $|\phi\rangle$ to some system, whereas you ascribe $|\psi\rangle$. As we have laboriously teased out of this correspondence, from your perspective, I am simply wrong. My judgment is not to be trusted (from your perspective). This much we agree upon. But I sense that you want to conclude more: Not only am I not to be trusted in my conclusions about the given system, but that I am not to be trusted about anything. You conclude that I am truly insane just because I adamantly disagree with you about one thing (as captured by our differing pure states). I say that goes too far.

I tell too many stories, but here is a true story. In discussing the cardinality of the natural numbers versus the even numbers, Kiki will accede that there is a one-to-one and onto mapping between the two sets. Nevertheless she contends that there are more natural numbers than even numbers. I have never had more annoying conversations than the one we revisit about once a year on this subject. I simply cannot convince her that she is not being logical on this issue. But still I do find that I trust her judgments on other issues.

A "misstep" on a quantum state (even a pure state), it seems to me, is not the end of the world precisely because of this.

Granted, I am a bit confused on what I think the ascription of a pure state actually does capture, but I think making it carry the weight of an agent's rationality or irrationality goes too far.

Schackcosm 37: *Yet another comment: You have said nothing in all your notes (to my best knowledge...) that tells me why this situation is different from Chris being certain that there are two chairs in this room, and Carl with Hideo's help convinces him that he was wrong. I'd say either Chris was tricked, or he had hallucinations. Todd said: "This is why we say that insane people 'out of touch with reality'". I said the same thing in a different way.*

Since you ask this question more pointedly in another note, I'll wait on answering it until I get there.

Schackcosm 38:

> CAF Said: So, in saying this, I don't think one has to do anything fancy with a measurement to get this thing you call fuchsian consistency off the ground. No intricate constructions of states seem to be needed. Choose ANY (mutually agreed upon) von Neumann measurement you like, and Carl and Chris will have to agree at the end of the day. One of them will feel foolish for having been so wrong, but as long as he is rational, agreement can still be had. As Rüdiger said, the idea is that this is a property of quantum mechanics (in its capacity as a law of thought), not of the initial states.

I found the idea quite attractive that in a quantum world, differences can be resolved (using a well-chosen measurement) that would be impossible to resolve classically. I thought that this was what you had in mind.

As far as I can tell, no physical statement (no ascription of a phase space point) is impossible to resolve classically. What is different is that quantum mechanics can do that even without the preexistence of a phase-space point ... and that surely is a property of quantum theory. But I said I'd come back to this in another note.

04-10-01 *Replies to a Conglomeration* (to C. M. Caves & R. Schack)

Now let me reply to a conglomeration of notes from you two.

First to Rüdiger:

Schackcosm 39 : *I remember you writing something to the effect that the click in a measurement is the closest thing to a fact one could come up with (sorry for not looking it up, but you write TOO MUCH).*

Did the reply I wrote to Mermin yesterday make any sense to you? I am now in the habit of drawing a distinction between a "fact" and a "proposition." The difference was not so important classically, but I now think it is paramount quantum mechanically. The fact (or consequence of our intervention) is the raw, uninterpreted, unclassified stuff of the world. It is the real stuff that makes its way to our senses and then to our brain to be pondered. The proposition, on the other hand, by its very nature attaches a meaning to the fact and, as such, is a subjective judgment. What this means in the quantum case is that to say there is a "click" is one thing: Presumably that is not a subjective judgment if I say it, and Steven says it, and everyone else we talk to says it. However, to say that that means the particular outcome E_b occurred in the POVM $\{E_b\}$ is to lay down a proposition, a subjective judgment.

The reason we could get confused in the classical case, and think that a proposition was more than a subjective judgment is because in the classical case, propositions don't entail probability assignments.

This distinction I'm drawing is not so different than the one Marcus Appleby draws in criticizing the Meyer-Kent-Clifton "nullification of the Kochen-Specker theorem." See his paper, `quant-ph/0109034`.

Schackcosm 40 : *I am afraid that I still don't know precisely where you stand, despite of your effort at explaining.*

Let's start from the classical notion of certainty. Let's consider the case where a physicist is certain that some outcome will not occur. Dutch book consistency implies that the outcome will be in his nullspace. That's the quantum part.

Do you agree?

Yes.

Schackcosm 41 : *1.) Carl is certain that up will occur and Chris is certain that down will occur.*

2.) Carl is certain that there are three chairs in the room, and Chris is certain that there are two chairs in the room.

In both cases their beliefs are contradictory in the same, classical sense.

Do you agree?

No, I do not think the statements have the same meaning. In the first case, in order to find out which of us is "right" and which of us is "wrong" we must elicit the world to produce something that it didn't contain beforehand—namely, the result of the measurement. In the second case, we can go blissfully along thinking that one of us is "right" and one of us is "wrong" simply because the world has something in it that one of the two of our brains is mirroring correctly.

There is a difference. In the classical world, reality is the ultimate arbiter of truth. In the quantum world, where we are fairly convinced that "unperformed measurements have no outcomes," we are actually lucky in a way that there is still an arbiter of agreement—we just can't identify it with a preexisting reality. It seems to me this is a feature of quantum mechanics: The theory can still bring us to agreement even without a preexisting truth value for our propositions. One might have imagined a more malicious world where we would not have even been able to rely upon that.

Schackcosm 42 : *If we make claims, we are COMMITTED to those claims (e.g., via betting behavior implied by the claims). I believe that starting from the notion that different scientists (different agents in the same linguistic community) should not have contradictory beliefs is eminently reasonable. To throw this notion over board, one needs excellent reasons. I enjoy playing with the idea, but I am far from converted.*

This hits upon what I wrote to Carl yesterday. The gulf that separates us seems only to be in where we think this agreement must be applied to get the engine running. I say both of us accepting QUANTUM MECHANICS as a structure is good enough. (I.e., accepting the theory is our common agreement.)

Now to Carl:

Cavesism 49 : *Your point, as we see, is that we can think about life going on after finding a result deemed to be impossible. As you point out, there is a limit (add on the null subspace with epsilon eigenvectors, get a result in the epsilon subspace, update, and then take the epsilon goes to zero limit) in which we can think of updating a state assignment based on outcomes in the null subspace. But I think this misses the point. This isn't updating a prior belief. It's realizing that your prior belief is full of it and abandoning it in favor of life going on, as you put it.*

Moreover, the really nice distinction between classical and quantum Fuchs consistency is lost if we adopt your point of view. If we adopt your way of formulating Fuchs consistency, then it has no content either classically or quantum mechanically.

I'm just repeating myself now, I but I don't see that as contentless at all. In fact, though the effect is the same in both theories, the content is quite different across the two of them. In the classical case, we can always "pick up the pieces" as you say, by realizing that there is something really there and just checking what it is. In the quantum case, we can always bend the world into something we *will* agree upon, even if we violently disagree upon the meaning of some subset of our previous interventions.

Here's the way, Josiah Royce put it in a letter in 1888:

> Thus called upon to explain amid the trade-winds, and under the softly flapping canvas, the mysteries of [quantum mechanics], I put the thing thus: "There was once a countryman," I say, "from Cape Cod, who went to Boston to hear Mark Twain lecture, and to delight his soul with the most mirth-compelling of our humorists. But, as I have heard, when he was in Boston, he was misdirected, so that he heard not Mark Twain, but one of Joseph Cook's Monday Lectures. But he steadfastly believed that he was hearing Mark. So when he went home to Cape Cod, they asked him of Mark Twain's lecture. 'Was it *very* funny?' 'Oh, it was *funny*, yes,—it was *funny*,' replies the countryman cautiously, 'but then, you see, it wasn't so *damned* funny.' Even so, Captain," say I, "I teach at Harvard that the world and the heavens, and the stars are all *real*, but not so *damned* real, you see."

Cavesism 50 : *The parties can always come to agreement, no matter what their state assignments, simply by getting amnesia regarding their prior beliefs and then picking up the pieces in the only way they know how.*

But there's really more to the story. They can always come to agreement, indeed—regardless of how disparate their initial opinions—if they are willing to make an essentially infinite expenditure toward laboratory technique. That is to say, the only thing that will give assured agreement in all cases is a set of Kraus operators all of rank-one. Jacobs and I called those infinite strength measurements: the idea being that they are hard to actually do. In more real-world measurements, where the operators are never really rank-one, coming to final agreement will generally require some initial agreement. Whence the point in my August 7 letter to Mermin.

Cavesism 51: *I believe that Fuchs consistency is about coming to agreement in the light of the outcome of an agreed-upon measurement where no party has to abandon his prior beliefs (certainly one has to agree that this is a legitimate case to consider). It could be that all parties are dumbfounded by the result, but let's put that case aside. For all other outcomes, the point is that all the parties be able to come to agreement by updating their prior beliefs. This imposes a strong constraint classically—all parties must have the same support—but appears to be no constraint quantum mechanically. That's an important distinction, it seems to me.*

I do agree that that is a legitimate case to consider. What I am not seeing presently is that its study will shed some foundational light. But I think I'm open-minded on this one: I might be convinced yet; I just don't see it now.

06-10-01 *Your Spelling Conscience* (to J. Bub)

I was committing one of your notes to my computer archive, and I caught such an intriguing spelling variation in it that I had to write. In describing Steane's article below, you write "inciteful." Wonderful! How apropos for the context. Did you do that on purpose?

Jeff's Preply, 08-08-01

> [**Chris said:**] Maybe some of the most interesting discussions centered around Andy Steane's paper "Quantum Computation Only Needs One World" (which Richard Jozsa presented). Doug Bilodeau had the wonderful idea that perhaps some combination of it and [Wootters'] old Ph.D. thesis could give us a deeper insight into where quantum computing derives its power from: Quantum computers are not powerful because they perform so many calculations in parallel (as the many-worlds pundits imagine), but rather because they do so *few* calculations! I.e., Their power derives from not doing anything they don't have to do for the final result.

I think this is exactly right. I've been thinking along these lines myself. I just wrote an article on 'Quantum Entanglement and Information' for the *Stanford Encyclopedia of Philosophy* (an online encyclopedia at `http://plato.stanford.edu`). It's not posted yet because I have to make a few minor changes. The concluding section says:

> The favoured explanation among Deutsch and others of how a quantum system processes information is the so-called 'many worlds' interpretation. The idea, roughly, is that an entangled state of the sort that arises in the quantum computation of a function, which represents a linear superposition over all possible arguments and corresponding values of the function, should be understood as a manifestation of parallel computations in different worlds.

The quantum circuit is designed to enable the computation of a global property of the function by achieving some sort of 'interference' between these different worlds. (For an insightful critique of this idea of 'quantum parallelism' as explanatory, see Steane.)

An alternative view, not much discussed in the literature in this connection, is the quantum logical interpretation, which emphasizes the non-Booleanity of the structure of properties of quantum systems. (The properties of a classical system form a Boolean algebra, essentially the abstract characterization of a set-theoretic structure. This is reflected in the Boolean character of classical logic, and the Boolean gates in a classical computer.) A crucial difference between quantum and classical information is the possibility of computing the truth value of an exclusive disjunction – for example, the 'constant' disjunction asserting that the value of the function (for both arguments) is either 0 or 1, or the 'balanced' disjunction asserting that the value of the function (for both arguments) is either the same as the argument or different from the argument – without computing the truth values of the disjuncts. Classically, an exclusive disjunction is true if and only if one of the disjuncts is true. In effect, Deutsch's quantum circuit achieves its speed-up by exploiting the non-Booleanity of quantum properties to compute the truth value of a disjunctive property, without computing the truth values of the disjuncts (representing the association of individual arguments with corresponding function values.)

08-10-01 *Larger, Smaller* (to J. Summhammer)

Thank you for your wonderful, thoughtful, long letter! I have now read it several times, and each time I think I've gotten a little more from it. Thank you also for your concern over my family and associates in light of the September 11 attack: As far as I know, all my friends, and my friends' friends were left unscathed physically. But it is all a very frightening affair, and it is certainly weighing on everyone's life on this continent and the world.

Concerning the content of your letter, let me especially thank you for the large number of YES's you wrote into the margin of my paper! Let me make a couple of small comments on your one NO.

Summhammerism 5: *It appears to me that quantum theory is the correct way of reasoning, and classical probability theory is a certain limit of it. But both spring from the SAME way of reasoning. So far, physics has stood in the way of clarifying this. The perennial talk of systems and properties of systems one is forced to carry along when dealing with quantum theory is a real hindrance to clear thought. Remnants of mass points, forces, fields, etc. always sneak in, and with it the need to allude to an objective world out there. As if repeatably detectable structures in the statistics of probabilistic events and their efficient description weren't objective and "out there" enough (to me mathematical truths and the Himalayas are equally "out there"; the former are mastered by acts of mental climbing, the latter by acts of physical climbing, but both require willful action to be conquered. You call it free-standing reality.)*

This issue has now come up in my email a couple of times since the Sweden meeting. Here's the way I put it to Jeff Bub on the last round:

> The main thing is that it sounded like a good opportunity to pound out the similarities and distinctions between our points of view on quantum mechanics without being interrupted every three minutes.
>
> I know I suggested I would write a longer letter soon, but I'm going to wimp out of it again for now. It would concern the main point of distinction I see between us (and also between myself and Pitowsky). Namely, A) that I view a large part of quantum mechanics as merely classical probability theory (which on my view may be an a priori "law of thought") PLUS an extra assumption narrowing down the characteristics of the phenomena to which we happen to be applying it to at the moment, while B) you are more tempted to view quantum mechanics as a *generalization* of classical probability theory (and with it information theory). I know that my view is not fully consistent yet, especially as I have always distrusted mathematical Platonism—which you pointed out to me I am getting oh so close to—but it still feels more right (to me, of course). Ben Schumacher, Rüdiger Schack, and I had a long discussion on this (on a long walk) the day after the round table, and I'd like to record that too. Ben took a stance quite similar to yours, and maybe even Rüdiger did too (despite his overwhelming Bayesianism). So, I may be the lonely guy out on this. And my view may be subject to change.

To some extent, I can understand both motivations, i.e., to see quantum theory as the larger of the two structures, and alternatively to see it as the smaller of the two. My thoughts are not completely set yet about which direction is the best direction, but let me try to explain a little about what I mean by probability theory possibly being a larger structure than quantum mechanics. (As evidenced in my paper, this is certainly the direction I lean most toward presently.)

Consider some physical system, say my house. And consider some set of questions you might ask about it. For instance, what color is it? (The answers being R, O, Y, G, B, I, V.) Or, what kind of flooring does it have? (The answers being wood, tile, vinyl, or carpet.) On so on: Consider every question you might ask about it. If you were a Bayesian, you would not hesitate taking all the information you know about me and applying it to the construction of a probability function for the outcomes of each such question that could be asked. For instance, if you had gained the impression in Växjö that I am a sentimentalist, you might place a higher probability on my floors being wooden than otherwise.

However it is also part of the Bayesian creed that there is no such thing as an invalid probability assignment. If there are no logical connections between a set of questions, then there are no constraints on the probability assignments I might give for their potential answers. So, for instance, though you might put a peaked distribution on the answer to the question about my floors, you might put a flat distribution on the colors. And so forth, for every elementary question that might be asked about my house.

However, when we come to quantum mechanics something changes about this. Now, the elementary questions correspond to POVMs. But, using Gleason's theorem, we are no longer free to assign probabilities to their outcomes willy-nilly. All but a very few probabilities assignments are tied together via the existence of a density operator. For instance, viewing quantum probabilities as Bayesian probabilities, one is completely free to assign any probabilities one deems relevant to the outcomes of a σ_x, σ_y, and σ_z measurement. However, once that is done, one is no longer free to specify an arbitrary assignment for spin in the n direction, for any other n.

From this point of view then, quantum mechanics allows only a subset of the vastly larger set of probability assignments one might make to the answers of the physical questions one might ask. And one might think that restriction is accounted for by some physical fact—the yet-to-be-discovered fact that is the essence of quantum mechanics.

Summhammerism 6: *Still, it is in this connection that I wrote a NO into your paper. On p. 28 you say "Probability theory alone is too general of a structure.", and at some other place you say there must be an input from nature. Based on my own games with these questions I doubt this. I think quantum theory is already contained in the basic notions that lead to probability theory. The sum rule of probability is no obstacle, if you ponder what "mutually exclusive" means from an operational point of view. For this reason I see a valuable contribution in Lucien Hardy's attempt of starting from a few axioms, although an axiomatic approach is unsatisfactory as long as the axioms aren't simple truths instead of formal assumptions.*

So, indeed, what I said above is what I meant as an input from nature: It is whatever binds us to Gleason's theorem. (Gleason's theorem being the string that ties all the various distributions for a physical system together.)

You'll note actually that Hardy is almost an antithesis to this idea. He starts with structures that are larger than both classical probability theory AND quantum mechanics. By adding an extra postulate he can narrow it down to either one or the other (or any of a number of other structures). What I want is start with classical probability and then narrow it down to quantum mechanics.

It could be the wrong direction, but it is the one that feels predominantly right to me (and the one that seems to me to have the highest probability of leading to some interesting physics). It is a subjective judgment of course, but that's all that each of us has.

By the way, Caves and Schack and I have been thinking about applying for a visit to the Erwin Schrödinger Institute next spring or early next summer. The plan would be to write (the bulk of) an RMP article on all this Bayesian business while we're there. It'd be great to have your ear to test it out on, if we do follow through with the plan.

Johann's Preply, "Quantum Foundations in the Light of QI"

> Now I come to your paper "Quantum Foundations in the Light of Quantum Information". I had gone through it in summer, then practical problems intervened, and now I scanned through it again. I liked it very much, although I tended to skip the formal parts, due to a dislike of density matrices. (The unspoken opinion of the experimentalist is this: If you can't produce a pure state, you are not in control of the experimental conditions. And while it is true that a pure state can never be produced accurately, it can be approached arbitrarily well.)
>
> Let me write down a few of the qualitative statements which captured my interest because I agree with them:
>
> - p.6 Information about what? ... nothing more than the consequences of our experimental interventions into nature ...
> - p.7 The whole structure of quantum mechanics — it is speculated — may be nothing more than the optimal method of reasoning and processing information (in the light of the world's sensitivity to our touch)
> - p.9 The quantum state is subjective incomplete information. (Meanwhile I understand in what sense you, Carl Caves and Rüdiger Schack use 'incomplete' in this context. A kind of respectful bow towards Einsteinian expectations from physics.)
> - p.19 Quantum states are states of knowledge, not states of nature
> - p.22 The uncertainty that decreases in quantum measurement is the uncertainty one expects for the results of any possible future measurements.

- p.26 on maximal state of knowledge. I like the clarification that 'nothing new' is learnt when already having maximal state of knowledge, but that 'mental readjustment' happens. It is in line with the everyday truth that *each* observation tells you something, in other words, that by additional observation your overall information about the world can only increase, never just remain the same. A fundamental theory must reflect this. (I had remarked on this in my letter to Carl Caves.)
- p.28 ... perhaps the better part of QM is just a law of thought
- p.28 ... $|\psi\rangle$ must be information about the potential consequences of our interventions into the world.

From the paper I also got an appreciation of POVMs, which I had always looked at as an unnecessary extension. The hint of getting more information from a two-level system if measured via a three-level ancilla was important.

I did not delve much into what you wrote about Gleason's theorem, although it is probably important for the paper. It appears that contextuality and counterfactuality are surprises only, if one is rooted in classical thought.

I also appreciated the part on why the tensor product of Hilbert spaces (p.29 ff).

The application of Bayesian theory to quantum theory. It appears to me that quantum theory is the correct way of reasoning, and classical probability theory is a certain limit of it. But both spring from the *same* way of reasoning. So far, physics has stood in the way of clarifying this. The perennial talk of systems and properties of systems one is forced to carry along when dealing with quantum theory is a real hindrance to clear thought. Remnants of mass points, forces, fields, etc., always sneak in, and with it the need to allude to an objective world out there. As if repeatably detectable structures in the statistics of probabilistic events and their efficient description weren't objective and "out there" enough. (To me mathematical truths and the Himalayas are equally "out there"; the former are mastered by acts of mental climbing, the latter by acts of physical climbing, but both require willful action to be conquered. You call it free-standing reality.)

Still, it is in this connection that I wrote a NO into your paper. On p.28 you say "Probability theory alone is too general of a structure.", and at some other place you say there must be an input from nature. Based on my own games with these questions I doubt this. I think quantum theory is already contained in the basic notions that lead to probability theory. The sum rule of probability is no obstacle, if you ponder what "mutually exclusive" means from an operational point of view. For this reason I see a valuable contribution in Lucien Hardy's attempt of starting from a few axioms, although an axiomatic approach is unsatisfactory as long as the axioms aren't simple truths instead of formal assumptions.

Quite generally, you seek an understanding of QM on the basis of two or three statements of a crisp physical nature. And these should be about information. In line with what I just said, I share this wish wholeheartedly. Having grown up collecting clicks of individual neutrons I would start with "clicks" as the only point of contact between observer and observed. ("clicks" = outcomes of trials of probabilistic experiments). On the other hand, you want to distill out certain features of the established theory, take them as nature's core message, and attempt an understanding of the whole theory from these.

Here, I hesitate a little. Quantum theory itself is already too remote from the experience from which it was distilled. We may continue marveling at our extracts from nature, and perhaps overlook that they are trivial consequences of the scientific method applied to data which we have decided to view as intrinsically probabilistic. In other words, the theory may indeed just be a law of thought, but straightforwardly so, and not in the sense that we first have to accept its results (nonlocality, interference, etc.) and from these be pointed to laws of thought which make these results self-evident.

In clear moments I wonder why we still have to discuss quantum theory, just as you did in the beginning of the paper, but from a slightly different vantage point. All we have is probabilistic events. Simple events, and coincidence events of arbitrarily high level. And all we can do is calculate random variables from them. What should be difficult about finding the random variables with the highest degree of invariance and the possible, statistically testable relations between such random variables? That will be our structure, and quantum theory will be contained in it. And nature will fit in, or the idea that probability is basic is wrong.

So much for today. The priors-question in Bayesian theory and "How Much Information in the State Vector" I must postpone to another time. Perhaps I will think of a practical question in interferometry.

P.S.: I hope that neither you, nor anybody close to you, have been affected by the recent terrorist attacks.

09-10-01 *Writing Physics* (to N. D. Mermin)

I'll place the new supplement to the Samizdat in the next email as plain text. Please let me know whether you're able to TeX it up fine; if that doesn't work out, I can post it on my web page (as a PostScript file) as I did before.

Collecting it up, it's hard to believe I've written this much in the little time since Växjö. I guess it's been an active time for me. I think there's no doubt that I've gone through a phase transition. For all my Bayesian rhetoric in the last few years, I simply had not realized the immense implications of holding fast to the view that "probabilities are subjective degrees of belief." Of course, one way to look at this revelation is that it is a *reductio ad absurdum* for the whole point of view—and that will certainly be the first thing the critics pick up on. But—you wouldn't have guessed less—I'm starting to view it as a godsend. For with this simple train of logic, one can immediately stamp out the potential reality/objectivity of any of a number of terms that might have clouded our vision.

You'll find the most useful stuff in here starting at my last note to you, i.e., page 78 onward. In particular you might enjoy the chart on page 84. It shows, I think, that when this exercise of epistemologizing so much is over and done with, there's still a fair amount left that one might be willing to call concrete reality.

I do hope you get something out of this. Two of your questions were the sources for the vast majority of the pages in the document. If you hadn't pushed me, I may have never seen that so much was waiting in the wings to be made sense of.

10-10-01 *Lunch Thanks* (to J. N. Butterfield)

Reviewing the day, it dawned on my that I hadn't thanked you for lunch! Thanks so much. I very much enjoyed our conversation, and I have to commend you for giving me a lead: you helped me realize that I really should read the principal principle paper again, and come up with a firm opinion.

I hope you will have a gander at my QFILQI paper: the more criticism you can heap on it the better. I know that I'll certainly come out the stronger for it.

It was wonderful seeing you again. Don't forget to remind me about your particular dates for Princeton: by the then I should know something for real about pragmatism, pro or con. (But knowing me, I'll probably lie somewhere in the middle.)

11-10-01 *Returning Home and Future Plans* (to A. Peres)

Asherism 13 : *Can you imagine Swissair bankrupt? I remember flying Braniff in one of their last flights, but Swissair???*

Computers get exponentially faster at the same time that world change gets exponentially faster. The two phenomena are probably not so untethered to each other!

Asherism 14 : *I sent you my preliminary draft "Imprecise quantum measurements", which nullifies the Meyer-Kent nullification of the KS theorem. I got a few comments, from Appleby, Cabello and Mermin.*

I apologize, but I didn't have a chance to look at your manuscript. (I'll also apologize for the format I'm using to reply to your present letter! When time slips away, so does my good composition sometimes!) Also, I figured with your large distribution list, you would get some good comments in any case. How were Appleby's comments? I met him for the first time in Ireland, and he struck me as a very clear thinker there. But I have not read any of his papers.

Asherism 15 : *Soon there will be the deadline that Khrennikov gave for his book about the Växjö conference. I talked there about my work with Petra, and this is well documented in two PRL and a forthcoming JMO. No need of repeating that. So I thought that an extended version of my "Imprecise quantum measurements" could be a nice chapter in such a book. What is your opinion?*

I think that's an excellent idea.

16-10-01 *Craters on the Moon* (to J. A. Waskan)

In a way you stole me away from the family tonight; the little ride home was full of thoughts about craters on the moon. You said something like, "That there are a thousand craters on the dark side of the moon, is a true statement regardless of whether it's useful or not."

Below is a passage I took from David Darling. It once made a good impression on me (long before my James days), and I can't help but feel that it is relevant to tonight's conversation too. If all the world is but atom and void—or substitute your favorite metaphysic, for that matter—then, it seems to me, there is no strict sense in which there are "craters" on the moon at all. To interpret the coarse information I have about some aspect of my experience as a map of the craters of the moon, seems to me an ultimately subjective judgment—one that I make because it is more or less useful.

Of course, I'm not wedded to these ideas: the game I play is to pick and choose anything from any philosophy that will help me make sense of the physics I'm doing and to promote it to a new level. Sometimes, after a sufficient amount of play, I change my philosophical mind. But I think the observation below is not a completely idiotic one, and it sends me some way toward the pragmatic conception of truth. No proposition I use in my daily life can be strictly true or false in the sense of reflecting the world as it is independently of me. And if not the case in my daily life, then why should it be the case at some more "ultimate" level (i.e., fundamental physics) that—after all is said and done—was intellectually derived from that daily experience in the first place?

> The interface between mathematics and everyday reality appears sharp and immediate at this point: one sheep, one finger, one token; another sheep, another finger, another token, and you can take away tokens or add them, as you can with your fingers. The tokens—the numbers—are just abstracted fingers; the operations for dealing with the tokens are just the abstracted raising or lowering of the fingers. You make a one-to-one correspondence between the tokens and whatever it is you want to reckon, and then forget about the fingers.
>
> At first, it seems clear from this that mathematics must be somehow already "out there," waiting to be discovered, like the grain of the stone. One sheep add one sheep makes two sheep. Two sheep add two sheep makes four sheep. That is certainly the practical end of the matter as far as the shepherd and the merchant are concerned. But already, even in this most simple mathematical maneuver, something strange has happened. In saying "one sheep add one sheep" we seem to be implying that any two sheep will always be identical. But that is never the case. Physically, the first sheep is never exactly equal to the second: it may be a different size, have different markings. It takes only one molecule to be out of place between the two, and they are not identical. Indeed, because they are in different places they are inevitably not the same on that basis alone. We have extracted a perceived quality to do with the sheep—namely, their "oneness," their apartness—and then merged this quality by means of another abstraction—the process of addition. What does it mean, physically, to "add" things? To put them together? But then what is "putting together" two sheep? Placing side by side, in the same field—what?
>
> All this may seem like nit-picking. But on the contrary, it brings us back to the central mystery—the relationship between the inner and the outer, the world of the rational mind and the world "out there." In the physical world, no two sheep are alike. But, more fundamentally, *there are no "sheep."* There are only some signals reaching the senses, which the left brain combines and then projects as the illusion of a solid, relatively permanent thing we call a sheep.
>
> Like all objects, sheep are fictions: chimeras of the mind. It is our left hemispheres, having through natural selection evolved this skill for extracting survival-related pieces of the pattern, that trick us into seeing sheep, trees, human beings, and all the rest of our neatly compartmentalized world. We seek out stability with our reasoning consciousness, and ignore flux. We shut our eyes to the continuous succession of events if those events seem not to substantially affect the integrity of what we see. So, through this classifying and simplifying approach we make sections through the stream of change, and we call these sections "things." And yet a sheep is not a sheep. It is a temporary aggregation of subatomic particles in constant motion—particles which were once scattered across an interstellar cloud, and each of which remains within the process that is the sheep for only a brief period of time. That is the actual, irrefutable case.

17-10-01 *Quick Single Point* (to J. A. Waskan)

Thanks for the note: I'm still digesting it. But let me quickly reply to the one thing that I can reply to.

Waskanism 1 : *Also, it seems strange for the fellow you quote to concede that there is light, eyes, the left half of one's brain, a process known as natural selection, and at the same time to deny the existence of sheep.*

Indeed, it seems strange to me too: He's pretty clearly not being consistent. But the role of the quote for me was as a motivating piece. (I read Darling a few years ago, and the main reason I used the quote last night was because it was already in my computer and I could, thus, send off a quick note to you.) My sentences above the quote were meant to show that I toy with the idea of going a further (more consistent) step:

> And if not the case in my daily life, then why should it be the case at some more "ultimate" level (i.e., fundamental physics) that—after all is said and done—was intellectually derived from that daily experience in the first place?

It is going that extra step that seems to me to be heading down the track to Jamesianism. Let me read your note again ...

17-10-01 *Quick Second Point* (to J. A. Waskan)

Sorry, I can't wait for the beer.

Waskanism 2 : *Here's a daily affirmation for you. Look in the mirror (not Rorty's mirror of nature, the one in your house), and say the following:*

I am pretty darned sure that I exist. I think there is other stuff in the universe too. I'm pretty darned sure that I have beliefs about (i.e., representations of) the other stuff that might be out there. My beliefs are true insofar as the world is how I represent it to be. If the world outside of my mind is in no way how I represent it as being (e.g., IF THERE ARE NO SHEEP, etc.), then all of my beliefs are false. Even if all of my beliefs about the things outside of my mind are false, this should have no bearing on the nature of truth itself (i.e., correspondence).

If you want to use true and false that way, then—in my present state of mind—yes, I would say that, strictly speaking, most all my beliefs are false. Beliefs play the role of coordinating our actions, and, in that way, can be more or less useful. But (in the too small of thought I've given this) I can't find a role for the concept of belief outside its use.

Like you, I am pretty darned sure of the existence of a world outside myself. But I would say that that surety comes NOT from some (transcendental?) knowledge that my beliefs mirror that world as it is. It is just the opposite. I believe in a real world outside myself because, throughout my life, things continue to take me by surprise. Significant numbers of my beliefs are systematically INvalidated with each new day. There's my evidence of the real world. Below is the way I put this point in a recent paper.

The point of view is not completely worked out yet—and it may never be—but my experience in quantum mechanics makes it feel more right than the other options I've seen so far.

Knowing me, I'll probably give your note another read, and be back again tomorrow. I hope you don't lose patience with me.

17-10-01 *Quick Third Point* (to J. A. Waskan)

OK, one more for the day.

Waskanism 3: *Also, sure no two sheep are exactly alike. Neither are any two bachelors. That doesn't entail that there are no bachelors. X is a bachelor if X is unmarried, and X is male, and X is eligible (e.g., not a priest).*

Granted. But what I thought was at issue is whether there are "bachelors" without the agents who make up (and use) all these judgmental categories. If all the world is BUT atom and void—to use an allegory I like but which should not be taken literally as my view—then where do all these extra distinctions come from if not the judgmental agent?

I say I thought this was the issue because my reading of the pragmatic conception of truth is more to the following point: Without agents, there are no "propositions." Therefore "propositions" cannot be true or false in any absolute sense. Without the agent, there is the world, and it is just whatever it is. A proposition adds something to the world that it itself did not possess before the agent's attention was drawn to it (via the act of dreaming it up, writing it down, acting upon it, etc.).

There's probably nothing worse than to have an armchair philosopher in your presence ...

17-10-01 *Final Point* (to J. A. Waskan)

I just read my last note over again and, to my great fear, I discovered you might read my closing two ways!!

> There's probably nothing worse than to have an armchair philosopher in your presence ...

I hope you understood that I was calling myself the armchair philosopher and not being a complete jerk.

18-10-01 *No Doobies Here?* (to J. A. Waskan)

Waskanism 4: *Lots of folks want to say that there are joints in the natural order (though, admittedly, bachelorhood probably isn't one of those joints).*

I'm not sure what you mean by the term "joints." Can you define it precisely? (Not knowing what you mean, it leaves me unable to reply to most of your message.)

Waskanism 5: *If, however, you take a step back and look at the big picture, ... I suspect that the same can be said for the relationship between atoms and tables.*

You know what my worry is (fueled by the 75 year debate on the interpretation of quantum mechanics). It's that we just can't step far enough back. We are immersed in this thing called existence, and there's just no way to get a view from outside it. We do the best we can from the inside, and that's called science. (For me, the phrase "best we can" means to eliminate impredictability—i.e., (only half jokingly) to delete reality as much as possible. Cf. yesterday's note about Emma.)

Tell me what a joint is, so I can think a little more about what you said.

21-10-01 *Danny, Dewey, David, Delight, and Dilemmas* (to A. Peres)

Thanks for the note; I'm glad to hear about Danny's opportunities.

I suppose because of the war and a couple of other details closer to home, I've gone into a bit of an email slump for the last couple of weeks. I am so far behind with all my correspondents. At least in my spare time I have been motivated to read more philosophy. And indeed I have come across a very good strain of that for once: I've been absolutely taken away by the writings of William James and John Dewey.

I have written David Mermin about our scheme for the ITP, but I have heard no reply yet. But he is enjoying the beauties of Vienna, so in a way, I expected no less.

22-10-01 *Sad and Happy* (to R. Cleve)

Of course, I'm sad to hear that we couldn't entice you to New Jersey ... but, really, I expected as much. Please do keep us in mind though in case anything ever changes.

On the other hand, I was really happy to hear that you liked the little sound bite in *Discover Mag.* I expanded those vague thoughts into a (slightly less vague) paper: `quant-ph/0106166`, "Quantum Foundations in the Light of Quantum Information". There is a small technical mistake in there—that I'm presently writing a comment on—but on the whole, I think the paper is relatively sound, and maybe entertaining. I'd enjoy hearing any reaction you might have, and more importantly, any ideas you might have about how to proceed on the program posed there.

22-10-01 *The Dilemmas of Subjectivism* (to R. Schack & C. M. Caves)

I apologize for holding off so long in a reply to Rüdiger's letter concerning the RMP article. The difficulty has come in that I didn't know how to reply. (I guess I still don't.)

The point of some potential consternation is this:

Schackcosm 43: *What I think we should be doing is a paper on "Interpretations of probability in quantum mechanics (with special emphasis on the Bayesian viewpoint)". The paper would NOT be on the interpretation of quantum mechanics.*

The problem is, I don't see how to separate the two issues. Where does the interpretation of probability fall off and the interpretation of quantum mechanics kick in? How can one have an interpretation of quantum mechanics wherein the wavefunction is objective, but still think of probabilities as being strictly subjective? Similarly, vice versa?

Let me do this: Let me ship to you both the mini-samizdat of my thoughts that came out of my post-Växjö broodings. You tell me which *sentiments* will be banned and which won't if we end up skinny-dipping together. I can foresee some being excluded—like the stuff in my letter to Wootters—and I can accept that; but for the greater majority of the writings, I can't see myself drawing a line, and I'm wondering where you will draw it.

Looking back on the BFM debate, I think the most important thing to come out of it for us three in particular is that it makes it absolutely clear that we need to get our thoughts straight on the "principal principle" before we can embark on a consistent statement of our position. For I see no way to erase the dilemma: Either we accept that the ascription of one Kraus rule over another in a measurement intervention is a subjective judgment, or we accept a quantumatized version of

Lewis's principal principle. Why did we all reject the principle before if we find ourselves accepting it now? This is something we ought to reflect upon more deeply.

23-10-01 *Increased Surveillance* (to N. D. Mermin)

My spies tell me you gave a talk on "Whose Knowledge?" yesterday. How did it go? Did anyone in the audience (unintentionally) lobby any shells on my behalf? If so, hand over their names, and I'll put out a recruiting effort for them.

23-10-01 *United We Stand Airlines* (to R. Schack)

Schackcosm 44: *Do you think you could give me a lift at such an early time?*

I do whatever it takes for the greater good of quantum mechanics. Of course I can give you a lift! (If you can stand to listen to me babble that early in the morning.)

26-10-01 *The Feynman Cult* (to J. N. Butterfield)

I don't remember railing against the Feynman cult in your presence, but your letter gives some evidence that I must have. (Doing such things was a common pastime for me at Caltech, but I've mellowed a little in my efforts since leaving, i.e., since the cult hasn't been in my face on a daily basis.)

But, anyway, yes I told Brandt that I would come. (I hope you will come too.) I've even thought about how I will open my talk: with the Feynman quote below. Lord knows I'm no materialist, so you can rest assured that I'll do my best to "zing" it up afterward. (See my Samizdat, page 237.)

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis (or the atomic fact) that all things are made of atoms—little particles that move around in perpetual motion, attracting each other when they are a little distance apart, but repelling upon being squeezed into one another.
>
> Everything is made of atoms. That is the key hypothesis.

27-10-01 *Coming to Agreement* (to C. M. Caves & R. Schack)

Schackcosm 45: *In any case, do you know a reference for strong Dutch-book consistency?*

Nothing comes to mind: I think I first learned of it from you (was it in Cambridge?). Might it have been in either of those papers we looked up in a trip to the university library one day? This was one of them (though I don't have it anymore):

> J. G. Kemeny, "Fair Bets and Inductive Probabilities," J. Symb. Logic **20**, 263–273 (1955).

Ahh, here was the other one:

> R. S. Lehman, "On Confirmation and Rational Betting," J. Symb. Logic **20**, 251–262 (1955).

It might be a good idea to dig those up again.

I'll try to answer your other questions next week, after I get a chance to get a better grasp on the paper.

27-10-01 *Literature* (to R. Schack)

Schackcosm 46: *A question to you: How much literature is there on interpretations of probability in q.m.? Would it be feasible to review it all?*

Attached is everything I had collected up previous to the Cerro Grande fire. A skim of that document might answer your question in the most direct way.

By the way, I've asked Maria Carla Galavotti for her criticisms (and any others that she knows) of the principal principle. But I haven't gotten a reply from her yet.

Thinking back on it, I can't remember if we ever had any NONquantum reasons that were substantially different than J. S. Mill's argument against a "substance" underlying the phenomenal world:

> If there be such a *substratum*, suppose it at this instant miraculously annihilated, and let the sensations continue to occur in the same order, and how would the *substratum* be missed? By what signs should we be able to discover that its existence had terminated? Should we not have as much reason to believe that it still existed as we now have? And if we should not then be warranted in believing it, how can we be so now?

Similarly one could say of objective probability: Bayesian coming-to-agreement would work precisely the same whether the objective probability is there or not. But did we have any other arguments than that (that did not depend on upon quantum mechanics, for instance in the nonuniqueness of the density operator decomposition)? Did we have any examples where believing in objective probability in Lewis's sense would be downright misleading in how it might suggest tackling a practical problem?

I so wish I had my old file cabinet back again; writing you this morning has made that feeling more acute.

01-11-01 *Your Limerick* (to I. Duck)

A couple of days ago I opened up a package of old mail that Los Alamos had finally forwarded to me. To my great delight, I found your letter and your limerick from February!!! Thank you so much; this is a very great honor. After reading your limerick I think I smiled all day.

I wish so that I had gotten your mail before May. I would have then been able to include (with your permission) your limerick in my large samizdat posted on the web at:

`http://xxx.lanl.gov/abs/quant-ph/0105039`

It would have been a great addition. And I think you are right: "the best we can ever surmise" catches the situation quite precisely.

Thank you again.

01-11-01 *Delayed Reactions* (to A. Peres)

Two days ago, I received a package of old mail that was finally forwarded from Los Alamos. Some of it was very old indeed, going back to November 2000. In any case, two pieces were relevant for our *Physics Today* articles. One was from someone named Fritz Froehner, and it looks like he sent an identical copy to you. But the far more interesting one was from a professor at Rice University (where, incidentally, Carl Caves got his undergraduate degree).

I scanned the letter into my computer this morning, and will put the whole thing below. I have just written him thanking him for the warm feelings he gave me.

> Dear Dr Fuchs:
>
> I am sending you a copy of a limerick which was prompted by your OPINION article with Asher Peres in MARCH 2000 PHYSICS TODAY. I hope it amuses you, and that it catches the gist of your 'No Interpretation' thesis. It is a fascinating subject which I believe to be completely rationalized by your point of view. I'm sure you have thought about this much more deeply than I. For instance, I just now realized that one does not have to invoke anything more miraculous than uninterrupted Hamiltonian evolution for the system. I had been thinking that it was a mystery that we should have gone from the mechanics of an inanimate object to a theory of knowing; but that is not what is happening. I think 'the best we can ever surmise' catches the situation quite precisely.
>
> I tried to interest Alan Alda in *Scientific American* with this, but I missed the deadline for his limerick contest and I don't think they ever forwarded it to him.
>
> With best regards,
>
> Ian Duck, Professor of Physics
>
> LIMERICK on the INTERPRETATION OF QUANTUM MECHANICS
>
> ON SCHROEDINGER'S CAT
>
> It comes as a total surprise
> That what we learn from the ψ's
>   Not the fate of the cat
>   But related to that:
> The best we can ever surmise.
>
> IAN DUCK, FEB 2001

02-11-01 *Positive Operators and Measurement* (to K. Jacobs)

Thanks for including me in the correspondence with Keiji Matsumoto. Did his question arise from having read our paper? (That would mean we might have 3 or 4 readers!!) I was supposed to visit his institute in Japan last month, but I wimped out of leaving the country. (In any case, I had already flown overseas four times this year ... and five seemed a bit too much.)

Let me make one comment on your points about the "identity" (as the unitary in the polar decomposition) giving a natural analog to Bayes' rule. Have a look at Section 5 in my paper: `http://xxx.lanl.gov/abs/quant-ph/0106166`. What I show there is that by appropriately adjusting the various U_i (but not to the identity), one can make a precise analog to Bayes' rule even in the noncommuting case. Of course, it's a trivial point that we probably both understood while

I was at LANL. What's new though, is that I've now come to think it is an important conceptual point.

04-11-01 *Dreams of an Ever-Evolving Theory* (to A. Peres)

Asherism 16: *I am reading a wonderful book "Dreams of a Final Theory" by Steve Weinberg. Chapter 7 is "Against Philosophy" and I highly recommend it. I got that book for $7.00+tax in a used books shop in Santa Barbara, where Aviva was looking for something else.*

Thank you for the tip; I did read Weinberg's chapter 7. He writes in a very crisp and no nonsense way, and I like that. (You know I constantly fight tendencies in the opposite direction in my own writing.) His points are well taken, especially the ones about how a preset philosophy can create immense blinders for the scientist:

> "... in rejecting it the [PHILOSOPHY-X]ists were making the worst sort of mistake a scientist can make: not recognizing success when it happens." — page 177, paperback version.

But, I think a deeper point is the one he makes near the beginning of the essay:

> "I do not want to draw the lesson here that physics is best done without preconceptions. At any one moment there are so many things that might be done, so many accepted principles that might be challenged, that without some guidance from our preconceptions one could do nothing at all." — page 167

And I agree with the numerical tally of the next sentence:

> "It is just that philosophical principles have not generally provided us with the right preconceptions." — page 167

However, I part company with him in thinking that that is a strong argument against pursuing philosophy as a sideline to science. That is, I don't know what the preconceptions can be if they're not philosophies.

When it comes to philosophies and, not unrelated to that, scientific research directions, I tend to take a lot of stock in a Darwinian kind of conception. That is, we each should do precisely what we feel compelled to do; we each should research precisely what we feel compelled to research. There's probably nothing we can do about it anyway. Indeed ninety-nine percent of the time we will be on the wrong track: The world supplies a selection pressure for our thoughts, just as it does for the lifespan of the drosophila. And just as it is not possible for the drosophila to change its genetic makeup before it meets its demise, I think the only thing we can do as scientists is cultivate to the best of our ability the philosophical preconceptions that led us down our own paths. Ninety-nine percent of us will be forgotten from the history books, but the ones of us that remain will do so because the world is such that it is less likely that we should fall.

For myself, I have DISCOVERED that I have chosen a direction of thought that is very closely aligned with the philosophical movement of pragmatism from the early part of the 20th century—a movement the details of which have been nearly forgotten in modern times. Interestingly, the thing that set me on to this realization was Martin Gardner's essay "Why I Am Not a Pragmatist" in his book *The Whys of a Philosophical Scrivener*. (You probably remember Gardner from his column in *Scientific American*.) This happened about three months ago. I really recommend you read the article if you get a chance. Maybe your library has a copy of the book. I think in reading it, you will discover that the essay might just as well have been titled, "Why I Am Not in Agreement with Fuchs and Peres's Physics Today Article." For, with each reason Gardner used to explain why he was *not* a pragmatist, I found myself thinking of quantum mechanics and saying to myself, "ahh, I guess that means I *am* a pragmatist." Really, the analogy is *that* close even though the article has nothing to do with quantum mechanics per se.

The issue is no less than whether "unperformed measurements have no outcomes." The pragmatists, for various reasons, thought it was *safer* to assume that they didn't. The movement then spent the greater part of its time developing the (liberating) consequences of this supposition. Gardner (and Bertrand Russell and G. E. Moore and gazillions of others) thought "how silly" and "how contrived" when it is so much easier to use standard realist language to describe the outcomes of experiments—to assume the outcomes are there before one has a look. But you and I know better, of course. And, I think it is quite useful to know that there was a set of people carrying through the detailed consequences of this line of thought for their broader worldview long before you and I were on the scene. The way I view it, these old thoughts can be a resource to our explorations of quantum mechanics just as much as any other: However, their use is in setting the directions for potentially fruitful lines of thought ... but that should always be the use of any philosophy for any scientist.

Below I'll attach a letter I wrote Bill Wootters a while ago on a similar subject. [See 20-09-01 note "Praise, Folly, Enthusiasm" to W. K. Wootters.] What I write just after the quote of William James better explains why I chose the title that I did for this note.

14-11-01 *Samizdat II* (to N. D. Mermin)

Merminition 76: *Expanding slightly on my reaction to Samizdat II: (Please don't conclude that I've missed the point yet again until we meet):*

It seems to me that all interpretations of QM have to come up against what is loosely called the "measurement problem" in one form or another, which I would describe as how we can reconcile a world empty of "facts" to the "facts" of our own experience. (I would not describe it as having to do with how wave-functions "collapse" or "decohere".)

Various people (at least those who don't want to decree that there is a "cut" between two domains) deal with the problem by extending the facts of our experience to everything else (Bohmians, GRW collapsists). Others deal with it by extending the indefiniteness of QM to our apparently definite experience (many worlders). It seems to me you're following the second strategy (which is all I meant by my irritating jokes about your Everettism — your version of the second strategy is obviously different from theirs) by insisting that the subjective character of quantum probabilities requires us to take the same subjective approach to classical "facts" — i.e. to insist that they too are beliefs that can also be dealt with only through a (subjective) probabilistic treatment.

This is intriguing and well worth exploring. I do worry (with Carl, I think) that it's getting rather far from how physicists do physics – or at least from how they think they do physics. But I wouldn't say it means the end of science. Whatever that means.

Thanks for the extended comments. I won't say you've missed the point: I think you've got it. But I don't quite understand the Everett analogy yet. I would say their world—Deutsch's world—abounds with facts. Facts far, far in excess of what any of us ever see. (I.e., all their worlds.) But I'll think much harder about your note. There's no need to reply yet again.

16-11-01 *Great Quote* (to J. W. Nicholson)

Now this is one I love! Which supersedes which? Christianity or humanity? I didn't realize the two notions could be considered so opposed.

> "It was very dramatic, right until the end," Taubmann said at a news conference Thursday night. He was wearing a new pair of pants that still sported the store tags, and he had sheared the scraggly beard and long hair he had when he had arrived in Islamabad.
>
> "I am a Christian — I have forgiven them [the Taliban] for what they have done," he said. "But as a human being, I hate what they did to us."

19-11-01 *A Lot of the Same* (to C. M. Caves & R. Schack)

Schackcosm 47: *Before you have me burned alive, please tell me why I am wrong!*

Come on, you know it's the Thanksgiving season. I would never burn you, only roast you. (Though my brother-in-law once fried his turkey in hot oil.)

Sorry for the hiatus, but I just got inundated with email last week, and I didn't have the proper mentality for replying to any of it. So I shut down for a while. Now I'm stuck with trying to clean out an even bigger pile of old mail. But let me compliment you by letting you know that I'm tackling your letter first! (It's the only interesting one in the lot.)

Schackcosm 48: *I started writing up a summary of our discussions, and hit upon a difficulty when I tried to formulate exchangeability for models. Here is the problem.*

In the traditional formulation of exchangeability, we say that we have N identical systems (same Hilbert space). At this stage, it is thinkable to assign a different state to each system. We then make the judgement that the state of the N systems is exchangeable.

For models, we say that we have N identical apparatuses. Alternatively, we say that we use the same apparatus to perform N measurements, let's say on different, independently prepared systems. At this stage, I can't think of a good reason to even consider assigning different models to the apparatuses. It seems to me that one is forced to say that each apparatus performs the same operation, so should be described by the same model. That leads immediately to a heresy: there exists a true model. If we don't know it, we assign probabilities to models. What we wrote down on your whiteboard is consistent with this viewpoint. The difficulty we encountered formulating exchangeability could mean that it is an unnatural concept in this case. Writing down a mixture of N-fold products of models is completely natural however.

Let me try to allay your fears. I think the issues here are almost precisely the SAME as they are in our old de Finetti considerations. To say it in a way that maybe Carl would endorse, "It's really all about learning." Or in a way that I'm more tempted to these days, it's all about demonstrating a willingness to update one's beliefs—one's commitments, one's pragmatic strategies for action, one's betting behavior—in the light of factual data.

Let me start with an example that's essentially already well-worn for us by now. Suppose we have a rather complicated quantum measurement device whose manufacturer purports it to be the best σ_z measurement device ever built. Furthermore, suppose we have a fresh supply of 10^8 calcium atoms, all meticulously prepared to have spin-up in the x-direction. What do we expect to happen if we individually dump all the atoms into the measuring device? We expect about 50% of them to get registered as spin down and about 50% of them to get registered as spin-up. But what happens if one after another, all the registrations are of the spin-up variety? Well, that outcome sequence would be no less likely or no more likely than any other outcome sequence if we walked into the laboratory with such a radically adamant prior belief. In a real-life situation, however, we would be shocked; we would update our beliefs accordingly—for we would have allowed for the possibility of "learning."

But in this situation, notice that there are at least two extreme cases to which we could attach the possibility of learning. The learning could be about the device or it could be about the preparations. Who's to say that the learning is about something more objective in the one case than the other? Prosaically, it takes both ingredients (the preparations and the device) to certify the device, and you can't get away from that.

Let me try to tighten this up by sketching how we ought to start thinking about a de Finetti theorem for unknown quantum models. I run a measurement device on N independently and identically prepared quantum systems. Suppose I am absolutely confident of these preparations—i.e., with respect to them, there is nothing left to learn in the technical sense of i.i.d. statistics for any repeated and KNOWN measurement. Then, what can it mean—from a Bayesian point of view—that the measurement device works according to an unknown model? It means that after all the outcomes are gathered, there's still something left to be learned from the posteriori quantum state for the systems that were measured.

That is, more simply, the best judgment we can make about the systems that passed through the measurement device is that they are exchangeable CONDITIONED on the registered measurement outcomes. For instance, suppose the device spits out an index i at each round. The issue is, what pragmatic meaning should we give to each such i? Quantum mechanically, the predictive meaning of an index i is specified by the Kraus operator A_i we associate with that outcome. (Its retrodictive meaning is given by the positive part of A_i—the POVM.)

If we think we don't exactly know what the device is doing to each individual system, then we shouldn't yet dare to make an association $i \to A_i$. (To make an extreme point of it, for all we know, the device might be entangling all our test systems.) We should just rest confident that no matter what order we send the systems through the device, we will end up with the same subjective beliefs in the end. Thus, if we gather up all the systems for which an outcome i occurred (as opposed to some other outcome j), then the subjective density operator we assign should be exchangeable. Using the standard quantum de Finetti theorem, we then get that that density operator must be of de Finetti form. Writing each of the final density operators as a linear map acting on the initial density operator, we (should) get something like the desired theorem for unknown quantum models. If we believe that we can learn something about the model, then the probability distribution that appears in the de Finetti form is restricted to being something other than a δ-function.

In summary, our belief that the best we can say of the outputs is that they ought to be exchangeable (conditioned on the factual outcomes), leads directly to a notion of mixture of models—i.e., that the output density operator is controlled either by a Kraus operator A_i, or a Kraus operator B_i, etc., etc., about which we capture our ignorance through some subjective probability distribution.

Now, just as the regular de Finetti theorem cannot put an end to the principal principle, we cannot use this (proposed) theorem to put a stake through the heart of the true believer of objective quantum models. That is, David Lewis might say of the regular de Finetti theorem, "That is a very nice theorem, but it doesn't change the fact that there really is always a 'man in the box.' His name is God." And so he would probably also say of our quantum models (if he knew quantum mechanics). Instead, all the (proposed) theorem can do is show that it is *possible* to get by without a man in the box. We don't need him; all we need is something like the judgment of exchangeability for the outputs (conditioned on the outcomes) along with i.i.d. on the inputs.

So that's the sketch. Now, how to dot the i's and cross the t's? I can foresee at least one difficulty that I'm not clear-headed about right now. That is, using the description above, for each index i, we will generate a probability distribution over models. But by what regularity condition can we assure that $p_i(\mathcal{A}) = p_j(\mathcal{A})$? What I mean by this notation is that $\mathcal{A}$ stands for the model in total (i.e., all the Kraus operators in it) and $p_i(\mathcal{A})$ stands for the probability distribution in the de Finetti theorem derived for each index i. It is probably so simple as this: If we were to imagine doing tomography on the posterior states for each index, then the states derived from that should always average up to a valid density operator. But I'm not exactly sure how to put that idea into action.

Oh, and here's another intriguing point that ought to be explored. Suppose we focus our attention on a given exchangeable density operator. There are many ways that operator could arise, but suppose that it came about as the posterior state arising from many identical measurements (in the sense above). The question I have in mind is how much freedom do we have for trading off between an unknown preparation and an unknown model for getting to the final state? Can one always find a fixed initial preparation and a mixture of models that will give rise to the final exchangeable state? Can one always find a mixed model and a mixture of initial preparations? Probably yes and yes, but I'm not completely sure.

I'm so glad to hear that you may have reversed your opinion on William James (at least a little). By the way, I hope you notice how these de Finetti considerations are drawing out a lot of the considerations I was trying to express to you in Samizdat-II and our subsequent discussions. For it helps draw the distinction between the amorphous index i that arises in a quantum measurement and the meaning A_i that I ascribe to that event. The symbol A_i plays the role of a proposition that I write about i: It carries the information about how I will react after having seen it, how I will place my bets.

I'm willing to believe this whole debate about "truth" might be a red herring—i.e., that we might easily be able to get away with never uttering the word. But I think the realization of the last paragraph had a primitive expression in James's worries about "truth" nevertheless, and to that extent maybe he and his movement of pragmatism are worth contemplating (though of course not subscribed to in toto).

19-11-01 *William James* (to R. Schack)

Schackcosm 49: *You will be pleased to learn that I bought a copy of "Pragmatism" and enjoy reading it a lot! His rhetoric is the best I have ever seen from a philosopher. He is definitely not tender minded.*

It's a curious mix, though. Sometimes he seems to be very close to Kantian ideas on truth, then he seems to subscribe to a naive correspondence theory of truth, at least for simple facts such as "this detector has clicked."

Yes, I'm more attuned to that now, and I'm trying to get it straight through extended readings (such as A. J. Ayer's book *The Origins of Pragmatism* that I picked up in New York City with you).

Schackcosm 50: *I am also a little disturbed by his praise for Ostwald.*

I don't remember his praise for Ostwald. Who was Ostwald? And what does James praise him about?

Schackcosm 51: *I am half way through writing a much improved draft of our paper.*

Sounds great.

20-11-01 *A Bathtub Moment* (to C. M. Caves & R. Schack)

I'm sure I've already told you both the story of the time I happened to end up at a British pub with Caroline Thompson, the famous Bell-inequality conspiracy theorist, but let me repeat it for the purpose of having it in this box. Somebody at the table was speaking of the great importance of intuition, of being able to "see beauty in a theory." I, with my usual example, piped up that I thought that was hogwash: I always thought Mary Ann was the prettiest girl on Gilligan's Island; my best friend thought it was Ginger. Anyway, I followed that comment with, "I never use any intuition in my calculations; I don't even know what intuition can be in that context." Caroline Thompson harrumphed, "Well we could see that from your talk!"

OK, so it won't be an intuition, but here's a hunch that hit me while I was taking a shower this morning. It's connected to the long note on de Finetti I sent you yesterday.

Among the thirty other reasons I have been thinking that trace-preserving quantum operations (and now measurement models explicitly) are subjective entities is because one can make a one-to-one correspondence between them and the density operators on a larger Hilbert space. That is, they have the same structure as the states of belief that we've already toyed with.[16] This has suggested to me that there ought to be a Gleason-like theorem for quantum operations (which I pursued a little bit but never could quite make things connect). But now this idea is rearing its head again in the context of a de Finetti theorem for models.

Yesterday, I blithely said something to the following effect. To get at a concept of an unknown model, what you do is 1) to 4) :

1. Start with many copies of a quantum system, for which you believe 1) that they are exchangeable and 2) that there is nothing left to learn. The standard quantum de Finetti theorem then gives us that the density operator we ascribe to the collective system will be a tensor product of identical quantum states.

2. Now drop each of those systems into a measurement device and note the outcomes i. Separate the post-measurement systems into bins according to those outcomes.

3. Finally suppose we believe that the quantum state we ascribe to each bin ought to be an exchangeable state for which we *can* learn something. The standard de Finetti theorem gives that this state must be of de Finetti form (with a nontrivial support).

4. The hunch was that the conjunction of 1), 2), and 3)—or them along with some minor additional regularity condition—would specify the content of the phrase "an *unknown* quantum measurement model." The unknown model is simply given by making explicit the form of the linear maps connecting 1) to 3) for all possible inputs into A.

Notice that nowhere in there did I say anything about these maps being completely positive. I just chose the word "linear" for some reason. But surely the assumption of complete positivity must come into this too. So now my question: UNDER THE ASSUMPTION that a measurement model is a state of belief, can one adequately explain the notion of an unknown measurement model by de Finetti techniques WITHOUT the technical assumption that these linear maps are completely positive? Or, instead, is complete positivity absolutely crucial to the program?

[16]By the way, Carl, as I recall, was never happy with thinking this point had any significance for our program. I think he saw it as little more than a coincidence.

It strikes me that Carl's superoperator calculus has got to be the way to go for exploring this question.

20-11-01 *James's Loose Lips* (to R. Schack)

Schackcosm 52: *In lecture 2, James says "I found a few years ago that Ostwald, the illustrious Leipzig chemist, had been making perfectly distinct use of the principle of pragmatism [...]"*

In my own words, Ostwald rejected as meaningless any statement that did not have observable consequences. A very pragmatic attitude indeed. The trouble was that Boltzmann's ideas about atoms fell into this category, at least that was the public opinion, led by Ostwald, at the time. No cash value in the "atom hypothesis". I imagine somebody like Ignacio (I pick him only because of his obvious scepticism at my talk in Benasque) to ask: where is the cash value in the Bayesian approach to quantum mechanics?

I agree with you now that that is a troubling praise coming from James. I noticed similarly somewhere else in the book that he also classified Mach in the ranks of the pragmatists.

But there is a grave distinction between positivism (Mach, Ostwald, etc.) and pragmatism (James, Dewey, etc.) as I see it. The positivists eschewed all metaphysical assumptions—thus the egg on their faces for not coming up to speed on the statistical mechanics an atomic hypothesis can afford. The pragmatists, on the other hand, are willing to glorify any metaphysics with a cash value. This relates to the passage by James on Mill that I read you while you were visiting. With metaphysics, the cash value is not in its explanation of any previously discovered facts, but in the concrete actions its BELIEF will give rise to in the agent believing it. Thus James's argument, for instance, for everyone's right to believe in a God, even if that God will never have the opportunity of being confirmed or falsified in an objective fashion. A God's validity in an agent is in his cash value for the agent's ethics, morals, and mode of action for his daily life.

To put this in concrete terms for Carl versus me: I would say that the ontological hypothesis I'm shooting for will show some cash value in the amount of interesting physics it leads to, to the opening up of new quantum computing and quantum control and quantum cryptography methods. And I think (or, more accurately, BELIEVE) it will help us make the leap to the next stage of physics. Whereas I would say—but it's just a gut feeling—that the ontology Carl has been shooting for (i.e., the Hamiltonian) has no such cash value. Only the money flow in the banks will ultimately tell. (And unfortunately, that can only be done with hindsight.)

James—I think in his essay "The Sentiment of Rationality"—has a beautifully worded passage on these considerations that I'll try to get scanned in tonight and sent to you. But I think James himself is either not consistent in his writings, or he's pretty sloppy in reading the other writers he wants to praise. (What I know of him now, I think it's probably mostly the latter.)

Sorry for writing all this. I got carried away. I hope to get your new draft printed out today, and studied partially tonight.

By the way, I think Ignacio is a good benchmark with all this. If we can't find a way to impact him in five or seven years, say, then maybe indeed all this is for naught.

20-11-01 *One Horse's Mouth* (to J. M. Renes)

I finally get to the last note I owe you.

Renesism 2: *You make this point in the nato paper explicitly (that the unitary taking the quantum Bayes rule post measurement state to the orthodox post measurement state is a "mental readjust-*

ment" and does depend on the input state). There's a lot here, though, especially since Carl initially balked pretty hard at the idea since "we're physicists so I don't know what he's (you) talking about."

I'm intrigued by your phrase "there's a lot here, though." I don't quite understand what you're trying to get at—that you agree that it's a difficulty, or that it's a good thing? Or that if Carl balked, that might be an unintentional mark in its favor? Can you explain a little better?

Renesism 3: *Some days I don't feel like a physicist (happily coinciding much of the time with the days I don't want to) so I'm not initially troubled by this. However, as I said, there's a lot of "stones unturned" here. Are you saying that there is no physical picture of what's going on, there must be some subjective element "uncaptureable" by a physical picture? This seems to fit with your rep as being an "extreme subjectivist" but I'd rather hear it from the horse's mouth.*

I'm not sure exactly how I should reply to this. You probably know my thoughts at this point better than I know them myself. Maybe I should say it like this: My pet idea at the moment is that there was a world here before humankind ever appeared on the scene; there'll be a world here after we disappear. But I would say the world is still under construction; there is no sense and no ultimate level at which it is already complete. To that extent, I believe our beliefs, our passions, our actions, our inventions, and our dreams modify the world and form part of its construction in a nonnegligible way.

And I think our greatest hint of that comes from quantum mechanics. I would say that what we're learning in a precise way from it is that there is something about the stuff of the world that makes it uncaptureable with a purely physical picture. We find that we cannot even draw a picture of the world without including our beliefs and belief changes as a crucial background in the sketch. (How could we if the world's not completed yet?)

Does that make me an extreme subjectivist? I don't know. Whatever it is though that I should be called, I think this willingness to accept a substantial part of quantum mechanics as simply "law of thought" will keep me from going down a misguided path. I.e., the path of trying to ascribe all the easiest terms in the theory a kind of physical reality independent of our presence as active agents.

One horse's mouth.

21-11-01 *Pragmatism versus Positivism* (to R. Schack)

Both quotes are taken from William James's essay "The Sentiment of Rationality."

Quote I:

> The necessity of faith as an ingredient in our mental attitude is strongly insisted on by the scientific philosophers of the present day; but by a singularly arbitrary caprice they say that it is only legitimate when used in the interests of one particular proposition—the proposition, namely, that the course of nature is uniform. That nature will follow tomorrow the same laws that she follows today is, they all admit, a truth which no man can *know*; but in the interests of cognition as well as of action we must postulate or assume it. As Helmholtz says: "*Hier gilt nur der eine Rat: vertraue und handle!*" And Professor Bain urges: "Our only error is in proposing to give any reason or justification of the postulate, or to treat it as otherwise than begged at the very outset."

With regard to all other possible truths, however, a number of our most influential contemporaries think that an attitude of faith is not only illogical but shameful. Faith in a religious dogma for which there is no outward proof, but which we are tempted to postulate for our emotional interests, just as we postulate the uniformity of nature for our intellectual interests, is branded by Professor Huxley as "the lowest depth of immorality." Citations of this kind from leaders of the modern *Aufklärung* might be multiplied almost indefinitely. Take Professor Clifford's article on the "Ethics of Belief." He calls it "guilt" and "sin" to believe even the truth without "scientific evidence." But what is the use of being a genius, unless *with the same scientific evidence* as other men, one can reach more truth than they? Why does Clifford fearlessly proclaim his belief in the conscious-automaton theory, although the "proofs" before him are the same which make Mr. Lewes reject it? Why does he believe in primordial units of "mind-stuff" on evidence which would seem quite worthless to Professor Bain? Simply because, like every human being of the slightest mental originality, he is peculiarly sensitive to evidence that bears in some one direction. It is utterly hopeless to try to exorcise such sensitiveness by calling it the disturbing subjective factor, and branding it as the root of all evil. "Subjective" be it called! and "disturbing" to those whom it foils! But if it helps those who, as Cicero says, "*vim naturae magis sentiunt*," it is good and not evil. Pretend what we may, the whole man within us is at work when we form our philosophical opinions. Intellect, will, taste, and passion co-operate just as they do in practical affairs; and lucky it is if the passion be not something as petty as a love of personal conquest over the philosopher across the way. The absurd abstraction of an intellect verbally formulating all its evidence and carefully estimating the probability thereof by a vulgar fraction by the size of whose denominator and numerator alone it is swayed, is ideally as inept as it is actually impossible. It is almost incredible that men who are themselves working philosophers should pretend that any philosophy can be, or ever has been, constructed without the help of personal preference, belief, or divination. How have they succeeded in so stultifying their sense for the living facts of human nature as not to perceive that every philosopher, or man of science either, whose initiative counts for anything in the evolution of thought, has taken his stand on a sort of dumb conviction that the truth must lie in one direction rather than another, and a sort of preliminary assurance that his notion can be made to work; and has borne his best fruit in trying to make it work? These mental instincts in different men are the spontaneous variations upon which the intellectual struggle for existence is based. The fittest conceptions survive, and with them the names of their champions shining to all futurity.

The coil is about us, struggle as we may. The only escape from faith is mental nullity. What we enjoy most in a Huxley or a Clifford is not the professor with his learning, but the human personality ready to go in for what it feels to be right, in spite of all appearances. The concrete man has but one interest—to be right. That for him is the art of all arts, and all means are fair which help him to it. Naked he is flung into the world, and between him and nature there are no rules of civilized warfare. The rules of the scientific game, burdens of proof, presumptions, *experimenta crucis*, complete inductions, and the like, are only binding on those who enter that game. As a matter of fact we all more or less do enter it, because it helps us to our end. But if the means presume to frustrate the end and call us cheats for being right in advance of their slow aid, by guesswork or by hook or crook, what shall we say of them? Were all of Clifford's works, except the *Ethics of Belief*, forgotten, he might well figure in future treatises on psychology in place of the somewhat threadbare instance of the miser who has been led by the association of ideas to prefer his gold to all the goods he might buy therewith.

In short, if I am born with such a superior general reaction to evidence that I can guess right and act accordingly, and gain all that comes of right action, while my less gifted neighbor (paralyzed by his scruples and waiting for more evidence which he dares not anticipate, much as he longs to) still stands shivering on the brink, by what law shall I be forbidden to reap the advantages of my superior native sensitiveness? Of course I yield to my belief in such a case as this or distrust it, alike at my peril, just as I do in any of the great practical decisions of life. If my inborn faculties are good, I am a prophet; if poor, I am a failure: nature spews me out of her mouth, and there is an end to me. In the total game of life we stake our persons all the while; and if in its theoretic part our persons will help us to a conclusion, surely we should also stake them here, however inarticulate they may be.

Quote II:

Now, I wish to show what to my knowledge has never been clearly pointed out, that belief (as measured by action) not only does and must continually outstrip scientific evidence, but that there is a certain class of truths of whose reality belief is a factor as well as a confessor; and that as regards this class of truths faith is not only licit and pertinent, but essential and indispensable. The truths cannot become true till our faith has made them so.

Suppose, for example, that I am climbing in the Alps, and have had the ill-luck to work myself into a position from which the only escape is by a terrible leap. Being without similar experience, I have no evidence of my ability to perform it successfully; but hope and confidence in myself make me sure I shall not miss my aim, and nerve my feet to execute what without those subjective emotions would perhaps have been impossible. But suppose that, on the contrary, the emotions of fear and mistrust preponderate; or suppose that, having just read the *Ethics of Belief*, I feel it would be sinful to act upon an assumption unverified by previous experience—why, then I shall hesitate so long that at last, exhausted and trembling, and launching myself in a moment of despair, I miss my foothold and roll into the abyss. In this case (and it is one of an immense class) the part of wisdom clearly is to believe what one desires; for the belief is one of the indispensable preliminary conditions of the realization of its object. *There are then cases where faith creates its own verification.* Believe, and you shall be right, for you shall save yourself; doubt, and you shall again be right, for you shall perish. The only difference is that to believe is greatly to your advantage.

The future movements of the stars or the facts of past history are determined now once for all, whether I like them or not. They are given irrespective of my wishes, and in all that concerns truths like these subjective preference should have no part; it can only obscure the judgment. But in every fact into which there enters an element of personal contribution on my part, as soon as this personal contribution demands a certain degree of subjective energy which, in its turn, calls for a certain amount of faith in the result—so that, after all, the future fact is conditioned by my present faith in it—how trebly asinine would it be for me to deny myself the use of the subjective method, the method of belief based on desire!

In every proposition whose bearing is universal (and such are all the propositions of philosophy), the acts of the subject and their consequences throughout eternity should

> be included in the formula. If M represent the entire world minus the reaction of the thinker upon it, and if $M + x$ represent the absolutely total matter of philosophic propositions (x standing for the thinker's reaction and its results)—what would be a universal truth if the term x were of one complexion, might become egregious error if x altered its character. Let it not be said that x is too infinitesimal a component to change the character of the immense whole in which it lies imbedded. Everything depends on the point of view of the philosophic proposition in question. If we have to define the universe from the point of view of sensibility, the critical material for our judgment lies in the animal kingdom, insignificant as that is, quantitatively considered. The moral definition of the world may depend on phenomena more restricted still in range. In short, many a long phrase may have its sense reversed by the addition of three letters, *n-o-t*; many a monstrous mass have its unstable equilibrium discharged one way or the other by a feather weight that falls.

23-11-01 *Bossa Nova* (to C. M. Caves)

Cavesism 52: *P.S. Jim [Hartle] also mentioned that the Perimeter Institute is currently still in its big-shot hiring mode as far as foundations is concerned.*

As far as I can tell the "big-shots" haven't done a useful thing in this area for 66 years.

25-11-01 *quant-ph/0106166* (to R. Cleve)

Thanks for the thoughtful comments! Let me fire back a couple of my own.

Cleve-ism 1: *What would the classical information theory version of the table on page 4 look like? Somehow the axioms of probability theory don't come out as crisply in English as those of relativity, do they?*

I think that's a superbly relevant point; it's one that Charlie Bennett keeps making to me too. Indeed, I do get a little worried about the axioms of information theory. But, I'm fairly optimistic on the axioms of probability at least. While you're at the ITP, you might take the opportunity to talk to Rüdiger Schack or Carl Caves about the "dutch book argument" for the classical probability axioms. They can sketch the argument for you, which I think you will find really intriguing. In essence, with respect to a fairly natural game, it does squeeze the structure of classical probability theory into a couple of English sentences.

This sort of thing gives me a lot of hope for both classical information theory and quantum theory too.

Cleve-ism 2: *Your comment on page 15 that starts on line 6 made me think of something I recently worked out. Consider the case of a one-qubit system in three possible states with an angle of 120 degrees between each pair and the goal being to determine which state as well as possible – is this the example you had in mind?*

If you're thinking 120 degrees in Bloch-sphere space, then yes that was the example I had in mind. On the other hand,

Cleve-ism 3: *I had thought this was a good simple example where one can illustrate how using a POVM is better than using von Neumann measurements (assuming one isn't willing to go into a larger Hilbert space). But it isn't. Instead of using the natural three-element POVM, one can first randomly pick one of three orthogonal bases (120 degrees rotated from each other) and then perform that von Neumann measurement. It turns out that the net result of this process will yield exactly the same information about which of the three states it was as doing the POVM would.*

Well, do you agree? My point here isn't against POVMs; I think there are other examples where one really can do more with a POVM (without enlarging the Hilbert space)

if you were thinking of what I said above, then I'm confident you made a mistake in your calculation. What is unique about the famous three-outcome POVM is that no matter what the outcome, one of the three possible preparations is eliminated, leaving the posterior probability of either of the other two preparations at 1/2. Thus the mutual information gained about the preparation is log(3/2). On the other hand consider the randomized measurement you speak of. First flip a (three-sided) coin, and then do one of the von Neumann measurements. If you get a "no" answer for that two-valued measurement, then you will have indeed eliminated one of the three possible preparations … giving log(3/2) bits. On the other hand, if you get a "yes" answer—which happens some fraction of the time—you will get less than that. For a "yes" does not eliminate any of the three possible preparations. It only makes one of them more likely than the other two.

Does that make sense? (It's a proof that goes back to one of Holevo's 1973 papers.) On the other hand, maybe I didn't really understand the example you had in mind.

Cleve-ism 4: *I am going to stop writing here (being halfway through the paper is a good point to pause) and hope to follow up with more comments (for what they're worth) soon.*

They're worth quite a lot, and I'm very grateful. More important than anything to me is that part of our community start thinking along these lines and make some progress (more than I can make with my feeble mind). If you've got the interest, I've got the enthusiasm!

Richard's Preply

I made it out to ITP where I'll be for the last three weeks. I've been meaning to read your paper for a while and on this quiet – and rainy! – day where I'm experiencing some frustration with a technical problem I decided this was as good a time as any to read and think about `0106166`. Of course, your engaging literary style gives a certain pleasure to the process. Here are some (admittedly brief) comments.

The NATO meeting sounded nice. I agree with you that decoherence doesn't occupy a sacred role in the foundations of quantum information. And – probably not surprisingly for someone with my background – the aspect of quantum mechanics that I also find the most interesting is its underlying information theory.

I recall the table on page 4 from one of your talks and I always find it compelling to see it, seeing the contrast between the two columns. I guess that special relativity is about geometry (more precisely a "generalization of geometry" that includes space and time). The fact that our brains and language are well equipped for spatial concepts, may be a reason why column one can be expressed so eloquently in words in column two.

An analogous table for quantum information would be nice. It is not really about geometry (in spite of geometric structure of Hilbert spaces) but about information theory. (Like many others, I think of quantum information as a "souped-up" version of classical information – i.e., a generalization of classical.) What would the classical information theory version of the table on page 4 look like? Somehow the axioms of probability theory don't come out as crisply in English as those of relativity, do they? On the other hand ... people don't seem to have that much trouble with probability theory. Card players, actuaries, casino managers all seem to do okay, in spite of numerous subtleties and the deep philosophical issues that arise when really thinks about what happens when one, say, flips a coin.

Your comment on page 15 that starts on line 6 made me think of something I recently worked out. Consider the case of a one-qubit system in three possible states with an angle of 120 degrees between each pair and the goal being to determine which state as well as possible – is this the example you had in mind? I had thought this was a good simple example where one can illustrate how using a POVM is better than using von Neumann measurements (assuming one isn't willing to go into a larger Hilbert space). But it isn't. Instead of using the natural three-element POVM, one can first randomly pick one of three orthogonal bases (120 degrees rotated from each other) and then perform that von Neumann measurement. It turns out that the net result of this process will yield exactly the same information about which of the three states it was as doing the POVM would. Well, do you agree? My point here isn't against POVMs; I think there are other examples where one really can do more with a POVM (without enlarging the Hilbert space).

I'm just reading the Bayesian stuff on page 21 and really like the idea – does the analogy really hold in some sort of sense? I am going to stop writing here (being halfway through the paper is a good point to pause) and hope to follow up with more comments (for what they're worth) soon.

26-11-01 *PRA Proofs* (to C. M. Caves & R. Schack)

I'm in the office again finally, and I've read over the PRA proofs. Of course, as always, I can't see that any of their changes were for the betterment of the paper ... but this time at least, none were overly annoying to me.

Here are my notes (which one of you two might want to incorporate into a reply).

1. page 2, "If one accepts this conclusion ... " : Note they take away the "a" in front of the kets. That grammatical change takes away the impression that the final state is unspecified within the set. I vote that we force them to reinstate the "a"s unless you can figure out a smoother way to express the proper idea.

2. page 2, "The physical basis of Einstein's ... " : They changed the end of the sentence to "amenable to experimental testing." That seems odd to me. Should we protest?

3. page 3, "We then use a version of the so-called Dutch-book ... " : They want us to explain "Dutch-book argument." That's pretty stupid, given that they didn't ask us to explain "Bayesian probability theory" or even "Gleason's theorem." We even used the warning sign "so-called." I don't know what more can be said without inserting Section II into the middle of this paragraph. Perhaps you guys have a nicer way to approach it than I would.

4. page 5, "The probability assignment is thus inconsistent ... " : You'll note in the proof that the equation at the end of the sentence is broken at the end of the line in an awful way. Can we ask them to keep it together? Or perhaps we can simply display the equation.

5. page 5, "For example, normalization of the probabilities . . . " : This is not a problem of theirs but a question of mine. We end the sentence with "so obvious that it needs no justification." Do we really need to insert that phrase? I find it a little distracting from the main point of the sentence.

6. page 6, "The keys to these results are . . . " : I would change the very last word to "paragraphs."

7. page 10, "The data gathered from the measurements are said . . . " : What is this, England? "The House of Commons have voted . . . " Horrible. I view "data" as a collective noun.

That's it really.

But let me take this opportunity to give one last ramble from the heart. It has nothing to do with changing the present paper: I just want to say it because it's on my mind again.

After reading the paper once more, I found myself feeling awful again. I don't think there is a reader out there besides Schack and Caves that will come away with the feeling that the Dutch-book argument is *purely* an internal consistency argument (or rationality check). The English in a sentence like,

> Given the assumptions of Gleason's theorem, if a scientist has maximal information, any state assignment that is different from the unique pure state derived in the last paragraph is inconsistent in the Dutch-book sense; i.e., it leads to a sure loss for a bet on the outcome of a measurement on a single system that includes the unique pure state among the outcomes.

is just loaded with imagery. Who out there will read "sure loss" as anything other than a factual state of affairs—something dictated by the world independent of the agent? Who out there will read the word "unique" as really meaning "unique with respect to the agent's belief of certainty"? Who out there will not interpret the phrase "maximal information" in an objectivistic way—i.e., that there is one and only one way to have maximal information? Or here's a better acid test: If a reader were to be confronted with our paper in its present form at the same time as the BFM paper, would he be able to see any philosophical differences in the approaches of the two papers? I can't imagine it. All that troubles me very deeply: A small change of language really could have made all the difference in the world.

29-11-01 *Observation* (to J. M. Renes)

Renesism 4: *Here's some stuff relating to what I wrote the other day from "The Taboo of Subjectivity" by B. Alan Wallace:*

> The disdain of scientific materialism for subjectivity has also shaped the very concept of scientific observation. While nonscientific kinds of observation also detect phenomena—such as our joys and sorrows, hopes and fears, ideas and inspirations—they are thought to be tainted by human subjectivity and are therefore suspect. From the perspective of scientific materialism, human sensory perception may be deemed not only unreliable but irrelevant. For a scientific observation to take place, all that is required is a detector, or receptor. The human eye is one type of receptor, which detects a certain range of electromagnetic frequencies, but other instruments also measure this and other types of information, and they are regarded as more reliable.

> In common parlance, for an observation to take place, the received information must be transformed into humanly accessible information that is, sooner or later, perceived and understood by a human being. But according to scientific materialism, *observation* is assimilated into the general category of *interactions*, thereby freeing it from the subjectivity of its normal associations. This interpretation is said to be central to grasping what is involved in scientific objectivity in the search for knowledge and the justification of belief.

[reference to Dudley Shapere "The Concept of Observation in Science and Philosophy," Philosophy of Science **49** *No. 4, 1982, pg 485.]*

Beautiful quotes! Do you have the whole Shapere article? Do you know whether he is for or against that conception of observation (i.e., as interaction)? If you do have the article, could you make a copy and send it to me? And what about the Wallace thing—is that a book or an article? How is it?

29-11-01 *Observation, 2* (to J. M. Renes)

Renesism 5: *As for Wallace, it's a book — pretty good so far. He's arguing that "scientific materialism" (i.e. the way someone like Feynman thinks of the world) is a bit like its own religion, with a major piece of dogma being the exclusion of subjectivity from consideration.*

Aha. Have a look at William James's essay "The Sentiment of Rationality" (in most any collection of his essays) if you get a chance. He also argues (for what amounts to) what you said above.

I'll try to see if I can find the Wallace book up here.

29-11-01 *Community* (to W. K. Wootters)

What a beautiful letter; thank you.

In a way, I've been going to my own seminar on science and religion lately—but with me, through my reading choices. I've gotten stuck on the "pragmatism" movement, predominantly William James's version. I had never realized before what a wealth of material was there (for the kinds of thoughts I'd like to think, about quantum mechanics in particular). Nor had I realized how Wheeleresque and Woottersesque James was in his outlook—hanging so much on the idea that the universe is (in part) a product of our collective experience.

In that regard, but with respect to religion, I have never been so impressed by the possibility—necessity even—of "faith" than when I read James's articles "The Sentiment of Rationality" and "The Dilemma of Determinism." They have made a huge effect on me. If you happen to read them and have any reaction, I would love to hear your thoughts. (For fun I just had a look at the Williams College library; you guys have a collection of 63 items penned by James! In contrast, not one hit in the Bell Labs library … but who would have guessed otherwise.)

I sympathize with your unease with telepathy. Here's how Stephen Brush put it:

> 'Wheeler's dilemma' is this: how can one maintain a strong version of the Copenhagen Interpretation, in which the observer is inextricably entangled with that which is observed, while at the same time denying that our consciousness affects that which we are conscious of—and thus accepting the possibility of telekinesis and other psychic

effects? For Wheeler himself there is no dilemma at all; one simply has to recognize 'the clear distinction between (1) the strange but well verified and repeatable features of quantum mechanics and (2) the pseudo-scientific, non-repeatable and non-verified so-called extra sensory perception.' But Wheeler's own views are likely to strike a non-physicist as being just as bizarre as those of the parapsychologists he deplores. Indeed, no one has yet formulated a consistent worldview that incorporates the Copenhagen Interpretation of Quantum Mechanics while excluding what most scientists would call pseudo-sciences—astrology, parapsychology, creationism, and thousands of other cults and doctrines.

The issue whether there is something besides unitarity—whether it can ever "breakdown" as you put it—is acute, but I think a lot of the problem hinges on how one views unitarity's status in a physical theory. I am inclined to believe that unitarity—or more generally "trace-preserving complete positivity"—does not breakdown either. But that is because I am inclined to view the time-evolution mapping one ascribes to a system as an epistemological entity (rather than an ontological one), much as I view the quantum state one assigns the same system. That is to say, I am struck that there is a deep reason one can make a bijective correspondence between the completely positive maps on one Hilbert space and the density operators in a larger space, a reason that has nothing to do with imagining ancillary systems: A CP map *is* a density operator, it is a state of knowledge in just the same way any other quantum state is.

There are other (peculiarly quantum) reasons for saying what I just did above, but it is also part of a larger program I have in mind (and one I think James had in mind too)—namely, to find a little slippage between the notion of a physical theory and the world itself. Below I'll quote a little piece I wrote to Carl Caves on the idea. It gets at the sentiment, even if not at the technical details! [See 04 October 2001 note to Caves-Schack, "Replies on Pots and Kettles".]

30-11-01 *Ramsey Theory* (to W. K. Wootters)

Just a small note. I want to bring up a reference that may or may not be relevant to thinking about your toy graph model—it's something I just learned about last week, called Ramsey Theory. Here's an example, "In any collection of six people either three of them mutually know each other or three of them mutually do not know each other." And more generally, "Every system of a certain class possesses a large subsystem with a higher degree of organization than the original." It's a subfield of graph theory, quite developed, with loads of examples like that.

Anyway, for what it's worth, I was told that the following is a good starter reference:

> Ronald L. Graham, Bruce L. Rothschild, and Joel H. Spence, *Ramsey Theory*, second edition (John Wiley and Sons, New York, 1990).

30-11-01 *Some Thoughts on Your Paper(s)* (to K. Svozil)

Svozilism 1: *I would kindly like to ask two questions:*

(i) are the Bayesian consistency requirement in any way related to Boole's "conditions of possible classical experience" (which have been interpreted by Pitowsky in terms of the faces of classical correlation polytopes)?

(ii) have you ever attempted to apply the methods to "exotic" but non-quantum event structures? It might at least be pedagogically helpful to consider such cases.

Thanks for your letter. You ask two good questions, and unfortunately I don't know the answer to either one! I will however have a good look at your papers and start thinking about them.

I think the Dutch-book argument for probabilities is not so strongly tied to the Boolean structure of propositions (as, say, Cox's argument for the probability axioms is). For instance, just look at Eqs. (1) and (2) in our paper. It seems to me that that part of the argument does not care one iota whether there is a distributive law for this event structure or not. If that's the case throughout the remainder of the Dutch-book mechanics, then maybe this is an important point.

30-11-01 *Later in the Book* (to J. M. Renes)

More great quotes! Keep 'em coming (as you like).

Joe's Preply

Wallace says

> James's philosophy of radical empiricism rejects the absolute duality of mind and matter in favor of a world of experience, in which consciousness *as an entity*, in and of itself, does not exist; nor is it a function of matter, for matter *as an entity* in and of itself, does not exist either. According to this view, the postulation of mental and physical substances is a conceptual construct, as is the metaphysical distinction between subject and object. Mind and matter are constructs, whereas pure experience, which is neutral between the two, is primordial. One implication of the hypothesis that we are directly acquainted with reality is that the contents of consciousness can no longer be regarded as being "in the mind" (let alone in the brain). Reality just *is* the flux of experience.

So my thoughts on this subject aren't very original! But to be in such company is far better. Wallace goes on to laud James some more, but does mention some work of Hilary Putnam:

> If all valid statements concerning the world of human experience have both a conventional and a factual element, it follows that the referents of language are also inseparable fusions of convention and reality. Thus, the existence of a concrete object like a tree is also a matter of convention, and our observation of a tree is possible only in dependence on a conceptual scheme. The reason for this, according to Putnam, is that 'elements of what we call "language" or "mind" penetrate so deeply into what we call "reality" that the very project of representing ourselves as being "mappers" of something "language-independent" is fatally compromised from the very start.' [from Putnam, *Realism with a Human Face* 1990]

This ending quote from Putnam reminds me a lot of the squabbles about the wavefunction and density matrix.

Okay, can't stop quoting.

> Once we have chosen a conceptual scheme, there are facts to be discovered and not legislated by our language or concepts. Our conceptual scheme restricts the range of descriptions available to us, but it does not predetermine the answers to our questions. As Putnam comments,

> "The stars are indeed independent of our minds in the sense of being causally independent; we did not make the stars ... The fact that there is no one metaphysically privileged description of the universe does not mean that the universe depends on our minds."
>
> On the other hand, if there were no language users, there would not be anything true or anything with sense or reference. Thus, the rich and ever-growing collection of truths about the world is the product of the experienced world, with language users playing a creative role in the process of production.

04-12-01 *The Power of Advertising* (to A. Peres & D. R. Terno)

Actually, reading over this again from your upcoming paper,

> The root of the difficulty we have to transform quantum expressions from one Lorentz frame to the other is that the process called "quantum measurement" is an intervention in the quantum dynamics by an "exosystem" [finkel], namely by an apparatus which is not completely described by the quantum formalism [interv].

I wonder if I can ask a favor of you? In my own paper,

- C. A. Fuchs, "Quantum Foundations in the Light of Quantum Information," to appear in *Proceedings of the NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer*, edited by A. Gonis (Plenum Press, NY, 2001 or 2002). (Until then, see `quant-ph/0106166`.)

I go to great lengths to try to explain why the term "intervention" is a better one than "measurement," and I try hard to promote its more frequent use in future dialogues on the subject. Of course, I cite Asher for the original introduction of the term in that. Nevertheless, might I ask you guys to cite this also? Certainly feel free to ignore this request as you wish: It just struck me that I might increase my readership if I happened to get a citation from you!

05-12-01 *Yep* (to R. Pike)

Regarding "Challenging Particle Physics as Path to Truth" by George Johnson in yesterday's *New York Times*:

`http://www.nytimes.com/2001/12/04/science/physical/04SQUA.html`.

Yep, you know the anti-unificationists have my sympathy.

05-12-01 *Dear Prudence* (to C. M. Caves & R. Schack)

Fellow Bayesians,

(Let's see if Carl's knowledge of 1960s music can help him guess the origin of this note's title.)

Anyway, to the real subject. Dutch-book coherence? Dutch-book consistency? Neither of them seem to be an accurate account of what's going on with the theorem. I know neither one of you will accept it, but I think "prudence" gets significantly closer to the mark.

- **Consistent** – 1. In agreement; compatible: "The testimony was consistent with the known facts." 2. Being in agreement with itself; coherent and uniform: "a consistent pattern of behavior." 3. Reliable; steady: "demonstrated a consistent ability to impress the critics." 4. In mathematics, having at least one common solution, as of two or more equations or inequalities.

- **Coherence** – 1. The quality or state of cohering, especially a logical, orderly, and aesthetically consistent relationship of parts. 2. In physics, the property of being coherent, as of waves.

- **Prudent** – 1. Wise in handling practical matters; exercising good judgment or common sense. 2. Careful in regard to one's own interests; provident. 3. Careful about one's conduct; circumspect.

- **Prudence** – 1. The state, quality, or fact of being prudent. 2. Careful management; economy.

How did Mermin's talk go? How did Rüdiger's talk go?

05-12-01 *Lucky Seven* (to B. W. Schumacher)

5) I was a little disappointed to learn from your prospectus that you'll be going to Caltech for your sabbatical, but on the other hand I can easily imagine several reason's why that's the best choice for you. Still, I'm going to miss having you around ... as will all the other guys in Bell Labs and the Bell Labs metro area, Charlie Bennett, David DiVincenzo, Steven van Enk, Danny Greenberger, Lov Grover, Mark Hillery, Alexander Holevo, Debbie Leung, Eric Rains, Terry Rudolph, Peter Shor, Dick Slusher, John Smolin, Barbara Terhal, Bill Wootters, and so forth. ☹

6) You know I'm always scheming, and this is one of the latest ones. Actually it's an older one, but revived with a new head of steam. Jeff Bub and I have decided to edit a special issue of *Studies in the History and Philosophy of Modern Physics* (SHPMP) on ... (you can guess) "Quantum Foundations in the Light of Quantum Information." But there are two things going on with this: 1) We pretty much want to get it done and published *before* the Montréal meeting, and 2) we really only want to go through with it if we can get commitments for some really good papers. Jeff wrote this in one of our emails:

Bubism 1: *We'd really have to think hard about how to ensure that the authors all deal with some conceptual or foundational question relevant to quantum information in a philosophically serious way. We want to avoid the issue just being 'intro to quantum information for non-specialists,' with some anecdotal interpretative comments as a nod to philosophy. And it also shouldn't just be a forum for one point of view.*

The difficulty, of course, is that very few philosophers have actually worked in this field. So we have to rely mostly or perhaps entirely on scientists for the papers. It will be hard getting papers from physicists and computer scientists for a philosophy journal. On the plus side, it does seem to be the case that some people working in quantum information have thought hard about the relevance of their research for foundational questions. Somehow, we have to motivate people to take it as a worthwhile and exciting challenge to write a paper that tackles a philosophical issue in an intellectually disciplined way — as intellectually disciplined as these authors invariably are when they write a technical scientific paper. Too often, all analytical controls seem to be switched off when scientists venture into philosophical terrain.

We've already gotten a commitment from Andrew Steane to beef up his "quantum computing needs only one universe" paper for it. Moreover, I know that you're also capable to the task, and already somewhat semi-committed to a paper on "doubting Everett." So I'm hoping I can get an official nod from you in the YES direction. If I can secure that, then I think I can make a weighty case to Charlie Bennett to record (more importantly defend) his thoughts on why he thinks Everett provides an adequate and conceptually simpler picture of quantum foundations than some other interpretive attempts.

Then beyond that I'm hoping to get some papers from *possibly* Jozsa, Caves & Schack, Mermin, Wootters, and maybe a couple of others (and Bub and myself of course). That's already more than enough.

Once I hear from you and Charlie, then Jeff and I will kick into "phase two" of our baiting, i.e., getting the other guys to say yes to getting a paper in before next summer.

7) I really loved the "How does it know?" story in your prospectus! Let me give you two stories from my Samizdat in connection to that below. Knowing and flying: the two questions might be a little bit of the same thing.

> **From a 17 December 1997 note to G. L. Comer, "It's a Wonderful Life"**
>
> Good holidays to you. This morning, as I was driving to work, it dawned on me that roughly this day 10 years ago, I was conferred my degrees at the University of Texas. Time does fly.
>
> It made me think of a little anecdote about John Wheeler that I heard from John Preskill a few days ago. In 1972 he had Wheeler for his freshman classical mechanics course at Princeton. One day Wheeler had each student write all the equations of physics s/he knew on a single sheet of paper. He gathered the papers up and placed them all side-by-side on the stage at the front of the classroom. Finally, he looked out at the students and said, "These pages likely contain all the fundamental equations we know of physics. They encapsulate all that's known of the world." Then he looked at the papers and said, "Now fly!" Nothing happened. He looked out at the audience, then at the papers, raised his hands high, and commanded, "Fly!" Everyone was silent, thinking this guy had gone off his rocker. Wheeler said, "You see, these equations can't fly. But our universe flies. We're still missing the single, simple ingredient that makes it all fly."
>
> Merry Christmas.
>
> **From a 02 December 1997 note to J. Preskill, "Flying Equations"**
>
> I couldn't help but think of the anecdote about John Wheeler's (non-)flying equations you told the other day when I came across the following little passage (presumably Biblical in origin):
>
>> "I forbade any simulacrum in the temples because the divinity that breathes life into nature cannot be represented."

06-12-01 *Baudrillard Maybe?* (to B. W. Schumacher)

Schumacherism 2: *I certainly loved the samizdat stories you included. Where did the second quotation, the one about "the divinity that breathes life into nature", come from? What a counterpoint to Wheeler's futile "Fly!"*

That's a good question; I really don't know anymore. Looking further through the Samizdat I see that I reported reading Jean Baudrillard on November 23. And I sort of faintly remember learning the word "simulacrum" from whatever book that was. So, I'd bet it was from there.

Schumacherism 3: *Well, yes, dammit. I owe you this paper. At this moment Mike W. and I are finishing off a paper on approximate error correction, but I will try to get something down afterwards. [...] We have also promised a paper to Mary Beth Ruskai for JMP. [...] But I owe you longer; I will put together a very rough draft over the vacation, and run it past you — I'd much value your comments.*

That's great!!! I'll inform Jeff Bub, and move to Phase 1.5 — i.e., coercing Bennett. Wish me some success there; it's gonna take a lot of work to loosen him up.

06-12-01 *Investigative Reporting* (to B. W. Schumacher)

It looks like Baudrillard was the right path. I just got on `altavista.com` and did a search on simulacrum, Baudrillard, and divinity. And I found the following:

> Baudrillard has interesting views on the nature of postmodern nihilism. In Simulacra and Simulation, he begins his discussion on the precession of simulacra by describing what he calls the "simulacra of divinity." He points out that "divinity that animates nature can never be represented" (p. 4). The idea of God embodied within religious icons and iconography is a complex system of human belief, in which the visible is supposed to evoke the religious all-being, the divine-referential. Yet when God's omnipotence and presence can only be felt through these icons, it is becomes probable that the existence of God is questioned, if the simulacra of divinity becomes the only visible image of God's presence.

Then I found an exact replica of the quote in someone's class notes about Baudrillard. So, I'm sure I got it from him.

So the question now is where did Baudrillard steal the quote from?

07-12-01 *A Sinking Ship?* (to C. M. Caves & R. Schack)

Fellow Bayesians,

Especially in light of what Carl wrote me yesterday, perhaps you should both have a serious look at Gavriel Segre's Ph.D. Thesis at `quant-ph/0110018`. Van Enk pointed it out to me this morning, and believe me it'll be worth your time! In particular, pay attention to Theorem 5.2.21 "Impossibility of a Subjectivistic Bayesian Foundation of Quantum Probability Theory" on page 193. Maybe we ought to get off this ship before it sinks?

Also, make sure you don't miss reading his acknowledgments on page 5. This is surely the best quote I've ever seen on `quant-ph`!

07-12-01 *A Sinking Ship?, 2* (to C. M. Caves & R. Schack)

Schackcosm 53: *Are you serious? Of course, the thought has crossed my mind.*

The sinking ship remark was a joke. The reference to the (unaccepted) thesis was a scoff (or at least a little). You did read the "acknowledgment" of the thesis, didn't you? Also look at the other contents of the "thesis."

I have never been more confident about anything scientifically than that we are on the right track in our quantum Bayesianism. No Bayesian that I've ever read called a probability assignment a "state of knowledge." What's different about quantum states? Why should they be above the "degree of belief" that every classical assignment is? Taking the "state of belief" appellation seriously is simply the cross we have to bear ... at least as I see it. (Sorry for the Christian motif, Carl.) But I'm also confident that it's not that bad anyway; it's just a question of packaging.

10-12-01 *Lost It* (to C. M. Caves)

I'll try to get in touch with you later today. Sorry we missed each other over the weekend.

Cavesism 53: *I managed to lose your last e-mail, where you expressed the view that no Bayesian views a probability as based on a state of knowledge. In the absence of your full statement, here's a thought on that point.*

I don't balk against "facts" having a say in determining probability assignments. I balk against "facts" *uniquely* determining them.

10-12-01 *The Spirit of Gandhi* (to N. D. Mermin)

Well, you took away some of my fun with the letter you just sent me! Over the weekend, I ran across your talk at the ITP and listened and watched the whole thing: In fact, I was going to send you some direct comments on it. Now some of my steam is taken away.

Still, let me gingerly point out some things I had wanted to point out earlier ... even if they may not be quite as relevant anymore.

Somewhere around 36 minutes into the talk—actually it was in your reply to a question (which sounded to come from Jeff Kimble)—you said:

Merminition 77: *Our presentation, at least in the paper we submitted on the web, can be read as being tinged with a view that quantum states are more than a reflection of knowledge ...*

the implication seeming to be that in the present talk you strove to get around that.

However, at 23 minutes into the talk you said this:

Merminition 78: *Of course there's the question of what it means for the combined knowledge of all observers to constitute a consistent body of knowledge about S, which is an interesting question. I'm taking a kind of dumb-physicist view, which is that there should be—at least in principle—there should be one observer who has a lot of data about various measurements and mutually commuting observables made on. The observer having access to all the data will realize that if ...*

I know I have said all this before, but let me "focus in" on these two passages and try to say it again (per your warning about me "focusing out" too often). I would say that what you said in the second passage has nothing to do with it being a "dumb-physicist view" of things. It is, however, the sine qua non of a view "tinged" with making the quantum state more than a simple epistemic entity. For it in essence says, there *should be* (your words) a "right" quantum state, or a range of "right" quantum states, and that has nothing to do with any *actually existent* observers.

Please do think about the similarities between your "dumb-physicist view" and the old limerick:

> There was a young man who said, "God
> Must think it exceedingly odd
> If he finds that this tree
> Continues to be
> When there's no one about in the Quad."
>
> REPLY
>
> Dear Sir:
>
> Your astonishment's odd:
> I am always about in the Quad.
> And that's why the tree
> Will continue to be,
> Since observed by
>
> Yours faithfully, God.

I would say that in supposing THERE SHOULD BE a superobserver, you are supposing that the quantum state should—in essence—already be there without any observer at all. When Bishop Berkeley ran into trouble with the question of where the trees go when there's no observer, he invoked the idea that God was there all along. Your superobserver is in essence a God, who—through his own objectivity—re-endows the quantum state with an objectivity I THOUGHT you were trying to get rid of in the first place.

But maybe that was never your goal.

Martin Gardner said something very clear in this regard in his essay "Why I Am Not a Solipsist," and so let me quote it:

> In this book I use the term "realism" in the broad sense of a belief in the reality of something (the nature of which we leave in limbo) that is behind the phaneron, and which generates the phaneron and its weird regularities. This something is independent of human minds in the sense that it existed before there were human minds, and would exist if the human race vanished. I am not here concerned with realism as a view opposed to idealism, or realism in the Platonic sense of a view opposed to nominalism or conceptualism. As I shall use the word it is clear that even Berkeley and Royce were realists. The term of contrast is not "idealism" but "subjectivism."

(The phaneron, by the way, was C. S. Peirce's term for "the world of our experience—the totality of all we see, hear, taste, touch, feel, and smell.")

If I were to give the BFM paper and your quest to make sense of Peierls (and Bohr and Heisenberg, as evidenced by your talk) a reading, I would say that what you are trying to do is give the quantum state an idealistic interpretation (via the word "knowledge") and thinking that that somehow contrasts with a realistic interpretation (and in doing so might fix everything up quantum interpretation-wise). But—in analogy to Gardner—for me, "realistic" and "idealistic" interpretations of the quantum state amount to the same thing. What I'm worried about is whether one can make any sense of the quantum state at all without simultaneously positing the active agent who makes use of it—I would claim you can't. And thus I'm left with the kind of "subjectivism" (for the quantum state) that frightens so many people.

PS. In case you're wondering why I titled this note "The Spirit of Gandhi," it is because I am hoping you will think of it as a form of nonviolent protest. Too many times in your talk you pointed out what a violent reaction I had to the BFM result, and I just can't think that that helped my reputation as a rational thinker or the seriousness with which a Bayesian-kind of quantum-foundation attempt should be viewed. (But I love you just as much as ever—maybe more with all the advertising you gave me—and there are no grudges.)

10-12-01 *Trumps and Triumphs* (to N. D. Mermin)

Also,

Merminition 79: *I did urge Carl and Rüdiger to do something about the phrase (in your other paper) "Gleason's theorem is the greatest triumph of Bayesian reasoning" which I read as a claim that Bayesian reasoning is the only way (or at least far and away the best way) to derive and understand Gleason's theorem. They explained that what you really meant was that Gleason's theorem, by providing stringent constraints on possible prior distributions, provided a powerful tool for Bayesian reasoning. But I think they did an inadequate job of removing the ambiguity, which is too bad, since I am rather sure it will put off most of the people you ought to be addressing.*

I'm glad you urged them. Here's what I had said to Carl and Rüdiger in a September 20 note:

> Another thing I guess I really didn't like—and this is only a new one for me, it's not something that had eaten away at me before—is the slogan "Gleason's theorem can be regarded as the greatest triumph of Bayesian reasoning." To say that is to imply that Bayesian reasoning IMPLIES Gleason's theorem. I don't think you mean that. I think what you mean is that it is one of the most valuable additions to Bayesian reasoning ever. But I won't cause trouble here: I know you both like the saying.

But they generally just view me as a trouble-maker.

10-12-01 *Reread (pronounced re-red)* (to N. D. Mermin)

I'm just reading through all my emails of the day again.

Merminition 80: *P.S. Spent a lot of time talking with Carl and Rüdiger in SB, which I enjoyed very much. I think the first half of your draft joint paper formulates the basis for the "necessary" condition of BFM much more coherently than we did: i.e. it is necessary if there is to be any density matrix that does not contradict anybody's strongly held beliefs. Much better than talking about "subsets of a body of data that anybody with access to all of which would agree constitutes nothing but valid results of measurements, none of which invalidate earlier measurement outcomes. . . ." It's hard even to put into a grammatical sentence. The other four cases strike me as interesting extensions of the case we (and Peierls) were addressing, which concerned existing knowledge prior to any subsequent interventions. My feeling was that there was no serious disagreement among the three of us, but maybe they will give you different reports.*

Thanks, that's a very nice compliment. Here's another way to put it, which might be my preferred way. Suppose two strongly consistent agents want to stand a chance of coming to agreement, just by talking to each other. Then BFM is the necessary condition. If it is not satisfied, then the only way they will ever get anywhere is by "consulting" the system with a (mutually agreed upon) measurement: With it, they can bend the world into something more congenial to both.

10-12-01 *Peace Pipe* (to T. Rudolph)

Thanks for the peace pipe.

I have seen the paper by Fivel before. We discussed it at a Maryland conference once. It somehow doesn't have the right flavor for what I've been seeking though: In the end, I want to find a kind of quantum ontology I can be happy with, not just something that feels more like a propositional calculus. But it's a little like the debate about art versus porn: You kind of only know porn when you see it. And I'm looking for porn.

Rudolphism 1: *But as I said its just an attempt to get something going, and I'm completely open to any suggestions you have – in particular I'd like to read any papers that you're thinking a lot about at the moment ...*

Are you kidding, I never read any papers any more! I'm becoming a bit solipsistic. But let me substitute this instead. Below I'll attach my notes on the big phase transition I've made from "knowledge" to "belief" for my view of the quantum state. If you need more of a context, first read the Brun-Finkelstein-Mermin paper on `quant-ph`. I start up with criticism on that at about page 19, and then it goes pretty thick into it thereafter. After October 9, Caves, Schack, and I started to work to turn a lot of that into a more technical argument: we're hoping to finish the paper by the end of the week and post it soon thereafter. (It stands at 14 pages at the moment.)

Anyway, the more radical stuff in those notes leads very quickly to some meaty technical questions. For instance, what does it mean to be an "unknown" quantum operation? It suggests a de Finetti theorem for completely positive maps, for instance, which Schack and I are working on in our spare time.

10-12-01 *Big Dreams* (to G. Brassard)

Brassardisme 3: *I've been talking around about our little dream of founding quantum mechanics on information theory. The more I talk about it to various people, the more I realize this is the only scientific topic that really excites me at this time. And the more I am sorry that I'm not doing anything about it.*

By the way, thank you for the very touching note. You are misplaced, however, in saying, "I am sorry that I'm not doing anything about it." At the very least, you are doing loads about it by organizing these meetings in Montréal!

The truth of the matter is, just like you, nothing excites me more. We're gonna prevail eventually. In fact, at the very moment, I'm writing another (pleading) paper titled "Quantum Mechanics as Quantum Information" (for the Växjö proceedings). For your private fun—but please do keep it private—I'll attach a new mini-samizdat I've recently put together with some of my latest musings. (It's probably hard to tell from it, but it has given rise to some new good theorems, that will be coming out soon in a paper with Caves and Schack.)

14-12-01 *Strong Consistency* (to C. M. Caves & R. Schack)

I know that I'm terribly slow to be coming up to speed on this project. But now I have another possibly frivolous question.

What was the origin of strong consistency? What motivated Rüdiger to invent the concept? And indeed, did either of you ever find it appearing in the literature otherwise? It, of course, has a very different flavor than normal Dutch-book—I know that's no surprise to you—but for me, for the moment anyway, it seems to me to be introduced more for mathematical nicety than anything else. (I.e., It seems more to be introduced just for the purpose of cleaning things up a bit axiom-wise.) It doesn't have the same operational/pragmatic flair that the rest of Dutch book has.

Indeed, it might be useful to lay this sort of issue on the line in the paper.

I can think of at least one case where one would never want to enforce strong consistency, namely for infinite event spaces. Consider an infinite number of draws from an i.i.d. distribution. One has that the probability of a typical sequence is one; however one should never then say that that implies the bettor believes a typical sequence will occur with certainty.

Reading ever so slowly (but I hope thoroughly),

15-12-01 *Bah, Humbug* (to C. M. Caves)

Humbug. Did I ever tell you I hate Christmas: It's the most intrusive holiday I know of, with people pushing me all over with obligations that they've invented for me. (Susie, Kiki's mom, arrived yesterday, by the way.) Now Thanksgiving, that's my favorite holiday: you cook, you eat, you watch a parade, and you end up rubbing your belly in a deep soporific ecstasy.

By the way, speaking of rubbing bellies, Kiki went into false labor last night. At first, when it was over, I thought, "Thank God it's false." But now, in the light of the morning, I'm thinking, "When are we ever going to get this over with?"

Cavesism 54: *There is evidence that it's out there in the literature (Sklar mentions it without any references), but we haven't dug up any references. In fact, Rüdiger and I had the impression that you might know where to find references.*

This is one of my biggest gripes about not being in a university. I don't have easy access to anything outside of the physics and mathematics journals. (In fact strangely, last night I dreamed that I was back at the University of Texas, surrounded by books, books, books on all subjects, and I was telling myself how glad I was to be back.)

Did you have a look at the Kemeny and Lehman references I recommended in an October 27 note? That at least would be my starting point. Also Bernardo and Smith, as I recall, had a lot of references on Dutch-book—in particular a lot of references dissatisfied with the argument (as Bernardo and Smith, in fact, are).

Cavesism 55: *I don't know about the flair point. I'll grant that strong consistency isn't as compelling as ordinary consistency, but it still registers on the flair scale: in a single trial, you never win, but definitely sometimes lose. Moreover—and this is the crucial point—strong consistency is absolutely essential so that probability assignments and density operators incorporate sufficient information about one's beliefs that one can use them as surrogates for the beliefs in our arguments.*

I wouldn't say that it's not "as compelling", it's just of a different flavor. To say that it's not as compelling, means it's directly comparable to the Dutch-book argument. But it's not clear to me that it is. Presently it seems to me much more like a Cox kind of thing. It says (at least in one case) my outward commitment *must* reflect my internal belief—not because I will be imprudent if I don't do so, but because I don't want my friends to see any discrepancy between my beliefs and my actions.

I believe I understand what you call the "crucial point." But I'm guessing—just a psychological point—you're more attracted to it, because it gives rise to (a better version of) the BFM statement.

Thus it gives the impression of giving something very solid. Whereas simple Dutch book lets everything fly in the wind—maybe that's the very thing I'm attracted to it.

In any case, I don't see anything wrong with exploring what strong consistency leads to. I'm just trying to get it straight in my head—and possibly also for the reader—what it's all about.

Cavesism 56: *Chris said, "Indeed, it might be useful to lay this sort of issue on the line in the paper."*

Perhaps you could be more specific. It seems to me that it is pretty well laid out in the paper, but you're looking at it with a fresh eye, so suggestions are welcome.

I'm sorry, I don't quite know how to be more specific right now. Maybe I would just like the reader to see more of the debate about it. And it should come across that it is (possibly) an invention of C and S, not de Finetti or Ramsey.

Cavesism 57: *You're absolutely right here: probability 1 cannot mean certainty and probability 0 cannot mean impossibility in the limit of an infinite number of draws from an i.i.d., but it's not clear to me that the problem is strong consistency. To apply probabilities to infinite sets, you need countable additivity for exclusive events, which doesn't follow from the finite additivity given by the Dutch book. To apply probabilities to continuous sets requires measure theory. These things are additional mathematical structure beyond what the Dutch book can provide. Perhaps the best philosophy is Rüdiger's: probabilities really apply only to finite sets of events; the generalization to infinite sets is an idealization, where additional mathematical structure is added to make things work nicely.*

Point well taken. But on the other hand, I'm still not convinced either. I think all three of us are aligned that the only thing really worth considering, conceptual-wise that is, are finite event spaces. However, it still strikes me as a chink in the armor that I can think of a limiting case where I would not want probability $1-\epsilon$ to have anything to do with internal certainty. That is to say, maybe there's a deep reason that Dutch-book consistency is NOT strong consistency ... and maybe we're missing something by plastering over its unsightliness so quickly.

Cavesism 58: *Let me know if you have specific recommendations. Rüdiger and I have agreed on a number of changes in presentation and a large number of additions on the POVM front. I will be attempting to incorporate these this weekend.*

I'm trying my best in the light of the present events. (But I'm sure you're not holding your breath for me.) The main thing I'm shooting for now, is to make sure I understand every aspect of the paper—in a defendable way, that is—before Mermin arrives Tuesday.

Did, by the way, you understand my issues with the *sufficiency* of the det $= 0$ conditions? Was I missing something?

15-12-01 *One So Far* (to C. M. Caves)

Just looking at the end of Section III, I was reminded that BFM posted their final version of the paper on `quant-ph` this week. So, they have made some changes. It might be worthwhile to check that they didn't sneak in too drastically different a formulation of the question.

Also, there you (or Rüdiger) write, "In contrast, our derivation is couched wholly in terms of the beliefs of the parties and does not appeal to a real state of affairs; it is therefore preferable in a Bayesian approach to quantum mechanics." I'm not sure—at least as hinted by the present structure of the manuscript—that any nonBayesian will have a clue why it's a preferable statement of the problem. "What's wrong with letting the quantum state reflect an objective fact, that what cannot happen cannot happen?," the reader might ask. "Aren't these CFS guys just paining over inconsequential points?"

Personally, I would like to see some discussion, perhaps in the introduction or in Section III, along the lines of those Bernardo and Smith quotes that I like so much. I'll place them below in case you forgot. I think they give the whole motivation for the work.

Finally, the present manuscript gives pretty short shrift to the Peierlsian ideas that got David's blood up in the first place. Nor do we say much about the apparent inconsistency between the goals and the "solution" of BFM in relation to the Peierls quest.

17-12-01 *Births and Mermins* (to R. Pike & S. J. van Enk)

Well, Kiki informed me a little while ago (it's 3:00 AM now) that she's been in labor since about 9:00 PM last night. So, I'm guessing—it's just a guess—I won't be in tomorrow. (That's a joke.)

I'm hoping you two can hold down the quantum fort while I'm away. In particular, David Mermin should be showing up Tuesday between 2:00 and 2:30. If by wild chance I'm not there—though really I have every plan to be—can one of you escort him around, make sure that he gets to his talk, and then make sure he gets to his room at the Turkey Hill Inn? (And criticize his paper a little bit while you're at it.)

But I think that'll be an utter emergency: this baby should certainly be born before today is out. In particular, I'm planning to be at Bell Labs by 1:00 on Mermin day (Tuesday).

Mermin will be coming to Bell Labs directly from a meeting at Rutgers in New Brunswick. For the heck of it, let me give you his contact number through Tuesday morning: New Brunswick Hyatt, 732-873-1234. But as I say, I should be at Bell Labs before his arrival.

Oh, and I'll cc this note to Caves and Schack so they'll understand why I might be silent on their latest draft.

19-12-01 *Katie Viola Fuchs* (to the world)

Dear Family, Friends, and Colleagues,

Katherine Viola Fuchs came to the world with Monday's sunrise, 17 December 2001. She was 8 pounds, 19.5 inches, and a delight to her parents' eyes—she is beautiful in every way. And like her big sister Emma, Katie has already made it clear that the future will not just unfold before her, but be made in crucial part by her presence and will.

With the greatest of expectations (and a modicum of pride), we send greetings to everyone on Katie's behalf.

Chris, Kiki, and Emma Fuchs

20-12-01 *Even Less Important* (to B. W. Schumacher)

Schumacherism 4: *Mike W. and I just posted the paper I mentioned to you about approximate error correction. (It ought to appear tomorrow at* `quant-ph/0112106`*.) This is not earth-shaking stuff, but it does mean that I can start working on the "Doubting Everett" paper next.*

That's great news, both about your paper with Mike and your "continuing doubt." Jeff Bub and I have just started to firm up the plans for the special issue. I'll let him know that we can count on you.

Schumacherism 5: *By the way, I did mention that we could bring you here to give a talk. [...] It turns out we have some open dates in late January and early February, too. Not that you should drop everything and come amuse undergraduates [...] but if you'd like to come, we'd be delighted to have you.*

Let's think of February as a possibility (but not too firm of one). What would be the latest date I can give you a firm decision? It would certainly be great to get a chance to talk a bit: I've recently made a further, more radical, Bayesian transition (and believe it or not, have had to drag Carl and Rüdiger kicking and screaming into it with me), and I'd like to test it out on as many moderately-sympathetic people as possible before opening my mouth to the general public.

23-12-01 *Princeton and the Bud of Bayesianism* (to J. N. Butterfield)

Thanks so much for the Italian lesson!

The last time we met, you mentioned something about a meeting in Princeton that you were organizing with Bas van Fraassen. Can you remind me of when it will be, so I can get it onto my calendar? Will it be a general philosophy of science meeting, or just something devoted to quantum mechanics? If it's the former, and you don't mind, I would like to bring a local friend-of-the-family with me while I attend. He has a Ph. D. in philosophy from Washington University in St. Louis and is a philosopher of mind and a cognitive scientist; he teaches at a small college nearby (can't remember the name). So, if the meeting is a general one, he'd fit right in, and I would certainly enjoy his company both there and on the drive over, etc.

I sure wish we could get together more often and talk. I very much enjoyed and appreciated your knowledge of *both* de Finetti's and Lewis's ideas. I believe I've made substantial progress in my Bayesianism in the last few months, and it would certainly great to have another test-ear to try it out on (and see it stand or fall) before I foolishly put it in print.

30-12-01 *The Two-Girl Rule* (to D. Petz)

Thanks for the happy note!

Petzism 1: *There is a saying in Hungarian, something like that "A clever man has daughters". I do not know if it is valid in the US but you may try further experimental checkings. (I have two daughters, too.)*

Here are the people (including myself) in quantum information that I can think of with two girls:

Herb Bernstein
Gilles Brassard
Ignacio Cirac
Claude Crepeau (adopted children)
Chris Fuchs

Jeff Kimble
Seth Lloyd
Asher Peres
Dénes Petz
John Preskill
Rüdiger Schack (2 girls, 1 boy)
Ben Schumacher

It is an interesting coincidence.

02-01-02 *Food for Thought* (to C. H. Bennett & J. A. Smolin)

The more I think about it today, the more I'm tickled about the idea of either Charlie or the two of you writing something on thermodynamics and quantum information (rather than Charlie writing a pro-many-worlds piece). I really hope you'll take the bait and view this as an opportunity to get some thoughts straight (including a response to Earman as Charlie suggested) and also to suck the philosophical audience into the excitement of quantum information ... and to let them see that quantum information is *actually* relevant to their subject matter.

Below I'll put a compendium of quotes from some of Jeff Bub's letters to give you a little more orientation on the goal of the project. As soon as I get off my lazy butt, we should be sending out a more formal set of invitations soon.

1. Rob Clifton has asked me whether I'd like to guest-edit a special issue of *Studies in the History and Philosophy of Modern Physics* (SHPMP) on quantum information. Apparently, he first made the suggestion to you some time ago, but since he hasn't heard back from you, he assumes that you are not too keen to take on the job. [...]

   The difficulty will be getting non-philosophers to write a paper that is not just informative for philosophers, but also philosophically convincing and articulate.

2. Well, Bub and Fuchs as editors would be fine with me. But we'd really have to think hard about how to ensure that the authors all deal with some conceptual or foundational question relevant to quantum information in a philosophically serious way. We want to avoid the issue just being 'intro to quantum information for non-specialists,' with some anecdotal interpretative comments as a nod to philosophy. And it also shouldn't just be a forum for one point of view.

   The difficulty, of course, is that very few philosophers have actually worked in this field. So we have to rely mostly or perhaps entirely on scientists for the papers. It will be hard getting papers from physicists and computer scientists for a philosophy journal. On the plus side, it does seem to be the case that some people working in quantum information have thought hard about the relevance of their research for foundational questions. Somehow, we have to motivate people to take it as a worthwhile and exciting challenge to write a paper that tackles a philosophical issue in an intellectually disciplined way – as intellectually disciplined as these authors invariably are when they write a technical scientific paper. Too often, all analytical controls seem to be switched off when scientists venture into philosophical terrain. [...]

I don't think we should tie this to the Montréal meeting. It would probably be better to go with people who have been thinking about a particular foundational issue for a while and whom we can persuade to write a paper on that issue, with the emphasis on philosophical relevance rather than the technical details.

3. This is probably not going to be easy, but I think it could be a very worthwhile project if we can get these people fired up about the idea of writing articles for a special issue that will clearly articulate the significance of the new work on quantum information and quantum computation for foundational and interpretational questions about quantum mechanics. SHPMP is right now the premier journal for foundational and philosophical issues in modern physics. So this is the right place to do this. As I said before, what we don't want is a bunch of articles on dumbed down quantum information for philosophers. Rather, we want a serious and analytically sophisticated attempt to explore foundational issues relevant to quantum information, and to show precisely how looking at quantum information sheds new light on the old questions that plagued Bohr and Einstein.

02-01-02 *Solipsism Story* (to C. H. Bennett & J. A. Smolin)

Here's the other story from Martin Gardner's "Why I Am Not a Solipsist."

> Russell once spoke on solipsism at a meeting chaired by Whitehead. As Russell tells it in his autobiography, he said he could not believe he had written those parts of Whitehead's books which he (Russell) could not understand, although he could find no way to prove he hadn't.

02-01-02 *Greedy Pragmatist* (to C. H. Bennett)

Kiki and I just got through looking through your photo-book. Here are probably the three that strike us the most (in that order):

1. Boat on Dam 1990
2. Surf, Bass Hocks 1967
3. Plant Saucers 1975

It'd be truly wonderful if we could hang any or all of these up in our new house. How about this: If you'd mat them, and label and sign the mats of course, we'd frame them and reimburse you for all the materials (except the love you put into them). Then, soon after that—after we get the house prim and proper—we would invite you and Theo up for the first showing.

Is that greedy of me? Anyway, despite what John says, I'm not a solipsist; I do believe in a reality external to me.

03-01-02 *Your Note* (to R. Balian)

Carl Caves recently shared a note with me that you had written concerning our paper `quant-ph/0106133`. Thank you so much for all the kind words! I am very flattered. Moreover, I will certainly read the papers you recommended to us: I wish we had known of them before sending off our final proofs to the publisher. In any case, I will not make the same oversight twice.

Let me address one of the points in your note:

Balianisme 1: *What I appreciated the most in your work is the use of Gleason's theorem. In my paper of 1989 I had assumed additivity of expectation values even for non-commuting observables, and it is an important progress, both conceptually and technically, to restrict to commuting ones. However, what do you do in the case of a 2-dimensional Hilbert space where Gleason's theorem fails?*

Yes, the two-dimensional case is interesting. There actually turns out to be a way to handle it that continues with the spirit of Gleason, though not by the exact letter of his original assumptions. The trick is to expand one's notion of measurement from the standard von Neumann type to the class of positive operator-valued measures (POVMs). If one does that while retaining the remainder of Gleason's assumptions, then the theorem works even for two-dimensional Hilbert spaces. Moreover the theorem also starts to work for Hilbert spaces over the rational number field—strangely, the original Gleason theorem seems to require the full power of the continuum to be valid—and beyond that, the proof of the theorem becomes essentially trivial (the sort of thing one can sketch on the back of an envelope).

I give what I believe is a fairly convincing argument for such a set of assumptions and a proof of the result in `quant-ph/0106166`. I think you will find the spirited discussion in that reference easy and enjoyable reading and also see that it promotes our common quantum-foundational position in spades.

03-01-02 *Prenuptial* (to R. Jozsa)

In a few days Jeff Bub and I will be sending out some more formal-looking invitations, but I wanted to discuss this with you a little privately first. The plan is to put together an issue of SHPMP (Studies in History and Philosophy of Modern Physics) on the impact of quantum information on quantum foundations and various related philosophical issues.

I know that you've been thinking hard for a long time about writing an article of this nature, and you pretty much (verbally) committed to me once that you'd do it if I could find a decent home for it. (OK, maybe it was during one of our late-night outings!) But, I really think this will be a decent home, and I hope you'll consider doing it.

So, far we have firm commitments from Andy Steane (he's going to put an extension of his "Quantum Computing Needs Only One Universe," which so far hadn't found a home) and Ben Schumacher (who has started to write up his "Doubting Everett" lecture). Beyond that, Charlie Bennett has pretty much said "yes" verbally and even seemed to relish the idea a bit. We'll also be going after Kent, Mermin, Pitowsky, Popescu, and a couple of others (yet to be decided), but we haven't contacted them yet. So I think I can promise you some "relatively established" company ... in case you're worried about reputation and such (since it is a philosophy journal).

As with Charlie Bennett yesterday, I'll place below some quotes from some of Jeff Bub's letters so you can get a better flavor for what the project is about.

I think whatever you wanted to write, it would be great, and we can give you about six months to do it. I hope you know that I honestly believe this stuff and this debate is important (and will be ever more so for the generation after us). Indeed, I'm guessing that a paper from you—it's just a guess, being tinged with mathematical Platonism and a fairly common-sense philosophical realism—would be a good counteragent to anything you might see Mermin and I spurt out ... and I hope you'll see all the more need for your own efforts because of that!!

So I'll leave it at that. I'll hope and pray you say "yes" and await your reply.

04-01-02 *New Breach of Faith* (to C. M. Caves)

Cavesism 59: *I agree wholly with your statement in the following:*

> *Also, there you (or Rüdiger) write, "In contrast, our derivation is couched wholly in terms of the beliefs of the parties and does not appeal to a real state of affairs; it is therefore preferable in a Bayesian approach to quantum mechanics." I'm not sure—at least as hinted by the present structure of the manuscript—that any nonBayesian [would] have a clue why it's a preferable statement of the problem. "What's wrong with letting the quantum state reflect an objective fact, that what cannot happen cannot happen?," the reader might ask. "Aren't these CFS guys just paining over inconsequential points?"*

But I don't quite know what to do about it. Your B& S quotes are nice, but they will just get in the way, in my view, of getting people even to grasp the setting for what we're doing.

Perhaps you misunderstood: I am not asking that the Bernardo and Smith quotes actually be used in the text. What I am asking is that we find a way to convey to the reader why any of this is important. One thing you've got to realize is that more than once, I've heard Mermin describe the reception of the BFM paper with words like these:

Merminition 81: *Sorry, I had the feeling that those few who understood anything at all thought it was pretty obvious, but they were polite anyway.*

What—it seems to me—is our task, is to convey to any readers who had such a reaction as above that they were actually snookered by BFM. The Bernardo and Smith report of what Bayesianism is about builds a context for our efforts—they make it clear why one should expect a hierarchy of conditions (like the ones we explore in the paper) rather than an *absolute* answer.

It is in saying terse, "only established clique"-interpretable (i.e., only preformed-radical-Bayesian interpretable) things like, "In contrast, our derivation is couched wholly in terms of the beliefs of the parties and does not appeal to a real state of affairs; it is therefore preferable in a Bayesian approach to quantum mechanics" ... and just kind of leaving it at that, without any further buffer ... that is going to get us in trouble. Or at least, that is one of my continuing fears whenever I work with you and Rüdiger.

What is being laid in this paper is the groundwork for viewing the quantum state in a way that people—even the Bayesians among us—are not at all accustomed to: Namely, taking the quantum state's subjectivity absolutely seriously and to the extreme. I can't understand how leaning heavily on the motivation for this work can hurt the paper. Indeed it seems far more crucial than the technical results in Section V if you ask me: if no one cares about the results, no one will read them in the first place. (It's not like this stuff can, with almost a single word, be advertised as a new quantum algorithm like Shor's.)

What's a little annoying is that I know that you (from among the three of us) are the one most up to the task of writing a beautiful, yet businesslike introduction. The only thing it seems that you need to be convinced of is that people will actually read this paper if it is well-written (and won't read it otherwise). Strangely, enough, my wacky 45-page papers get read (or at least skimmed): Explain that. It's not the substance a priori (and maybe not even a posteriori), but it might be the style.

Finally, in a last-ditch effort to shore up this point, I'm going to perform a new breach of faith (and just reconcile it with St. Peter when the day comes). [. . .]

OK, I'll say no more on the subject.

Cavesism 60 : *Example: Kimble sat through Rüdiger's talk at Caltech, nodding his head in agreement the whole time, but then it emerged in the discussion afterward that he thought each party had his own copy of the system. He and his student, Andrew Landahl, really had a hard time with understanding that there is any [difference] between "different states" and "different systems." They really just couldn't help using these interchangeability.*

This is an oddity I have encountered before with several people. I don't understand its origin, but it is weird. Even taking an extreme realist view that there are objective quantum systems and objective quantum states, one should be able to detect the matter-property dichotomy in that and not confuse the two. If you have a theory of the confusion's origin, I'd be interested to hear it.

04-01-02 *Once Again* (to C. M. Caves & R. Schack)

Cavesism 61 : *It IS interesting that strong consistency is required so that the firm part of your belief structure can be read off your probability assignments. I would have thought that a Bayesian would want to know what assumptions are required to translate beliefs into probability assignments.*

Let me put the relevant part of my take on that down again:

> It [strong consistency] says (at least in one case) my outward commitment *must* reflect my internal belief—not because I will be imprudent if I don't do so, but because I don't want my friends to see any discrepancy between my beliefs and my actions.

"Outward commitment" means acceptable odds for the bettor. What strong consistency says is that there are cases I should lay my precise beliefs on the table (for public view), even if I didn't have to in order to avoid a sure loss.

Nowhere else does Dutch book do this. For instance—with standard Dutch book—I could internally believe $p(X, Y)$, but nevertheless only accept bets according to $q(X, Y)$ so long as both these are coherent assignments. Is it part of the Bayesian creed to also require honest reporting of my internal beliefs? Maybe it is: but it seems to me that that is something on top of Dutch book (and in fact we know that it is).

I'm not "against" strong consistency, I just want to understand what motivates it other than that it leads to more equations in our paper . . . and thus leads to a more scientific look.

I'm going to try to get to a real library this weekend and dig up those J. Symb. Logic papers by Shimony etc. Maybe that'll demystify things for me.

07-01-02 *Correlation without Correlata* (to N. D. Mermin)

Let's see what kind of reaction the longer quote below gets out of you. "There are, so to speak, relations all the way down, all the way up, and all the way out in every direction: you never reach something which is not just one more nexus of relations."

> In the rest of this essay I shall be trying to sketch how things look when described in antiessentialist terms. I hope to show that such terms are more useful than terminologies

which presuppose what Dewey called 'the whole brood and nest of dualisms' which we inherit from the Greeks. The panrelationalism I advocate is summed up in the suggestion that we think of everything as if it were a *number*.

The nice thing about numbers, from my point of view, is simply that it is very hard to think of them as having intrinsic natures, as having an essential core surrounded by a penumbra of accidental relationships. Numbers are an admirable example of something which it is difficult to describe in essentialist language.

To see my point, ask what the essence of the number 17 is—what it is *in itself*, apart from its relationships to other numbers. What is wanted is a description of 17 which is different *in kind* from the following descriptions: less than 22, more than 8, the sum of 6 and 11, the square root of 289, the square of 4.123105, the difference between 1,678,922 and 1,678,905. The tiresome thing about all *these* descriptions is that none of them seems to get closer to the number 17 than do any of the others. Equally tiresomely, there are obviously an infinite number of other descriptions which you could offer of 17, all of which would be equally 'accidental' and 'extrinsic'. None of these descriptions seems to give you a clue to the intrinsic seventeenness of 17—the unique feature which makes it the very number that it is. For your choice among these descriptions is obviously a matter of what purpose you have in mind—the particular situation which caused you to think of the number 17 in the first place.

If we want to be essentialist about the number 17, we have to say, in philosophical jargon, that *all* its infinitely many different relations to infinitely many other numbers are *internal* relations—that is, that none of these relations could be different without the number 17 being different. So there seems to be no way to define the essence of seventeenhood short of finding some mechanism for generating *all* the true descriptions of 17, specifying all its relations to *all* the other numbers. Mathematicians can in fact produce such a mechanism by axiomatizing arithmetic, or by reducing numbers to sets and axiomatizing set theory. But if the mathematician then points to his neat little batch of axioms and says, 'Behold the essence of 17!' we feel gypped. There is nothing very seventeenish about those axioms, for they are equally the essence of 1, or 2, of 289, and of 1,678,922.

I conclude that, whatever sorts of things may have intrinsic natures, numbers do not—that it simply does not pay to be an essentialist about numbers. We antiessentialists would like to convince you that it also does not pay to be essentialist about tables, stars, electrons, human beings, academic disciplines, social institutions, or anything else. We suggest that you think of all such objects as resembling numbers in the following respect: there is nothing to be known about them except an initially large, and forever expandable, web of relations to other objects. Everything that can serve as the term of a relation can be dissolved into another set of relations, and so on for ever. There are, so to speak, relations all the way down, all the way up, and all the way out in every direction: you never reach something which is not just one more nexus of relations. The system of natural numbers is a good model of the universe because in that system it is obvious, and obviously harmless, that there are no terms of relations which are not simply clusters of further relations.

To say that relations go all the way down is a corollary of psychological nominalism: of the doctrine that there is nothing to be known about anything save what is stated in sentences describing it. For every sentence about an object is an explicit or implicit description of its relation to one or more other objects. So if there is no knowledge by acquaintance, no knowledge which does not take the form of a sentential attitude, then there is nothing to be known about anything save its relations to other things. To insist that there is a difference between a nonrelational *ordo essendi* and a relational *ordo cognoscendi* is, inevitably, to recreate the Kantian Thing-in-Itself. To make that move is to substitute a nostalgia for immediacy, and a longing for a salvatory relation to a nonhuman power, for the utopian hope which pragmatism recommends. It is to reinvent what Heidegger called 'the ontotheological tradition'.

For psychological nominalists, no description of an object is more a description of the 'real', as opposed to the 'apparent', object than any other, nor are any of them descriptions of, so to speak, the object's relation to itself—of its identity with its own essence. Some of them are, to be sure, better descriptions than others. But this betterness is a matter of being more useful tools—tools which accomplish some human purpose better than do competing descriptions. All these purposes are, from a philosophical as opposed to a practical point of view, on a par. There is no over-riding purpose called 'discovering the truth' which takes precedence. As I have said before, pragmatists do not think that truth is the aim of inquiry. The aim of inquiry is utility, and there are as many different useful tools as there are purposes to be served.

Common sense—or at least Western common sense—has trouble with the claim that numbers are good models for objects in general because it seems counterintuitive to say that physical, spatiotemporal objects dissolve into webs of relations in the way that numbers do. When numbers are analysed away into relations to other numbers, nobody mourns the loss of their substantial, independent, autonomous reality. But things are different with tables and stars and electrons. Here common sense is inclined to stick in its toes and say that you cannot have relations without things to be related. If there were not a hard, substantial autonomous table to stand in relation to, e.g., you and me and the chair, or to be constituted out of hard, substantial, elementary particles, there would be nothing to get related and so no relations. There is, common sense insists, a difference between relations and the things that get related, and philosophy cannot break that distinction down.

The antiessentialist reply to this bit of common sense ...

08-01-02 *Correlation without Correlata, 2* (to N. D. Mermin)

Merminition 82: *I don't like the stuff about 17 — very unconvincing. But after that it gets more interesting. It starts to get most interesting just as you cut it off.*

Of course it's not convincing: philosophical mumbles—it seems to me—can't serve that purpose. I was just curious whether it struck any chords with you on how to convey (whatever it is you've been trying to get at with) your "correlation without correlata." Is it a good metaphor? Does it carry any Ithacan soul?

Personally, I thought the analogy was nice from the second I read it. Even the primeness of a number, for instance, can only be defined by invoking the existence of all the other numbers. Numbers just don't have any properties in and of themselves (or at least none that I could think of).

You're not going to QIP at IBM next week, are you?

08-01-02 *Information → Belief → Hope ??* (to N. D. Mermin)

I started reading through the essay again and realized that if I were to fulfill your request, I might just have to copy the whole damned article. I give up! But at least I was kind enough to check that you have the source in your library at Cornell (call number and location below).

The essay is titled "A World without Substance or Essences." It should probably be read in conjunction with the essay previous to it, "Truth without Correspondence to Reality." The book as a whole is entertaining, but it is a little skimpy on firm argument. (Though, the author admits it is an attempt at popularization ... so that kind of makes it OK in my mind.) He's certainly not the devil that Steven Weinberg labeled him. But I will admit, his version of pragmatism may go too far for my tastes: I'll hold on to the final verdict for a while. (William James is still the best bet in my eyes.)

PS. The title to this note is a joke, based on Rorty's own title (and some of the discussions in his book). However, I am seriously toying with the idea of making a distinction between "beliefs" and "commitments." I.e., saying that a quantum state ascription is explicitly a "commitment" rather than a "belief"—a commitment to behaving one way or the other in the face of some experimental data (yet to be gathered). Sometimes commitments explicitly correspond to beliefs (as Dutch-book takes to be a definition), but it seems to me not always the case.

But as I say, I'm just toying with the idea: I know Caves and Schack will have a cow and beat me up if I have enough nerve to say anything about it. So, I'd better be sure of myself.

08-01-02 *Help with ET Quote* (to C. M. Caves & R. Schack)

Cavesism 62: *I've been trying to remember without success where ET discusses his ability to toss coins à la what we saw from Dan Greenberger in Växjö. Can you help?*

I don't know that I've ever actually read the example: I've just heard stories of it (probably through Rüdiger), and maybe words to that effect as a section heading or something. I have a faint memory that it was somewhere in Jaynes's big book, but I couldn't find it in the table of contents. Rüdiger's probably a better person to ask.

One interesting thing I did find in looking for the answer to your question though, is a Jaynesian diatribe against a Dutch-book foundation for probability theory. It's in Appendix A of his book. The fear he expresses there strikes me as not so different from what I was reading into your worries about my fall into "radical Bayesianism."

08-01-02 *Term Origin* (to C. M. Caves & R. Schack)

Here's a little (unfortunately inconclusive) discussion on the origin of the "Dutch book" term:

`http://web.archive.org/web/20060524181532/http://www1.fee.uva.nl/creed/wakker/miscella/Dutchbk.htm`.

08-01-02 *Rorty on Religion* (to J. W. Nicholson)

I wish I had had these quotes in stock when we were having our conversation the other night. But I hadn't gotten that far in the book yet ... if I were only a quick reader like you. [Disclaimer:

My copying these quotes for your thought (and our continued discussion) neither represents an endorsement for or against their content.]

From the essay "Religious Faith, Intellectual Responsibility and Romance" in the R. Rorty, *Philosophy and Social Hope*:

page 153:

> If one accepts that claim, one will have reason to be as dubious as James was of the purportedly necessary antagonism between science and religion. For, as I said earlier, these two areas of culture fulfill two different sets of desires. Science enables us to predict and control, whereas religion offers us a larger hope, and thereby something to live for. To ask, 'Which of their two accounts of the universe is true?' may be as pointless as asking, 'Is the carpenter's or the particle physicist's account of tables the true one?' For neither question needs to be answered if we can figure out a strategy for keeping the two accounts out of each other's way.

page 156:

> Pragmatists are not instrumentalists, in the sense of people who believe that quarks are 'mere heuristic fictions'. They think that quarks are as real as tables, but that quark talk and table talk need not get in each other's way, since they need not compete for the role of What is There Anyway, apart from human needs and interests. Similarly, pragmatist theists are not anthropocentrists, in the sense of believing that God is a 'mere posit'. They believe that God is as real as sense impressions, tables, quarks and human rights. But, they add, stories about our relations to God do not necessarily run athwart the stories of our relations to these other things.
>
> Pragmatist theists, however, do have to get along without personal immortality, providential intervention, the efficacy of sacraments, the Virgin Birth, the Risen Christ, the Covenant with Abraham, the authority of the Koran, and a lot of other things which many theists are loath to do without. Or, if they want them, they will have to interpret them 'symbolically' in a way which MacIntyre will regard as disingenuous, for they must prevent them from providing premises for practical reasoning. But demythologizing is, pragmatist theists think, a small price to pay for insulating these doctrines from 'scientific' criticism. Demythologizing amounts to saying that, whatever theism is good for, it is not a device for predicting or controlling our environment.

page 157–158:

> I said earlier that many readers of 'The Will to Believe' feel let down when they discover that the only sort of religion James has been discussing is something as wimpy as the belief that 'perfection is eternal'. They have a point. For when Clifford raged against the intellectual irresponsibility of the thesis, what he really had in mind was the moral irresponsibility of fundamentalists — the people who burnt people at the stake, forbade divorce and dancing, and found various other ways of making their neighbours miserable for the greater glory of God. Once 'the religious hypothesis' is disengaged from the opportunity to inflict humiliation and pain on people who do not profess the correct creed, it loses interest for many people. It loses interest for many more once it is disengaged from the promise that we shall see our loved ones after death. Similarly, once science is disengaged from the claim to know reality as it is in itself it loses its appeal for the sort of person who sees pragmatism as a frivolous, or treasonous, dereliction of our duty to Truth.

09-01-02 *Doin' the Dutch-Book Zombie* (to H. Mabuchi)

Hey MacArthur boy,

It's a funny thing being a parent of a newborn child. It leads to a kind of zombie-like state most every night: You're never neither really awake nor asleep. And then your mind gets hung up on some little thing—tonight being the Dutch-book argument—and you just repeat it over and over, as if in a trance. Does that build any imagery for you?

Anyway, in looking up Andrew's email address last night (or was it this night? — time stops), I ran across your web page. The flattery to be listed among your collaborators! (Keep it there!) But you need to get the affiliation right — Bell Labs, Lucent Technologies.

Philosophically, lately, I've been taken away by William James and John Dewey. I've been reading their stuff with a pretty voracious appetite (and there is a heck of a lot of it). But of course one thing leads to another—just like that marijuana—and last week I found myself picking up a copy of Richard Rorty's *Philosophy and Social Hope*. His flavor of pragmatism goes maybe too far even for me, but he writes well and I find it easy to read him. I guess I just write you all this to tell you he speaks highly of your friend Derrida! One of these days, I am going to get the nerve to approach that man. (By the way, Mermin told me recently that your old professor Bas vF has been reading my papers ... and, apparently, disagreeing with them ...)

I'm going to slip back under the covers now; like Nosferatu I keep a little fresh earth there for comfort.

Just a sleepy note to let you know I miss you sometimes.

> He did the mash
> He did the monster mash
> The monster mash
> It was a graveyard smash

12-01-02 *Raussendorf-Briegel* (to S. J. van Enk)

Just thinking a little more deeply about the Raussendorf-Briegel model, here's another thing I like about it. Note that in the Nielsen-Leung models, one has to make a strict assumption about the post-measurement states in order to make it work. That is, one is not free to have any old quantum operation (allowed by the Kraus specification) associated with the POVMs that are measured: The measurements must be of the class of "nondestructive measurements" that Debbie talked about yesterday. On the other hand the RB model has no such restriction. The reason for this is because as soon as each measurement is done, they can simply throw the measured qubit away; it's never touched again.

I know this was obvious, but it just had never quite stood out for me before. The main point it brings out—for me anyway—is that the exponential speedup of quantum computations really must just be a property of the nonclassical correlations that one can generate from such an entangled state. And in that way, the exponential speedup appears to be deeply connected (or at least deeply analogous) to the violation of Bell inequalities.

13-01-02 *Princeton Envy* (to H. Mabuchi)

Mabuchism 1: *Sounds like domestic life is treatin' you good!*

Wasn't it Freud who said, once a school girl sees your Princeton, she'll know what's missing from her life and be envious thereafter?

Well I'm not a school girl, but I saw your Princeton the other day (ahem), and it certainly did start a deep yearning in me. What a wonderful place! This was my first real trip there since moving to New Jersey, and I was enchanted all day. I found myself thinking, what I wouldn't give to live the rest of my life in this little cloister.

The thing that really struck me was the immense resources at one's finger tips. I found myself copying a little over $35 of articles in Firestone Library! I couldn't believe it: They had the complete collection of the *Danish Yearbook of Philosophy* and (shockingly) 35 years of the *Transactions of the Charles Sanders Peirce Society*, and I knew that those were just the tip of an iceberg. I was in heaven.

Emma and I play this game: I say, "When you go to college, I hope you'll go to Harvard." She says, "I want to go to Princeton." Or if I say Princeton, she says Harvard—it's always the opposite. (You can see a trend in our relationship starting to form.)

But that's just an aside (to tell you that the grass is always greener). Keep up the good work with all those good students. Get them to read William James' *Pragmatism* and tell them that quantum mechanics is a much better motivation for all that he said there ... but to never lose sight that the real goal is to get to where he wanted to go.

13-01-02 *Reality in the Differential* (to N. D. Mermin)

Merminition 83: *Actually I'm leaving it in the original form for now, but you're giving me a very hard time here.*

Let me say touché, and then little more than that. Strangely, actually, I've been having a little conversation of my own with You this week. And I've been planning to write you—i.e., the one with the little y—all about it. But what happens? Now that I've got a little time this Sunday morning, I'm finding that the inclination is leaving me.

So this note is going to come out far weaker than I had planned. The main thing was to build a conversation around another Rorty quote, and to tell you how pleased I am with the pragmatism movement in general. I'm finally finding a philosophy so close to what I'm looking for that I'm willing to advertise it to my friends.

Somehow they go a little too far for me, but there are so many beautiful gems I keep finding in their stuff that I find it better not to dismiss it outright. The second paragraph below struck me especially last week. It might as well be about my latest quantum interpretation thoughts.

The reality is in the differential. The quantum state represents a (gambling) commitment on the part of the agent; it never represents anything external to that agent. If you're looking for where the "reality" of the external world creeps into the formalism, you should look to how these commitments change. That's what I've been trying to say for a few months now ... but I guess you already understood that (as maybe witnessed by our last real conversation, during your visit to Morristown).

I had a pretty happy-sad week this week you might say. The happiness was that I visited Princeton for the first time since moving up here. I was like a kid in a candy store!! I ended up copying over $35 of articles in their wonderful library (which even subscribes to the *Danish Yearbook of Philosophy*!). The sadness was in that I had to leave that environment and go home at the end of the day; it wasn't home itself.

I'm going to write you that longer James-Dewey note eventually. But right now I'll just leave you with a reminder that the reality is in the differential. From: R. Rorty, "The Pragmatist's

Progress: Umberto Eco on Interpretation," in his book *Philosophy and Social Hope*, (Penguin Books, New York, 1999), pp. 131–147:

> As I see it, the rocks and the quarks are just more grist for the hermeneutic process of making objects by talking about them. Granted, one of the things we say when we talk about rocks and quarks is that they antedate us, but we often say that about marks on paper as well. So 'making' is not the right word either for rocks or for marks, any more than is 'finding'. We don't exactly make them, nor do we exactly find them. What we do is to react to stimuli by emitting sentences containing marks and noises such as 'rock', 'quark', 'mark', 'noise', 'sentence', 'text', 'metaphor' and so on.
>
> We then infer other sentences from these, and others from those, and soon—building up a potentially infinite labyrinthine encyclopedia of assertions. These assertions are always at the mercy of being changed by fresh stimuli, but they are never capable of being *checked against* those stimuli, much less against the internal coherence of something outside the encyclopedia. The encyclopedia can get *changed* by things outside itself, but it can only be *checked* by having bits of itself compared with other bits. You cannot *check* a sentence against an object, although an object can *cause* you to stop asserting a sentence. You can only check a sentence against other sentences, sentences to which it is connected by various labyrinthine inferential relationships.
>
> This refusal to draw a philosophically interesting line between nature and culture, language and fact, the universe of semiosis and some other universe, is where you wind up when, with Dewey and Davidson, you stop thinking of knowledge as accurate representation, of getting the signs lined up in the right relations to the non-signs. For you also stop thinking that you can separate the object from what you say about it, the signified from the sign, or the language from the metalanguage, except *ad hoc*, in aid of some particular purpose.

13-01-02 *Indulgence* (to G. Brassard)

By the way, did I ever tell what I ended up doing on the single malt end? I took all your suggestions: I bought a Glenmorangie 18, a Talisker 10, and a Laphroaig 15. (The Laphroaig wasn't cheap: $77.50.) It made for quite a Christmas!

Too bad you're not going to QIP this week. I'm pretty sick (some kind of ear infection along with conjuctivitis in my eyes), but I'm going to be there. (Just to infect everyone!) By the way, have you seen the Raussendorf and Briegel model of quantum computation? I.e., the model where one starts with a fixed entangled state for all computations (of a given spacetime size), and then simply performs a sequence of single qubit measurements thereafter to enact the particular computation one cares about. I think this is a really very deep achievement. Briegel will be at the meeting; I'm really looking forward to seeing that.

21-01-02 *R, B, and P* (to A. Peres)

Asherism 17: *I am happy that QIP2002 went well. I saw the papers of Briegel and Nielsen on* `quant-ph` *(please remind me the numbers, if you have them ready). I was not favorably impressed, maybe I misunderstood them, and I should read them again.*

Actually, the better work is the Raussendorf/Briegel stuff. If you were not favorably impressed, then I think you should give it another chance. I think it is a beautiful construction, and, in fact, the deepest thing I've seen in quantum computing for 2 or 3 years now. The first paper to start with is `quant-ph/0010033`. Then more details can be found in `quant-ph/0108067`, `quant-ph/0004051`, and `quant-ph/0108118`.

What is deep about this work is that all computations start off with the SAME given entangled state for the qubits. That is to say, a given entangled state is taken as a resource for the task. Thereafter the particular computation one is interested in is enacted by making single-qubit measurements alone: there are no further unitary evolutions. I think that is quite remarkable and quite lovely.

An interesting feature of the Raussendorf/Briegel model (as opposed to the Nielsen and Leung models) is that one need to take into account NO details of the post-measurement state for the measured qubits: they can be thrown away immediately after the measurement. And because the measurements are localized, the post-measurement state of the remainder of the qubits is fixed completely by the POVM (rather than the operation). That is to say, for the relevant qubits in this model, "an effect only has one operation."

23-01-02 *Book Review of Nielsen & Chuang's Book for American Journal of Physics* (to L. K. Grover)

Good review, except I disagree with the following statement violently.

Groverism 1: *Another rule that classical systems satisfy is something called the locality principle (anything that happens depends on local conditions, not on something that exists or happens far away). This is something that quantum systems frequently violate. This led to a number of paradoxical situations such as the EPR paradox. When Einstein and others first discovered this, they thought they had disproved quantum mechanics.*

If one takes the view (as, say, Caves, Cleve, van Enk, Heisenberg, Mabuchi, Mermin (somewhat), Milburn, Pauli, Peierls, Peres, Rudolph, Schack, Wheeler, Zeilinger, and I do) that quantum states are not objective properties of systems, but rather subjective states of belief or knowledge about systems, then quantum mechanics presents no evidence that the locality principle—as you define it above—is violated.

Beyond that, I would say you have the Einstein argument incorrect. Einstein held fast to the *assumption* of locality and never budged from it. The conclusion he drew from EPR situations—where localized measurements on entangled systems cause quantum-state updates for far away systems—is that the quantum state must stand for one's information/knowledge/belief rather than for something objective and observer-independent. In his own words, the quantum state can only amount to an "incomplete description." (See typical Einstein quote below.) Thus he never felt that he had "disproved" quantum mechanics: he only thought that there was *more* to say about nature than quantum mechanics had to offer.

You can find wordier versions of this argument, along with historical references, in Section 3 of my paper `quant-ph/0106166`.

The difference between Caves, Peres, Schack and myself—these guys' opinions I know for sure—from Einstein, is not that we reject his EPR-type argument for concluding that the quantum state must be subjective (i.e., dependent on the information any particular observer has gathered, etc.), but that we find no indication in this that quantum mechanics is incomplete ... and, thus, that there is more to say about nature than quantum mechanics can offer.

> Here is the way Einstein put it to Michele Besso in a 1952 letter [found in J. Bernstein, *Quantum Profiles*, (Princeton University Press, Princeton, NJ, 1991)]:

What relation is there between the "state" ("quantum state") described by a function ψ and a real deterministic situation (that we call the "real state")? Does the quantum state characterize completely (1) or only incompletely (2) a real state? ...

I reject (1) because it obliges us to admit that there is a rigid connection between parts of the system separated from each other in space in an arbitrary way (instantaneous action at a distance, which doesn't diminish when the distance increases). Here is the demonstration: ...

If one considers the method of the present quantum theory as being in principle definitive, that amounts to renouncing a complete description of real states. One could justify this renunciation if one assumes that there is no law for real states—i.e., that their description would be useless. Otherwise said, that would mean: laws don't apply to things, but only to what observation teaches us about them. (The laws that relate to the temporal succession of this partial knowledge are however entirely deterministic.)

Now, I can't accept that. I think that the statistical character of the present theory is simply conditioned by the choice of an incomplete description.

27-01-02 *What Would William James Say?, 1* (to J. W. Nicholson)

Of course I'm reading email in Texas! (Or at least that's what I'd say.) I'll send your regards to my Mom.

Nicholsonism 1: *I had just settled down to a lovely lunch with my new issue of* Scientific American *and was perusing the various articles, when I came across a regular column by Michael Shermer titled "Skeptic." Now normally I enjoy this column (it's all about debunking psuedo-science and public misconceptions) and this month's topic was evolution vs creationism. It got off to a good start with a quote by Richard Dawkins — "the universe we observe has precisely the properties we should expect if there is, at bottom, no design, no purpose, no evil and no good, nothing but blind, pitiless indifference." I rather liked that.*

That may have been true at one time, i.e., at some stage in the development of the world. That's how it got off to a start so to speak. But now, I go the experimentalist (who does the hard work in helping the theorist construct his theories), and ask him, "What ya doin?" He says, "Twiddlin' knobs." I ask, "How come?" He says, "I'm tryin' to hep this wacky friend of mine. He wants to get a theory of how iodine reacts to this and that? I'm chartin' it out, givin' him some clues." I ask, "How do you do that?" He says, "By twiddlin' these knobs."

The present stage of the universe doesn't look like it's full of blind indifference to me.

The initial condition is always left separate from the theory; there's a reason for that. How else would the experimentalist be able to twiddle his knobs?

27-01-02 *What Would William James Say?, 2* (to J. W. Nicholson)

Nicholsonism 2: *Anyway, I was far more interested in your reaction to the quote requiring God's absence from standard scientific theories.*

The point was, it doesn't even require the scientific agent's absence from the ultimate gears and pinions of the world, much less a god's. The scientific world view consists essentially of two components: theories and initial conditions. And, it seems to me, it is a tacit assumption of the whole scientific enterprise that the experimentalist can freely set the initial conditions he wishes to.

Nicholsonism 3: *If the universe really wasn't blindly indifferent, people wouldn't fly airplanes into buildings, convinced they were on their way to heaven.*

There's something about this sentence I just don't like. I'll try to put my finger on it in the next couple of days.

29-01-02 *Quotes That Moved Me Once* (to J. W. Nicholson)

Nicholsonism 4: *Only in the best of cases does an experimentalist have access to initial conditions. Consider astronomers or evolutionary biologist who are left to mine a data trail, long after the fact. And, I imagine a developmental psychologist who tried to monkey around with initial conditions would quickly find himself slapped with a malpractice lawsuit.*

Concerning your last note to me, here are some quotes that moved me once. Maybe they do a more convincing job than I did, maybe they don't. I'd be interested to hear your reaction.

- D. J. Bilodeau, "Physics, Machines, and the Hard Problem," J. Consc. Stud. **3**, 386–401 (1996). The parts of this paper on quantum mechanics and the practice of science in general are absolutely excellent.

> Even apart from the limits to measurement revealed by QM, there have been good reasons to doubt the power of physical observation to penetrate to ontological foundations. I will focus on one of these, which is fundamental but not widely understood—the idea of dynamics.
>
> The geometric idea of the physical seems simpler than it really is—there are simple geometrical entities (perhaps Newtonian particles) which inhabit physical space, which move and interact according to simple physical laws and form larger structures which are the objects of our ordinary experience. It might seem at first that, with the tools of modern science, the structure and nature of such a world would be open to observation with no conceptual difficulties. But, of course, actual observations require that we *use* this world in order to observe it. We must manipulate and modify certain parts of it in order to create situations (experiments) in which information about the structure of other parts can be conveyed to us. Some measurements necessarily preclude others. We cannot dissect a microscope at the same time we are using it to study a sample of brain tissue (much less use the same brain tissue to think about the microscope, etc.). In pure geometry, the intellect can wander over every detail of a geometric structure, as one can gaze over a blueprint or electronic schematic diagram, going here and there and back again at will. But the physical observer has no such freedom. It is impossible to examine every point in space and time. We will never be able to obtain more than an exceedingly tiny fraction of the total information contained in the structure of the universe (to the extent that it can be considered a structure).

and

The physicist *acts* and intervenes in nature. Typical situations are limited and determined in part by what the experimenter can manage to contrive. The idea of causality in physics depends on the freedom of the experimentalist to alter the conditions of the experiments. I first gained an appreciation of the importance of this idea from an argument by Roger Newton:

> The most practical and only foolproof method of scientifically testing a causal connection between A and B is 'wiggling' one of them and watching the response of the other. We are not interested here in what might be called 'historical causality' (establishing a causal connection in a single chain of events) but in 'scientific causality' (establishing such a connection in repeatable events) . . . It is the external control of A together with the correlation with B that establishes, in a good Humean sense, the causal connection between them, as well as the fact that A is the cause and B, the effect.

This observation contains a great profundity. The laws of physics are dynamical—they are not laws of being but laws of action. They are human constructions based on our experience of nature and in form and concept derive from our role as creative agents. [Footnote: When I write of the physicist's 'freedom' of action in setting up experiments and controlling parameters, I am not taking a position on the philosophical question of 'freedom of the will.' I mean here only a pragmatic freedom which is independent of the physical entitities being observed.] The importance of action can be seen in the advance of celestial mechanics from Kepler to Newton. Kepler observed the patterns of the motion of the planets and distilled them into three laws which described the elliptical shape of orbits and the speed with which the planets move along them at each point. These are purely geometrical and kinematical ideas. Newton took Galileo's work on the motion of physical objects developed in experiments and refined it and extrapolated the concepts of gravitational force and mass (both dynamical rather than purely geometrical notions) out into the solar system and so was able to derive Kepler's laws from his own dynamical laws of inertia and gravitation.

The dynamical description of cause, force, and law stands in contrast to the 'historical' denoting of particular things and events in the course of time. These are two 'modes of description' which are equally essential to any account of the physical world.

- D. Bilodeau, "Why Quantum Mechanics is Hard to Understand," LANL eprint `quant-ph/-9812050` (1998). This paper was submitted to and rejected from the journal *Foundations of Physics*; its most likely permanent home will be the LANL eprint archive.

A thing is historical insofar as it is objective (can be observed and treated as an object). It then enters into the realm of recordable objective occurrences which can be ordered in historical space and time. It is dynamical insofar as it is defined as an abstract element of the dynamical theory which explains causal relationships between objects. . . .

Imagine that we could see the universe as omniscient external observers, all space and time at once, and that what we "see" is a tangle of intersecting particle world-lines (cf. Ch. 1 of Misner, Thorne, and Wheeler). We might detect some patterns which would constitute physical rules or laws in some sense, but it would be quite difficult or impossible to know whether we had found all the important patterns, or to distinguish significant relationships from accidental ones. Even more difficult would be to translate this omniscient description into the kinds of relationships and laws which would be observed by the huge clumsy bunches of world-lines which constitute ourselves.

When we set out to investigate Nature, we are not like that external omniscient observer at all. We look for relationships and patterns in the behavior of objects we know. We want to find out—does this kind of object always behave this way under these circumstances? The phrases "this kind," "this way," and "these circumstances" imply the ability to abstract relevant or significant features from what are really unique events. They also imply that we can find or (even better) set up many instances of these typical situations. The result is that the concepts we develop to describe physical phenomena depend not only on what we can observe, but also on what we can do.

To say that A affects or causes or influences or interacts with B implies a counterfactual: If A had been different, B would have been different, too. The most convincing way to establish a connection is to "wiggle" some parameter in A more or less randomly and then observe the same odd pattern showing up in some property of B. If I want to know whether a wall switch controls a certain light, I can flip the switch on and off and observe whether the light follows my actions. There is always the possibility that the light is being controlled by someone else or goes on and off spontaneously; but if I put the switch through a very irregular and spontaneous sequence of changes and the light still follows along, then the probability of a causal connection is very high (barring a conspiracy to deceive the experimenter).

Physical theory is possible because we *are* immersed and included in the whole process—because we can act on objects around the us. Our ability to intervene in nature clarifies even the motion of the planets around the sun—masses so great and distances so vast that our roles as participants seems insignificant. Newton was able to transform Kepler's kinematical description of the solar system into a far more powerful dynamical theory because he added concepts from Galileo's experimental methods—force, mass, momentum, and gravitation. The truly external observer will only get as far as Kepler. Dynamical concepts are formulated on the basis of what we can set up, control, and measure.

- H. Primas, "Beyond Baconian Quantum Physics," in *Kohti uutta todellisuuskäsitystä. Juhlakirja professori Laurikaisen 75-vuotispäivänä* (Towards a New Conception of Reality. Anniversary Publication to Professor Laurikainen's 75th Birthday), edited by U. Ketvel (Yliopistopaino, Helsinki, 1990), pp. 100–112.

The methodology of experimental scientific research and engineering science is to a large extent characterized by the regulative principles emphasized by Francis Bacon. It is a tacit assumption of all engineering sciences that nature can be *manipulated* and that the initial conditions required by experiments can be brought about by interventions of the world external to the object under investigation. That is, *we assume that the experimenter has a certain freedom of action which is not accounted for by first principles of physics.* Without this freedom of choice, experiments would be impossible. Man's free will implies the ability to carry out actions, it constitutes his essence as an actor. We act under the idea of freedom, but the topic under discussion is neither man's sense of personal freedom as a subjective experience, nor the question whether this idea could be an illusion or not, nor any questions of moral philosophy, but that *the framework of experimental science requires the freedom of action as a constitutive though tacit presupposition.*

> The metaphysics of Baconian science is based on the confidence that only the past is factual, that we are able to change the present state of nature, and that nothing can be known about nature except what can be proved by *experiments*. Francis Bacon's motto *dissecare naturam* led to a preferred way of dividing the world into object and observing systems. An experiment is an *intervention* in nature, it requires artificially produced and deliberately controlled, reproducible conditions. In *experiments* in contradistinction to *observations* – one *prepares* systems in initial states, *controls* some of the variables, and finally *measures* a particular variable. The regulative principles of Baconian science require *power to create initial conditions*, stress *the facticity of the past* and *the probabilistic predictability of the future*, and reject *teleological considerations*.

29-01-02 *Qunix* (to J. M. Renes)

Renesism 6: *In other news, I discovered a really interesting paper (actually an undergrad thesis) by a guy at Oxford, who argues against Deutsch's view of the quantum world. One thing he takes to task is the notion that information is physical (which I think I've now discarded).*

Yeah, I met Timpson in Ireland and really enjoyed his company. I'll definitely look at his thesis. Right before Rolf died, I had wanted to write him a letter telling how much I had come to disliking the phrase "information is physical." I wanted to tell him that I think a far more appropriate phrase would be "information carriers are physical." In fact, I told Charlie Bennett about this—at the time—and he told me, "Too late; Rolf just had some fraction of his brain removed last week."

Concerning myself, I've gotten further carried down the path of pragmatism. I've even read some of the Rorty blend now. Indeed my latest little epiphany hit me last week (during an operating-systems talk in our center) when I came up with the following slogan: "A physical theory really amounts to little more than a programming language." Its rules, its specifications are more about the ways we've come to naturally manipulate the world than anything intrinsic to that same world. I tried to say this in my Oct 4 letter to Carl ("Replies on Pots and Kettles" in the new mini-samizdat), but I think the new slogan says it better.

29-01-02 *Växjö Contribution* (to C. M. Caves)

You know, if my true love is philosophy—Bennett calls it theology in my case, actually—yours is certainly sports. You're just not going to let this go without one hell of a fight, are you?

Cavesism 63: *So it appears that I'm in a pickle with evolutions that are mixtures of unitaries, since quantum operations don't have unique decompositions. But of course, all the decompositions into things other than unitaries aren't of interest, and I've been able to "show" (a number of half-baked steps here) that mixtures of unitary EVOLUTIONS are unique, at least in a sense that's good enough for me. To put it more precisely, I've shown it for qubits and think I can go further.*

I'm wondering what the "sense that's good enough for me" is? For instance, I already think of the depolarizing channel, where it doesn't matter which x, y, and z axes I use for defining the Kraus operators. But you probably have something up your sleeve that will be more instructive than that.

Cavesism 64: *For pure states the problem shows up as an inability to make a clean distinction between objective and subjective probabilities,* And for the channel maps, I would say the problem shows up as an inability to make a clean distinction between objective and subjective probabilities. [Just like my double footnote in the NATO paper, I meant this to be taken seriously.] The only difference now is that the probabilities are of a conditional type, $p(y|x)$. But I know that this is too cryptic for you to make any sense of it at the moment, and I already write you too much preachy email: I just need to try to write a damned paper and be done with it.

29-01-02 *Lock In* (to A. M. Steane)

I'm sorry, I haven't kept you up to date on how this special issue project of SHPMP is going. Anyway, it is now definitely going to happen, and I hope we can still count on your commitment of publishing the "one universe" paper there.

So far, we've got definite yes's from Ben Schumacher, Howard Barnum, Richard Jozsa, and Itamar Pitowsky. We've got a high-probability yes from Charlie Bennett. And we've just heard from Carl Caves clamoring to write a paper on "Can Hamiltonians Be Taken as an Ontology for Quantum Mechanics?" if Jeff can be convinced that it's appropriate. Also there's a good chance that David Mermin will be saying yes.

All papers will be due mid-June of this year.

Hopefully Jeff Bub and I will be making an official announcement to all concerned sometime next week.

29-01-02 *A Summer Masterwork?* (to N. D. Mermin)

Merminition 84 : *Funny that you should find Rorty appealing. I've liked a lot of what he says too, although he is Public Enemy Number 1 for most of the scientists engaged in the Science Wars. No Geneva convention for him!*

You emerge! It's so good to hear from you again. If you have read Rorty, then why in the hell did you never tell me, "Chris, what you're shooting for in quantum mechanics really sounds a lot like pragmatism!"? You know you really might have saved me—more importantly this program I dream of—a lot of time! For instance, I might not have wasted years thinking that a pure-state assignment ought to be unique, if I had just read a little James, Dewey, and Rorty.

Anyway, I continue to go further off the deep end. Here's what I wrote Renes this morning:

> Concerning myself, I've gotten further carried down the path of pragmatism. I've even read some of the Rorty blend now. Indeed my latest little epiphany hit me last week (during an operating-systems talk in our center) when I came up with the following slogan: "A physical theory really amounts to little more than a programming language." Its rules, its specifications are more about the ways we've come to naturally manipulate the world than anything intrinsic to that same world. I tried to say this in my Oct 4 letter to Carl "Replies on Pots and Kettles" in the new mini-samizdat), but I think the new slogan says it better.

However, I write this letter for another reason. The way I view it, you play a unique role in our community: You really do "write physics" every bit as much as you exhort your readers to in that nice essay on your webpage. I say this because I would dearly love you to make a statement, a solid statement, of where you think quantum information can have its greatest impact on settling quantum foundations questions. Our community needs this kind of incitement, and you have the writing skills that might even convince someone to do something about it. Even if you wrote a paper of nothing but questions, it would be great and a great service.

Would you think hard about doing that? It could be your masterwork for the year. Jeff Bub and I have finally gotten to the point of organizing a special issue of SHPMP, and your contribution could help set a good tone for it.

Below, let me copy a couple of letters I've already written in this regard. They'll fill you in on all the details of the way Jeff and I see the project. [...]

What say you?

30-01-02 *Sweet Talk* (to N. D. Mermin)

Merminition 85: *Saying that I've read Rorty is a gross exaggeration. I've dabbled around and found a curious mixture of interesting and outrageous assertions. Beware of becoming a "postmodernist" yourself.*

Funny that so many are viewing this as a new kind of onslaught to science. By his own admission, Rorty is not saying anything particularly different from James and Dewey. And my reading is starting to confirm that. So, it looks like the train of thought started before the turn of the (last) century. What is postmodern about it?

That said, the only thing I have ever wanted is a sensible of view of what quantum mechanics is about. If it takes rearranging our thoughts about what the classical world (and classical physics) is about, then so be it. To that extent—I think—I run the danger of becoming a postmodern. The other day Hans Briegel was visiting and he saw James' book *The Meaning of Truth* sitting in the back seat of my car. He ended up asking what pragmatism is about. I said, "You know, these guys didn't know anything about quantum mechanics, but I might venture to say it can be summarized as a Copenhagen interpretation of classical physics."

Merminition 86: *Actually it's interesting the way foundations of QM have come up again and again in the science wars, from Shelly Goldstein's attack on all of physics in the notorious NY academy volume, to Mara Geller's attack on Bohr, to my occasional mutterings that nobody whose thought hard about foundations of QM could possibly think science is as simple as Gross and Levitt would like it to be.*

1) What is the "notorious NY academy volume"? You've piqued my interest.

2) I love it: you made the same spelling slip in Mara Beller's name in an email of November 28, 1999!

3) Who are Gross and Levitt?

Merminition 87: *As for your latest attempt to sweet talk me into doing something reckless, if I thought I had anything useful to say on that subject I would have said it, instead of beating "Whose Knowledge" into the ground (and possibly many feet under the ground). I will, however, keep the invitation in mind as I work my way through the second edition of my Qcomp course.*

If I thought it might help, I'd even say "Pretty please, with sugar on top." More seriously, I think the only thing that would be reckless would be for you to NOT use these post-65 years to act a little like a more sober version of John Wheeler and get the physics community going to its next great stage. I know that you believe that quantum information and computing have something deep to contribute to quantum foundations studies. Why do you believe that? Can you articulate it? Why do you think that Peierls might have been on the right track? Or do you even think that? Why have you changed your stand somewhat on nonlocality? Why have you privately backed out a little on your initial statement of the IIQM? What problems did you foresee? What problems were brought to your attention? What part of the interpretation still stands a chance of being useful? What troubles you about Everett and Deutsch's interpretation? What troubles you about Griffiths' interpretation? What troubles you about Bohm and Goldstein's interpretation? What troubles you about Zurek's interpretation? *All* of these people *claim* to have long since solved all the greatest mysteries of quantum mechanics. Why should we not be listening to them? Have you ever gathered all your thoughts on all these questions?

You can't tell me that you don't already have a LOT to say, even while . . . all the while . . . you are telling yourself that you have nothing useful to say. Most importantly, you have the means at your disposal for saying these things in a way that people will listen and think about them. Even a study in self-indulgence, i.e., a reflection on why you've come to the positions that you have and why you remain perplexed would be *immensely* useful to the community.

Have you ever analyzed why I send so much email to you? I mean, "you" in particular? If I had to put my finger on it, it would be for two reasons: 1) Because I've always felt that your high standard for your own writing has induced me to a higher standard for mine. And 2) because you are the least dogmatic and most clear-thinking devotee of quantum foundations I've ever met. It has made it a pleasure for me to discuss my ideas with you; it has induced me to sharpen and present in a more convincing fashion everything I have ever wanted to say. What I imagine for your contribution in this volume is that same charm working on a public (archival) scale.

I really want to be able to reserve you a spot in the volume.

30-01-02 *Qunix Redux* (to J. M. Renes)

Renesism 7: *OK i'm done ranting. I hope this makes some sense, or is at least enjoyable to read!*

Indeed it was, and I'll read it many times over!

Joe's Preply

> [CAF said:] I came up with the following slogan: "A physical theory really amounts to little more than a programming language." Its rules, its specifications are more about the ways we've come to naturally manipulate the world than anything intrinsic to that same world. I tried to say this in my Oct 4 letter to Carl ("Replies on Pots and Kettles" in the new mini-samizdat), but I think the new slogan says it better.

Ok, I see the pragmatism coming through — the concepts of the world arise because we get them from our observations and they're useful in our manipulations. Now that I've been learning computer science and complexity theory for the last few days (who knows why), this reminds me of Solomonoff's notion of science as data compression. He's got some notion of universal prior probability that I don't really know what to do with. somehow this subject seems interesting, important, and useful, but I can't quite put my finger on how, or if it jives with Bayesian views. A lot of that complexity stuff seems aimed at justifying "random number" so we can use Kolmogorov probability. Ugh.

However, things are beginning to come together in my mind, organized, as ever, in the logico-algebraic approach. A while back Carl and Howard were interested in bayesian views of computation; so am I. My strategy is to always link these things to some logical/algebraic framework and then make up probability distributions on the framework. Maybe computability can be thought of the same way; maybe it's possible to "understand the power of quantum computation" via this method. (Of course, I'm not particularly interested in quantum computation per se, but the idea of using the same framework to apply to physical theory and computer science is certainly interesting.)

I really like this overall approach; there's many levels to it philosophically, and tons of practical things to work out. I'm very amenable to the notion that physical theory isn't true because it corresponds to reality, but rather makes the most sense and use of our data and our models. Within this context, however, it will be useful to think of things as objective — there's a nice section in kant's critique of pure reason (in the transcendental aesthetic, the part on 'space') in which he goes to some pains to assert that space is empirically real, as it's objectively present in our sensation of the world, but it's transcendentally ideal since it's really/ultimately/truly a part of our intuition.

Perhaps we don't have to be that aggressive about what is really/truly/ultimately, and instead take the fact of our experience as given. Now we're really in with the pragmatist flow: from here on out what's true is what's useful. And part of that will be thinking that things are objectively real because it makes the most sense. So, maybe Hamiltonians can be 'real.' as real as anything, I suppose; though certainly not real in the ultimate sense. We're out of that game now. there's really not a lot we can say about it. It exists, inasmuch as 'it' is our experience. But not much more than that. Besides, we don't have to; we can get along quite nicely using what works.

That's it, back to computation, because I just realized what my mind has been trying to think all these days. I don't think quantum computation is fundamental, or as fundamental, in the logical sense. Computing, logic, math, all that is about true statements and can you prove them. Quantum logic, in this sense, is like fuzzy logic — you can't assign truth values to all the propositions. OK, this is the logical angle. QC is not in the class of things we think of when we think of computing because its logical structure is all wrong.

Now to the physical! In the physical domain there is a truth of the matter, of sorts. The result of experiment. In the physical realm we shift our focus a bit — our probability distribution is over what will happen, not what is the underlying truth value of the proposition in question. So we can use our physical manipulation rules and probability distributions for computation, if we like. But we're still performing a computation in the classical sense: the factoring algorithm will give the correct factors or not; there's a truth of the matter. But the fully quantum view of logical processing there's no such thing as the truth of the matter, as we know. Thus, in my mind quantum computation is divorced from the logical business of computation; it's merely concerned with the physical business.

Still not coming out well. Put it this way — it's the same idea as there's no such thing as quantum information. When we talk about using a quantum computer, we want one that we can put classical propositions into, and get classical propositions out of, albeit probabilistically. There's just no sense to the idea that it's doing some logical processing on quantum propositions. Physical processing to be sure, but not logical. We can use physical systems to perform computation, just as we can use them for information transmission, but we shouldn't conflate the physical carrier with the logical or semantic content.

OK I'm done ranting. I hope this makes some sense, or is at least enjoyable to read!

31-01-02 *Clean Sweep* (to A. J. Landahl)

Let me just say again how much I really enjoyed our conversations yesterday. Your visit prompted me to think, and it prompted me to re-look at your old emails to me.

Maybe it's time I moved at least a little on your last email, even if I don't attempt to give you an answer anywhere close to my own satisfaction.

Landahlism 23: *That said, I'm still not sure what you believe reality "is". It seems as though you wish to go in two different directions:*

reality = that which we cannot control/predict [Emma analogy]

reality = free will [Bacon citation].

Are these supposed to be the same? They don't appear that way on the face of things.

The short answer is, 1) yes, I take both these things to be components of reality, and 2) they are not supposed to be the same things. For each of us, one is an inward reality and one is an outward reality.

At heart, I think I am a "pluralist." Reality is made of lots of "stuff," samples of which—by definition—cannot be reduced to each other.

So, I say, randomness and free will appear to be two components of reality. At least as of today, I'm willing to say there may well be a few more things that we can glean from the structure of quantum mechanics and hypothesize as elements in the external world. Among these is the concept of systems (each of which with associated integer properties, commonly called the Hilbert space dimension) and certain ideals of behavior (usually going under the name of collapse rule, etc.).

I don't expect this last sentence to make too much sense to you on a first reading. But maybe a couple of passages from some of my correspondence with others will help. To that end, let me attach a small samizdat that I've put together. The relevant things to read are: A) a note to Mermin titled "Kid Sheleen" starting on page 78, and B) a note to Caves and Schack titled "Replies on Practical Art" starting on page 82. [See 03-10-01 note "Kid Sheleen" to N. D. Mermin and 03-10-01 note "Replies on Practical Art" to C. M. Caves & R. Schack.] In particular look at the table on page 84. (However, please keep this samizdat private: I haven't edited it for complete public distribution yet.)

Landahlism 24: *The part of your paper which smells the most like a lack of belief in reality is your portrayal of Bayesian agents. You say that Bayesian agents would like to align their predictions with some kind of underlying reality, and "they would if they could but they don't because they can't." Of course that doesn't mean that an underlying reality doesn't exist, only that it doesn't allow that kind of alignment. (The use of the word alignment here is unfortunate; it sounds like some kind of astrological prediction by Miss Cleo.) Do I understand correctly that you believe Bayesian agents instead attempt to align their predictions with each other as a sort of "plan B"? If so, then it sounds like all Bayesian agents learn through this process is what other Bayesian agents will have to say about things. How can it be that Bayesian agents learn anything about reality in such a process? Unless, of course, that is all reality is—correlations with other Bayesian agents' predictions.*

Maybe the most effective way to answer this is to include a note I wrote Mermin the other day. It is pasted below. [See 13-01-02 note "Reality in the Differential" to N. D. Mermin.] A hint of the external world is not to be found in the quantum states themselves, but in their changes. The world pings me when I ask a question of it, and I update from one (possibly nonsensical) belief to another (possibly nonsensical) belief, i.e., from one quantum state to another. Reality creeps into this description in two ways: 1) through the pressure that caused that very change, and 2) through the formal structure of quantum mechanics, as it makes possible—through its belief-update rules, i.e., the Krausian collapse rules—a convergence of our disparate beliefs.

I hope that helps. I understand that it is partially vague. But I view that predominantly as an effect of the fact that it is a point of view still under construction. I am not a prophet, just someone searching for a clear view of what QM is all about.

31-01-02 *Words, Only Words* (to A. J. Landahl)

I did a search on "information-information tradeoff" in my big samizdat `quant-ph/0105039` and found two hits, one in a letter to Mabuchi and one in a letter to Mermin. I'll place them below in that order. [See notes "Destinkifiers" and "Zing!" on pages 216 and 265, respectively, of C. A. Fuchs, *Coming of Age with Quantum Information*, (Cambridge University Press, 2011).]

Another reason I send you these, is that they also both explicitly tackle the question you keep asking me. What is real? What is real?! What is real!?!

The tentative answer here is the locus of all possible information-disturbance (or information-information) tradeoff curves for a system. The main idea that connects this to what I was saying to you yesterday is that that locus should be characterized by a single number. We call it the dimensionality of the Hilbert space. A little further explanation can also be found in the letter to Caves titled "The First Eye" on page 26 of the new mini-samizdat I just sent you. [See 08-08-01 note "The First Eye" to C. M. Caves.]

01-02-02 *Dublin Visit* (to C. King)

Here's my itinerary: [...] I'll place my title and abstract below.

> Title: Squeezing Quantum Information through a Classical Channel: Measuring the "Quantumness" of a Set of Quantum States
>
> Abstract: In this talk, I propose a general method to quantify how "quantum" a *set* of quantum states is. The idea is to gauge the quantumness of the set by the worst-case difficulty of transmitting the states through a purely classical communication channel. Potential applications of the notion arise in quantum cryptography, where one might like to use an alphabet of states that promises to be the most sensitive to quantum eavesdropping, and in laboratory demonstrations of quantum teleportation, where it is necessary to check that quantum entanglement has actually been used in the protocol. A more devious purpose for the talk, however, is to introduce some new concepts into current quantum foundations discussions. Time permitting, I'll tell you what I think is "real" about a quantum system.

02-02-02 *Colleague* (to C. G. Timpson)

Well, I still haven't read your anti-Brukner/Zeilinger paper, but I'm writing this small note to tell you that I just finished reading your undergraduate thesis *Information and the Turing Principle*. It's quite a work, and I very, very much enjoyed reading it!

I have to tell you, one of my first reactions after reading about the first third of the thesis was, "Finally there's something sensible coming out of Oxford!" It really was such a relief: I had been thinking that Deutsch had essentially brainwashed everyone in the quantum information community there (except for possibly Hardy and Steane).

Of course, what I like most is what I see as a significant overlap between our attitudes toward scientific theories, the Church-Turing thesis, the homunculus fallacy, and the misleadingness of the slogan "information is physical." But I learned a lot from you, and you helped me sharpen several points.

Here were some of my favorite pages: 4, 6, 8, 16, 24!, 32, 35-36, 38!, 46!, 47-48!, 52!, 60, 65!, 66, and of course chapter 5. (An exclamation means that something especially intrigued me.)

I know there are a few places where I distanced myself from the phrase "information is physical" in my large Samizdat (`quant-ph/0105039`), but I'm having trouble finding them right now. One is in a letter to Bennett starting at the bottom of page 34. However, the most intriguing moment in my mind is one instance I have not recorded in email before. Let me record it here.

I was visiting Bennett at his weekend home in Wendell, MA one weekend and somewhere in the night I got on a roll about how much I had started disliking the phrase "information is physical." In place of it, I was arguing that a much better, much more accurate call-to-arms we ought to be sending the physics and information-theory communities is that "INFORMATION CARRIERS are physical." Taking that into account is what is behind all the new questions we are asking in quantum information theory. Then I told Bennett, "I am starting to plan a long email that I would like to write to Rolf on this subject. But I know that I'm going to have to word it delicately if I'm going to stand any chance at all of catching his ear and not getting an immediate dismissal as a fool." Charlie replied, "Too late; Rolf just had about a quarter of his brain removed last week." Then he explained that Rolf was just found with cancer in the brain, and that chances were strong he would be dead soon. About a week later Rolf died.

The Shanker 1987 paper looks especially intriguing to me. I'll try to pick it up the next time I'm at Princeton.

By the way, the homunculus fallacy struck me on several levels. I think one might view William James's argument against the correspondence theory of truth in his a little book *Pragmatism* as a little bit along the same lines, for instance. But it also got me to thinking about one of the things that has long bugged me in the Zurek-style versions of quantum foundations. There, the starting point is how bad the word "measurement" is and how it should be banished from the foundation of the theory. Yet, inevitably (just watch them), whenever push comes to shove in their explanation of the true importance of decoherence, to get the idea across, they start saying things like "in essence the environment 'measures' the system." (Zurek always makes little quote motions with his fingers when he says it.) And that's supposed to lead us to a deeper understanding of that tabooed subject?!?!? (I thought I had put that complaint in a footnote in some recent paper, but for the present I couldn't find it either! I must be losing my memory or losing my mind ... or both!)

Anyway, again, I really enjoyed the thesis. Keep up the good work.

P.S. I also had a look at your webpage. From the schedule of your discussion group, it looks like you're trying hard to make sense of the notions of 'objective chance' and 'propensity' to yourself. I went down that path once—roughly from 1991 to 1996—and it was instructive. I found that I couldn't buy any of the theories, and that's what ultimately pushed me down the long road to Bayesianism. I keep my fingers crossed that you meet the same frustration!!

04-02-02 *Reverse Shannon* (to C. H. Bennett and P. W. Shor)

Thanks for sending me a preliminary version of the reverse Shannon write-up; I'm having fun reading it. And I'm happy to see that my little contribution—Eq. (5) and the eqnarray after it—was a useful thing.

Besides the full theorem, Lemma 1 is just plain nice conceptually. In fact, it seems pretty closely connected to the development between Eqs. (62) and (65) of my wacky paper `quant-ph/0106166`. There I kind of had a "teleportation" of a POVM, but I hadn't thought about applying it in addition to an arbitrary ancillary state. In my case, it was just half of a partially entangled pair. Your additional piece tickles me, and is making me think about its implications for my devious (theological) purposes.

Strangely, Andreas's matrices $\hat{\rho}_j$ in Theorem 1 also played a significant role in the same wacky paper. They played a crucial role in the rewrite of the Kraus rules that I was seeking—Eqs. (54) to (59)—in order to make quantum-state updating look more like Bayes' rule. In this case, though, it's probably just a coincidence. But it leaves me wondering.

I'll try to read the draft more thoroughly tomorrow, after I awake from this medicated fog. (Another middle ear problem.)

05-02-02 *Fighting Windmills* (to C. M. Caves & R. Schack)

Thanks for the revealing note on Feller (which I've finally had a chance to read).

Schackcosm 54: *Chris, do you know about any other book or paper that explains a "modern understanding of probability"?*

No I don't really. In fact I don't even really have a strong understanding of where most physicists have gotten their prejudices toward probability. Maybe a good place to start in such a quest is to look at the discussions of probability in the main graduate and undergraduate quantum mechanics textbooks used today (Cohen-Tannoudji or however you spell it, Merzbacher, Liboff, Bohm, and whoever else).

All I remember from my undergrad statistics course was that the professor told us that the Bayesian flavor of statistics was nonsense. Unfortunately, I don't remember the text we used. In fact I got very little out of the course that I remember at all.

06-02-02 *The Commitments* (to C. M. Caves & R. Schack)

Schackcosm 55: *The other problem is that your conversation is far too playful. State assignments are compilations of betting odds. They are COMMITMENTS."*

Dear old friends,

I'm going to try to write a note today that essentially has been sitting in my head for over a month. I'm sorry that I've held on to it for so long, but the great difficulty has been that time has just been stolen from me left and right ever since a few days before Katie's birth. (Katie herself, by the way, is the least of my problems; she's really a dream of a child.) Anyway, I think there were times in this period where the issues were so at the top of my mind that this note could have (or would have) turned out far more passionate and, thus, maybe clearer. But that time has past. Still I'll give it my best shot with the time I can muster today, and also hope that it's not so late as to clash with anything Carl has written for the newest version of the post-BFM paper. Here goes.

In a nutshell, what I'm going to say is that I now take Schackcosm 55 absolutely seriously—indeed probably far more seriously than it was ever meant to be taken.

I used to say that quantum states are "states of knowledge," or "states of information." But, you both know that brooding over the BFM paper caused me to disabuse myself of that. Then I got into the habit of calling quantum states "states of belief" in analogy with what the more left-wing, de-Finetti-flavored Bayesians say of classical probabilities. Now, what I'm going to tell you is that in contemplating the points you *two* think are important in our reply to BFM, I've gotten into the habit of calling quantum states "states of commitment." A quantum state should be viewed most properly as a compendium of commitments, gambling commitments.

Indeed you guys almost nudged me directly there by your love affair (or at least Carl's love affair) with strong coherence. I'll come back to say what I mean by this in much greater detail in a minute, but for the moment let me bring the postulate into a clearing so that you can go ahead and start aiming your arrows at it:

> A quantum state corresponds to a compendium of gambling commitments (i.e., just like the gambling odds of a Dutch-book argument) one is willing to make in various given practical situations. However, the key new point is that these commitments are with respect to ALL THINGS CONSIDERED. There are times when one's commitments correspond to one's (internal) beliefs—and because of that the quantum state remains just as subjective as ever in my mind—but that need not always be the case. There are times (and these are probably the vast majority of all real cases) where the quantum state one ends up ascribing to a system is something less than a compendium of ANYONE's beliefs.

I know that I don't have to remind you that I tried pretty darned hard to choose all my words very carefully in that definition.

Now let me tell you about the sorts of thoughts that lead to this by explicitly replying to some of your old notes to me.

Rüdiger wrote:

Schackcosm 56 : *I don't quite understand Chris's problem. Strong consistency DOES have a motivation which is very similar to ordinary consistency, and which DOES have the same flavor. It's just stronger. If you violate strong consistency, you are imprudent in the sense that you accept a bet in which, according to your own state belief, you never win, but you lose for at least one outcome that you believe is possible. That qualifies as imprudent, I think. Actually, I believe that the term "imprudent" fits better here than in the case of ordinary consistency, where "outright crazy" seems more appropriate.*

And then Carl wrote:

Cavesism 65 : *I agree completely with the above, especially with the distinction between "imprudent" and "outright crazy." I think a violation of strong consistency is somewhere between imprudent and outright crazy, but I haven't been able to think up the right [word]. People who take imprudent actions expect to win big, I think; they are judged imprudent because soberer people can see that the chance of [a] big win is small, whereas the chance of serious losses is large.*

First, let me try to settle the issues of language in these two remarks, or at least try to say more clearly why I was concerned—actually in a rather offhand way in the beginning (see my note of 12/5/01, titled "Dear Prudence")—with the appropriateness of the usual terms, "coherence" and "consistency." Then I'll tackle the more substantial issue Rüdiger brings up before that.

I once had an officemate who committed suicide. That is an action I would call (and did call) "outright crazy." Seeing the pain and the soul-searching it put everyone through who was near him, I might even have called it "moronic." And that is important. For, the point I was trying to make explicit to you in my earlier (shoddy) note, is that that kind of craziness is of a much less absolute character than the kind of craziness one would be committing by asserting both A and $\neg A$. In the first case, the craziness is conditioned by one's culture and one's customs, or you might say by the instinct for one's survival. In the second, the craziness is in the breaking of a timeless, Platonic, a priori, ideal "law of thought."

To me, the word "inconsistency"—and therefore the word "consistency"—seems far more to connect to such an ideal Platonic stasis than one's willingness to be taken to the cleaners by a Dutch-bookie. Similarly with the word "incoherent." However, being Dutch-book coherent strikes me much more as an expression of the survival-instinct type than anything else. It is a formal expression of "thou shalt not commit suicide." Why should that be considered an inherently "logical" commandment? Why should it have the right to live in the Platonic realm of Boolean logic? If you ask me, I would say it is probably much more a manifestation of simple Darwinian evolution. We try to stay Dutch-book consistent—it is our ideal of behavior—precisely because of the survival tool it represents for our kind. However, we know by recent experience, that some cultures bask in the idea that, at times, there are reasons to override one's personal aversion to suicide. Do you know of a culture that, at times, finds it useful to override Boolean logic in its mathematical proofs?

When I wrote my original offhand note, I never imagined the backlash I would get from you guys. Nor did I intend to make another proposal to change established nomenclature. I thought I would just simply get a reply of the sort, "Yes, we understand your concerns, but in this case it really is too late to change the nomenclature. A good fraction of the Bayesian community has been using the terms 'coherence' and 'consistency' for almost 70 years. The best you should hope for is an extra paragraph in the paper's appendix assessing why one ought to consider Dutch-book coherence compelling." And I'll still stand by that.

But I actually think there is a point of more substance in this—one that starts to bite much harder when one debates the relative merits of regular Dutch-book coherence versus its stronger cousin (i.e., strong consistency, strict fairness, or what have you). And the new problem is no longer just a problem of language. To say it again, Rüdiger writes:

Schackcosm 57: *Strong consistency DOES have a motivation which is very similar to ordinary consistency, and which DOES have the same flavor. It's just stronger. If you violate strong consistency, you are imprudent in the sense that you accept a bet in which, according to your own state belief, you never win, but you lose for at least one outcome that you believe is possible.*

You don't know how I ALMOST agree with the first sentence of this! In fact, I could agree with it completely if you would just let me change the first instance of "does" to "can."

> Strong consistency CAN have a motivation which is very similar to ordinary consistency, and which DOES have the same flavor.

I said before that strong consistency was of a different flavor than standard consistency, but let me be more careful now—i.e., now that you've helped me sharpen my point. It is not that the flavor is a priori different; it is that in paying the price of strong consistency, you actually get two flavors for the price of one. That is to say, I cannot agree that strong consistency is "just stronger"—it is that and something more.

Here is a very different (and ostensibly much less satisfying) way of motivating strong consistency:

> AXIOM: Whatever an agent's personalistic beliefs, i.e., whatever personalistic probabilities he has managed to write down in his head for some event, when placing a bet (with a Dutch-bookie or otherwise), he MUST place it precisely according to the gambling odds his beliefs afford.

Clearly a special case of this is that when a person believes $p = 1$ for some event, then he MUST bet as if he is certain that it will occur. I.e., he must be strongly consistent.

But you probably ask, "What is wrong with that?"

I will tell you. In doing such a thing, you would throw away loads of freedom, loads of wiggle room that the standard Dutch-book argument generally leaves in your command. In essence you throw away the freedom to concatenate your Dutch-bookie-game commitments with any of a number of other games. Or still another way to say it—though it's not so nice—you throw away your freedom to lie, even when it is in your best, overall, total interest.

An easy case to see this in is the "double agent" Dutch book argument that I wrote you about on 8 Sept. 2001 in a note titled "Negotiation and Compromise" (starting on page 64 of the mini-samizdat). Suppose that Kiki and I draw our money from the same bank account, and that for some given event I ascribe $p = 1$, whereas Kiki ascribes $p = 1/2$. We've talked about our disparity in assignments many times, but I simply cannot convince her to accept the evidence that led me to $p = 1$; she holds her ground, and I know she would do that in any circumstance, come hell or high water. Now suppose I'm later approached by a Dutch-bookie that I am fairly sure has already gambled with Kiki. What should I do? If I don't want to lose my shirt, then I had better not declare my adamant belief in the ultimate occurrence of the event we are betting. Instead, I should adjust my GAMBLING COMMITMENT to agree with Kiki's (sorely) less than optimal one. (Sorely less than optimal from my point of view, that is.)

If on the other hand, you say that in order to play the game of Bayesian probability, I must be strongly Dutch-book consistent, then I no longer have the option to save my own fortunes in this new game. The point is, standard Dutch-book consistency concatenates well with such a further, ancillary game, whereas strong consistency does not.

But this is just a contrived example. Similar, but maybe better, examples can also be found by looking at the old "Keeping the Expert Honest" game (see pages 20–21 in Ph.D. thesis). And I'm sure there are still other more realistic situations that we could come up with if we just tried.

So, what I am saying is Dutch-book consistency is only the tiniest check on one's gambling strategies, and that is a GOOD thing. A very good thing. If one imposes too much structure—apparently almost anything more than simple DB consistency—then one will be left in a lurch in the real world, a world where negotiation and compromise are the keys to survival.

I truly, truly, truly hope you will see the point of this, but I guess I have gained enough experience in the last email war to be prepared for the worst. Here is what I am mainly afraid of. By holding fast to strong consistency as a reasonable addition to standard DB consistency, one ends up in the quantum case with a nice, tightly mathematical looking theorem (almost) in the BFM style. You cannot tell me that at least the Carl among you is not far more attracted to results like that, than the willy-nilly result one is left with if only standard DB consistency is enforced. At the very least, it makes the paper look far more constructive than destructive . . . and that's got to be deemed a good thing, right?

What I guess I am saying is that, concerning what Carl wrote:

Cavesism 66: *I, as you know, like strong consistency and hope that in discussions of it, we can separate the a priori reasons for liking or disliking it from reasons based on the conclusions it leads to.*

I believe that I have done that. However, I hope that you will live up to these hopes too! Every bit of the discussion above (excepting the last paragraph) had nothing to do with quantum mechanics. The argument is purely classical and divorced from the BFM issue. In our post-BFM paper, I certainly have no problem whatsoever delineating the whole hierarchy of conditions, regular DB, strong DB, etc. What worries me though is how the present draft gives pretty short shrift to plain old regular DB — the very one that I myself find the most reasonable (for all the reasons above).

Of course, suspecting that you guys will try to wring me out after spouting this blasphemy, I had hoped to come armed with good knowledge of the literature. Unfortunately, as I already expressed way above, time hasn't been on my side these last few weeks. What I was at least able to do though, was run to the Princeton library one day and amass copies of a lot of papers. I ran from one paper to the next, following the citation trail that each gave. Below is the result: My complete collection of Dutch-book papers.

I wish I could say that I had read these, but for me the skim of a title and abstract doesn't count as "reading" a paper ... as it does for XXX, say. Nevertheless, a couple of points did stand out for me. The most important one is that maybe I'm not alone in thinking that strong consistency goes (far) too far. Hacking (1968) below writes, for instance,

> Abner Shimony called it *coherence*; John Kemeny called it *strict fairness*; today many people speak of *strict coherence*. According to Shimony's definition, a set of betting rates on a series of propositions h_i and e_i is strictly incoherent, when "there exists a choice of stakes S_i such that, if X accepts the series of bets at these stakes, then no matter what the actual truth values of h_i and e_i may be, X can at best lose nothing, and in at least one possible eventuality he will suffer a positive loss." De Finetti had a less demanding concept which is called *coherence*. A set of betting rates is incoherent if, no matter what the actual truth values of h_i and e_i may be, X will suffer a positive loss in every eventuality. Logicians usually think that Shimony has improved on de Finetti's concept of coherence, but statisticians, including de Finetti himself, have seldom been persuaded.

After that, he starts writing about nonBoolean algebras, and I haven't had a chance to try to decipher it. But what is really important to me is that this is the first indication I've run across that de Finetti himself thought about strong coherence and then rejected it. I ask, "Why?" I know that Carl has little use for relying on authority—I'm referring to his quick rejection of even wanting to hear de Finetti's opinion—but that is not what is at issue here. de Finetti is someone who thought long and hard about probability from the Bayesian view. I cannot see how it would not be worthwhile to at least hear his arguments. We might save ourselves time, and we might save ourselves from making mistakes that will make us feel foolish.

Then there is a large set of papers debating whether Dutch-book coherence really has to do with "rationality" or rather something else (as I alluded to above). The titles should give those papers away. I've hardly even skimmed those at all, but you can see from above where my present opinion lies. DB consistency defines an expedient for our actions, but it is hardly more rational or logical than that. Skyrms87 in particular, I am told, argues the opposite point of view. If that is a position that is near and dear to your heart, then maybe it would be worth understanding what he has to say.

OK, clearly I'm petering out and it's after midnight now. Soon I won't even be coherent myself; I'm already starting to see signs of it. Let me try to quickly summarize the whole argument in a few sentences, rather than checking and editing all the above to make it more eloquent, more complete, and more connected.

1. DB coherence strikes me as much more a pragmatic requirement than as any rule of rationality (as the law of the excluded middle is).

2. Thus one is more compelled to consider the pragmatic consequences of standard DB versus strong DB.

3. From that, one sees that strong DB is not just more of the same, but carries with it whole new flavors of behavior. In particular it forces us all to be little George Washingtons—"I cannot tell a lie"—when we have a $p = 1$ assignment in mind.

4. Eschewing that, I am forced to divorce our (pragmatic) gambling commitments from our actual beliefs. Our beliefs can be our commitments, but our commitments need not be our beliefs.

5. Thus it is better to say that "probabilities are our gambling commitments, ALL THINGS CONSIDERED." (with apologies to NPR)

6. Quantum states being compendia of probabilities are thus "states of commitment" full stop.

That's the argument. I'm sure this letter is riddled with typos, but I don't want to hold on to it anymore. France Telecom is coming Thursday and I've been tapped to convince them that quantum information is interesting and that our dabbling in it is a little value-added perk they'll get if they stay our customer rather than running away to Alcatel. Can you believe that?

Anyway, maybe Rüdiger will be a little happy to see this when he gets into the office tomorrow . . . to see that I haven't really abandoned you.

1. Brad Armendt, "Is There a Dutch Book Argument for Probability Kinematics?," Philosophy of Science **47**, 583–588 (1980).

2. Patricia Baillie, "Confirmation and the Dutch Book Argument," British Journal for the Philosophy of Science **24**, 393–397 (1976).

3. David Christensen, "Clever Bookies and Coherent Beliefs," Philosophical Review **C**(2), 229–247 (1991).

4. Barbara Davidson and Robert Pargetter, "In Defence of the Dutch Book Argument," Canadian Journal of Philosophy **15**(3), 405–424 (185).

5. Richard Foley, "Being Knowingly Incoherent," Noûs **26**(2), 181–203 (1992).

6. Bas C. van Fraassen, "Belief and the Will," Journal of Philosophy **81**(5), 235–256 (1984).

7. Ian Hacking, "On Falling Short of Strict Coherence," Philosophy of Science **35**, 284–286 (1968).

8. Frank Jackson and Robert Pargetter, "A Modified Dutch Book Argument," Philosophical Studies **29**, 403–407 (1976).

9. John G. Kemeny, "Fair Bets and Inductive Probabilities," Journal of Symbolic Logic **20**(3), 263–273 (1955).

10. Ralph Kennedy and Charles Chihara, "The Dutch Book Argument: Its Logical Flaws, Its Subjective Sources," Philosophical Studies **36**, 19–33 (1979).

11. R. Sherman Lehman, "On Confirmation and Rational Betting," Journal of Symbolic Logic **20**(3), 251–262 (1955).

12. Abner Shimony, "Coherence and the Axioms of Confirmation," Journal of Symbolic Logic **20**(1), 1–28 (1955).

13. Brian Skyrms, "Coherence," in *Scientific Inquiry in Philosophical Perspective*, edited by Nicholas Rescher (Center for Philosophy of Science, Lanham, MD, 1987), pp. 225–242.

14. Jordan Howard Sobel, "Self-Doubts and Dutch Strategies," Australasian Journal of Philosophy **65**(1), 56–81.

15. Lyle Zynda, "Coherence As an Ideal of Rationality," Synthese **109**, 175–216 (1996).

06-02-02 *How Did? What Did?* (to N. D. Mermin)

Merminition 88: *I don't remember the precise content of some of the issues you seem to be addressing, notably what is "strong consistency"?*

Then how on earth did you word your slide (where you gave our version of the theorem) when you gave your talk here in Murray Hill. Now my curiosity is piqued; maybe I didn't notice some inanity in your talk. One only gets something that looks even remotely like the BFM criterion if one assumes strong consistency. If you stick with standard Dutch book, you get precisely what I had been trying to tell you since the very beginning: namely, there are *no* constraints on the density operators required at all.

06-02-02 *Actually, the Both of You* (to C. M. Caves & R. Schack)

This morning I woke up and re-read the long note I sent you last night. In doing that, and looking back at your other notes again, I now think I was too harsh in singling out Carl when I wrote:

> By holding fast to strong consistency as a reasonable addition to standard DB consistency, one ends up in the quantum case with a nice, tightly mathematical looking theorem (almost) in the BFM style. You cannot tell me that at least the Carl among you is not far more attracted to results like that, than the willy-nilly result one is left with if only standard DB consistency is enforced. At the very least, it makes the paper look far more constructive than destructive ... and that's got to be deemed a good thing, right?

That's not fair to him. In particular, both Rüdiger and Carl wrote:

Schackcosm 58 : *Of course it is nicer to base one's approach on the weaker concept. What we are discussing in the paper, it seems, is reasons for why the weaker concept does not quite give us what we would like. These reasons are secondary, they have nothing to do at all with the Dutch book justification of strong consistency itself.*

Cavesism 67 : *Rüdiger's two paragraphs nicely illustrate what I was hoping for when I wrote that I "hope that in discussions of it, we can separate the a priori reasons for liking or disliking it from reasons based on the conclusions it leads to." The first paragraph is about the a priori reasons, and the second is about why its conclusions are important.*

Let me focus in particular on Rüdiger's sentence, "What we are discussing in the paper, it seems, is reasons for why the weaker concept does not quite give us what we would like." What is it that *we* would like? And, why would *we* like it?

Clearly, I am most happy with the willy-nilly result that standard DB coherence alone gives. I'm glad you guys made that nice and rigorous. For me, it says that B, F, and M were just way off the mark in trying to dictate what various observers MUST ascribe for their quantum states. It tucks nicely with my very first note to them where I registered some protest.

Now, what is nice about the strong coherence version of the argument is that it does at least get us the "if" part of BFM, and thus gives us something solid with which to compare to their campaign. But I suppose, I have always viewed that as a secondary, rather than the primary, point of our criticism. Its role is simply in that it tells us what we have to ADD to pure (de Finetti flavored) Bayesianism, to get something that comes close to resembling BFM.

So, my apologies to Carl, and my consternation to the both of you!

06-02-02 *Definition* (to C. M. Caves & R. Schack)

Let me go back to the NPR thing briefly while I've got a couple of moments. "All things considered," what does it mean?

I know that your knee-jerk reaction is going to be that it is a hopelessly vague term. However, I'm going to suggest that it is no more and no less vague (and mysterious in its origin), than "belief" was in the first place. It is just broader in scope. In fact, I think it amounts to little more than the sum total beliefs one possesses. (What more could one consider?)

Thus in setting a quantum state, one sets it according not only to what one believes about the system of particular interest . . . but also according to the situation one believes he will be encountering in the laboratory, the purposes of the information he will gather and how it will be used, who will share that knowledge so gained, etc., etc. It may even depend upon how the financial markets are doing, which political party happens to be in power, and so forth.

It just takes seriously the idea, that to any quantum system, what we say about it—i.e., what we are openly willing to bet on it—depends upon many things beside the system itself.

Maybe all of this goes back to a conversation I had with Marcus Appleby while I was in Northern Ireland, just after 9/11. I tried to explain my new point of view—that a quantum state is a state of personal belief—and he replied that he though that had the right feeling, but that he had some kind of "unpinpointable" fear that maybe the idea didn't go far enough. He seemed to be saying something to the effect that beliefs can never truly be considered in isolation from other beliefs. I didn't understand his worry at the time, but I think everything I wrote you yesterday starts to pick up on this line. In this regard, by the way, Appleby suggested I read a book by Michael Polanyi, *Personal Knowledge*. (Polanyi was apparently also a chemist of some renown, I believe he said.) Though I haven't had a chance to dig it up yet.

In any case, the COMMITMENTS one (potentially) makes in the sum total of all gambling situations quantify the quantum state. They give it an operational meaning—in terms of its "cash value"—which goes beyond niggling over the details and merits of "beliefs" versus "culture" versus "all relevant things considered," etc.

06-02-02 *The Great Quantum Well* (to C. M. Caves)

Cavesism 68: *Not willing to discuss the technical issues much at present because I'm not going to have time to flesh things out for a while. I get a long way by not being interested in Kraus decompositions, which aren't generally convex combinations of unitaries, and I get a good deal farther by insisting on time evolutions, not just single-time stuff. The rest is going to come from having available some operators that rigidify (and thus provide a physical interpretation) for the vectors in Hilbert space, without which the quantum questions (and Hamiltonians) aren't anchored to anything at all.*

I think the last sentence really captures what sets our goals apart. The image I carry around in my mind presently is that the quantum system is very much like an oracle. We ping it, and it provides us with something that we did not have before, something that we could not foresee ... and therein lies its reality. Therein lies its independence from us. Each quantum system is an instantiation of your great quantum well.

But the "interpretation," the "meaning," of the gifts those oracles give us is set from the outside. Completely from the outside. Or at least that's the point of view I'm pursuing.

I just don't see how that line of thought lies anywhere along the lines of your accusation that quantum questions will thus not be "anchored to anything at all."

07-02-02 *The Heisenbergs* (to J. B. Lentz)

I'm just reading some new revelations about Heisenberg's involvement in the Nazi A-Bomb project in the New York Times this morning, and I ran across the following:

> But others say questions about the meeting remain. One of Heisenberg's sons, Dr. Jochen Heisenberg, who is now a physicist at the University of New Hampshire, and Mr. Powers, say the documents show that Bohr never understood the message Heisenberg meant to convey in Copenhagen.

Didn't you tell me you met this guy?

Anyway, the whole article is at `http://www.nytimes.com/2002/02/07/science/07BOMB.html`.

08-02-02 *Samizdats and Dutch Book* (to B. C. van Fraassen)

I was talking to David Mermin a while ago and he mentioned that you had brought my name up in some email and seemed to be aware of some of my papers. (I hope I got the story straight.) Anyway, that piqued my interest: If true, then you are very likely the only philosopher who has ever noticed my existence! I would certainly love to hear your opinion—both con and pro—of my quantum foundational thoughts (forming as they are). Having the critique of a true-blue philosopher would be most useful for steering me to clarity, or even steering me away from the abyss!

My two most close-to-philosophical pieces can be found on the Los Alamos preprint archive: `quant-ph/0106166` and `quant-ph/0105039`. Or, they can both be found at my (almost empty) website with a couple of other pieces of supporting material.

Also I'm in Princeton from time to time making use of your wonderful library system. Maybe I could drop by for a chat? (Sometime in March or later, actually, since I'll be in Japan until essentially then.)

By the way, I was trudging through various Dutch-book arguments recently, and I came across your paper "Belief and the Will," [Journal of Philosophy **81**(5), 235–256 (1984)] in the process. It looks good. I'll let you know if I form an opinion.

11-02-02 *Oh Modern Wittgenstein, 1* (to N. D. Mermin)

Concerning points 1 and 2 in your *Tractatus Quantico-Philosophicus*, read the anecdotes below. [See letter to Chris Timpson, dated 2 February 2002.]

I exclusively use the word "qubit" for the physical system, i.e., the "carrier" of the information, i.e., the object of one's belief, i.e., the oracle the receiver consults at his end of the game. I never use qubit to mean a quantum state (and I don't think I ever have), but I know it's a serious problem in the community.

11-02-02 *Oh Modern Wittgenstein, 2* (to N. D. Mermin)

Merminition 89: *It's the Cbits I'm more concerned with. "bit" – which Charles wants to reserve for the classical physical system clearly has an important abstract meaning as well, relevant to both Qbits and Cbits.*

Aristotle called it matter, the receptacle for accidental properties (presumably some of which could be binary valued). When we turn our attention to a subset of such properties, and are completely ignorant of which of the two is inherent, then we say that we are missing a bit of information.

11-02-02 *A Wackier Idea* (to J. Preskill)

I wonder if you would indulge me with still another wacky idea (actually a wackier idea than usual) to distract you from your usual workaday duties. The only compensation I can give is my promise that in the long run—the long, long run—the world will be a better place if you make this effort. (As if I'm in a position to talk that way!)

Here's the wacky idea. Jeff Bub and I are putting together a special issue of the journal *Studies in the History and Philosophy of Modern Physics* on the impact (if any) of quantum information on quantum foundations and other philosophical issues. Here's what we've managed to line up so for for the issue. The *definite* commits are:

- Andrew Steane: "A Quantum Computer Only Needs One Universe," (extended version of `quant-ph/0003084`)
- Ben Schumacher: "Doubting Everett"
- Richard Jozsa: "On quantum information, and the sense in which it is a distinct notion from its classical counterpart"

- Itamar Pitowsky: "On Bayesian quantum probability (deriving the quantum rules from a gambling scheme in the spirit of de Finetti or Ramsey)"

- Howard Barnum: "On quantum logic and quantum information"

Beyond this we have "probable yeses" from Charlie Bennett and David Mermin. Charlie would write something on Maxwell demons and David would write something Merminesque.

You might be able to tell from this that the lineup is starting to get a little one-sided against Everett. I'm writing you because I'd like to find a little counterbalance to that. In particular, I'd like a scientist—a real scientist—to explain as best he can, what he finds pleasing in the Everett picture, in what ways he finds it useful, how it might make quantum computing's power easier to stomach, and to what extent he finds the view problem-free or, instead, to what extent he foresees it needing a patch.

I've already tried to get Charlie Bennett to take this bait (since in private he is such an avid fan of Everett and calls my own quantum foundations quest "theology"), but in his words, he "just can't get it up to write an article on that subject." (See the note below, along with further details about the goals of the project.) But I really think someone needs to do it, or at least try to do it. Honestly, you're the only person besides Bennett that I trust with such a task.

I want a scientist … not a counter-theologian like Deutsch or Vaidman. I think only in that way, will we really get at the root of what's really good in the Everett view or where it remains weak.

Would you do this? Could you do this? (If I were a doctor, I'd send a sample of Viagra along with the note!) We'd be able give you until about mid-June to get the article finished. And of course, I could volunteer as much (constructive) feedback as you'd like for your drafts, if you're worried about the oddball audience you'd be writing this for.

I hope you'll think hard about the proposal. There are few people that have such a broad overview of the field as you, and I'm banking that'll give you the resources to make a first-class contribution along these lines.

As soon as we hear from you one way or the other, Jeff and I will send out a more formal invitation to all involved, including deadlines, etc.

11-02-02 *A Wackier Idea, 2* (to J. Preskill)

Preskillism 3: *Seems to me that you need someone who is passionate about this, and I'm not.*

No, you're hitting the nail on the head with the latter part of the sentence, and that's exactly why I would like for YOU to write an article. To the extent that you seem to find Everett a pleasing or satisfying picture, you have *always* seemed dispassionate about it. That's a perspective that this debate on the subject seems to sorely need—the working scientist's point of view, without a religious fervor. (I tried to convey that to you in my last note.) A calm, collected account of why you find it a useful working picture.

Preskillism 4: *Actually, Deutsch seems like a natural, since he ought to be able to pitch his message to an audience of philosophers, and you seem to want that.*

Deutsch really is only about religious fervor (probably much like I am, if Charlie's accusations are on the right track). I think what needs pitching to an audience of philosophers—for this particular subject—is what I said above. You, like Bennett, think that the Everett picture has a certain cash-value in your everyday work (or, you wouldn't have taken the time to write about it in your lecture notes). I think it would be useful for us all to understand that better (in general, but also with some explicit examples).

Deutsch would say the same things he has said a thousand times before. He would invoke Popper, and Plato, and whatnot, and say that anyone who does not believe as he does is "refusing to contemplate the implications of science." You would say things that John Preskill would say, in John Preskill's way. And they may turn out to be the same things that Deutsch would have said, but then that will be a useful datum too.

11-02-02 *Viagra Sample* (to C. H. Bennett and J. A. Smolin)

Below I'm gonna send the note again that I sent you on January 2 concerning the special issue of SHPMP that Jeff Bub and I are putting together. Plus let me also send you the lineup of other authors for the issue we've put together since then. I'm hoping you'll find it as stimulative as I find it!

Beyond these guys, Mermin and Preskill are also giving it some serious thought.

I sure would like to hear a "definite yes" from you guys soon. (The deadline on the paper would be mid-June.) Jeff and I are hoping to send out a more formal "invitation letter," closing the ranks and sort of tying the whole project together by the end of this week.

The *definite* commits are:

- Andrew Steane: "A Quantum Computer Only Needs One Universe," (extended version of `quant-ph/0003084`)
- Ben Schumacher: "Doubting Everett"
- Richard Jozsa: "On quantum information, and the sense in which it is a distinct notion from its classical counterpart"
- Itamar Pitowsky: "On Bayesian quantum probability (deriving the quantum rules from a gambling scheme in the spirit of de Finetti or Ramsey)"
- Howard Barnum: "On quantum logic and quantum information"

11-02-02 *Viagra Sample, 2* (to B. W. Schumacher)

I thought you might enjoy this too. (And I send it as a subtle reminder.) [See 11-02-02 note "Viagra Sample" to C. H. Bennett and J. A. Smolin.]

But let me also ask you a technical question related to your project. Have you ever thought about what (if anything) it would change of your conclusions if one didn't augment the universe's Hilbert space with a fixed (preferred) basis—which definitely would demolish your whole point, as you have made clear in your talks—but instead one augmented it with a fixed (preferred) tensor-product decomposition? Indeed, does that even quite make sense?

12-02-02 *The Will to Believe* (to B. C. van Fraassen)

Thanks for the note. You warmed my heart with the sentence, "We were all very intrigued with this 'Bayesian' approach to probability in QM." We (i.e., Caves, Schack, and myself, and sometimes Mermin) know that it's all very much at the beginning stages, but things are starting to fall into place so exponentially fast it evermore gains the air of an inevitability. We've now got a load of material that we haven't published yet (and are working very hard to write up), and I in particular have made a strong phase transition in my attitude toward quantum time evolutions (i.e., their level of subjectivity versus objectivity). So things are just flying.

Thanks very much for putting me on the meeting list. But also I hope to meet you before then. In any case, know that I am always, always available on email. (And as one of the documents I advertised in the last note will attest, that is even my preferred means of communication!) So, please feel free to get a dialog going if you wish: I would relish it.

12-02-02 *Samizdats and Dutch Books* (to P. F. Scudo)

I'm glad to hear that you are not frightened by de Finetti's ghost! My whole life is tied up in it now. Sometimes when one hits the right path to a solution, one just knows it—the feeling is overpowering. And that is the case with me on this round. I dare say there is no deeper, no more direct path to the quantum foundations than through Bayesianism.

13-02-02 *Another Kent Paper* (to G. Brassard)

Brassardisme 4: *What do you think of this?*

I think "nonlocality" and especially questions about the restrictions enforced on physical theory—in particular, the description of quantum phenomena—due to no-signaling criteria are red herrings. Our brain pulp is better left for deeper matters. You see, in playing the sorts of games that people have been playing along these lines, the very starting point is to take the idea seriously that the quantum state is a physical property, rather than a description of information, knowledge, belief, betting-odds or what have you. And that is an idea I stopped taking seriously a good while ago.

See Section 6 (and its closing paragraph) in my paper `quant-ph/0106166`. By the way, there is a (relatively minor) technical mistake in that section; let's see if you can find it!

13-02-02 *One More for James* (to C. M. Caves)

Cavesism 69: *Curt? Emphatically so. Snide? I don't think so. But I can see that my habitual curtness has led to more than its usual negative reaction, so let me go back to square one.*

William James likes to say that all beliefs are "numerically additional" to the reality they take as their target, even "true" beliefs.

Thanks for the note.

I similarly need to automate my Outlook better: maybe that'll be one of the great triumphs of our time together in Brisbane.

15-02-02 *Friendship Call* (to L. Hardy)

I discovered yesterday that you'll be in New Jersey in March for the Wheeler-meets-rich-man-desperately-wanting-to-live-forever thingy. I'm not sure whether I'll be going to that yet, but I wonder if I could interest you in dropping by my neck of the woods while you're in the state. I should be around March 11 through 19 (inclusive). March 19 at 6:00 PM I fly out for the little meeting in Dublin (quantum channel capacity stuff).

I can't offer you any travel funds, but I could put you up in our guest room and offer you some of my wife's home cookin'. Also I've got a decent collection of single malts that might lead us to some insight. (There's a great line in a Tom T. Hall song, "'We must've drank 10 quarts of German beer; my conscience and my sinuses were clear.") Also we're a 1 hour and 10 minute straight-shot train ride to New York City; so you could use us as a base of operations for a couple of days if you wished.

If you've got the time and haven't settled your plans yet with respect to this trip, I'd certainly love to see you. I've got a load of things quantum I'd like to talk to you about. (I'll be the poetaster—that's actually a word—and you can be the concrete master.)

I'm off to Japan for 10 days tomorrow, but I'll be in email contact every day.

16-02-02 *Some Things Should Not Pass* (to several friends)

Some things should not pass without our best effort to make them indelible. Yesterday, February 15, Kiki and I had to put our golden retriever Wizzy to sleep. He was the most loving and faithful dog either of us had ever had.

As things happen, Wizzy's last day of life marked exactly eight years from the time he first entered my apartment in Albuquerque; it was my second date with Kiki. I had schemed all day about how I might meet her that evening, and the solution was to cook a meal, a large meal. Hopping around the corner from my apartment to hers, I said, "Would you be interested in dinner tonight? I accidentally made too much." She said, "Sure; I was only going to warm up some potatoes and cheese anyway. I'll be over in about 20 minutes." A couple of minutes later, I got a phone call asking if she could bring her dog with her. I said, "No problem." It was a sweet and touching sight: Wizzy was a dog so insecure at the time, he never left Kiki's side—he didn't sniff around or explore like most any other dog would have; he stayed in bodily contact with her from the moment he entered until the moment he left.

About four months before that night, Kiki had rescued Wizzy from an animal shelter. He must have had a hard life, we surmise, judging from the scar on his head and the fear he had of brooms at the beginning. We'll never really know how old he was, but comparing him to our other golden, Albert, he was probably 12 or 13 years old when he passed.

The day Kiki met him, she had the intention of looking at two dogs before making a decision of which one to take home. Wizzy was the first. When the attendant let him out of the cage, he so leaned his whole body into Kiki and seduced her with his big, loving eyes, that she knew she couldn't put him back.

Wizzy in fact played a predominant role in my meeting Kiki. For some time I had seen her walking him around the neighborhood, and it dawned on me that since no one was ever with her, she had a chance of being single. I waited for my moment, and it came one morning as I walked across a neighborhood park. I introduced myself by going directly for Wizzy. I said, "What a beautiful dog; what's his name?" She told me, and then I asked in a sort of quizzical way, "Is he a purebred?" She said, "Yes." I said, "I don't think so. I have a golden myself, and they don't look very much alike." Why she accepted a date with me a few weeks later remains a mystery.

This morning I broke the news to Emma, and she became sad. She asked, "Where did he go?" I said, "Back to nature." When she's ready to think about it harder, I'll tell her my (presently) favorite metaphor for what happened: Our finite lives are like little drops of water that have parted from the sea. For a small time we have the chance to move around and determine our courses as we please—to leave a trail behind us. But we all eventually run back into the sea. We never stop being; we just become part of something bigger.

Kiki and I put Wizzy into the ground at sunset yesterday, like Egyptians. We gave him his blanket of eight years so that he would never be cold. We gave him his leash so that he could have an infinity of walks, his bone so that he could have an infinity of chews, and his rubber ring so that he could retrieve it for eternity. I told him that I had always known he was a purebred.

17-02-02 *Long Letter, Way Longer Flight* (to D. B. L. Baker)

"Baker-san. Kon-nichiwa. Fuchs-desu." [...]

I've got such a long day ahead of me still, I don't even know where to start. This time I'm going to the city of Sendai. I think it's maybe 200 miles north of Tokyo. So, once I get to Narita airport, I have to take a train to Tokyo station. That ride lasts a little over an hour. But then, after a 45 minute wait, I have to take a train from Tokyo to Sendai. Even though it's labeled a "super-express," that will also take over two hours.

For a while I was pretty frightened about navigating Tokyo station: You wouldn't believe the ant hill that place is!! But luckily after enough whining, my hosts thought it best to contact someone in Tokyo to meet me at the incoming train and lead me to the outgoing one! These people—the Japanese—are so sweet and polite. I've never seen anything else like it anywhere in the world. [...]

About myself, I'm just plodding on—trying to act the part of a scientist/philosopher. And trying not to worry too hard about the downfall of Lucent (and hence Bell Labs). Philosophically, I've become an absolute junkie for the "pragmatism" movement. There's hardly a day when I can be parted from my Willy. William James that is. But also, John Dewey, F. C. S. Schiller, George Herbert Mead, Richard Rorty and so on. Do you know any of these names?

It's sort of interesting how the first letter in my big samizdat—it's one to you—opens up by describing what a great respect I have for William James' writing style. But the truth of the matter is I really didn't start reading the man in depth until last August. So, my knowledge of him is really very young. Here's how I finished that letter to Mabuchi:

> But that's just an aside (to tell you that the grass is always greener). Keep up the good work with all those good students. Get them to read William James' *Pragmatism* and tell them that quantum mechanics is a much better motivation for all that he said there ... but to never lose sight that the real goal is to get to where he wanted to go.

I'll tell you the same thing. Read him; it's a philosophy for living, and writing the future.

Concerning the quantum world—the real world of my every waking moment—in the last six months, I think I've made the greatest progress I've made in years. Go have a look at my paper: `quant-ph/0106166`. That was just the beginning of the transition in me. Since then, I really believe I've climbed a peak that no one else has even been able to see from the ground. Presently that insight is captured in about a 120 page document of emails (much like the bigger samizdat I published on the web), but I'm hoping to distill it into a real paper within the next month or so. (As you can already tell from the size of this letter, I'm hoping to use this Japan trip to get a whole lot of writing done.) [...]

I haven't been to New York City nearly as much as I would like to; but I keep hoping for the day when I'll be able to increase the frequency of my visits. The last time I was there (about a month ago), I had a pretty amazing experience. I went to see this philosopher friend of mine who was in town to visit his sister. He sent me her address and suggested that we meet at her apartment and then go to lunch. The address was essentially the corner of Broadway and Bleeker Street, right in the middle of the Village. I thought, "Wow, that's in the thick middle of things. The apartment has got to be an absolute shoe box; how else would an academic afford it?" So I got quite excited about going there: I thought I'd be able to see first-hand what squalid lengths people will go to just to live in an intellectually stimulating environment. Well, it was anything but squalid. It turned out to be a penthouse apartment on the top of the building. Three levels of its own! In square feet, it was essentially the same size as my house in Morristown! Many millions of dollars worth of Manhattan space. Here's the rub: Arkady's sister was not an academic as I had assumed. She's an investment banker, and her husband is an architect. Arkady's girlfriend told me, "You ought to see her place in the middle of Paris; it's ten times this big."

It was funny: One of the windows was placed explicitly so as to "frame" the World Trade Center. It's kind of an empty view now. I visited the old WTC site, by the way, sometime in early December. It was quite a stirring experience. The whole affair certainly caused me to think much harder about our finitude. And it caused me to think much harder about what we all, each of us, might hope to do in this life. Hence also the extracurricular reading of James. (But I'm not quite sure which came first, the chicken or the egg—i.e., my seeking the books or the books seeking me.)

17-02-02 *The Process* (to C. H. Bennett)

Bennettism 8: [Thoughts on Wizzy] *Sorry to hear about the big change in your lives, lives that would have been unimaginably different without Wizzy. My favorite metaphor for death is being dropped into a black hole. The main worthwhile thing left behind is not your physical remains, nor even their information content (which presumably reemerges as Hawking radiation) but rather the relative state you leave behind in the Church of the Larger Hilbert Space. If I were you, though, I wouldn't beat about the bush with Emma, but just tell her that Wizzy died. She's plenty smart, and probably knows about death already, so euphemizing will not make it any less painful.*

I'm sorry; I didn't mean to give a misimpression to you. Emma's asking "Where did he go?" came a while after we had already discussed that Wizzy died (literal phrase). That was exactly how I introduced the subject in fact: "I have some bad news. Wizzy died last night." (All this happened, by the way, as I was getting her dressed so that she and Kiki could take me to the airport at 6:30 yesterday morning. I'm in Sendai, as I write to you.)

I knew that she was a little familiar with the concept since she had seen a Babar movie where Babar's mother gets shot. So she understood what was going on a little, but she didn't have all the aspects of it down yet. She started by saying, "I don't want Wizzy to die. Why did he die?" I said, "Because he was old and his time had come." She said, "I don't want him to be old; I want him to be new." I explained that we had buried him in the back yard, and that she could go to the spot and talk to him—"pretend talk"—whenever she wanted. It was when I finally took her outside to show her the grave that she asked "Where did he go?" She was moving into a little more contemplative mode by then, so I thought I'd run with it.

Kiki said that after they returned from the airport, Emma wanted to go to the grave and talk. When they got there she asked, "When's he going to come out, Mom?"

Bennettism 9: *My favorite metaphor for death is being dropped into a black hole. The main worthwhile thing left behind is not your physical remains, nor even their information content (which presumably reemerges as Hawking radiation) but rather the relative state you leave behind in the Church of the Larger Hilbert Space.*

I don't suppose it's ever struck you what an excessive sort of universe that would be. In a way, everything appears twice over. Once in the state and once in the relative state. (If you accept the existence of the universal wave function, the one determines the other uniquely.) What so moved God that he should make two copies of everything? (Redundancy for the purpose of error correction won't do as a reply!)

18-02-02 *Psychology 101* (to J. Preskill)

Let me reply to some of your points in a way that doesn't reflect their original order.

Preskillism 5: *In the past I have sensed that you and I differ in how we regard ourselves. I believe that I am just another physical system governed by the same fundamental laws as any other system. You seem to think there is a fundamental distinction between yourself and the system you are observing. To me the Everett view is appealing because it turns away from this egocentrism.*

It's funny, but when I read this, my reaction went in two rather peculiar directions. First I thought, "I wonder if, in the end, the only thing the great quantum foundations struggles will leave behind is a few psychological observations? If so, what a shame." But secondly, I imagined Galileo hoisting me up to the top of the Leaning Tower of Pisa and dropping me off it along with his two famous stones. Even though I cursed and screamed the whole way down, I went "splat" at the same time that they went "thud."

Here's the psychological thought in a little more detail. One of the things that bugs me about the Everett view is what *I* consider *its* extreme egocentrism! Now, how can that be—both of us accusing the other's view as *the* egocentric view? I'll tell you what I think, trying to express the problem from both sides of the fence.

My side gets to go first. What I find egocentric about the Everett point of view is the way it purports to be a means for us little finite beings to get outside the universe and imagine what it is doing as a whole. And what is it doing as a whole? Something fantastic? Something almost undreamable?! Something inexpressible in the words of man?!?! Nope. It's conforming to a scheme some guy dreamed up in the 1950s.

This whole fantastic universe can be boiled down to something representable within one of its most insignificant components—the brain of man. Even toying with that idea, strikes me as an egocentrism beyond belief. The universe makes use of no principle that cannot already be stuffed into the head of an average PhD in physics? The chain of logic that leads to the truth of the four-color theorem (apparently) can't be stuffed into our heads, but the ultimate operating principle for all that "is" and "can be" can?

It's a funny thing: I don't think I've met anyone who would imagine that mathematics will ever come to an end. Or even that it *can* come to an end. There'll always be new axiom sets to play with, new formal structures to write down. But with physics it's a completely different story. People are always wanting to say, "Well we've finally gotten there." Or, "Even though we're not there, we're pretty damned close." It's OK, even condoned, to have Dreams of a Final Theory. From this point of view, all the mathematics yet to come is worthless as far as the essence of the universe goes; the wad was already shot.

You get the point. It's a psychological one, but it's one that I find overwhelmingly powerful. It is that anytime any of us ever has the chutzpah to say, "Here's an ultimate statement about reality," or even a potentially ultimate one, what we're really doing is painting the world in the image of man. We're saying that the measly concepts we've managed to develop up to this point in time fit the world in a way that none of our previous concepts have, that none in the future will ever do better, and, most importantly, we view this not as a statement about ourselves and the situation set by our present evolutionary and intellectual stage, but rather as a property of the universe itself.

Now let me start moving toward the other side of the fence. The question someone like me—someone who has these kinds of blasphemous thoughts—has to ask himself is, how can I ever hope to be a scientist in spite of all this? What can science and all the great achievements it has given rise to in the last 400 years be about if one chooses to suspend one's dreams of a final theory at the very outset? (Or, to tribute Johnny, how can one have law without law?)

I think the solution is in nothing other than holding firmly—absolutely firmly—to the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettistas holding to it—we have to recognize that what we're doing in the game of science is swimming in the thick middle of things. We're swimming in this undulant sea, and doing our best to keep our heads above the water: All the concepts that arise in a physical theory must be interpreted to do with points of view we can construct from *within* the world.

That is to say, we have to loosen the idea that a physical law is a mirror image of what "is" in the world, and replace it with something that expresses instead how each of us can best cope with and hope to take advantage of the world exterior to ourselves. This, it seems to me, is something that by its very definition can be stuffed into the human brain. The current state of science is our presently best known means for survival. A scientific theory indeed, from this point of view, is yet another expression of Darwinian principles. Scientific theories evolve and survive because the survivors have a kind of staying power that none of the rest of the competition have. Not because they are part of the blueprint of the universe.

The situation of quantum mechanics—I become ever more convinced—illustrates this immersion of the scientific agent in the world more clearly than any physical theory contemplated to date. That is because it tells you you have to strain really hard and strip away most of the theory's operational content, most of its workaday usefulness, to make sense of it as a reflection of "what is" (independent of the agent) and—importantly—you insist on doing that for all the terms in the theory.

I know you're going to find the last sentence debatable, but that is what I see as the danger in the Everett point of view: You are able—or at least purportedly so—to view the universal state as a reflection of something, but at the cost of deleting all the concrete things it was meant to reflect in the first place. What I mean by this is, if we take any concrete situation in quantum mechanics—a system, a measuring device, and some kind of model for the beginning stages of a measurement—we can indeed construct a Church-of-the-Larger-Hilbert space description of it. I'll grant you that. But try to go the other way around without any foreknowledge of the "measurement": Start with the Church, and try to derive from it that a concrete measurement has taken place, and you encounter an embarrassment of riches. You don't know how to identify the valid worlds, etc., etc. (And, if you ask me, invoking decoherence as a cure-all is little more than a statement of faith that some guy from Los Alamos has the all the answers to all the tough questions the rest of us are too lazy to work out.)

So, I myself am left with a view of quantum mechanics for which the main terms in the theory—the quantum states—express nothing more than the gambling commitments I'm willing to make at any moment. When I encounter various other pieces of the world, if I am rational—that is to say, Darwinian-optimal—I should use the stimulations those pieces give me to reevaluate my commitments. This is what quantum state change is about. The REALITY of the world I am dealing with is captured by two things in the present picture: 1) I posit systems with which I find myself having encounters, and 2) I am not able to see in a deterministic fashion the stimulations (call them measurement outcomes, if you like) those systems will give me—something comes into me from the outside that takes me by surprise.

OK, now let me put myself squarely in your pasture. You worry that having those main terms in the theory refer to *my* (or *your*, or Joe Buck's) gambling commitments, is committing a kind of egocentrism. What respectable theory would refer to my particular vices, my desires, my bank account in making its most important statements?

This is going to surprise you now, but I agree with you wholeheartedly. Even enthusiastically so. Where I seem to disagree is that I do not find this a good reason to promote those vices, those commitments to an unearthly realm and call them "states of the universe" (or relative states therein). Instead, it seems to me to be a call to recognize them for what they are and to redouble our efforts for getting at the real nub of the matter.

Let me try to give you a way of thinking about this that you might respect. What was Einstein's greatest achievement in getting at general relativity? For the purposes of the present exposition, I would say it was in his recognizing that the "gravitational field" one feels in an accelerating elevator is just a coordinate effect—it is something that is induced purely with respect to the description of an observer. In this light, the program of trying to develop general relativity thus boiled down to trying to recognize all the things within gravitational and motional phenomena that should be viewed as consequences of our coordinate choices. Or to use a phrase I've come to like, it was in identifying all the things that can be viewed as "numerically additional" to the observer-free situation which come about purely by bringing the observer (scientific agent, coordinate system, etc.) onto the scene.

Now the point is, that was a really useful process. For in weeding out all the things that can be viewed as "merely" coordinate effects, the fruit left behind could be seen in a clear view for the first time: It was the Riemannian manifold that we call spacetime.

What I dream for in my foundational program for quantum mechanics is something just about like that. Weed out all the terms that have to do with gambling commitments (I used to call it information, knowledge, or belief), and what is left behind will play a role much like Einstein's manifold.

This much of the program, I hope and suspect you will understand even if you are not sympathetic to it. But, I don't know, you might be sympathetic to it. (Especially if I've done a good job above.) However, it is also true that you have rightly suspected some tendencies in me that go further. In particular, in opposition to the picture of general relativity, where reintroducing the coordinate system—i.e., reintroducing the observer—changes nothing about the manifold (it only tells us what kind of sensations the observer will pick up), I do not suspect the same of the quantum world. This is why I recommend to all my friends that they read William James's little article "The Sentiment of Rationality." It sort of sets the right mindset, even though it has nothing to do with quantum mechanics (other than in the efficacy of taking gambles) and goes much further on religion than I myself would go.

Anyway, here I suspect that reintroducing the observer will be more like introducing matter into pure spacetime, rather than simply gridding it off with a coordinate system. "Matter tells spacetime how to curve *when it is there*, and spacetime tells matter how to move *when it is there*." Observers, scientific agents, a necessary part of reality? No. But do they tend to change things once they are on the scene. Yes. Or at least that's the idea.

Does that mean that the scientific agent is something outside of physical law? Well, to give this an answer, you've got to go back and be very careful to use the picture of "physical law" that I built up at the beginning of the essay. What we are "governed" by, God only knows. He's the one, if anyone, who sits outside the physical universe and has a chance to look back at it whenever he pleases. Our task is to build up as good and solid a set of beliefs as we can from within it. In that way, we increase our survival power, and use our spare time to try to bring forth a few progeny of our own. (I used the word "governed," by the way, because you had used it above.)

If Galileo had dropped me from the tower, I feel pretty confident that I would have gone splat.

Aye yi yi, I wrote a lot. That's the dangers of being jetlagged in a foreign country without one's wife and kids. (I'm in Sendai visiting Ozawa.)

I'm going to have to reply to the other points of your note later.

18-02-02 *Still Thinking* (to W. K. Wootters)

I've been meaning to write you for some time about your inside/outside distinction for entangled systems, i.e., the stuff you wrote in your last philosophical note to me. Well, I'm still not quite able to say what I want to say, but let me set the stage by forwarding you the note I just wrote to John Preskill this morning. [See note just above.] It's probably just my jetlag, but I felt like I was being particularly clear in it. The note is almost completely self-contained and refers to John's embrace and my disembrace of the Everett stance. A good part of it was greatly inspired by two discussions I had with you (one in Princeton and one in Montréal). Maybe it'll show you the monster you created!

I hope to get my reply on your inside/outside idea off before this Sendai visit is over.

Bill's Preply

> You apologized for taking 20 days to respond to my note, and now I've taken a couple of months to respond to yours! Sorry about that!
>
> You quoted William James on the temperaments that underlie apparently dispassionate philosophical arguments. His observations are certainly correct. Maybe one reason I have not written up my little graph model is that it doesn't fully jibe with all my underlying prejudices. It captures one idea—that the universe we experience gets its form partly from being a *collective* experience—but in other respects I'm not satisfied with it.
>
> This semester I've been participating in a seminar at Williams on science and religion. It's been very interesting, and it has forced me to articulate, as least to some extent, my interpretation of quantum mechanics. I certainly don't have a fully worked out interpretation, but I do seem to hold firmly to two principles: (i) there is no physical breakdown of unitarity, and (ii) mind is crucial to the establishment of definite "facts" (such as a definite pointer reading, as opposed to endless entanglements and superpositions). In these respects I find myself in line with most versions of the many worlds interpretation, in which definite "facts" occur in one's subjective experience, not in the true reality. (Of course there are facts about the wavefunction of the universe, but that's not what I mean by "facts" in quotation marks.) But I deviate from the many worlds interpretation in that I would like to attach a greater ontological status to the world of our experience and a lesser ontological status to the wavefunction of the universe. That is, I want to say that the world of experience is a closer approximation to *reality* than is the wavefunction of the universe. (The latter would be more like a mathematical device, something that defines the range of possibilities.) This is pretty clearly a metaphysical distinction that cannot be tested by any conceivable experiment. But it makes a difference for the questions that arise in the science/religion dialogue. For example, I am led to say that mind is a crucial component of the structure of reality; and this makes it easier to say that God has something to do with the world. If

the ultimate reality is the wavefunction of the universe, then the ultimate reality seems much more mechanical and less God-friendly, like the world of classical physics.

Or maybe there *is* a possibility of testing the above distinction, at least in principle. (It depends on how I finish my interpretation.) Let's consider David Deutsch's thought experiment in which a sentient computer measures, in a reversible way, some property that doesn't have a definite value for the object being measured. Later, the computer will report that he observed a definite outcome but did not record which one, and the various possible paths will have been brought together successfully to exhibit interference. That's David's prediction. I would agree that interference should be possible, but I may disagree about what the computer will report. If it is true that one needs to have a *shared* world in order to have a sensible world, then when the computer was all alone in his private entanglement with the object he measured, who knows what he experienced? Maybe he experienced nothing more about that measured object than what one electron in a singlet pair experiences about the other electron. Maybe he cannot honestly report later that he saw one outcome or another.

But I have to admit that this idea of "sharing" a world with others is very spooky. I wonder if, by pushing the idea to its logical conclusion, I will have to embrace the possibility of telepathy. I don't automatically reject this possibility, but I think it's time to change the subject!

What I'm actually working on these days is the subject of entangled rings. I'm looking for a possible analogy between the *entanglement* among particles arranged in a lattice and the *action* associated with a lattice gauge field, but so far my lattices are just one dimensional (rings) and I have very little to offer in the way of an analogy. Nevertheless, I'm in the process of writing a short paper on the subject to be posted on `quant-ph` in the next week or two; so you'll see it there.

I hope your work is going well.

19-02-02 *Re-Tackle* (to L. Hardy)

I'm in Japan for a couple of weeks at the moment, and I'm finally getting some time put in the 16-hour days again. (Like I used to in the good old days.)

Anyway, I thought I'd tell you, though it is long overdue, I am finally tackling your 5-Axiom paper again. I'm starting to appreciate it much more for sure. If you just weren't so damned non-Bayesian!!! There's a lot of good stuff in it. My main difficulty at the moment is that you have a couple of moves that I know I don't want to allow into my porn: 1) taking mixtures of states (i.e., allowing probabilities of probabilities) as a fundamental step, and 2) invoking extensions to the Church of the LHS.

But I definitely think you are on the right track. And it's probably just a matter of my searching harder for some Bayesian ways of looking at what you've already done. (I'm doing that as we speak.) The most essential things that strike me are 1) the move to column vectors and thinking of measurement as a decomposition of the state, and 2) invoking a relation between K and N. I think those ingredients are definitely here to stay in my mindset. Also I'm warming up to the continuity axiom. I'll try to write you the reasons why soon. (But I've made promises before.)

Anyway, I'm super-looking forward to your stay.

19-02-02 *Where to Stop?* (to J. Preskill)

You know I've got a million ways of saying why I don't like the Everett interpretation—none of which you find very convincing—but here's a new thought that dawned on me as I was writing my last note to you, and I wonder what you think. Let me try it out on you.

Everett says, "You know Chris, all these silly things you do like leaving measurement as an undefined primitive, etc., will disappear and find a more satisfactory solution if you'll just lay back, relax, and recognize that the quantum states you're working with are really just relative states ... ones derived from the universal wavefunction under one or another decomposition."

I say, "Aha, OK. Then what is this wavefunction of the universe?"

He says, "Well for that, we ought to consult the Hartle-Hawking paper. Here it is: It's $|\psi\rangle$."

Then it dawns on me. How do I know that that state they wrote down isn't just the relative state of our universe with respect to some super-universe? And how do I know that that state is not itself some relative state with respect to some super-super-universe? And so on ad infinitum.

The point is, what principle of science tells you where to stop? None, I'd guess. Is that troubling? I don't know. But it seems a little fishy to me.

Everett tells me, "You've just got to recognize that the wavefunctions you use on a daily basis simply don't have the same ontological status as my universal wavefunction. You might call them 'states of knowledge' in a way. But my universal wavefunction, now that's the real thing; it's here independently of every man, woman, and child." But then I ask, "Well, why does yours get that exalted position? I claim that it itself is a relative state and you can't prove me wrong."

Like I say, I don't know what I think of this yet, but it does strike me as fishy. Once you get into the game of building a Church of the Larger Hilbert Space, who tells you how many pews to put there? That's not something it seems to me you can ever discern from within the universe. It's an article of religion, it seems to me, much like the imagery the appellation seems to provoke.

19-02-02 *One More Before Lunch* (to J. Preskill)

Here goes. Let's see if I can be brief enough to finish in time. No easy task for me!

Preskillism 6 : *Still, I'm flattered by your persistence. Or are you (as I can't help but suspect) slyly recruiting an Everettite who will make a weak case?*

No, I was honest in all that I wrote in the flattering note.

... Damn! Didn't make it. It's after lunch now.

What I was going to say was that I was absolutely honest in why I want you to write the sort of paper that you might for the special issue. The point being that if a physicist really, really does find Everett completely adequate to his needs ... and can argue that it's not a superfluous addition to what he's actually doing when he's doing a calculation, then that would be an interesting datum for the freaky types like me who see it as an ugly picture of the world.

I wasn't slyly recruiting you to make a weak case. But, of course, I actually did have an ulterior motive—something much bigger in my mind than the needs of the special issue—and now that you've forced my hand, I ought to be up front about it. I was banking that if you really did put your heart into making a convincing case for the Everettistas—i.e., the sort of thing that having to write a paper on the subject might draw out of you—then your intellectual honesty would cause you to see how much of the point of view really hasn't been worked out (yet? or maybe ever will be?). I.e., that they have no convincing/relevant argument for the probability rule, that they seem to require a preferred basis, that they seem to require a preferred tensor product structure, that to make sense of two systems, they need to invoke a third, and so on. And when you started to add all those things up, you would also realize that the Everett picture really wasn't much help after all in getting you to the point understanding what measurement is. That's how I was being sly.

19-02-02 *More Psychology 101* (to J. Preskill)

Preskillism 7: *Sure, scientists are arrogant. That our puny brains can grasp anything about how the world works is a miracle, and I can't pretend to be able to explain it. But I believe it is so.*

It's not the claims of "anything" that worry me so much. It's the claims of "everything." I.e., that our puny brains can grasp everything (in the sense of an "ultimate physics") is the thing that seems implausible to me. And if we can't have that, then we—or more realistically, those who are inclined to do so—ought to be asking what it is we're shooting for.

Sorry I hit a nerve.

19-02-02 *Sendai Morning* (to N. D. Mermin)

Thanks for helping pick me back up from my preskillsplat. (That could almost be a real German word.)

I'm enjoying Sendai and Ozawa's company greatly. And I'm once again thinking harder about complete positivity. The fact that the trace-preserving completely positive maps are isomorphic to the density operators on a larger space has got to be a truly deep point (in my quest to shore up my argument that the time evolution map is itself a subjective belief). But I just can't quite figure out how to put that into a convincing physical context.

Also, by the way, I'm going through Hardy's five axioms again. It's making a much bigger impression on me this time around. It's got a lot of good stuff in it. If it just weren't so damned non-Bayesian! The point is, I think it's got a hell of a lot of cleaning up to be done on it, but it really does have potential.

20-02-02 *Out Loud* (to W. K. Wootters)

Thanks for thinking out loud. I'll just respond to one point.

Woottersism 1: *Let me think out loud for a minute here about your note to John. I can think of a* pragmatic *reason for being an Everettista. (At those times when I am particularly attracted to the Everett view, this is what attracts me.) Even if we can't hope to* know *reality, if we can* guess *a model of reality, this guessing helps science progress. What Everett gives us is a guess at ultimate reality. So let's guess that Everett is right, and then work to falsify this guess.*

It's about the guessing part. I had meant to cover that case with the word "potentially" in this paragraph:

> You get the point. It's a psychological one, but it's one that I find overwhelmingly powerful. It is that anytime any of us ever has the chutzpah to say, "Here's an ultimate statement about reality," or even a potentially ultimate one, what we're really doing is painting the world in the image of man. We're saying that the measly concepts we've managed to develop up to this point in time fit the world in a way that none of our previous concepts have, that none in the future will ever do better, and, most

importantly, we view this not as a statement about ourselves and the situation set by our present evolutionary and intellectual stage, but rather as a property of the universe itself.

That's actually the point you first inspired in me with your aphorism about the dog. There are some things a dog will never understand; there are even questions he can never understand. Why should we expect the evolutionary chain to stop with us? In a way, this cluster of thoughts that I'm starting to think is a rather strong kind of anti-Church-Turing thesis. That is, I think we've gotten into the habit—and Deutsch tried to codify it in his 1985 paper—of thinking that the Church-Turing thesis implies that once you've got a universal machine (people like to say the human brain is one), then you've reached the end of the line. But one should not forget that what Turing was up to in his 1936 paper was to formalize the notion of what is "humanly computable." This was a point brought home to me by Chris Timpson's excellent undergraduate thesis

`http://web.archive.org/web/20010307183244/http://users.ox.ac.uk/~quee0776/thesis.html`.

By the way, not that it matters too much, but I refined one of my paragraphs in the note before I archived it away:

> The situation of quantum mechanics—I become ever more convinced—illustrates this immersion of the scientific agent in the world more clearly than any physical theory contemplated to date. That is because it tells you you have to strain really hard and strip away most of the theory's operational content, most of its workaday usefulness, to make sense of it as a reflection of "what is" (independent of the agent) and—importantly—you insist on doing that for all the terms in the theory.

And that—by the way again—may have been a point also inspired by someone else, namely Schrödinger. Though I haven't been able to completely track its origin in my mind. Somewhere—maybe his 1935 paper in Wheeler and Zurek—he says something like, "understanding quantum mechanics may not require the addition of more variables, but rather taking some of them away."

20-02-02 *WQRST Revision with Figure* (to C. H. Bennett)

Thanks for the new draft. Apparently we're both in Asia. I'm in Japan right now. I'm visiting Ozawa. (By the way, I think in the past you've confused him with Ohya. It was Ohya who said the wacky things about quantum computers solving NP-complete problems ... or at least that's what Ozawa says. Ozawa is quite a good guy and not so silly ... and he likes to laugh, so you'd probably like him.)

BTW, you never answered my last two questions:

1. Are you going to that Wheeler thing?
2. Can I count on you for a Maxwell-Demon and/or I-Love-Everett article?

Wish you were here, instead of there.

21-02-02 *Quantum Fest, Bell Labs Style?* (to H. Mabuchi)

I just read your proposal for the Wheeler book. Sounds good. (And by the way, you smooch ... adding that flattering remark.)

I've been having a good time with Ozawa. He's got some awfully good stuff, though he's not Bayesian enough for my taste. I suggested to him that he ought to visit Caltech. (And I'll suggest to you that you ought to invite him. He'll be in America in the July-August time frame, with a little time free between QCMC and the Feynman Fest.) I told him about your experimental interest in going beyond the SQL. But also he's got some interesting new stuff that even goes beyond the uncertainty principle! (Well ... that is, if you mean by that roughly the thing Heisenberg meant with his microscope model of attempting to infer position and moment simultaneously.) Anyway, I think it could make an awfully good splash for you, and he tells me he's in the process of distilling a potentially relevant / Q-optics realizable model that would do the trick. I've promised to help him work a bit on the style of the paper, so he can send it to *Nature*. (I always get myself into trouble with my promises.)

BTW, Ozawa is contemplating visiting Bell Labs in March too. So I might have a bang-bang-bang quantum fest: Hirota comes on March 12, Hardy will come for maybe two days before the Wheeler meeting, though the dates aren't settled yet. And you'll give us a talk on March 18. When were you thinking of coming into town? Would you be staying the night of March 17, or the night of March 18, or both, or neither. If Ozawa does come, he said he might try to line up with you.

On another subject, let me send you a little essay of my own. [See 18-02-02 note "Psychology 101" to J. Preskill.] I got carried away with replying to one of John Preskill's comments in a note to me the other day, and the end result was something I was particularly proud of as a statement of my quantum foundational ideas (broad sweep). John, of course, hated it and almost reacted violently. But Mermin and Wootters loved it. So, I'm probably doing something right.

22-02-02 *Sendai Thoughts* (to H. J. Folse)

Greetings from Sendai, Japan! I'm here for a small time of about 11 days, visiting Masanao Ozawa. (He's done some of the most interesting technical work on the uncertainty relations that's been done in a long, long time.)

Anyway, I just thought I would tell you I was thinking of you. Just a few moments ago at the university bookstore, I bought two volumes of Bohr's philosophical writings, translated into Japanese. The total came up to maybe about $11, so it seemed like a good investment. I think I will write a little something in each and give them to my daughters.

By the way, while I'm here, let me remind you again to send me that article of yours I still haven't gotten: H. J. Folse, "The Formal Objectivity of Quantum Mechanical Systems," Dialectica **29**, 127–136 (1975).

Also, if you could, I'd like to get your Växjö paper.

Let me also send you a little food for thought. It's a little essay I wrote in response to a remark John Preskill wrote me the other day. [See 18-02-02 note "Psychology 101" to J. Preskill.] I'm a little proud of it. I think it's maybe one of my best expressions yet of where I'd like to see quantum foundations studies go.

22-02-02 *Getting the Mindset* (to P. F. Scudo)

You asked for some materials to help you get more familiar with the problem I'd like you to work on.

OK, I'm ready to send you some now. And I'll inundate you, but don't let that frighten you. Only try to understand things to the extent that you've got some free time. (And I well expect you may have none at all!)

The first thing to do is read the Brun, Finkelstein, Mermin paper, "How Much State Assignments Can Differ," `quant-ph/0109041`. This is really the thing that started my thoughts off in the present direction, for I completely disagree with them. Their statement is in ultimate conflict with the Bayesian idea of what a quantum state can be. So, understand their argument.

After that, start reading my new samizdat (i.e., underground publication), which I will send you in a separate email. It's quite large (150+ pages), with plenty of repetition, but it mounts an attack on B, F, and M from just about every direction conceivable. Also there is the fact that as time went on, all the issues became clearer and clearer with me, and so I found crisper and crisper ways of expressing myself. Still reading it (and reading it carefully) might help you get in the right mindset for any number of problems we might be discussing.

The upshot of much of the samizdat is that, for consistency in one's Bayesianism, one must accept that the *assignment* of a POVM (living on a piece of paper) to a measuring device (living in a laboratory) is a subjective judgment at exactly the same level of subjectivity as the quantum-state assignment. Thus one is presented with a cross-roads. Either one accepts pure Bayesianism and gives up the idea that POVMs and quantum time evolutions have objective ascriptions (i.e., gives up the idea that they are independent of the agent assigning them), or one continues with the belief of objective POVMs and time evolutions and adjusts oneself to the idea that probability has to be objective too.

The direction I personally take is that probabilities are subjective, always. They are never objective. Therefore one must make sense of what one means when one speaks of an "unknown POVM" or an "unknown quantum operation." This is where a new kind of de Finetti representation must come in ... and hence your summer work.

But the most important thing for the present is understanding all the motivation leading up to that point. When you are ready to see a sketch of how the theorem ought to go, look at the note of 19 November 2001 to Caves and Schack titled, "A Lot of the Same."

For completeness sake, I will also send you a draft of the paper that Caves, Schack and I are presently constructing to make some of this official. The part that is maybe the most relevant for your education is the appendix on Dutch-book arguments. You might try to understand that. And I'll write you more about that later.

24-02-02 *Consistency* (to J. W. Nicholson & K. M. Fuchs)

Consistent coffee quality the world over! I'm just writing to report to both of you that I saw a Starbucks about an hour ago and ... despite my significant need for coffee and my overbearing homesickness for all things American, I want you to know I resisted it. More than that, I want you to know I shunned it! I'm sitting in a Japanese coffee shop as I write to you—itself a Starbucks wannabee, but not a Starbucks—drinking the most godawful mud-thick coffee, BUT I SHUNNED IT!!! And in the grand scheme, I know I'll come out the better for this.

A big hello from breezy cold Sendai,

25-02-02 *A Wonderful Life* (to W. K. Wootters)

Thanks for the two notes, and wow, thanks for reading the James essay. Your questions were anything but naive. In fact, they were much needed. In trying to answer them, I think I significantly clarified—to myself even!—what I'm hoping to get at. Besides, I certainly don't have a final stand yet; the whole point of view is in the process of formation and questions like yours really help.

I'll do my best to reply to your questions below, and in the process I think I'll finally compose what I've been wanting to say about your "private-world-within-entanglement" musings. At the end of the note, I'll list some of the open questions on my mind. (These are likely to be the naive ones!)

Woottersism 2: *Of course I'm very sympathetic to the perspective you express in this paragraph . . . but couldn't one still argue that as a matter of methodology, the tactic of pretending that we can know the whole story has served science well? We make up a model of the world, and this model gives us something to shoot at. We hang on to the model until we have found an explicit flaw in it (other than the flaw of hubris). And then we move on to a new model.*

I find this an interesting question. On the one hand, I think this strategy does work well in advancing science. On the other hand, scientists (and others) are much too prone to accept as true the pragmatic lie that says we can fully understand the world.

Your note to John P. goes some way toward laying out an alternative methodology. You speak of science in Darwinian terms: the most successful theories survive. How then do we proceed as scientists? I suppose the answer is that we still make up theories and test them, but the theories are not tentative descriptions of the world. Rather, theories are schemes for making predictions. But you obviously also want to say that our theories tell us something *about reality, even if they are not descriptions of reality. Moreover, our theories will tell us more about reality if we identify and remove from them those aspects that are subjective. So your view of science is not entirely operational. There is realism in the background.*

Have I understood you correctly?

Yes there is certainly a kind of realism working in the back of my mind, if what you mean by "realism" is that one can imagine a world which never gives rise to man or sentience of any kind. This, from my view, would be a world without science, for there would be no scientific agents theorizing within it. This is what I mean by realism: That man is not a priori the be-all and end-all of the world. (The qualification "a priori" is important and I'll come back to it later.)

A quick consequence of this view is that I believe I eschew all forms of idealism. Instead, I would say all our evidence for the reality of the world comes from without us, i.e., not from within us. We do not hold evidence for an independent world by holding some kind of transcendental knowledge. Nor do we hold it from the practical and technological successes of our past and present conceptions of the world's essence. It is just the opposite. We believe in a world external to ourselves precisely because we find ourselves getting unpredictable kicks (from the world) all the time. If we could predict everything to the final T as Laplace had wanted us to, it seems to me, we might as well be living a dream.

To maybe put it in an overly poetic and not completely accurate way, the reality of the world is not in what we capture with our theories, but rather in all the stuff we don't. To make this concrete, take quantum mechanics and consider setting up all the equipment necessary to prepare a system in a state Π and to measure some noncommuting observable H. (In a sense, all that equipment is just an extension of ourselves and not so very different in character from a prosthetic hand.) Which eigenstate of H we will end up getting as our outcome, we cannot say. We can draw up some subjective probabilities for the occurrence of the various possibilities, but that's as far as we can go. (Or at least that's what quantum mechanics tells us.) Thus, I would say, in such a quantum measurement we touch the reality of the world in the most essential of ways.

With that said, I now want to be very careful to distance this conception of reality, from what I'm seeking in the foundation game of quantum mechanics. Here's the way I originally put it to John the other day. Let me repeat a good bit of it so that it's at the top of your mind:

> OK, now let me put myself squarely in your pasture. You worry that having those main terms in the theory refer to *my* (or *your*, or Joe Buck's) gambling commitments, is committing a kind of egocentrism. What respectable theory would refer to my particular vices, my desires, my bank account in making its most important statements?
>
> This is going to surprise you now, but I agree with you wholeheartedly. Even enthusiastically so. Where I seem to disagree is that I do not find this a good reason to promote those vices, those commitments to an unearthly realm and call them "states of the universe" (or relative states therein). Instead, it seems to me to be a call to recognize them for what they are and to redouble our efforts for getting at the real nub of the matter. ...
>
> What I dream for in my foundational program for quantum mechanics is something just about like that. Weed out all the terms that have to do with gambling commitments (I used to call it information, knowledge, or belief), and what is left behind will play a role much like Einstein's manifold.
>
> This much of the program, I hope and suspect you will understand even if you are not sympathetic to it. ... However, it is also true that you have rightly suspected some tendencies in me that go further. In particular, in opposition to the picture of general relativity, where reintroducing the coordinate system—i.e., reintroducing the observer—changes nothing about the manifold (it only tells us what kind of sensations the observer will pick up), I do not suspect the same of the quantum world. ...
>
> Anyway, here I suspect that reintroducing the observer will be more like introducing matter into pure spacetime, rather than simply gridding it off with a coordinate system. "Matter tells spacetime how to curve *when it is there*, and spacetime tells matter how to move *when it is there*." Observers, scientific agents, a necessary part of reality? No. But do they tend to change things once they are on the scene. Yes. Or at least that's the idea.

From some of my choices of words, I think you probably got the impression that this thing—this structure within quantum mechanics—that I'm hoping to find at the end of the day is meant to be a model of "reality." Or at least our "current best guess" of what reality is. But no, that's not really what I want. And your questions helped make that much clearer to me. Remember, for me, the mark of reality is its indescribability.

What I'm asking for instead is something like what one finds in the old movie, *It's a Wonderful Life*. That is to say, in our scientific theories, we codify some fraction of what we know about manipulating the world and conditionally predicting the phenomena about us. However, suppose we wanted to get at a measure of our place in the world. How would we quantify it, or at least qualify it? That is, how might we ask how important our lives and agential actions are with respect to the theory we ourselves laid out?

Our only tool, of course, is the theory; for it defines the frame for optimal thinking (and imagination) at any given moment. We can only gauge our measure by deleting the free variable that is ourselves and seeing what is left behind. You surely remember what George Bailey found when his guardian angel granted his wish in *It's a Wonderful Life*. He found that his life mattered. So too is what I suspect we will find in quantum mechanics.

But all of that is the sort of thing I won't be able to say in a conference presentation for quite some time. It's the sort of thing that we discussed once before, in the context of some Jamesian quote. It's the underground reason for the philosophy.

At the level of convincing our peers, let me put it to you this way. Within quantum mechanics, there is an invariant piece which is common to all of us by the very fact of our accepting the theory. That is what we are in search of because in some sense—which need not pertain to a realistic conception of a theory's correspondence to nature—it is the core of the theory. It is the single part that we agree upon, even when we agree upon nothing else. In the direction I am seeking to explore, the quantum state is "numerically additional" to that core. (That is, the quantum state is a compendium of Bayesian "beliefs" or "gambling commitments" and is thus susceptible to the type of analysis James gives in his "Sentiment of Rationality." Our particular choice of a quantum state is something extra that we carry into the world.)

I hope that clears up some of the mystery of my thoughts for you—it did for me. Given John's implicit acceptance of the idea that "a true theory is a mirror image of nature," I should not have said in my note that I agreed with him "wholeheartedly." I do not intend for *any* part of the formal structure of quantum mechanics to be a mirror image of nature (in the sense of a proposed final theory). However, I do not intend to give up the reality of our world either.

From my point of view, the only "true" reality that creeps into quantum mechanics is "in the differential"—i.e., in the changes we induce upon our (personal) quantum states for this and that due to any stimuli we give to or take from the outside world. That, however, is a pretty amorphous thing as theoretical entities go. It is little more than what might have been called in older language, the measurement "click."

There is a temptation to go further—to say that the POVM element E_b associated with a measurement outcome b is itself an element of reality. But I think that has to be resisted at all costs. There are several arguments one can use to show that the *ascription* of a particular POVM to a measurement phenomenon is a subjective judgment at the same level of subjectivity as the quantum state itself. (In fact the two go hand in hand, one cannot support the subjectivity of the quantum state without also taking the subjectivity of the POVM.) Instead, one should view the (theoretical) ascription of a POVM to an actual measurement device as an attempt to set the significance and meaning of the "click" it elicits. Similarly for the Krausian quantum operation associated with the measurement: It describes the subjective judgment we use for updating our quantum-state assignment in the light of the "click." (If you want more details about these arguments, I can forward you some of my old write-ups on the subject.)

So, you probably ask by now, "What does that leave for the core of the theory? Aren't you throwing away absolutely everything?" And the answer is, "No, I don't think so." Let me give you an example of something which I think is left behind. Recall my favorite argument for why the quantum state cannot be an element of reality—it's the Einstein argument I wrote about in Section 3 of my NATO paper. Once I posit a state for a bipartite system, even though by my own admission my actions are purely local, a measurement on one of the systems can toggle the quantum state of the other to a large range of possibilities. Thus, I say that the quantum state of the far-away system cannot be more than my information or the compendium of subjective judgments I'm willing to ascribe to that system.

Notice, however, that in positing the original state, I had to also implicitly posit a tensor-product space for the bipartite system. Let me ask you this: Once this tensor-product space is set, is there any way to toggle one of the factors from afar just as with the quantum state? As far as I can tell there is not. Thus I would say that the Hilbert space of the far-away system is a candidate for part of the theory's core. Well, the Hilbert space—once the choice of a particular quantum state within it is excluded—really carries no substance beyond its dimensionality d. Thus, in a more refined way of speaking, what I really mean to say is that when I posit a quantum system, I am allowed to also posit a characteristic property of it. It is a property that can be captured by a single integer d.

There are some other things which I can argue will be "left behind" in such an analysis, but I don't want to clutter this note too much. Mainly I presented the example above so that I could give you a clearer sense of how I want to draw a distinction between the rawest forms of "reality" (the surprises the world gives us) and the "core of a theory."

It is the core of the theory (along with the theory as a whole) that I am starting to view in Darwinian terms. But don't we have every right to posit that core as a property of the world itself, at least as long as that belief serves us well? This, as you point out, has been the predominant image of what science is about heretofore.

The only answer I can give you is "yes, we can" (just as indeed we have heretofore). So, your point is well-founded. What I am worried about is whether we *should* posit it so. You say that this view has guided science well in the past. But how do you know? In a world with a view that there is no ultimate law, how do you know that we would not be a thousand years more advanced if we had only better appreciated our role as the substratum of our theories? I think it boils down to the difference between an active and passive view of what existence is about. Or maybe the difference between a positive and a negative view.

To make this point, let me try to put things back into the context of regular Darwinian evolution. Consider the word "elephant." Does it denote anything that exists in a kind of timeless sense, in a way that we usually think—or in my case, previously thought—of physical theories as existing? If the concept of an elephant is worthy of treating as a candidate for an element of reality, then so too will a theory's core.

Well, if we have bought into Darwinism in any serious way, then I would say, no, there is nothing particularly timeless about the concept of an elephant. There was once a chance that it might not even arise in the world. The "elephant" is merely a function of the selective pressures that cropped up in our world's particular history. And, ashes to ashes, dust to dust, the poor elephant may eventually disappear from the face of the universe, just like so many species that arose in the course of evolution only to be never discovered by a single archeologist.

But now, contrast the evolution of the elephant with the possible future evolution of the human species. The elephant was an accident pure and simple, from the strictly Darwinian view. But I would be hard pressed to apply pure Darwinism to the future of mankind. The birth of my oldest daughter, for instance was no accident. Her traits were selected based on personal visions that both her mother and I had for the future. Similarly, but not so excitingly, with the golden retriever, and all our other domesticated species. The key point is that in the present stage of evolutionary development, we have it within our power to move beyond strict Darwinism. This is what our industry of genetic engineering is all about.

However, we would have never gotten to this stage if we had not first realized that the concept of a species is not immutable. As strange—and as crazy and as scary—as it may sound, this is where my thoughts are starting to roam with physical theories. This does not mean, however, that we can have exactly what we want with our physical theories—that they themselves are little more than dreams. Just as the genetic engineer can make a million viruses that will never have a chance of surviving on their own, there is more to the story than our whims and fancies: There is the ever-present selective pressure from the outside. But that does not delete the genetic engineer's ability to make something that was never here before.

But now, I go far, far, far beyond what I needed to say to answer all your questions. Mainly, I just wanted to emphasize why I intentionally placed the words "a priori" in my definition of reality way above.

I fear now slightly that you're going to realize I'm one of the craziest people you've ever met! And, trust me, I'm not sure I really believe all that I said in the last three paragraphs. But it does strike me as a productive, or at least hopeful, train of thought that someone ought to explore. I guess I offer myself as the sacrifice.

— — — — — —

There. I think that's enough of my going around your questions in a rather wide way. Let me now zoom back to the center of one of them for purposes of a final emphasis.

Woottersism 3 : *But you obviously also want to say that our theories tell us* something *about reality, even if they are not descriptions of reality.*

I hope you can glean from all the above that I do indeed believe our theories tell us something about reality. But that something is much like what the elephant tells us about reality. Its presence tells us something about the accumulated selective pressures that have arisen up to the present date. A theory to some extent is a statement of history. It is also a statement of our limitations with respect to all the pressures yet seen, or—more carefully—a statement of our limitations with respect to our imaginations for classifying all that we've yet seen. (I for instance, cannot jump off the leaning tower of Pisa unprotected and hope to live; you, for instance, cannot get into your car and hope to push on the accelerator until you are traveling beyond the speed of light.) Finally, to the extent that we the theory users are part of nature, the theory also tells us something about nature in that way.

But for any theory, there is always something outside of it. Or at least that's the idea I'm trying to build.

PS. Way above, I said I would finally say a few words about your "private-world-within-entanglement" musings. But somehow it didn't quite fit in with the flow of the rest of what I wanted to say. So, let me try to present the statement in isolation. From my point of view, the quantum state, and with it entanglement, never pierces into the quantum system for which we posit a parameter d (the "dimension"). Similarly for any bipartite system for which we posit two parameters d_1 and d_2. The quantum state is only about what I'm willing to bet will be the consequences when I reach out and touch a system. Otherwise, indeed, a quantum system denotes a private world unto itself. And similarly with bipartite systems. We have very little right to say much of anything about the goings-on of their insides. (This part of the picture is something I've held firmly for a long time; it even shows up in my *Physics Today* article with Asher.) Thus, I guess what I'm saying is that I can find a way of agreeing with what you wrote me:

Woottersism 4 : *Or maybe there* is *a possibility of testing the above distinction, at least in principle. (It depends on how I finish my interpretation.) Let's consider David Deutsch's thought experiment in which a sentient computer measures, in a reversible way, some property that doesn't have a definite value for the object being measured. Later, the computer will report that he observed a definite outcome but did not record which one, and the various possible paths will have been brought together successfully to exhibit interference. That's David's prediction. I would agree that interference should be possible, but I may disagree about what the computer will report. If it is true that one needs to have a* shared *world in order to have a sensible world, then when the computer was all alone in his private entanglement with the object he measured, who knows what he experienced? Maybe he experienced nothing more about that measured object than what one electron in a singlet pair experiences about the other electron. Maybe he cannot honestly report later that he saw one outcome or another.*

But maybe I'm coming at it from a different point of view.

PPS. I also promised to end with some open questions. But I'm petered out now. And if you've gotten this far, you're probably exhausted too. So I'll just leave it for the future, depending upon how interesting you find the ideas above, or how much you think they're nonsense!

Only two and half days left in Japan.

26-02-02 *New Jersey Trip* (to L. Hardy)

I'm about to start on the long journey back to NJ this morning. (I'm in Japan.) There's a little good news on my end. Within your framework, I finally found an absolutely simple Bayesian justification for the linearity assumption of the probability rule. I hope to write you an explanation of that on the flight. I think there's no doubt in my mind now: Your axioms (or maybe only a very small variation thereof) are certainly the most promising starting point for what I've been hoping to see in quantum mechanics. For the first time in my life I've understood (roughly) where the vector-space structure comes from. Sorry it took me so long to come around. If I were only smarter …

So, again, hopefully I'll write you a longer explanation on my flight. (If I don't snooze the whole way.)

26-02-02 *A Tired Old Man* (to G. Plunk)

I wrote the note below to David Mermin the other day, and he gave you a pretty good grade. Besides that, I understand from my superiors that there is still a *chance* (nothing certain) of filling at least one more position in our summer program.

Thus what it boils down to is for me to decide whether I've got enough spunk in me to take on a summer student this year … and/or whether this note will frighten you away. You unfortunately have written me at a time when I have just had a bad experience with a summer student. That is, though the summer went well for her (by what she tells me), I felt that my summer was used for little more than giving basic lectures in linear algebra and giving emotional support to a kid on her first excursion to summer camp. I lost a lot of time during the summer when I could have and should have been writing papers. I hope that builds some imagery.

Thus if I'm going to take on a student, it has to be a student who will:

a) Ask *some* of his own questions of nature, and not rely upon me for every input beyond the bigger picture and a reasonable amount of direction.

b) Use the resources available at Bell Labs to seek out an answer, which includes knocking on other people's doors besides mine some fraction of the time. I.e., I want a team player.

c) Handle my being away for a good piece of the summer, with only email contact.

d) Be emotionally secure in the sense of not needing me as a den mother. And,

e) Believe that issues to do with quantum foundations are the most exciting ones in the physics of our day.

The pay here is good. The towns are dull. But you'd live near a train station, with Manhattan about 50 minutes down the line.

I'll give you a shot at convincing me. Feel free to talk with Mermin before composing your reply (or deciding I'm not worth the bother). In fact I'll CC this note to him, so he knows what's up. But I'll need a reply pretty quickly, due to Bell Labs' time constraints … and my own time to let a decision percolate through my mind.

I've got a long trip from Sendai back to New Jersey tomorrow, so I may not be in email contact again for a couple of days.

27-02-02 *An Even Tireder Old Man* (to N. D. Mermin)

I forgot to include you as a recipient of the note below as I had promised. Let me not tell a lie: It's attached this time around.

I made good progress on several fronts in Japan. Maybe the most important—though most trivial—was that I finally found a rather natural Bayesian motivation for the linearity of the probability rule in Lucien's system. (Lucien is a trickster; he sneaks axioms in left and right that he doesn't call axioms.) Anyway, at the very beginning of the paper already, I couldn't accept his motivation for linearity, falling back as it does on the idea that a state refers in an objective way to (a class of) preparations. That is, he didn't take states to be Bayesian beliefs pure and true, and that's crucial to me.

Here's the solution; it's as simple as can be (now that I see it). The upshot of Lucien's formalism is that a measurement is any "I-know-not-what" that causes a refinement of one's initial beliefs (consistent with the other axioms). Thus a measurement is simply an application of Bayes' rule by its very definition—moreover, any application whatsoever within the allowed range. OK, that's good enough for the single observer and already thrills me (because it expresses in a more rigorous way a claim I made in the NATO paper at the level of density operators).

But when there are two observers and you view measurement as nothing but an application of Bayes' rule, you have to have some method of saying when they agree that it is the same measurement (so that you don't fall into the infinite regress I tried to warn you of when you were going full-steam-ahead with BFM). Well, here again the solution is simple: Two observers should call a refinement of their beliefs the same measurement, precisely when they draw the same meaning for the data they obtain. Thus as long as both observers ascribe the same $P(\text{data}|\text{hypothesis})$, we will say they "agree" upon the measurement.

And that's it. That does the trick. That and that alone gives linearity. No Gleason's theorem; not even anything fancy. I'll make this prediction right now: Quantum mechanics will turn out to be one of the simplest structures we've ever seen in physics, and for 75 years we've just been too pigheaded to see it.

Somewhere over the Pacific,

28-02-02 *Bell Labs!!!* (to G. Brassard)

I'm just back from 11 days in Japan, and on what fuel I'm running I'm not sure. In fact I'm deeply asleep as I write this. But in my slumber I sifted through my pile of mail and found the CRM theme year poster. What a delight to see our meeting listed! But what horror to see Los Alamos as my affiliation!! I hope Kiki will wake me up from this nightmare …

28-02-02 *Which Day?* (to L. Hardy)

What title would you like me to give the talk? And while I'm at it, do you have an abstract? Certainly talking on your axioms would be great. Though—for business appearances—you might want to throw in a word or two (but no more than that) about how getting the essence of quantum mechanics straight may shed some light on what it is that powers quantum computing ... or some such money-grubbing sort of thing.

01-03-02 *Drunken Nights* (to J. W. Nicholson)

I almost forgot to send you the two letters I told you about. I'm very proud of these. In fact, I think they're my best written and most provocative pieces in a year. This is what we've been talking about those drunken nights. [See 18-02-02 note "Psychology 101" to J. Preskill and 25-02-02 note "A Wonderful Life" to W. K. Wootters.]

In what follows, `\bjp` and `\ejp` mean the beginning and ends of a John Preskill quote. `\bbw` and `\ebw` means the same for Bill Wootters. `\bq` and `\eq` mean the beginning and ends of some general quote. The Everett point of view refers to the many-worlds interpretation of quantum mechanics. Otherwise I think the two notes are self-contained.

You can see by the time stamp of the present note, jetlag has me by the nads. I kind of doubt I'm going to come in today, so, sorry, probably no usual-Friday-lunch thing.

02-03-02 *Mermin Festschrift, 2* (to C. M. Caves)

Cavesism 70: *I'm inclined to think that the Mermin festschrift would be just the right place for my speculations about the reality of Hamiltonians, especially since he has kind of encouraged this idea. What do you think?*

I think, "I wonder why you ask *me* this!?!?" Are you contemplating that we might have dual submissions? Abbott and Costello, Crosby and Hope, Lewis and Martin, the Smothers Brothers? The problem is, both of us would want to be the straight man.

OK, enough sarcasm. To answer your question, yes, that probably would be a good idea.

By the way, here's a little technical meat for the hungry wolf. Suppose I were willing to go with you on the objectivity of *the* interaction Hamiltonian or unitary in a measurement interaction. (I'm not willing in actual fact, but that's beside the point for this question.) If we write down the POVM such an interaction ultimately leads to, then we will get something like this for its elements E_b:

$$E_b = \mathrm{tr}_{\mathrm{A}}\Big((I\otimes\sigma)U(I\otimes\Pi_b)U^\dagger\Big)\ ,$$

where all the terms have the usual interpretation. I believe, according to you, there should be only one subjective piece to the right hand side of this equation, namely the ancilla's initial density operator σ.

Here's my (innocent, first) question. The subjectivity in σ will afford a range in our interpretation of which POVM was actually measured in any such interaction. What is that range? How large is it?

A corollary question—this one is more rhetorical and less innocent—is this. You know I have never liked the above formulation of what a measurement is because one has to invoke a second measurement (the Π_b) to explain the first (the E_b). This leads to an infinite regress because one can ask (as von Neumann did), where do the Π_b come from. Thus, just to make sure we're on board, let me reconfirm that I got the setting in your mind right in my elaboration above. Namely, that the only truly subjective term you would accept in the displayed equation is σ? The range in E_b is consequent only to that? If that's the case, I would also like to understand how you invoke a stopper at the level of the Π_b. That is, why do the Π_b not have some of their own range, induced by a higher level measurement model?

You can tell I'm still deeply jetlagged.

03-03-02 *De Finetti and Strong Coherence* (to P. F. Scudo)

Thanks for the ghost story.

Scudoism 1: *Will tell you more as soon as I finish my calculations for this paper.*

Not to worry at all. I have a hard enough time leading a rushed life. You should not expect that I expect that out of you when I cannot live up to that standard myself! When you arrive here you'll have plenty of time to practice the macabre.

But still, if you've got the interest, who am I to get in your way. Here's some historical investigative work that you might tackle if you find that have nothing better to do. In fact, your friend Regazzini might be a wonderful resource in this regard. In the draft of the anti-BFM paper that Caves, Schack, and I are putting together, we make a distinction between simple "Dutch-book consistency or coherence" (i.e., the notion that de Finetti and Ramsey first introduced) and "strong Dutch-book consistency." The latter is a notion that apparently Abner Shimony first introduced, though Caves and Schack stumbled across it independently in one of our many wars. (See reference 12 on page 139 of the samizdat I sent you for the original citation.) [See 06-02-02 note "The Commitments" to C. M. Caves & R. Schack.]

Starting on page 133 of the samizdat, I write a fairly strong polemic against the "requirement" of strong-consistency—it seems to me that regular Dutch-book consistency is the most we can demand. Here's my question. As you'll see from the quote of Ian Hacking on page 137, de Finetti was aware of Shimony's addition to coherence and apparently rejected it. Do you think it might be possible to find out where he wrote about this? More importantly, what were his particular reasons for rejecting the stronger notion?

04-03-02 *Sliding Off the Deep* (to H. J. Folse)

I was able to print out your paper without a hitch. And thanks moreover, for your detailed notes on my letter to John Preskill. Let me try to answer some of your questions (in particular the one about Popper and propensities) by sending you still more rubbish. It's in the form of a follow-on letter (to the Preskill one) that I sent Bill Wootters while still in Japan. [See 25-02-02 note "A Wonderful Life" to W. K. Wootters.] Of course, I would appreciate any further comments like your last ones if you've got the time!

Let me quickly reply to two of your points explicitly.

Folsesm 5: *Do any physicists still think this way? I realize cosmologists talk about a TOE, but surely the conduct of contemporary research is not animated by the thought that we're closing in on some "final" description of the universe? That was true enough a century ago, but I think that the weight of philosophy of science at least in the 20th century has pulled against that sort of image of science – certainly in a post-Kuhnian era. It is curious perhaps that this kind of attitude might* *still persist in so-called "foundational" studies, whereas I would suppose in something like biological research everyone would have become much more historicized by now.*

Nope, the attitude runs pretty rampant in the quantum information and computing community. (I'm definitely one of the outsiders there.) Have a look at David Deutsch's *The Fabric of Reality*. I think he expresses much of the majority opinion of our little clique there. But I think you'd be surprised to know that the "dreams of a final theory" attitude runs pretty rampant even in such workaday fields as laser physics. I just got the following message from an experimentalist friend of mine whose work is predominantly used for the design of better fiber optics:

Nicholsonism 5: *One of the thoughts that continues to strike me is your optimism in the continual evolution in physical theory. I will admit that on bad days I feel like we are in the twilight of physics - only incremental progress is left to be had.*

Of the physicists I have met who have even heard of Kuhn or Rorty—there aren't that many—almost all of them view what little they know of their thoughts (i.e., Kuhn and Rorty's) with a little contempt. (By the way, John Preskill's reaction to my letter was particularly violent. So I think it is the ideas, and not the men, that bug their sensibilities.)

By the way, I have indeed just discovered Rorty and I find him wonderful. I've read the two collections of essays *Philosophy and Social Hope* and *Consequences of Pragmatism*. It won't be long, and I'll move on to the book of his that you recommended. Also since Växjö, I've gotten a good feel for James (I read *Pragmatism*, *The Meaning of Truth*, and Perry's massive biography of him), and also of Dewey (though I've only read *John Dewey: The Essential Writings*).

Thanks for telling me about Kitcher; I hadn't heard of him. And thanks for the pointer to Toulmin; I'll look more deeply into him than I have in the past.

Concerning one of your other points,

Folsesm 6: *It is arrogant perhaps, but I don't see this as the egocentricity. Every attempt to sketch a conception of the universe from our best theories at any date in human history in effect commits such arrogance. Were the Newtonians of the end of the seventeenth century being "egocentric" to think that Sir Isaac had done nothing less than peer into the mind of the Divine and discerned God's blueprints for the universe?*

Yes. (In my opinion.) And you might interpret James and pragmatism in general as a reaction to that. However, I think in our modern age with quantum mechanics we have a motivation and opportunity in front of us that James did not have. Try to give quantum mechanics a naive realist interpretation—you can do it, or at least both Everett and Bohm tell us we can—and you find yourself contorting yourself beyond belief. It's as if nature is telling us for the first time, "Please don't interpret me in a naive realist fashion. I can't stop you, but please don't."

OK, the sun is rising and the vampire must return to his native soil.

04-03-02 *The Good Questions of Nicholson* (to J. W. Nicholson)

Nicholsonism 6: *So. My question to you is two-fold. Do you still feel the same way about particle physics experiments that try to continually increase the energy of interactions between particles? And, if not, what extrema in physical theory should we be poking at in order to continue the evolutionary progress?*

You ask a good question old friend—the second one—and I shall have to think a while before I reply. Or at least compose myself before I reply.

In the case of your first question the answer is easy. Yes, I still feel that way. And the feeling is not that the SSC would not have been worthwhile science per se; it might have been. It was just the cost versus insight-and-control-of-nature ratio that doomed it in my mind. Furthermore, the paradigm the high-energy types presuppose certainly puts me off. They think they are doing something deeper than tabulating the Hamiltonian of an iodine atom, say, but I don't think they are. They are just tabulating the Hamiltonian of another system, and that has the value it always has.

But let me think harder about how to answer your second question. For my present project (i.e., quantum mechanics), it strikes me not so much as poking at the extrema that we ought to be spending our time on, but in poking at the consistency of the worldview we wish to embed the theory in. To revert to my analogy of the construction of general relativity, it was only consistency that Einstein was seeking when he first started contemplating how gravitation fit within the framework of special relativity. Seeing that it did not (and weeding out all the coordinate effects as I expressed earlier) is what led to the bending of the manifold.

But don't take that as my definitive (or exhaustive) reply. I'm still thinking.

04-03-02 *Green Light* (to A. Y. Khrennikov)

Khrennikovism 7: *I am waiting for your papers for proceedings. Please send me a signal that you are working! If yes, I shall wait one-two weeks.*

Yes, please do wait for me. I am working, and I am trying to find a way to respond to your "Växjö interpretation" both directly and indirectly.

05-03-02 *A Hardy Reminder* (to C. H. Bennett)

OK, it's set, Hardy's going to give his talk on Thursday March 14, from 2:00 to 3:00. And as I told you, Osamu Hirota will also be around. (Not to mention the pleasures of my goofy laugh and van Enk's dry wit.) So, you've got every reason to come. We'll look forward to seeing you Thursday.

06-03-02 *Poetry on Concrete* (to L. Hardy)

I decided to write this letter in LaTeX so that I would have a small chance of being clearer. A few months ago, you wrote me this:

Hardyism 1: *[M]y basic picture of preparation/transformation/measurement is rather similar, though considerably less poetic, than your picture of humans interacting with a world. . . . After the poetry you need to make the journey from vision to concrete construction.*

You're quite right. I think your paper has finally started to mix the right sort of concrete to support a building like the one I want to see built.

In what follows, all I'm going to do is keep true to this word I learned—poetaster—and try to write a little amateur poetry at your construction site. There are three things that really intrigue me in your paper, and one that really confuses me. I'll take them one at a time.

The first is simply the absolutely beautiful ease with which you bring us to a vector space structure. I had known about the concept of "informationally complete" POVMs for years[17]—they're even a crucial part of the proof of our quantum de Finetti theorem[18]—but before your work I had never appreciated so clearly that the probability distributions they generate ought to be taken as the very most primordial representation of a quantum state.

> *What is a quantum state? Nothing more than one's gambling commitments with respect to the outcomes of a fiducial measurement. Full stop.*

Or at least, that's the way I want to view it.[19]

Now, what you write presently is not completely consistent with that characterization. I'm going to do my best to try to make that plain to you. You write, "I don't believe that my approach needs to adopt a particular approach to interpreting probability." There is a sense in which that is certainly true for the mathematics you've already performed. However, if we're ever going to get past this foundational impasse in quantum mechanics, I would say firmly that we cannot leave the story there.

Here is the difficulty. If you take an objective approach to probability, then you are necessarily left with the conclusion that a measurement "here" in general changes something objective (or physical) over "there." And you will never get around that. Just consider any entangled state. Making a measurement on Alice's side changes her ascription—without any considerations of time or distance—of the state on Bob's side. If a state is purely equivalent to a set of probabilities and probabilities are not subjective degrees of belief or gambling commitments, but rather objective and independent of the agent, then something physical must change at Bob's site with that far away measurement. And if you believe that, then you end up with conundrums out the wazoo.

I know that may not be your stand yet, but it is mine. Thus I'm left with the task of putting your mathematics into a framework I can accept. The first and clearest example of a technical mismatch between our views is what we each might consider as a valid motivation for the convex-linearity rule in your Eqs. (41)–(43).[20]

I think if we were to trace the roots of our mismatch, we would find it in that I do not subscribe to your "basic picture of preparation/transformation/measurement" ... which I think you think is common to both of us. Here's the way I put it to Asher Peres when we were constructing our Physics Today paper:

> In general I have noticed in this manuscript that you lean more heavily on the word "preparation" than we did in our letter to Benka. ... Unless I misunderstand your usage of the word, it may actually be a little too anthropocentric even for my tastes. The problem is this: consider what you wrote in the paragraph about the wave function of the universe. It seems hard to me to imagine the wave function of those degrees of freedom which we describe quantum mechanically as corresponding to a "preparation." Who was the preparer?

[17]I think the idea can be traced back to E. Prugovečki, "Information-Theoretical Aspects of Quantum Measurements," Int. J. Theo. Phys. **16**, 321 (1977).

[18]See Section IV of `quant-ph/0104088`.

[19]And I say this even though I was saying things right after the Växjö meeting like I said to Khrennikov in the letter of 4 July 2001 attached below. Somehow, I just never took the idea so seriously as I do now, i.e., after reapproaching your paper in Japan. "Fiducial measurement" + "a notion of transformation from one measurement to another that carries the probabilities with it" is what tells the whole story!

[20]By the way, I have to take a little exception with your accounting rules for your axioms. In similar treatments from the "operational school," say of Kraus, Holevo, and Ozawa, a convex-linear or affine assumption (of exactly the same spirit as your Eqs. (41)–(43)) is always listed as a distinct axiom.

> It is for this reason that Carl Caves and I prefer to associate a quantum state (either pure or mixed) solely with the compendium of probabilities it generates, via the Born rule, for the outcomes of all potential measurements. And then we leave it at that. Knowing the preparation of a system (or the equivalence class to which it belongs) is one way of getting at a set of such probabilities. But there are other ways which surely have almost nothing to do with a preparation. An example comes about in quantum statistical mechanics: when the expected energy of a system is the only thing known, the principle of maximum entropy is invoked in order to assign a density operator to the system. There may be someone beside me in the background who knows the precise preparation of the system, but that does not matter as far as I am concerned—my compendium of probabilities for the outcomes of all measurements are still calculated from the MaxEnt density operator.
>
> To help ensure that I was not jumping to conclusions on your usage of the term, I reread today your paper "What Is a State Vector?" [AJP **52** (1984) 644–650]. There was a time when I agreed with everything you wrote there .... But as of today at least, I think a more neutral language as in our letter to Benka is more appropriate.

Thus what I seek is a picture that involves only measurement and transformation. (And if the truth be known, I'd like to get rid of transformation *in a sense*, leaving only measurement behind from your trinity. But that's a longer story, and I'll only give hints of it below.) However, I do think your formalism is just about up to the task, despite your Figure 1 on page 4.

The way I see it, this thing called the "preparation" is just a token for the *right and true* quantum state you imagine for a system. But from a steadfast-Bayesian[21] point of view, there is no such thing as a right and true probability assignment for anything. Consequently, if a quantum state is to be solely a probability assignment for the outcomes of a fiducial measurement, from the steadfast-Bayesian view, there can also be no such thing as a right and true quantum state.[22]

Let me emphasize this once more at the purely classical level before going on. For the Bayesian there is no such thing as a *right* probability assignment; there is no such thing as a *wrong* probability assignment. A probability assignment is a set of numbers that signifies which gambles one is willing to make, period. How those numbers are set in any given instance is an issue that has nothing to do with nature. Instead it has everything to do with all the ugly things we try to keep out of science: one's emotions, one's intelligence, one's hopes, the rumors one has heard recently, and so on. In fact, a probability assignment has nothing to do with the world itself—in a sense it floats above the physical world.

What this boils down to is that—*within a Bayesian paradigm*—your argument for convex-linearity cannot make any sense:

> Assume that the preparation device is in the hands of Alice. She can decide randomly to prepare a state $\mathbf{p}_A$ with probability λ or a state $\mathbf{p}_B$ with probability $1-\lambda$. Assume that she records this choice but does not tell the person, Bob say, performing the measurement. Let the state corresponding to this preparation be $\mathbf{p}_\mathbf{C}$. Then the probability

[21] Some would say "radical Bayesian."

[22] I have argued this point at great length in a correspondence with Caves, Schack, and Mermin predominantly. I'm going to post that on my website at just about the same time I send this note off to you. The file is called "Quantum States: What the Hell Are They?," and the relevant part with respect to my remark starts on page 19 and winds up somewhere around page 97. However, the most relevant notes are the ones to Brun-Finkelstein-Mermin dated 7 August 2001, to Caves-Schack dated 22 August 2001, and to Mermin dated 2 September 2001. I hope you will have a look at those at the same time as reading the present note.

Bob measures will be the convex combination of the two cases, namely

$$f(\mathbf{p}_C) = \lambda f(\mathbf{p}_A) + (1-\lambda) f(\mathbf{p}_B) \tag{1}$$

> This is clear since Alice could subsequently reveal which state she had prepared for each event in the ensemble providing two sub-ensembles. Bob could then check his data was consistent for each subensemble. By Axiom 1, the probability measured for each subensemble must be the same as that which would have been measured for any similarly prepared ensemble and hence (41) follows.

Here's the difficulty. What does the distribution $(\lambda, 1-\lambda)$ refer to? From the Bayesian view, it can only refer to a belief (or better yet, a gambling commitment) that Bob possesses concerning Alice's actions. On the other hand, the numbers $f(\mathbf{p}_A)$ and $f(\mathbf{p}_B)$ refer to a couple of alternate beliefs a completely different person, Alice, might possess. However—*from the Bayesian standpoint*—there is nothing *a priori* that can be used to rigidly identify Bob's beliefs as a function of Alice's ... as Eq. (1) above would imply. Alice has every right to believe anything she wants; Bob has every right to believe anything he wants.[23]

For the Bayesian, all the action—all the science—is not in the probability assignments themselves that various individuals might make, but in how those assignments change upon the acquisition of new data (steered by the influence of rationality).[24] In particular, the important things to ferret out are the conditions in any situation where two observers will converge in their beliefs when they agree that they are acquiring the same data. Bernardo and Smith put it in a beautiful way:

> The subjectivist, operationalist viewpoint has led us to the conclusion that, if we aspire to quantitative coherence, individual degrees of belief, expressed as probabilities, are inescapably the starting point for descriptions of uncertainty. There can be no theories without theoreticians; no learning without learners; in general, no science without scientists. It follows that learning processes, whatever their particular concerns and fashions at any given point in time, are necessarily reasoning processes which take place in the minds of individuals. To be sure, the object of attention and interest may well be an assumed external, objective reality: but the actuality of the learning process consists in the evolution of individual, subjective beliefs about that reality. However, it is important to emphasize, as in our earlier discussion in Section 2.8, that the primitive and fundamental notions of *individual* preference and belief will typically provide the starting point for *interpersonal* communication and reporting processes. In what follows, both here, and more particularly in Chapter 5, we shall therefore often be concerned to identify and examine features of the individual learning process which relate to interpersonal issues, such as the conditions under which an approximate consensus of beliefs might occur in a population of individuals.
>
> — pp. 165–166, Bernardo and Smith, *Bayesian Theory*
>
> What is the nature and scope of Bayesian Statistics within this spectrum of activity? Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts

[23]Nor does it help to try to divert the discussion from a two-player situation and stuff all of these beliefs into a single head (Alice's say). For at any instance, Alice believes what she believes. And there is nothing within the Bayesian creed to force what she ought to believe in various other hypothetical situations—for example, where the distribution $(\lambda, 1-\lambda)$ might describe the amount of amnesia she has concerning a previous belief that she's just forgotten.

[24]Read my note to Mermin titled "Reality in the Differential" starting on page 128 of my previously mentioned posting.

> of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."
>
> — p. 4, Bernardo and Smith, *Bayesian Theory*

And, it is precisely this that I'm going to use to get to your Eq. (41) from a Bayesian starting point. But before I can do that, let me praise the second thing that I see as a deep contribution of your paper. This is the very *definition* of what a measurement is: It is *any* application of Bayes' rule consistent with your remaining axioms.[25]

To make some sense of this, let me put the problem into a notation that is slightly better for my purposes. Suppose the outcomes of a fiducial measurement are labeled by h and the outcomes of some other measurement we might contemplate are labeled by d. Then I will dually write the quantum state (via your identification of states with probabilities for the outcomes of a fiducial measurement) as a function $P(h)$ or as a vector $\mathbf{P}$. What I mean by an application of Bayes' rule is the supposition of a set of probability distributions $P(h|d)$ (or $\mathbf{P}_d$ in vector notation)—one for each d—such that

$$P(h) = \sum_d f_{\mathbf{P}}(d)\, P(h|d) \;, \tag{2}$$

or alternatively

$$\mathbf{P} = \sum_d f_{\mathbf{P}}(d)\, \mathbf{P}_d \;, \tag{3}$$

for some other probability distribution of $f_{\mathbf{P}}(d)$ over the variable d. When a particular data d is collected, one—at least tentatively—enacts the transition

$$\mathbf{P} \;\longrightarrow\; \mathbf{P}_d \;. \tag{4}$$

This is what I claim you have taught us is "what a quantum measurement is."[26]

A measurement is any "*I know not what*" that enacts a transition of the form Eq. (4). What is the variable d? How is it defined? It simply is not, except as a place holder in a particular instance of Bayes' rule. And that's it: That's the story of measurement.

Now, where does one get the convex-linearity of the probability rule $f_{\mathbf{P}}(d)$ in a Bayesian-happy way? The technique is to consider when it is that two observers will think they are performing

[25] There is no doubt that I have been predisposed to saying something like this for a long time. For instance, at the top of page 26 of my NATO paper `quant-ph/0106166`, I say, "Quantum measurement is nothing more, and nothing less, than a refinement and a readjustment of one's state of knowledge." The difference is, your paper brings this beyond just some metaphor. There I tried to capture the idea with my Eqs. (57)–(59), which are explicitly in terms of density operators. You on the other hand, do it with the real thing. I.e., you make it absolutely airtight that it is Bayes' rule operating in the background, and not just some noncommutative analog of it.

[26] Note carefully that I used the word "tentatively" in this description. That is because there is a possibility of the further quantum phenomenon that measurement can be more invasive than supposed classically. What this means operationally is that the $\mathbf{P}_d$ may not ultimately concern the original fiducial measurement, but may instead be about a completely different fiducial measurement at the end of the process. See my discussion centered around Eqs. (57)–(59) of `quant-ph/0106166`.

the same measurement. That is to say, I could walk into the room and think to myself that the measurement device in front of me gives me warrant for the decomposition in Eq. (2). You on the other hand, with a completely different set of beliefs and experiences, may think that the device warrants you to the decomposition

$$Q(h) = \sum_d f_{\mathbf{Q}}(d)\, Q(h|d)\ . \tag{5}$$

When shall we say that we actually agree upon the measurement? Classically, the answer is when the statistical model made use of by each of us is the same:

$$Q(d|h) \equiv \frac{f_{\mathbf{Q}}(d)\, Q(h|d)}{Q(h)} = \frac{f_{\mathbf{P}}(d)\, P(h|d)}{P(h)} \equiv P(d|h)\ . \tag{6}$$

That is, if we had hold of which one of the fiducial outcomes actually obtained, then we would draw the same meaning from it. Here, by "meaning" I mean how much we would feel warranted in revising our beliefs that a d would have popped up if we had instead performed an appropriate measurement for it.

By the way, notice one thing. If this account deviates from standard Bayesianism any at all, it is only in this. Never once did I invoke the *necessity* of a joint probability distribution

$$P(h,d) \equiv f_{\mathbf{P}}(d)\, P(h|d) \tag{7}$$

in my description of Bayes' rule. Of course, such a function exists as a mathematical artifice—i.e., it has all the properties of a joint probability distribution—but one should not try to make any meaning for it beyond that. In particular—and, especially with regards to the quantum context—one should not feel it necessary to interpret the function as a degree of belief of the simultaneous *existence* of a true h and a true d.

OK, let me use that now to start talking about convex-linearity again. Suppose there are at least three agents on the scene, Alice, Bob, and Charlie. And suppose Alice subscribes to Eq. (2) for her description of what the device in front of her is about, whereas Bob subscribes to Eq. (5), and Charlie still further subscribes to,

$$R(h) = \sum_d f_{\mathbf{R}}(d)\, R(h|d)\ . \tag{8}$$

If that is as far as it goes, then there is nothing whatsoever we can say about the relation between $f_{\mathbf{P}}(d)$, $f_{\mathbf{Q}}(d)$, and $f_{\mathbf{R}}(d)$. Indeed there need be no relation whatsoever.

However, suppose Alice, Bob, and Charlie share the minimal belief that actually the same measurement is being performed with respect to each of their descriptions. Then by definition

$$P(d|h) = Q(d|h) = R(d|h)\ . \tag{9}$$

Fine. Now consider those cases where, as it turns out,

$$P(h) = \lambda Q(h) + (1-\lambda) R(h)\ . \tag{10}$$

Letting

$$G(d) = f_{\mathbf{P}}(d) - \lambda f_{\mathbf{Q}}(d) - (1-\lambda) f_{\mathbf{R}}(d)\ , \tag{11}$$

we see immediately that

$$G(d) = \sum_h \Big[P(h)P(d|h) - \lambda Q(h)Q(d|h) - (1-\lambda)R(h)R(d|h)\Big] \tag{12}$$

$$= \sum_h \Big[P(h) - \lambda Q(h) - (1-\lambda)R(h)\Big]P(d|h) \tag{13}$$

$$= 0 \tag{14}$$

since

$$f_{\mathbf{P}}(d) = f_{\mathbf{P}}(d)\sum_h P(h|d) \equiv \sum_h P(h)P(d|h)\ , \tag{15}$$

and so on.

And that's it, from this perspective.

$$P(h) = \lambda Q(h) + (1-\lambda)R(h) \qquad \Longrightarrow \qquad f_{\mathbf{P}}(d) = \lambda f_{\mathbf{Q}}(d) + (1-\lambda)f_{\mathbf{R}}(d) \tag{16}$$

when and only when the agents who hold the beliefs **P**, **Q**, and **R** "agree" to the meaning of the d-"clicks" the measuring device will elicit.

Part of me fears that you're going to view all this as little more than rhetoric. It took me five pages of explanation to do what you did in one paragraph. So let me try to reiterate the point of this exercise from my perspective one last time, before I throw in the towel. Here's the point. If you believe that the quantum state is rigidly (or even loosely) connected to reality, then—it seems to me—you will never find a way out of the conundrum of "unreasonableness" associated with "state-vector collapse at a distance." I.e., our community will always be left with a search for the *mechanism* that makes it go. Our community will always be left with the embarrassing questions to do with its clash with Lorentz invariance. And, maybe most importantly, we will be left with the nagging question of why we can't harness this mechanism for more useful purposes.[27] I view these questions as a distraction from the ultimate goal we all ought to have, of building a more interesting, more fantastic physics.

In connection to the present discussion, I would claim that THE preparation is little more than an anchor for such a rigid connection. It is, even if implicitly, a reversion back to the old issues that led to all the trouble in the first place. Thus I am compelled to find a way to absolutely disconnect the quantum state from the physical world. Yet I am still required to make sense of what it is that we're doing when we practice quantum mechanics. For this, the trail has already been blazed by (radical) Bayesian probability theory, and thus I take that as my cue. Getting at Eq. (16) from within a subjectivist framework is one of those first steps you just have to take ... and then hopefully never have to think about again.

OK, with that, I'm ready to praise you my third and final time in this letter. But I think you'll probably hardly feel it's a praise. Let me tell you another goal of seeing how much of quantum mechanics can actually be run through with complete airtight consistency from the subjectivist standpoint. And that is to pick the theory apart. For, you see, I see no difficulty whatsoever in imagining that any theory can consist of two basic components—the completely subjective and agent-dependent, and the completely objective (or intersubjective if you will) and thus, agent-independent. What is the distinction between these components (I hope you ask)? It is this: Once

[27]If we can't see angels, why posit them? If we can't see superluminal communication, why posit it? Alternatively, if we do posit angels, the natural question to ask is how might they help save our souls. I would be suspicious of a world where the angels served little purpose other than rounding out a theology and not aiding in our souls' deliverance.

an agent posits one of the objective components in any particular instance (whether it's "really" there are not), there is no move *within* the theory that will allow him to change that supposition.

Let me give you an example of the latter. Posit a bipartite system with Hilbert spaces $\mathcal{H}_{d_1}$ and $\mathcal{H}_{d_2}$ (with dimensions d_1 and d_2 respectively) and imagine an initial quantum state for that bipartite system. Now, I would say that the quantum state must be a subjective component in the theory because the theory allows me localized measurements on the d_1 system that can change my quantum state for the d_2 system. In contrast, I would say that the number d_2 that was posited for the second system is an objective component in the theory. There is nothing I can do at the d_1 site that will allow me to change the numerical value of d_2. The only way I can change that number is to scrap my initial supposition. Thus, to that extent, **(on first pass)** I have every right to act as if the numbers d_1 and d_2 are potential "elements of reality."[28]

Here's where I really think you sell yourself short by advertising your system as an extension or generalization of classical probability theory (with classical probability theory as a special case that's gotten by deleting one of the axioms). For I would say that your framework of "states" as vectors and "measurements" as applications of Bayes' rule is *classical probability theory*, full stop. Or, I should just say "*probability theory*, full stop"—for, the word "classical" seems to imply that it is a subject somehow within empirical science (rather than "law of thought" that antecedes science). In showing me that even quantum "measurements" can be viewed legitimately as nothing more than applications of Bayes' rule, you have done me a great service. For you demonstrate to me more clearly than ever that the concept of POVM ought to be put onto the subjective side of the shelf when I tear quantum mechanics into its two components. But your other intriguing axioms—like the simplicity and composite-system axioms—which you think give the possibility of generalizing upon classical probability, I would say are nothing of the sort. Instead, I would say they express just the opposite. These axioms seem to me to say something about what we are positing of nature. They express something that is not subjective and is not "law of thought." They in fact form part of the restriction to probability theory that I asked for over and over in my `quant-ph/0106166`. Thus drawing those axioms out explicitly strikes me as great progress.[29]

Thus to come back to my example of Hilbert-space dimension. As I have already explained, I would say that quantity is a (potentially) objective feature of nature. But now, after understanding your paper better, I would say the same of your composite-system axiom. In fact, that axiom is a way of elucidating the very meaning of the dimension d. As such, it forms part of the "manifold-structure analogy" behind quantum mechanics which I tried very hard to explain to Preskill and Wootters in two further letters I'll paste below. (I hope you will read them along with this note, as I think they greatly elucidate what I was hoping to convey in the previous four paragraphs.)

There. Three praises; take them for what they're worth. They are the three things that really intrigue me about your paper. But I've only started my digestion process. I'll leave a discussion of the confusions until we get some single-malt in front of us next week.

08-03-02 *Your Webpage* (to H. J. Folse)

Thanks a million for the link to your webpage. I had a quick flip though it before my flight this morning, and it looks great. (I'm on my way to Atlanta for an AMS meeting.) I'll have a much deeper read through it upon my return; I can tell you that it will certainly help orient me with respect to the various schools. Just a snap judgment—maybe not to be taken seriously, since

[28] Please note that I emphasized the qualifier "on first pass." The reason for this emphasis will become clear if you read the letters to Preskill and Wootters that I have pasted into the present letter to you.

[29] And that is why this minor spanking counts as a praise!

I've only mildly skimmed the page—but it looks like I might be closer to an "entity realism" than anything else.

I'll tell you how my van Fraassen impressions go after I meet him. There's no doubt I'm more Bayesian-optimistic than ever. In particular, I've now become convinced (during my Japan stay) that a combination of my NATO paper (`quant-ph/0106166`) and Hardy's "five-reasonable-axioms" paper tells us unequivocally that collapse (or Lüders' rule or Kraus's generalization of it, or whatever you wish to call it), is NOTHING OTHER THAN an application of Bayes' rule along with a possible redefinition of which "observable" (POVM generally) the information is relevant to. In other words, collapse is after all an utterly trivial notion—it is refinement and readjustment (to a new context) of information and nothing more.

BTW, I met Plotnitsky and his girlfriend in Manhattan a few weeks ago. You should have heard the praise and the descriptions of beauty they gave your home! This report came, by the way, as were sitting in one of Plotnitsky's sister's homes: a three level penthouse on the corner of Broadway and Bleeker Street in Manhattan! Surely, a multi-million dollar affair.

12-03-02 *Wheeler Link* (to R. Pike)

Pike-ism 1: *Wheeler in* The Times*:*

- Dennis Overbye, "Peering Through the Gates of Time," *New York Times*, 12 March 2002, `http://www.nytimes.com/2002/03/12/science/physical/12WHEE.html`.

Thanks. That was good. Typical Wheeler.

15-03-02 *Ice Cream and Reciprocity* (to C. H. Bennett)

Thanks for taking care of Lucien last night. After a night of alternating between sweat and shivers, I seem to be on the road to recovery. A comment and a question.

Comment. I think you said that I should make an overlay for my quantum-axiom slide that says "Give an ice-cream reason, if possible," for each of the axioms. By this, you were indicating that my "Give an information theoretic reason, if possible," is a rather arbitrary thing to be asking. But, I say that it is not arbitrary precisely because the main object of our attention in the theory, the quantum state, specifies *probabilities*. It specifies how we ought to be taking nature into account when we make our mortal decisions. The quantum state does not specify flavors of ice cream. Thus it seems to me like an entirely natural question to ask: If the main object of the theory is of an information theoretic (or decision theoretic or call it what you will) character, then how much of its support structure might also be of the same character?

I think I put the goal of this program in a particularly clear way in a note to John Preskill. [See 18-02-02 note "Psychology 101" to J. Preskill.] I'll paste it below. Read it if you have the time.

Question. On the other hand, I rather liked what you said about wanting to base quantum mechanics on the idea that interactions are more symmetric in this theory than in classical physics. In interaction, both observer and observed are changed in the process. Could I get you to write your own version of that in a small paragraph, so that I can have something solid to read and think about?

24-03-02 *Leaving Dublin* (to C. King)

I just wanted to let you know, I absolutely enjoyed my little time alone with you today. Even if there had been nothing else to the rest of the meeting—and there was much to the rest of the meeting—today's discussions would have made it worthwhile for me to make the trip! I hope I can repay you one day likewise.

Concerning the part of today's conversation about Darwinist conceptions of scientific theories, I told you that I would give you an exact pointer to my write-up of that. The place to look is at the file titled *Quantum States: What the Hell Are They?* posted at my website (link below). In particular, have a look at the letter to Preskill titled "Psychology 101" starting on page 143 and the letter to Wootters titled "A Wonderful Life" starting on page 149.

The references I owe you concerning characterizations of completely positive maps are these:

- J. A. Poluikis and R. D. Hill, "Completely positive and Hermitian-preserving linear transformations," *Linear Algebra and Its Applications*, vol. 35, pp. 1–10, 1981.
- G. P. Barker, R. D. Hill, and R. D. Haertel, "On the completely positive and positive-semidefinite-preserving cones," *Linear Algebra and Its Applications*, vol. 56, pp. 221–229, 1984.
- L. J. Landau and R. F. Streater, "On Birkhoff's theorem for doubly stochastic completely positive maps of matrix algebras," *Linear Algebra and Its Applications*, vol. 193, pp. 107–127, 1993.

Take care and enjoy your visit with your wife and children.

24-03-02 *Packing Your Suitcase* (to F. Verstraete)

I'm just about to get on the road home from a week-long stay in Dublin. Today I had the greatest conversation with Chris King where we mapped out a good load of mathematical questions that arise naturally from my musings about the Bayesian nature of quantum time evolution operators. So, if you're amenable, I think I'll have you work on precisely those problems during your visit to Bell Labs. (CK, in fact, thought these questions were exactly up your alley ... based on a paper you wrote recently characterizing CPMs.)

Anyway, here's the stuff to be reading to start to get ready for the project. (That is, if you've got any spare time. If you don't, then certainly don't worry about this.) The easy stuff is my new collection of letters posted at my website (link below). The title is "Quantum States: What the Hell Are They?" and pay particular attention to the parts where I give arguments for the "subjectivity" of the Kraus operation (and POVM) that one associates with a measurement. Anyway, that forms my personal background and motivation for the technical questions I've got for you.

But you don't have to worry: The technical questions stand alone and will be worthwhile to you even if you buy in to none of my particular motivation. As a little preparation for that, you might obtain the following papers:

- J. A. Poluikis and R. D. Hill, "Completely positive and Hermitian-preserving linear transformations," *Linear Algebra and Its Applications*, vol. 35, pp. 1–10, 1981.
- G. P. Barker, R. D. Hill, and R. D. Haertel, "On the completely positive and positive-semidefinite-preserving cones," *Linear Algebra and Its Applications*, vol. 56, pp. 221–229, 1984.

- L. J. Landau and R. F. Streater, "On Birkhoff's theorem for doubly stochastic completely positive maps of matrix algebras," *Linear Algebra and Its Applications*, vol. 193, pp. 107–127, 1993.

Is everything settled now for your visit? Can you send me the schedule of when you'll be arriving? We're gonna have to find a place for you to live.

25-03-02 *Company from Hybernia* (to A. Plotnitsky)

This is just a small note to tell you I'll be traveling with you today. I'm in Dublin right now, but soon to go home. I was in a bookstore yesterday, and I ran across a copy of Paul Feyerabend's posthumous book *Conquest of Abundance*. Seeing your recommendation printed on the back pushed me over the edge and I bought it!

26-03-02 *Popper and Detractors* (to R. Pike)

Pike-ism 2: *From Adam Gopnik's article about Karl Popper in the latest* New Yorker*:*

> The reason science gave you sure knowledge you could count on was that it wasn't sure and you couldn't count on it. Science wasn't the name for knowledge that had been proved true; it was the name for guesses that could be proved false.

Thanks, I ought to have a look at the full article.

Here's the way I put my own take on exactly the same subject in a letter to Bill Wootters recently. [See 25-02-02 note "A Wonderful Life" to W. K. Wootters.]

27-03-02 *Still More Zing!* (to J. Gea-Banacloche)

Thank you for the long, beautiful note. I loved it, and it makes me so wish that I had had enough time to get to the rest of my talk in Dublin—i.e., the part where I try to give some substance to the word "Zing!" I introduced on one of the early slides. (Recall, "Zing!" was meant to be a place holder for the answer to the question "What is real about a quantum system?")

Gea-Banaclochesm 1: *You seem to have a pretty good idea of how to make most of the postulates (on probability, tensor space structure, and even wavepacket reduction) flow in a more or less natural way from some reasonable information-theoretic ideas, once you are given the basic formal structure of operators and Hilbert spaces. The main question would appear to be, where does this formal structure come from, and what does it actually say about the physical universe?*

That is the main question. And—*in spirit*—I believe our proposed answers appear to be essentially the same:

Gea-Banaclochesm 2: *the basic physical fact at the heart of quantum mechanics is the uncertainty principle, which one could formulate very generally as follows–*

(P1) The nature of quantum mechanical systems is such that, even when we have all the possible information we can have about them, we cannot, in general, predict the outcomes of all the possible experiments we could carry on them. Specifically,

The only issue in my mind is how to carry out the word "specifically" in a way that would satisfy the aesthetic I'm seeking. In particular, I would really like to pin down where the noncommutativity comes from in a way that does not make a priori use of the notions of "compatibility" and "incompatibility." In other words, I'd even like "incompatibility" to be a secondary notion, rather than a primary one. I think it is possible, and my present thinking is that it can be made to come out in a pretty way as a natural consequence of the mismatch between the number of bits that can be reliably stored in a quantum system and the number of measurement outcomes required for an "informationally complete" representation of a quantum state. (That is to say, something along the lines of the mismatch between the numbers N and K in Lucien Hardy's treatment in `quant-ph/0101012` and `quant-ph/0111068`.)

In any case, I have a discussion of "Zing!" in several places in my paper `quant-ph/0106166`. Especially in the closing section. Since then, I've come quite a way toward what I was trying to express above, but you'd have to dig harder for that—it's not exactly published properly yet. The place to look at the moment is in the file titled "Quantum States: What the Hell are They?" posted on my webpage (link below). The upshot is the following (working) statement: Each quantum system can be postulated to have an intrinsic amount of "sensitivity" to our experimental interventions upon it, and that sensitivity can be captured by a single parameter d (call it the dimension). From that, everything about the algebra of observables for a system follows from a basic statement about the very meaning of Bayes' rule in that context.

I'm working hard to get some of this in a proper paper presently.

By the way, I loved your Teilhard de Chardin quote:

> The history of the living world can be summarized as an elaboration of ever more perfect eyes within a cosmos in which there is always something more to be seen.

In my own way, I tried to express something similar in two pieces that I've come to be pretty proud of. Have a look at my letter to John Preskill titled "Psychology 101" starting on page 143 and my letter to Bill Wootters titled "A Wonderful Life" starting on page 149 of the file I mentioned above.

I hope that you yourself made it home safely and comfortably from Madrid, and also that you found your family doing well there. Emma's chicken pox are already clearing up: So maybe I was gone just the right amount of time!

27-03-02 *Invited Submissions* (to J. H. Shapiro)

Attached below are two pdf files. One, titled `QCMC02.pdf`, contains the abstract to my invited submission. The other, titled `honest.pdf`, represents a joint (contributed) submission with Patrick Hayden. I hope that the organizing committing will still consider it a valid contributed submission even though we have missed the deadline. (The missing of the deadline was entirely my fault, as I somehow got the impression that all submissions were due April 1.)

In any case, of the second submission, Hayden would give the actual presentation. Just to let you know, Patrick Hayden is presently a Prize Postdoctoral Fellow at Caltech and one of the young movers of quantum information. (He also had a Rhodes Scholarship for his graduate work at Oxford, and has recently been offered a faculty position at McGill University in Montréal.) So, having him there, would certainly do well for the conference's reputation.

28-03-02 *Men in Power* (to P. Hayden)

Sorry to get back to you so late. (If you haven't discovered by now, it's a habit with me.) The trouble was, I was in Ireland last week and part of this week, and, of course, I got carried away with all the conversations of the moment.

Anyway, I finally sent in the abstracts yesterday. Here's the reply I got from Jeff Shapiro today:

Shapiro-ism 1: *Thanks for submitting your title and abstract in such a timely fashion. As for the "*`honest.pdf`*" paper, I am sorry but I feel that I must be absolutely bureaucratic and reject it. We have 182 submissions for QCMC'02 as it is, and even with posters this number is considerably more than we'll be able to accept. I have already rejected several late submissions, and it would be unfair of me to create an exception for you and Patrick Hayden.*

I am sorry about this. Maybe just maybe if I had gotten off my duff and read your email before just leaving for Ireland ...

In any case, I do think we should pursue doing something for real this summer. I'll try to write you soon about scoring function issues.

Below, let me attach the abstract I actually sent Shapiro. Mainly I made only the most minor of changes so that it would fit in two pages: I.e., I killed the last section and took out the associated reference. Also I shortened the acknowledgements just a tad. Stylistically, I changed all the "non-"s to "non"s (it's a pet peeve of mine) and changed the title to "Keeping the Quantum Experimentalist Honest". Oh yeah, I also used a `\Big` in Eq. (28). And I think that's it.

Like I say, I've got a lot more to write to you about scoring functions, but let me give you the short of it. The main reason I'm attuned to the issue has to do with some of my recent debates with Caves and Schack concerning the strength of axioms one should assume in a Dutch-book foundation for probability theory. I say the axioms should not be so strong as to keep an agent from lying (no matter what the case). This is important when one considers concatenating probabilities for various events. Something of an account of where I last left the story can be found on my webpage in the file "Quantum States: What the Hell Are They?". The most relevant part appears in a letter titled "The Commitments" starting on page 133. A link to my webpage is below.

Again, I'm sorry about the QCMC thing, and I'm sorry you spent the time writing the abstract for nothing. But maybe still something good will come out of this in the end.

Keeping the Quantum Experimentalist Honest

Christopher A. Fuchs and Patrick Hayden

von Neumann entropy and the honest expert

There are a number of compelling axiomatic characterizations [AczelD75, OhyaP93] of the von Neumann entropy $S(\rho) = -\mathrm{Tr}\rho \log \rho$ of a density operator ρ and the quantum source coding theorem provides what is certainly its most important functional characterization [Schumacher95, JozsaS94, BarnumFJS96]. Here we give what could be the *simplest* characterization of the von Neumann entropy, generalizing an idea of Aczél's for characterizing the Shannon entropy [Aczel80]. The approach is to show that the entropy arises as the optimal expected payoff in a type of game between a cash-strapped experimentalist and her employer. Thus, it provides a meaningful interpretation of the von Neumann entropy in a completely nonasymptotic setting, when only one realization of the density operator ρ is available.

Alice, an ambitious scientist attempting to build a quantum computer, manages to produce states that, to the best of her knowledge, are described by the density operator σ. She then sends a note to her employer, Bob, saying that she has succeeded in producing the state ρ. Bob, as a conscientious scientific administrator, would like to ensure that Alice does not lie about her progress. In other words, he would like to guarantee that $\rho = \sigma$. Therefore, from time to time Bob will visit Alice's lab and perform a measurement, conditioned on the data ρ that she sent him. Her future funding will depend on the outcomes of these measurements. The question is, what measurement should Bob perform and how should he structure his payments to Alice such that she never has any incentive to deceive him?

We propose the following strategy for Bob. He should perform a measurement in the basis $\{|e_i\rangle\}_{i=1}^n$ that diagonalizes $\rho = \sum_i r_i |e_i\rangle\langle e_i|$ and, upon getting outcome i, pay Alice $C + D \log nr_i$ dollars, for nonnegative constants C and D. Notice that if ρ described a maximally mixed state then $r_i = 1/n$ and, regardless of the outcome, Bob pays Alice C dollars, ensuring that she will be able to support herself even if her lab produces completely random states.

Now, assume to start that Alice has prepared a state σ such that $[\sigma, \rho] = 0$. We can then write $\sigma = \sum_{i=1}^n s_i |e_i\rangle\langle e_i|$ and the expected payment from Bob to Alice is

$$C + D \log n + D \sum_{i=1}^{n} s_i \log r_i, \tag{17}$$

which is maximized if and only if $s_i = r_i$ for all i, giving an expected payment of

$$C + D \log n - DS(\sigma). \tag{18}$$

Thus, under the assumption that Alice reports a state ρ that commutes with her σ, she maximizes her payment by choosing $\rho = \sigma$. Moreover, there is a built-in incentive for her to try and produce pure states since she is penalized by an expected amount $DS(\sigma)$.

Now consider what happens in the general case. The probability that Bob will measure outcome i is $p_i = \mathrm{Tr}(\sigma |e_i\rangle\langle e_i|)$ and the expected payment is

$$C + D \log n + D \sum_{i=1}^{n} p_i \log r_i. \tag{19}$$

Again we find that

$$\begin{aligned} -\sum_{i=1}^{n} p_i \log r_i &\geq -\sum_{i=1}^{n} p_i \log p_i && (20) \\ &= H(p_1, p_2, \ldots, p_n) && (21) \\ &\geq S(\sigma), && (22) \end{aligned}$$

where the second inequality holds because the output entropy of a complete projective measurement is always at least as large as the entropy of the input density operator. (See, for example, Ref. [NielsenC].) Thus, the expected payment is

$$\begin{aligned} & C + D \log n + D \sum_{i=1}^{n} p_i \log r_i && (23) \\ \leq\ & C + D \log n - DH(p_1, p_2, \ldots, p_n) && (24) \\ \leq\ & C + D \log n - DS(\sigma). && (25) \end{aligned}$$

Equality in the first line occurs if and only if $p_i = r_i$ for all i and in the second line if the measurement is a complete projection in the eigenbasis of σ. (Again, see Ref. [NielsenC].) Therefore, under the assumption that the payment as a function of the outcome probabilities is unique, the proposed measurement strategy is the unique solution to the problem of keeping Alice honest.

The following theorem of Aczél's guarantees this last fact for density operators with $n \geq 3$. It is notable for the extremely weak assumptions made of the payment function.

Theorem 1 (Aczel80) *Let $n \geq 3$. The inequality*

$$\sum_{i=1}^{n} p_i F_i(q_i) \leq \sum_{i=1}^{n} p_i F_i(p_i) \tag{26}$$

is satisfied for all n-point probability distributions $(p_1, \ldots, p_n)$ and $(q_1, \ldots, q_n)$ if and only if there exist constants $C_1, \ldots, C_n$ and D such that

$$F_i(p) = D \log p + C_i, \tag{27}$$

for all $i = 1, \ldots, n$.

The efficient gambler and accessible information

The accessible information $\mathrm{Acc}(\mathcal{E})$ in an ensemble of states $\mathcal{E} = \{\rho_i; p_i\}$ is defined to be the maximum over all possible POVM measurements $\{M_j\}$ of the mutual information $I(i:j)$, where $p(j|i) = \mathrm{Tr}(\rho_i M_j)$. Despite the enormous effort that has been spent studying the accessible information [Fuchs95], however, justification for the definition remains unclear. One motivation, which we won't describe in detail, is via asymptotic coding [HausladenJSWW96, Holevo98, SchumacherW97]. The approach yields the accessible information as the maximal rate at which bits can be sent using quantum codewords whose marginal distribution is given by $\mathcal{E}$ but only if severe restrictions are imposed on the encoding and decoding. Namely, the codewords must be product states and the decoding must be performed by product measurements. If the second restriction is relaxed, it is the Holevo χ quantity

$$\chi(\mathcal{E}) = S\Big(\sum_i p_i \rho_i\Big) - \sum_i p_i S(\rho_i) \tag{28}$$

which answers the coding question, not the accessible information. Thus, the delicacy of the asymptotic problem provides yet another incentive for finding a single–realization interpretation of the accessible information.

Such a realization exists, as the quantum analog of an old idea of Kelly's [Kelly, CoverT]. We imagine that Alice is a bookie who takes bets on the mutually exclusive events $\{i\}$, which occur with probabilities p_i. For simplicity, we assume that Alice gives fair odds and does not collect a fee, so that if the gambler Bob wagers S_i on event i, he is paid S_i/p_i when i actually occurs. Now suppose that Bob is given private information about which outcome occurred prior to it becoming public knowledge, so that he could still place bets at the original odds. Moreover, imagine that Bob's source of information is a *noisy quantum channel* which outputs ρ_i when event i occurred. Bob's task, therefore, will be to perform some POVM measurement $\{M_j\}$ and then use the information gained from the measurement to make a wager, all done in such a way as to maximize the exponential rate of growth of his capital

$$G = \lim_{n\to\infty} \frac{1}{n} \log \frac{C_n}{C_0}, \tag{29}$$

where C_0 is his initial capital and C_n his capital after n rounds of betting. We show that this maximal rate of growth is the accessible information: $G_{max} = \mathrm{Acc}(\mathcal{E})$.

Acknowledgments

We thank Howard Barnum and Simon Benjamin for helpful discussions. P.H. was supported by a Sherman Fairchild Fellowship and US National Science Foundation grant EIA–0086038.

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28-03-02 *And a Short Reply* (to R. Campos)

Thank you so much for your kind letter. I just printed out your PLA paper, and I look forward to understanding it. [R. A. Campos and C. C. Gerry, "A Single-Photon Test of Gleason's Theorem," Phys. Lett. A **299**, 15–18 (2002).] Sorry it has taken me so long to reply, but I was tied up with a meeting in Dublin from the 19th through the 26th and then it took me still a couple days beyond that to recover from the travel.

Just a couple of very quick remarks on your manuscript. 1) In your second sentence, you say "Starting from the description of physical states as $\hat{\rho}$ as vectors in a Hilbert space, and observables as projectors $\hat{\Pi}$ in that space, Gleason proved that the ..." That however, is a little inaccurate and downplays Gleason's achievement. Gleason does not start at all with states as vectors in a Hilbert space. He only starts with the observables that you mention and then, in the process of his proof, *derives* that the states can be represented as density operators. Thus, in a sense, he gets the states for free. In any case, from the rest of the paper it looks that you do understand this. I'm just pointing out that the wording of the introductory paragraph of the paper is misleading. 2) In reference 5, you write C. C. Caves instead of C. M. Caves.

As I say I really haven't had a chance to understand your paper yet, but I will indeed study it. How could I but help to with so many flattering remarks in it about me!

You ask about simplifications to the standard Gleason theorem. There are two places to look (one you've already known):

- R. Cooke, M. Keane, and W. Moran, "An Elementary Proof of Gleason's Theorem," Math. Proc. Camb. Phil. Soc. **98**, 117–128 (1981)
- I. Pitowsky, "Infinite and Finite Gleason's Theorems and the Logic of Indeterminacy," J. Math. Phys. **39**, 218–228 (1998).

Unfortunately I don't have copies of either of these papers any more.

However, there is a far greater simplification than the above, which can be found if one is willing to embrace the notion that POVMs can be taken as a basic concept of measurement in quantum mechanics. I explain this in detail in my paper `http://xxx.lanl.gov/abs/quant-ph/0106166` and prove a version of Gleason's theorem in an absolutely trivial way. (Also you can pick it up off my webpage: the link for it is below.) In particular the proof by this method works also for 2-D vector spaces and even vector spaces over the field of rationals, in both cases of which the standard Gleason theorem fails.

Certainly keep up the good work. This quantum mechanics is a wonderful thing.

28-03-02 *Quantum-Information Information* (to R. Duvenhage)

I just want to write you a very short note to tell you how much I enjoyed your paper `quant-ph/0203070`, "The Nature of Information in Quantum Mechanics" and to express how much similarity I think I see between our points of view. In particular, I think you expressed some things so very clearly that I would love to co-opt your phrases!

Here's where I think we agree the most:

1. "A measurement is by definition the reception of information by the observer."
2. "This renders many problems surrounding the measuring process in quantum mechanics no more difficult than in classical physics." And consequently,

3. Your discussion of the Heisenberg cut.

You can find some reflection of these ideas in my own paper quant-ph/0106166, though not put quite so succinctly as in yours. In particular I'm thinking of my discussion on pages 27 and 28 of that paper, following the earlier discussion on page 11. Also, I agree with your point about B's information being "invalidated" in your discussion on page 13 of your paper. Similar ideas make an appearance on pages 39 through 41 of mine. Finally, I also enjoyed your discussion of the linearity of time evolutions. That was the sort of thing, I was trying to describe in my notes of 22, 26, and 27 September 1999 on pages 408, 284, and 285, respectively, of my samizdat quant-ph/0105039.

However, I think I've come a long way since that paper and those notes. In particular, I think I'm no longer really willing to say that "quantum collapse is a noncommutative [generalization of] conditional probability." I think there is a sense in which quantum collapse is *precisely* an application of Bayes' conditionalization rule, modulo only a final redefinition of what the posterior probability is relevant to. What I mean by this in more detail can be found in my letter to Lucien Hardy beginning on page 159 of my collection "Quantum States: What the Hell Are They?" (which can be found on my webpage, link below), especially toward the end of the letter. In fact, I'm presently striving to write that up in an updated version of quant-ph/0106166, and I hope to place it on the server soon.

Anyway, I really want to point out the similarities in our thoughts rather than the contrasts. I think you've done the physics community a good service with your paper. It's very well written, and a lovely piece actually.

I really have the greatest hope and enthusiasm that we, the quantum information community, are on the verge of something very big in our understanding of quantum mechanics. I'm glad to see some good young minds joining into the enthusiasm as well!

29-03-02 *Building with Bayes* (to B. C. van Fraassen)

Sorry to take so long to acknowledge your note: I've been running around Ireland with some bad phone connections and probably a few too many pints.

But I'm back now, and I did find your flyer waiting in my mailbox. I tacked it to the wall, but the subject's not likely to attract any of the physicists in my immediate vicinity. I would like to bring a visiting student with me though. Her name is Petra Scudo, and her present email address is scudo@techunix.technion.ac.il. She, as things turn out, did some undergraduate work in Pavia under a guy named Regazzini, who in turn was a student of Bruno de Finetti. While Petra is visiting (for a month and a week), I'm going to have her work on trying to pin down a kind of quantum de Finetti representation theorem for time-evolution maps. I.e., a theorem in answer to the question, "What is an 'unknown' quantum operation?" (In this context, the term "quantum operation" refers to the generalization of unitarity that is common in quantum channel theory — namely, the trace-preserving completely positive linear map. Sometimes people call it "open-system dynamics" but, from the Bayesian perspective, it is little more (nothing more?) than a noncommutative generalization of a conditional probability, connecting as it does the prior (quantum-state) assignment to the posterior (quantum-state) assignment.)

Technically, I've made a breakthrough of sorts recently. I now know how to think of quantum collapse as *precisely* an application of Bayes conditionalization (importantly, followed by a redefinition of which measuring instrument the posterior probability assignment is relevant to). I call this a breakthrough because until recently (for instance see page 25 of my NATO paper, quant-ph/0106166), I continued to think of collapse as a noncommutative *analogue* of conditionalization. But now, using a representation of the quantum state similar to the one Hardy harps on, I can see that what is going on is the true-blue thing (i.e., simply Bayes in disguise). At the moment I'm working hard to get this written up in a sensible way . . . or at least give my readers a hint of it, until I can do it properly.

By the way, in coming to all this, I've taken a more radical Bayesian turn than I had expected at the outset. I.e., though I started my career in an Ed Jaynes kind of "objective Bayesian" camp, I'm now finding myself in the left of the de Finetti camp and maybe a little beyond that. In case it interests you, I've documented a lot of this transition in a new samizdat which I've placed on my webpage. The title of the file is "Quantum States: What the Hell Are They?" and it contains a lot of new correspondence with Mermin and others along these lines. (As an aside, I've significantly revamped my webpage; it's not so minimalist anymore, and maybe thus a little more attractive.)

While I'm here and I've invested this much into a long letter to you, let me make the thing even longer by tacking on two pieces from the above-said collection. I'll put them immediately below—one is a letter to John Preskill and one a letter to Bill Wootters. Both letters should be self-contained. Anyway, I place them here because I had forwarded them to Henry Folse a while ago, and he wrote me back a rather excited letter saying that I'm starting to explore some philosophical ground not so dissimilar from where Bas van Fraassen has gone. I wish I were in a position to judge the validity of that! But I just haven't read enough yet. (I'm trying to, believe me. But, being a physicist, I've got a lot of material to catch up on.) Anyway, until then, I'll keep my fingers crossed that maybe I can get some reaction directly from the horse's mouth. Are there similarities between our views? And what other pieces of your work should I be reading if there are?

Finally, concerning your seminar with Elga, I'd love to attend! So, please do keep me on the list.

29-03-02 *Historical Rampage* (to A. Peres, N. D. Mermin, A. Cabello, H. J. Folse, and A. Plotnitsky)

I am finally sitting down to write an introduction to the Växjö proceedings, and I am wondering if I can bother any of you to give me some help in this regard. The help I would like is related to a paragraph I just wrote:

> Quantum theory in its full-fledged form has been with us for 75 years. Yet in a very real sense, the struggle for its formation remains. Indeed, not a year has gone by since 19?? when the world has not seen at least one meeting of physicists or philosophers devoted either explicitly or implicitly to the foundations of quantum theory. Our very meeting in Växjö, "Quantum Theory: Reconsideration of Foundations," is just one of a long lineage of meetings with this tormented soul.

What I would like to do is back up that claim with some substance, and also make the year "19??" look as dramatic as possible.

Here is the way you can help me if you have some time. Please just send me a list of the meetings you are aware of. What I would like is 1) the title of the meeting, 2) the location of the meeting, and 3) its date. Even if you only have partial information for any of these things, I would still like you to send me what you have: it may give me enough clues to piece together the rest. (Furthermore, if you know of any good resources that may help me in this quest, please let me know.)

For instance, just in looking in my own CV I have been able to dig up the list below.

For whatever help you can give, I will thank you lavishly in the article's conclusion!

- Workshop on Squeezed States and Uncertainty Relations, College Park, Maryland, March 1991
- Fundamental Problems in Quantum Theory: A Conference Held in Honor of Professor John A. Wheeler, Baltimore, Maryland, June 1994
- Sixth UK Conference on Conceptual and Mathematical Foundations of Modern Physics, Hull, England, October 1997
- Second Workshop on Fundamental Problems in Quantum Theory, Baltimore, Maryland, August 1999
- Chance in Physics: Foundations and Perspectives, Ischia, Italy, November 1999
- Quantum Foundations in the Light of Quantum Information and Cryptography, Montréal, Canada, May 2000
- NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer, Mykonos, Greece, June 2000
- Quantum Theory: Reconsideration of Foundations, Växjö, Sweden, June 2001
- 10th UK Conference on the Foundations of Modern Physics, Belfast, Ireland, September 2001
- American Mathematical Society Meeting, Special Session on Quantum Logic, Atlanta, Georgia, March 2002

31-03-02 *Progress Report* (to A. Peres, N. D. Mermin, A. Cabello, H. J. Folse, and A. Plotnitsky)

With the predominant help of Adán, here's what I can show so far. If any of you know of a way to fill in any of the gaps, that would be great. (Also if you know the data for the double question marks ??, that would be wonderful.) However, there's no need to waste your times sending me things from the years where I already have an entry. (Of course, I only really want the list for dramatic effect, not for complete completeness.)

Tomorrow, when I have the resources of a library, I'll try to fill in as much as I can. Of course it'd be great if I could get a continuous run ever since 1970 (or even earlier).

- 1970, International School of Physics Enrico Fermi. Course IL: Foundations of Quantum Mechanics, Varenna, Italy
- 1971
- 1972
- 1973
- 1974
- 1975
- 1976

- 1977, International School of Physics Enrico Fermi. Course LXXII: Problems in the Foundations of Physics, Varenna, Italy
- 1978
- 1979
- 1980
- 1981
- 1982
- 1983, Foundations of Quantum Mechanics in the Light of New Technology, Tokyo, Japan
- 1984
- 1985, Symposium on the Foundations of Modern Physics: 50 Years of the Einstein-Podolsky-Rosen Gedankenexperiment, Joensuu, Finland
- 1986, 2nd International Symposium on the Foundations of Quantum Mechanics in the Light of the New Technology, Tokyo, Japan
- 1987, Symposium on the Foundations of Modern Physics 1987: The Copenhagen Interpretation 60 Years after the Como Lecture, ??
- 1988, Bell's Theorem, Quantum Theory, and Conceptions of the Universe, George Mason University
- 1989, Sixty-two Years of Uncertainty: Historical, Philosophical and Physical Inquiries into the Foundations of Quantum Mechanics, Erice, Italy
- 1990, Symposium on the Foundations of Modern Physics 1990. Quantum Theory of Measurement and Related Philosophical Problems, Joensuu, Finland
- 1991, Bell's Theorem and the Foundations of Modern Physics, Cesena, Italy
- 1992, Symposia on the Foundations of Modern Physics 1992: The Copenhagen Interpretation and Wolfgang Pauli, ??
- 1993, International Symposium on Fundamental Problems in Quantum Physics, Oviedo, Spain
- 1994, Fundamental Problems in Quantum Theory: A Conference Held in Honor of Professor John A. Wheeler, Baltimore, Maryland
- 1995, Fundamentos de la Física Cuántica San Lorenzo de El Escorial, Spain
- 1996, 2nd International Symposium on Fundamental Problems in Quantum Physics, Oviedo, Spain
- 1997, Sixth UK Conference on Conceptual and Mathematical Foundations of Modern Physics, Hull, England
- 1998

- 1999, Second Workshop on Fundamental Problems in Quantum Theory, Baltimore, Maryland, August 1999
- 2000, NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer, Mykonos, Greece
- 2001, Tenth UK Conference on the Foundations of Modern Physics, Belfast, Ireland

01-04-02 *Last Little Push* (to A. Peres, N. D. Mermin, A. Cabello, H. J. Folse, and A. Plotnitsky)

Thanks for all the help you've given me so far. But now here's my goal: to exhibit 30 full years of conferences. I'm only missing entries for 1976, 1978, 1981, and 1982!! So, if any of you have any faint memories about those years, I'd really appreciate any lead you can give me!

- 1972, Symposium on the Development of the Physicist's Conception of Nature in the Twentieth Century, Trieste, Italy
- 1973, Foundations of Quantum Mechanics and Ordered Linear Spaces, Marbourg, Germany [See 15-05-02 note "Marburg, Strasbourg, Blunderburg!" to A. Peres.]
- 1974, Quantum Mechanics, a Half Century Later, Strasbourg, Germany [See 15-05-02 note "Marburg, Strasbourg, Blunderburg!" to A. Peres.]
- 1975, Foundational Problems in the Special Sciences, London, Canada
- 1976
- 1977, International School of Physics "Enrico Fermi", Course LXXII: Problems in the Foundations of Physics, Varenna, Italy
- 1978
- 1979, Symposium on Quantum Theory and Gravitation, New Orleans, Louisiana
- 1980, Quantum Theory and the Structures of Time and Space, Tutzing, Germany
- 1981
- 1982
- 1983, Foundations of Quantum Mechanics in the Light of New Technology, Tokyo, Japan
- 1984, Fundamental Questions in Quantum Mechanics, Albany, New York
- 1985, Symposium on the Foundations of Modern Physics: 50 Years of the Einstein-Podolsky-Rosen Gedankenexperiment, Joensuu, Finland
- 1986, New Techniques and Ideas in Quantum Measurement Theory, New York, New York
- 1987, Symposium on the Foundations of Modern Physics 1987: The Copenhagen Interpretation 60 Years after the Como Lecture, Joensuu, Finland
- 1988, Bell's Theorem, Quantum Theory, and Conceptions of the Universe, Washington, DC

- 1989, Sixty-two Years of Uncertainty: Historical, Philosophical and Physical Inquiries into the Foundations of Quantum Mechanics, Erice, Italy
- 1990, Symposium on the Foundations of Modern Physics 1990. Quantum Theory of Measurement and Related Philosophical Problems, Joensuu, Finland
- 1991, Bell's Theorem and the Foundations of Modern Physics, Cesena, Italy
- 1992, Symposia on the Foundations of Modern Physics 1992: The Copenhagen Interpretation and Wolfgang Pauli, Helsinki, Finland
- 1993, International Symposium on Fundamental Problems in Quantum Physics, Oviedo, Spain
- 1994, Fundamental Problems in Quantum Theory: A Conference Held in Honor of Professor John A. Wheeler, Baltimore, Maryland
- 1995, The Dilemma of Einstein, Podolsky and Rosen, 60 Years Later, Haifa, Israel
- 1996, 2nd International Symposium on Fundamental Problems in Quantum Physics, Oviedo, Spain
- 1997, Sixth UK Conference on Conceptual and Mathematical Foundations of Modern Physics, Hull, England
- 1998, Mysteries, Puzzles, and Paradoxes in Quantum Mechanics, Lago di Garda, Italy
- 1999, Second Workshop on Fundamental Problems in Quantum Theory, Baltimore, Maryland, August 1999
- 2000, NATO Advanced Research Workshop on Decoherence and its Implications in Quantum Computation and Information Transfer, Mykonos, Greece
- 2001, Tenth UK Conference on the Foundations of Modern Physics, Belfast, Ireland

01-04-02 *Broadcasting and Petra* (to A. Peres)

Asherism 18: *Only many years later there was the no broadcasting theorem, then some better understanding of distinguishability, the proof that completely positive maps did not improve distinguishability, etc. Could* you *please give me a brief survey with references?*

That CPMs never create distinguishability in the sense of "relative entropy" between two states—this is perhaps the very strongest statement of the notion of "distinguishability nonincrease" —was first proved by Armin Uhlmann. Unfortunately, I cannot find that reference.[30] However, I do know the paper is cited in Ohya and Petz's book *Quantum Entropy and Its Use*. The property to be looking for is called the monotonicity of relative entropy.

In terms of operationally measurable quantities (the relative entropy is not really one such thing), like the fidelity or the trace-norm distance, I think we, in our no-broadcasting paper were the first to prove the property in the former case. Here's is the reference:

[30]But 11 years later Blake Stacey could: It is A. Uhlmann, "Relative Entropy and the Wigner-Yanase-Dyson-Lieb Concavity in an Interpolation Theory," Commun. Math. Phys. **54**, 21–32 (1977).

- H. Barnum, C. M. Caves, C. A. Fuchs, R. Jozsa, and B. Schumacher, "Noncommuting Mixed States Cannot Be Broadcast," *Physical Review Letters* **76**(15), 2818–2821 (1996).

With respect to the trace-norm distance, I don't know who was the first. I've certainly done it independently of anyone else, but on the other hand, I've never published. (And, actually, I don't know if anyone has ever published it.)

Better work on the no-broadcasting theorem, i.e., an algebraic proof, can be found in the work of Göran Lindblad:

- "A General No-Cloning Theorem," Lett. Math. Phys. **47**, 189–196 (1999).

I am very proud of that reference because, in it, he called our proof of the no-broadcasting result "ingenuous" (though from the context he meant "ingenious")! He set as his task to simplify the proof, though he ended up with a stronger theorem.

The very best work yet and the strongest (possible) result along these lines is due to Koashi and his boss Imoto, `quant-ph/010114`, "What is Possible Without Disturbing Partially Known Quantum States?".

I hope that helps you. Incidentally, I knew that Ghirardi had rejected the Herbert paper. He told me the story in Ischia in November of 1999. Later, upon my request, he sent me all the documentation. Unfortunately, I lost those papers in the Cerro Grande fire.

Asherism 19: *Please take good care of her. Her health is not very strong.*

I will do my best to take care of her. (And extract a good, lasting result in physics from her too!)

Asher's Preply

> I found your "rampage" letter, and I'll look at home for all the conference proceedings I have — quite many! Now I'll need similar help from you. When I was at the Trieste meeting, someone who was speaking about the no cloning theorem said that the Herbert paper "FLASH…" was wrong. He repeated it so many times that this antagonized me. When time came for questions, I said that I was a referee of that paper and I had recommended its publication. Obviously it was wrong, because it violated special relativity, but Herbert knew that very well. I wanted someone to find what was actually wrong in the amplification mechanism that he proposed, and I thought that a serious discussion would lead to progress. Indeed it led!
>
> Then I had a big surprise: Ghirardi came to the podium, with a transparency that he had ready. He said that he was a referee of that paper and had rejected it. His reason: the no cloning theorem that he had proved first. The transparency was a letter from the editor of *Found. Phys.*, certifying "to whom it may concern" that Ghirardi had proved the theorem in his confidential referee's report. He never bothered to publish it, until it was too late! I want to write all that in my contribution to the proceedings.
>
> However I also said that Ghirardi should not have rejected the paper, because Herbert had no exact cloning. All he had in his laser was a mess, perhaps described by a density matrix, and the no cloning theorem did not apply. Only many years later there was the no broadcasting theorem, then some better understanding of distinguishability, the proof that completely positive maps did not improve distinguishability, etc. Could *you* please give me a brief survey with references?

I was so excited after this exchange with Ghirardi that I had to take a tranquilizer, something I do very rarely. Also, a young man came to me saying: thank you for accepting that paper. The no-cloning theorem is my main source of income.

The San Feliu conference was also very good. Petra was there, and soon she will be going to you. Please take good care of her. Her health is not very strong.

02-04-02 *Desist* (to A. Peres, N. D. Mermin, A. Cabello, H. J. Folse, and A. Plotnitsky)

Just in case any of you are still looking, I've now filled all the slots between 1972 and present. So, you can wipe your brow and call it a good day's work. Thanks to all of you for all the help.

02-04-02 *A Little Urgent* (to J. Bub)

I'm trying to put together a dramatis personae for my latest lambasting of the existence of quantum foundations conferences—i.e., I'm trying to make a case that we should work toward the conference to end all conferences—and I would like to exhibit 30 full years of conferences in it. And I'm only missing one!! It's for the year 1978. I wonder if you can help.

I discovered that you have a 1979 paper titled "The Measurement Problem of Quantum Mechanics" which appears in a book *Problems in the Philosophy of Physics* (72d Corso), Bologna: ... Could this paper, by chance, have been due to a Conference Proceedings in 1978?? If so, can you give me the following things: 1) the title of the conference, and 2) where it was held.

Thanks, and if you can help me, I'd be ever so grateful.

06-04-02 *The Early Morning* (to A. Plotnitsky)

I laid in bed this morning, reading and thinking about your review of Feyerabend's book. What a pleasant way to wake up. I think you are right: the most important issue is, what is the right balance of construction? It plays a role—I am convinced—but how big of a role?

You flatter me by sending me your newest book! How can you be so productive, while all the rest of us just twiddle by? I will cherish the book and have it read before you know it.

07-04-02 *Latour* (to A. Plotnitsky)

Plotnitskyism 2: *You got my point on Feyerabend exactly right. Besides, it is, I think, far more interesting, at least at this point, to think through that which is unconstructible, or, if one prefers, could be "constructed" as irreducibly unconstructible. (I am writing, among other things, a review of Bruno Latour's book, where this is a central issue as well.)*

Please send me that review when you've gotten it written!

07-04-02 *My (Hard-Earned) Pomposity* (to G. L. Comer)

Comerism 1: *Really? There's nothing to be learned from it* [Penrose's book]*? I mean I read Hawking's thing, didn't agree with everything, but did learn a few things.*

Sure, there's plenty to be learned by way of mathematical ideas and mathematical definitions and results. It's his implicit philosophy that I shun now, a philosophy that he's not even completely aware of (I think). And it's that philosophy (and the very troubles it causes) that makes up the two books' whole reason to be. [...]

Let me come back to this Penrose thing again. It has to do with the words "rational," "logical," and "sane." Penrose is a believer in a kind of other-worldly realm beyond the physical world called the realm of mathematical truth. In that realm logic and rationality are defined in a timeless fashion, in a way independent of human frailty.

I've now come to think that that is hogwash. In positing such a thing, one misses the whole point of this wonderful world — that it is still under construction, and can be made *to some extent* in the image we want to make it. By this account, what is rational and logical is not in what is timeless and "right," but in what gives us the most *long-term* survival value. Survival value so that we can give rise to the most progeny (genetically and intellectually); survival value so that we can stand a chance to shape things. And that, as Darwin taught us, is not something set intrinsically, but is a function of our cumulative environmental pressures.

09-04-02 *Thanks* (to H. J. Folse)

> **IMPORTANT**: See the correction to the present note in my note "Doing It and Doing It Right," dated 15 April 2002, to Henry Folse.

Folsesm 7:

> That is, the quantum state is a compendium of Bayesian "beliefs" or "gambling commitments" and is thus susceptible to the type of analysis James gives in his "Sentiment of Rationality." Our particular choice of a quantum state is something extra that we carry into the world.

Whoa, this paragraph eludes my comprehension. I thought the measurement chose the state of the outcome.

That's a long story. Yes, measurements can determine states, but my latest greatest realization is that they determine states in a way not so different from the way probability distributions $p(x)$ and $p(y|x)$ determine $p(y)$. $p(x)$ is the classical analog of the initial quantum state. $p(y|x)$ is the classical analog of the projection operator in the collapse rule. $p(y)$ is the classical analog of the final state. To the extent that in the Bayesian view *all* probabilities—even conditional ones like $p(y|x)$—are completely and utterly subjective, so are the meanings of the outcomes of one's quantum measurement (i.e., so are the basis vectors to which the collapses occur).

10-04-02 *Evidence 2* (to G. Brassard)

I did say two pieces of evidence: I almost forgot to go on to the second one. The one below is an excerpt from a long(er) letter by Jeff Bub. [See 10-04-02 note "Bitbol-ization" to J. Bub.] I'll try to read up on Bitbol soon. It sounds like he indeed embraces a position similar to ours. (I.e., that QM may come out of the idea that the observer cannot be detached ... i.e., (hopefully) that QKD exists.) So, the "waiting list" for our meeting grows.

But really, we need to give our more important attendants (i.e., the ones on the poster) some more concrete plans.

10-04-02 *NSERC Funding* (to R. W. Spekkens)

Spekkensism 8: *Incidentally, I was reading an old paper the other day which I thought might resonate with you. It is by Arthur Fine, "Do correlations need to be explained?", in* Philosophical Consequences of Quantum Theory*, edited by Jim Cushing.*

I am aware of that paper, and indeed was mightily impressed with it for a while. Have a look at my Samizdat, pages 373–379. Ultimately, however, I shook myself of the hidden hand. The only reason I'm a (subjectivistic) Bayesian is because of all the hard work I put into trying to be a propensitist.

10-04-02 *Bitbol-ization* (to J. Bub)

Sorry myself for taking so long to get back to you. Bitbol sounds interesting. I've put in a good word for him to Gilles; we'll see what happens. I absolutely love that phrase "blinding closeness."

Bubism 2: *Back to Bitbol and your earlier letter to me with replies to Preskill and Wootters. The essential point there seems to me very close to the neo-Kantian view about quantum mechanics that Bitbol has been developing in several books and articles.*

At the risk of getting into deep waters with a philosopher, I would say the point I'm pushing has a much deeper affinity to the philosophical tradition of pragmatism (James and Dewey's versions in particular) than anything of a Kantian flavor. But I've learned my lesson about saying the words "Copenhagen interpretation" to physicists. (See the story on the bottom of page 70 in the big Samizdat.) And similarly I'm learning a lesson about saying the words "James," "Dewey," and "pragmatism" in front of philosophers. The reactions I've gotten from Timpson, Brown, Donald, and Butterfield! Even when, in other contexts, I got such pleasant reactions from them about the ideas I was talking about ... namely pragmatism without the explicit label. Sometimes a few words are worth a thousand (mental) fences.

Jeff's Preply

Apologies for being out of touch. I am now in my house in Quinson in Provence for another couple of weeks, after spending an interesting week in Paris, where I met a philosopher of quantum mechanics by the name of Michel Bitbol whom I think you should get to know (more of Bitbol later).

To answer your question about my paper ...

Back to Bitbol and your earlier letter to me with replies to Preskill and Wootters. The essential point there seems to me very close to the neo-Kantian view about quantum mechanics that Bitbol has been developing in several books and articles:

> I think the solution is in nothing other than holding firmly—absolutely firmly—to the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettistas holding to it—we have to recognize that what we're doing in the game of science is swimming in the thick middle of things. We're swimming in this undulant sea, and doing our best to keep our heads above the water: All the concepts that arise in a physical theory must be interpreted to do with points of view we can construct from *within* the world.

> That is to say, we have to loosen the idea that a physical law is a mirror image of what "is" in the world, and replace it with something that expresses instead how each of us can best cope with and hope to take advantage of the world exterior to ourselves.
>
> ...
>
> The situation of quantum mechanics—I become ever more convinced— illustrates this immersion of the scientific agent in the world more clearly than any physical theory contemplated to date.

What seems to me closely related to Bitbol's position is your emphasis on replacing the idea of a physical theory (you say 'law') as a mirror image of what is in the world (in what Pauli called a 'detached observer' sense), with an opposing view that takes seriously the fact that our science can only reflect points of view we can construct from *within* the world. I agree with you that this is the right way to look at quantum mechanics (although in my book I argued for the 'detached observer' view, which I identified with an Einsteinian view). Now I would say that quantum mechanics is not so much a descriptive theory of new sorts of non-classical objects (particles that are also wave-like, or particles with properties that hang together in a non-Boolean way, for example), as a theory of mechanics constrained by certain explicit limits to the process of objectification. (So, as Bohr said, there are no quantum objects. Quantum mechanics is not about how nature is, but about what we can say about nature. In this sense, it's a mechanical theory at the information-theoretic level, unlike classical mechanics – and the claim is that we are stuck at this level just because we are not 'detached observers.') Bitbol talks about the 'blinding closeness' of the world in the title to one of his books – a much more apt image than d'Espagnat's image of a 'veiled reality.'

Here's a suggestion: Take a look at Bitbol's website (you'll find him just by looking up Michel Bitbol on Google). If you like what you see (most of his publications are in French, but there are some things in English you can download from his website), why not invite him to the Montréal quantum commune meeting in the Fall? Note that he's more of a philosopher trying to make sense of quantum mechanics than someone who works with the formalism, but he does seem to know what he's doing.

11-04-02 *Zing D* (to I. H. Deutsch & C. M. Caves)

I am finally writing up my contribution to the Växjö conference proceedings, and in it I plan to make a statement about which elements of quantum theory I would be willing to call "ontological" if push came to shove. As Carl knows well, my favorite is the D. That is, for each system, when I ascribe to it a Hilbert space of some dimension D, what I am really doing is ascribing it an integer parameter of some (potentially) ontological significance. I do not let myself, however, assume such a significance for the states *in* the Hilbert space or the operators *on* it.

When I have that discussion, I would like to cite the stuff I saw Ivan present at the ITP meeting. (I saw it via the web.) Do you have paper written on that? If so, what are its coordinates? What I'll say is something like: If you're looking for the magic ingredient that powers quantum computing, it's not going to be first and foremost in the subjective elements of the theory. It's going to be in those things that stand a chance of being objective.

By the way, Ivan. I remember seeing one slide where you talked about the various points of view for just the question above—i.e., what might give quantum computing its power. In it you had one bullet devoted to "information-disturbance tradeoff." That was flattering, but surely it must be such a minority opinion as to not be an opinion at all!! (However, you ought to know what I'm aiming for: that that and D are the same thing, after all. So thanks in advance!)

13-04-02 *Quantum Locality* (to R. Garisto)

Thanks for sending me your draft; I will try to have a deeper look at it after my present project lets me come up for a bit of air. But, yeah, you're right, I can already tell I'm going to disagree with it. "What is the speed of quantum information?" It doesn't have a speed. It could only have a speed if you endow the state vector with an objectivity it does not have. QUANTUM STATES DO NOT EXIST (in and of themselves). They merely express the gambling commitments one is willing to make when one encounters a quantum system. But that does not leave the world empty; it just means that the quantum state is not part of its substance. Why is it that I should choose my gambling commitments to be in accordance with the structure of quantum mechanics? When we can answer that, we will have learned something clean and simple about the substance of the world. But until then, encumbering the world with an idea—nonlocality—that is clearly wrong-headed (without a heck of a lot of contrivance to try to make it go) will only distract us from the straight course to that great goal. What is it that makes quantum mechanics go? It is something deeper and far more interesting than the quantum state—that much I firmly believe.

I'm glad to hear that things are going well with you cancer-wise. Every time I think of you, I cross my fingers mentally for your health and happiness.

13-04-02 *An Address* (to L. Hardy)

Can you send me the name and email address of the guy who invited you to the Wheeler conference? I want to get in touch with him for a little help with regards that big reference document I'm putting together ("The Activating Observer").

By the way, I think I've got a better Bayesian-styled argument for linearity than the one I sent you in the big note. [See note titled, "Poetry on Concrete."] It allows me to conglomerate the three protagonists there into one. I'll ultimately write that up and send it off to you, but probably not for a while. By the way, I've got a summer student coming in from Cornell this summer, and I've decided on a technical project for him that has to do with connecting our (i.e., yours and mine) views of your paper.

15-04-02 *Doing It and Doing It Right* (to H. J. Folse)

I was just looking my last note to you over again, and I was appalled at what I had written. Namely, I really botched my description of the classical analog to collapse. How I could do that, I don't know. And I am ashamed of myself.

Here's the proper way to say it, and in fact the way I am just writing it up for a paper:

> In Section 8, "**What Else is Information?**," I argue that, to the extent that a quantum state is a subjective quantity, so must be the assignment of a state-change rule $\rho \to \rho_d$ for describing what happens to an initial quantum state upon the completion of a measurement—generally some POVM—whose outcome is d. In fact, the levels of subjectivity for the state and the state-change rule must be precisely the same for consistency's sake. To draw an analogy to Bayesian probability theory, the initial state ρ plays the role of an a priori probability distribution $p(h)$ for some hypothesis, the final state ρ_d plays the role of a posterior probability distribution $p(h|d)$, and the state-change rule $\rho \to \rho_d$ plays the role of the "statistical model" $p(d|h)$ enacting the transition $p(h) \to p(h|d)$. To the extent that *all* Bayesian probabilities are subjective—even the

probabilities $p(d|h)$ of a statistical model—so is the mapping $\rho \rightarrow \rho_d$. Specializing to the case that no information is gathered, one finds that the trace-preserving completely positive maps that describe quantum time-evolution are themselves nothing more than subjective judgments.

16-04-02 *Oh Grammarian* (to C. M. Caves)

I don't think I ever got the rules for "farther" and "further" straight. Which would you have put in the paragraph below (and why)?

> The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information. Put in the right mindset, this is *not* so intolerable. It is a statement about our world. There is something about the world that keeps us from ever getting more information than can be captured through the formal structure of quantum mechanics. Einstein had wanted us to look further—to find out how the incomplete information could be completed—but perhaps the real question is, "Why can it *not* be completed?"

Thanks for (far?, fur?) your advice.

Carl's Reply

> Get yourself a copy of Strunk and White, and read it cover to cover. Here they are on your question (p. 46–47):
>
>> Farther, further. The two words are commonly interchanged, but there is a distinction worth observing: *farther* serves best as a distance word, *further* as a time or quantity word. You chase a ball *farther* than the other fellow; you pursue a subject *further.*
>
> I'd say you want *further*, unless you are thinking of Einstein as literally looking into the distance.

16-04-02 *My Own Homeopathy* (to C. H. Bennett, T. A. Brun, C. M. Caves, P. Grangier & N. D. Mermin)

I thought of all of you with a smile as I was writing the footnote below for an upcoming paper. I'll send it to you now. Maybe I really do practice homeopathy. (Wait till you see the undiluted version.)

> In the previous version of this paper, `quant-ph/0106166`, I variously called quantum states "information" and "states of knowledge" and did not emphasize so much the "radical" Bayesian idea that the probability one *ascribes* to a phenomenon amounts to *nothing* more than the gambling commitments one is willing to make with regards to that phenomenon. To the "radical" Bayesian, probabilities are subjective all the way to the bone. In this paper, I start the long process of trying to turn my earlier de-emphasis around (even though it is somewhat dangerous to attempt this in a manuscript

that is little more than a modification of an already completed paper). In particular, because of the objective overtones of the word "knowledge"—i.e., that a particular piece of knowledge is either "right" or "wrong"—I try to steer clear from the term as much as possible in the present version. The conception working in the background of this paper is that there is simply no such thing as a "right and true" quantum state. In all cases, a quantum state is specifically and only a mathematical symbol for capturing a set of beliefs or gambling commitments. Thus I variously call quantum states "beliefs," "states of belief," "information" (though, by this I mean "information" in a more subjective sense than is becoming common), "judgments," "opinions," and "gambling commitments." Believe me, I already understand full well the number of jaws that are going to drop by the adoption of this terminology. However, if the reader finds that this gives him a sense of butterflies in the stomach—or fears that I am or will become a solipsist[1] or a crystal-toting New Age practitioner of homeopathic medicine[2]—I hope he will keep in mind that this attempt to be absolutely frank about the subjectivity of **some** of the terms in quantum theory is part of a larger program to delimit the terms that actually can be interpreted as objective in a fruitful way.

1. P. Grangier, private communication, 2001.

2. C. H. Bennett, T. A. Brun, C. M. Caves, and N. D. Mermin, various vibes, 2001.

16-04-02 *Rushdie Quote* (to D. R. Terno)

> Scientists get angry when laymen misunderstand, for example, the uncertainty principle. In an age of great uncertainties it is easy to mistake science for banality, to believe that Heisenberg is merely saying, gee, guys, we just can't be sure of anything, it's so darn *uncertain*, but isn't that, like, *beautiful*? Whereas he's actually telling us the exact opposite: that if you know what you're doing you can pin down the exact quantum of uncertainty in any experiment, any process. To knowledge and mystery we can now ascribe percentage points. A principle of uncertainty is also a measure of certainty. It's not a lament about shifting sands but a gauge of the solidity of the ground.
>
> — Salman Rushdie, *The Ground Beneath Her Feet*

Thanks for the Rushdie quote. It's quite nice, especially the last line: "It's not a lament about shifting sands but a gauge of the solidity of the ground."

I think it's something a little in the spirit of what I wrote in my NATO paper:

> The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information. Put in the right mindset, this is *not* so intolerable. It is a statement about our world. There is something about the world that keeps us from ever getting more information than can be captured through the formal structure of quantum mechanics. Einstein had wanted us to look further—to find out how the incomplete information could be completed—but perhaps the real question is, "Why can it *not* be completed?"

18-04-02 *Urgent Reference* (to H. Barnum)

Can you send me some full references on where the idea of "noncontextuality" of the probability rule was introduced? I think you told me it was Mackey. Also, though, can you give me the reference, to the criticism of the idea: I think you said something like Hilgevoord?

Howard's Reply

The easiest thing is probably to attach my draft for the Cesena proceedings, which you may already have. Refs. 6 and 7 are probably what you want.

[6] G. W. Mackey, *The mathematical foundations of quantum mechanics*, W. A. Benjamin, New York, 1963.

[7] R. M. Cooke and J. Hilgevoord, "A new approach to equivalence in quantum logic," in Current issues in quantum logic, E. Beltrametti and B. van Fraassen, Eds., New York and London, 1981, Plenum.

I'll see if I can get the page nos. from my copy of Cooke & Hilgevoord (but they may be unnecessary since its an edited vol, not a journal). They are 101–114. Mackey's statement is part of his Axiom II on page 63. There is also an article which I haven't read, "Quantum mechanics and Hilbert space", *American Mathematical Monthly* **64**, pp 42–47 (1957), which was apparently "expanded into a book" (the one I cited), so this is probably the original source for Mackey. Note that Cooke and Hilgevoord, while citing Mackey, attribute the idea of probabilistic equivalence to Bohr, but I think they aren't specific about where (I can't find the article right now).

As you probably know, I think there was also a lot of discussion of noncontextuality in the papers around von Neumann's hidden variables result, notably Bell's paper on it, but I can't remember the details.

18-04-02 *Reference* (to F. E. Schroeck)

Can you give me the full reference for the paper in which you introduced the idea of informationally complete POVMs? (By full reference I mean title, journal, volume, beginning page number, ending page number, and year.)

Frank's First Reply

The earliest reference I can find to informational completeness is in a 1977 paper by Prugovečki. It is listed in my book *Q.M. on Phase Space*. The earliest reference in my own work is in a 1989 paper entitled "An Overview of Q. M. on Phase Space". I'll have to look up a reference to see where it was published! In this paper, I refer to it being present in two papers of 1989 — Busch and S., Found. of Phys. **19** (1989), 807–872; and Brooke and S., Nuclear Phys. B (Proc. Suppl.) **6** (1989), 104–106. I'll have to check these two references at home to see if "Info. Compl." is even defined in them. I also refer to it being in a paper that was not published, it being part of a package prepared for a grant; it was surpassed by another paper. Anyway, I'll send you a copy of that paper (and all other papers that I will mention here) in two days. The paper that surpassed this is "On Informational Completeness of Covariant Localization

Observables and Wigner Coefficients," D. M. Healy, Jr. and S., J. Math. Phys. **36** (1995), 453–507.

The first(?) unambiguous reference of mine to it appeared in "Unsharpness in Measurement Yields Informational Completeness," in *Symposium on the Foundations of Modern Physics 1990*, P. Lahti and P. Mittelstaedt, eds., World Scientific, 1991. In this paper I talk about its ramifications in quantum signal processing and natural biological systems.

There were a whole series of papers that I wrote, with an occasional collaborator, on representations of groups, pattern recognition, frames, ...

And then there is my book.

I'm going to send all this because the applications of info. compl. are everywhere, and the references are sometimes hard to come by. You have a day or two of reading ahead of you!

I'll get back to you on the three early references.

Frank's Second Reply

I found one that predates the one from 1990. It is in "Coexistence of Observables," F. E. S., Int. J. of Theor. Phys. **28** (1989), 247–262. I'll send that one also. I presume you don't any longer need the page no's from the paper mentioned below.

I also found info. compl. mentioned in a later paper from 1989. ("On the Reality of Spin and Helicity") It is enclosed as well.

So, there you have it!

19-04-02 *Lovely Circuits* (to N. D. Mermin)

Is there a typo in your paper, third paragraph? Two consecutive "before"s. If it's a clever construction, I didn't get it.

What do you use to draw your lovely circuits? I'm contemplating putting a figure of a circuit in my present paper, and I've never taken that bold step before.

"What the story demonstrates is the ability of entangled states to store interaction in a highly fungible form that need not be cashed in until the need arises." In comparing entanglement to classical correlation and making a point similar to yours, I once called entanglement "all-purpose correlation." This was great fun because it set me up to mention "Martha White's All-Purpose Flour," a product I remembered from my youth. What tickled me the most was that I got to cite Lester Flatt and Earl Scruggs, who sang the "Martha White Theme" during commercial breaks at the Grand Ole Opry. You might know them for the Beverly Hillbillies theme. It's the little things that keep me going, you know.

I'll tell you how if the paper stirs me.

20-04-02 *Yes* (to T. Rudolph)

It just turned 5:30 AM, and clearly I ain't got nothin' better to do.

This note is just to acknowledge that your question is a good one. Can one give a primary quantum state assignment $\rho^{(n)}$ that captures the idea of n draws from a fixed set $\{\rho_i\}$ of quantum states (distributed according to some probabilities p_i). Let me call such a source, a Bernoulli source.

That is, the question is how to characterize the class of density operators that look like this

$$\rho^{(n)} = \sum (p_{i_1} p_{i_2} \cdots p_{i_n}) \rho_{i_1} \rho_{i_2} \cdots \rho_{i_n}$$

where the summation is over all strings $i_1 i_2 \cdots i_n$. It would indeed be nice to have a characterization of the flavor of a de Finetti representation.

That is, in a Bayesian account, how can one take into acknowledge the receiver's state of knowledge is more than simply $\rho^{\otimes n}$, without inserting a man in the box? Or is the only way to do it to explicitly insert a man in the box? I'm not clearheaded enough right now. But it might indeed be the latter. See, for instance, in the case of quantum cryptography (B92, say), with respect to Eve there is always a "man in the box"—it's Alice, the source of the states. Similarly for all the other cases I've ever thought about: They're always communication games, where there is explicitly a sender and receiver. A sender sending messages (or a key), and a receiver (or eavesdropper) trying to decode them.

Concerning your A matrix account yesterday, Howard Barnum has thought a little bit about that. In particular he used the A to define some notion of "coherent superposition" of density operators. I'm not quite sure what he did with that, but it's written up as one of the chapters of his PhD thesis.

I'll cc this note to our friend Professor Schack, as he might have some interest in it too.

20-04-02 *Wacky Paper à Moi* (to G. Brassard)

I'm writing another wacky foundations paper, and I want to give one of the section headings a title like, "Le Bureau International des Poids et Mesures à Paris" (to express the phrase "International Bureau of Weights and Measures in Paris"). My question to you is, should the opening "Le" be there in a title? Also, am I safe assuming that the "à" translates to my "in"?

My French is atrocious. (In case you didn't know.)

20-04-02 *Helping a Friend* (to A. Cabello)

Yesterday, it was brought to my attention that you had increased the size of your massive bibliography since its first posting. I hadn't noticed. What an amazing document!

But really, I write you to ask if you have a BibTEX version of that database? One of our visiting grad students here, Frank Verstraete, had a hard disk failure on his laptop, and he very literally lost three years of work! (Of course, he is an idiot for not backing it up—he had never done it, not even once. But then again, I am an idiot for not grabbing my two-thousand pages of calculations as my wife and I fled Los Alamos.) I'm just trying to think of ways to help him. One thing he mentioned was that he had an extensive BibTEX file built up on quantum information things.

I noticed that your file is not written in BibTEX format. But is there a version of it in which it is? If so, it'd be nice if you could help him to this extent.

I am putting together a bibliographic project of my own—very different subject matter than yours. But I'll certainly cite your lovely work in it, for the purpose of cross-connections. I'm calling mine "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature." It's sitting at four hundred and something references right now, many of which I've got annotated extensively. You can read the abstract of it at my new webpage (link below), but I don't have it posted yet. I was hoping to get it complete for the second anniversary of the Cerro Grande Fire, but I'm almost surely going to miss that deadline. Maybe I'll be able to do it for the 1.5 year mark.

21-04-02 *No BibTEX Version, Sorry* (to A. Cabello)

Oh well; we tried.

Cabello-ism 2: *By the way, I've recently realized that there was an international symposium on "Quantum Theory and Reality" in Oberwolfach, in July 1966. The proceedings, edited by M. Bunge, were published by Springer-Verlag, NY, in 1967 (before I was born!).*

I actually knew about that one. We have its proceedings in our library here at Bell Labs. I didn't work too hard on the things before 1972 because I decided that 30 years might be a good cut-off mark.

It would kind of be interesting to know when the earliest conference ever on quantum foundations was. I know that there was at least one sometime in the 1950s. Let me dig up the reference. Here it is: *Observation and Interpretation: A Symposium of Philosophers and Physicists*, edited by S. Körner (Academic Press, New York, 1957).

The earliest ever can probably be found in one of Jammer's books.

21-04-02 *Walton's Mountain* (to G. L. Comer)

Today is the gloomiest, most drab day as far as the weather is concerned. But I like that. I always feel academic and reflective on such days. Kiki tells Emma, "If nature doesn't supply Daddy with a few clouds, he makes his own."

Lucent has been doing awfully poorly again in the market. [...] And the rumors are flying that about 10,000 more are going to get the pink slip Monday (after this quarter's losses are announced). [But] I'm not feeling too panicked at the present though. [...]

[F]or the present, I'm just happy being a high-payed philosopher, and I'm not worrying too much.

And the philosophy I've been doing! I can't put the pragmatists down; I read them in the morning, I read them in the night. I am so taken by the thoughts of James *et al.* I don't know how I could have missed these guys for years! It's a crying shame and certainly has stunted my growth. The pragmatists are all about the things John Wheeler was about (in his heyday), but oh so much deeper. John was an absolute amateur in comparison.

23-04-02 *Mermin's Mysterious Ways* (to A. Peres)

Asherism 20: *What happened to David Mermin:* `0204107` *"deconstructing dense coding", which follows deconstructing teleportation in* PRA **65** (2002) 012320*? Can you understand his motivation?*

Did you read his acknowledgements in the first paper? He writes, "I thank ... Chris Fuchs for asking why I found it interesting." I don't know that he ever gave me an answer that I understood. (He attempted to do it in the paper's present introduction.) I think part of it is surely in the fact that he is presently teaching a quantum information course, and he is trying to be very pedagogical for the students. But then, why he is not publishing in AJP, I don't know.

Petra went off to Amherst and then to Williamstown to visit Bill Wootters this weekend. She returns by bus (to NY) and train (to Murray Hill) today. She called me last night to tell me that everything is OK. She is doing quite well in her research, and I am very pleased with her.

Tomorrow I post a rather philosophical paper on `quant-ph`. [See "The Anti-Växjö Interpretation of Quantum Mechanics," `quant-ph/0204146`.] I think it will be my first document there with zero equations. (But Seth Lloyd does almost the same in many of his *Nature* papers!) The reactions it's gotten in pre-circulation have been quite interesting. For instance, Bill Wootters seemed to love it, but John Preskill seemed to hate it. Julio Gea-Banacloche seemed to love it, but Charlie Bennett seemed to hate it. And so on.

23-04-02 *Music in the Musician* (to G. L. Comer)

Thanks for the thoughtful note. I found myself thinking about it on and off all last night, in both my periods of wake and sleep. I think you expressed the issues to do with chemistry versus consciousness especially vividly.

I think we just have to get rid of this imagery that we are "made" of atoms. Or none of us are ever going to make any progress in our emotional lives OR our physical understanding. By my present thinking, a much better imagery is this. Take me and an old log: we both float in water. That is to say, we have that much in common. But there are a heck of a lot more things that we do not have in common. For any two entities, we can always find some characteristics they have in common, if *we* are willing to ignore all the ways in which they are distinct. And that, I think, is the story of atoms. The atomic picture has something to do with what we all have in common. (Or, maybe more potently, it has something to do with what is common in the *part* of existence that we have chosen not to ignore for the moment.) But to see the atomic picture shine through, we have to dim down all the things that are unique in us. Who said the particular shape of that rock is not important? Who said the pain you are feeling is only epiphenomena?

Such a picture of what physics and chemistry is about is every bit as consistent as the worldview Steven Weinberg, say, would have us believe. And I would say that it is more so; for it gives us a power and a hope for control in our lives that his can't even imagine.

Let me do two things for you. First, I'll paste in two old emails, that have to do with your music-on-the-page versus music-in-the-musician imagery (which I think it is so apt and so beautiful). [See stories from *Notes on a Paulian Idea* pasted into my 05-12-01 note "Lucky Seven" to B. W. Schumacher.] Mostly I'm pasting them because your note caused me to go back and read them this morning. And I'm just reconfirming that I'm on the same wavelength.

But then I want to quote William James. (That will come in a little later note.) I know you're not much in the mood to read any philosophy right now. But if you read the *right* stuff, I cannot see how it cannot help. My side of the conversations with you, in any case, is just a poor reflection of what William James already said with such flare.

23-04-02 *Installment 1* (to G. L. Comer)

From *Pragmatism*, pages 30–32:

> And do not tell me that to show the shallowness of rationalist philosophizing I have had to go back to a shallow wigpated age. The optimism of present-day rationalism sounds just as shallow to the fact-loving mind. The actual universe is a thing wide open, but rationalism makes systems, and systems must be closed. For men in practical life perfection is something far off and still in process of achievement. This for rationalism is but the illusion of the finite and relative: the absolute ground of things is a perfection eternally complete.
>
> I find a fine example of revolt against the airy and shallow optimism of current religious philosophy in a publication of that valiant anarchistic writer Morrison I. Swift. Mr.

Swift's anarchism goes a little farther than mine does, but I confess that I sympathize a good deal, and some of you, I know, will sympathize heartily with his dissatisfaction with the idealistic optimisms now in vogue. He begins his pamphlet on 'Human Submission' with a series of city reporter's items from newspapers (suicides, deaths from starvation, and the like) as specimens of our civilized regime. For instance:

> "After trudging through the snow from one end of the city to the other in the vain hope of securing employment, and with his wife and six children without food and ordered to leave their home in an upper east-side tenement-house because of non-payment of rent, John Corcoran, a clerk, to-day ended his life by drinking carbolic acid. Corcoran lost his position three weeks ago through illness, and during the period of idleness his scanty savings disappeared. Yesterday he obtained work with a gang of city snow-shovelers, but he was too weak from illness, and was forced to quit after an hour's trial with the shovel. Then the weary task of looking for employment was again resumed. Thoroughly discouraged, Corcoran returned to his home last night to find his wife and children without food and the notice of dispossession on the door. On the following morning he drank the poison.
>
> "The records of many more such cases lie before me [Mr. Swift goes on]; an encyclopedia might easily be filled with their kind. These few I cite as an interpretation of the Universe. 'We are aware of the presence of God in his world,' says a writer in a recent English review. (The very presence of ill in the temporal order is the condition of the perfection of the eternal order, writes Professor Royce (*The World and the Individual*, II, 385).] 'The Absolute is the richer for every discord and for all the diversity which it embraces,' says F. H. Bradley (*Appearance and Reality*, 204). He means that these slain men make the universe richer, and that is philosophy. But while Professors Royce and Bradley and a whole host of guileless thoroughfed thinkers are unveiling Reality and the Absolute and explaining away evil and pain, this is the condition of the only beings known to us anywhere in the universe with a developed consciousness of what the universe is. What these people experience *is* Reality. It gives us an absolute phase of the universe. It is the personal experience of those best qualified in our circle of knowledge to *have* experience, to tell us *what is*. Now what does *thinking about* the experience of these persons come to, compared to directly and personally feeling it as they feel it? The philosophers are dealing in shades, while those who live and feel know truth. And the mind of mankind—not yet the mind of philosophers and of the proprietary class—but of the great mass of the silently thinking men and feeling men, is coming to this view. They are judging the universe as they have hitherto permitted the hierophants of religion and learning to judge *them*. . . .
>
> "This Cleveland workingman, killing his children and himself [another of the cited cases] is one of the elemental stupendous facts of this modern world and of this universe. It cannot be glozed over or minimized away by all the treatises on God, and Love, and Being, helplessly existing in their monumental vacuity. This is one of the simple irreducible elements of this world's life, after millions of years of opportunity and twenty centuries of Christ. It is in the mental world what atoms or sub-atoms are in the physical,

primary, indestructible. And what it blazons to man is the imposture of all philosophy which does not see in such events the consummate factor of all conscious experience. These facts invincibly prove religion a nullity. Man will not give religion two thousand centuries or twenty centuries more to try itself and waste human time. Its time is up; its probation is ended; its own record ends it. Mankind has not aeons and eternities to spare for trying out discredited systems."

24-04-02 *Leibniz on de Finetti* (to T. Rudolph)

Rudolphism 2: *"Two things are identical if one can be substituted for the other without affecting the truth."*

But neither de Finetti nor I believe in "truth." How does that affect things?

24-04-02 *A Stapp in the Right Direction?* (to B. W. Schumacher)

I just ran across the abstract below on the archive. Maybe it's relevant to you doubting project.

John Preskill told me you'll be at Caltech for 10 months, starting in September. He was trying to use his siren song to lure me too for a little while. I'm pretty sure I can't do it this year. But I'm gonna try hard to do it next year while you're still there.

> Quantum Physics, abstract
> `quant-ph/0110148`
> From: stapp@thsrv.lbl.gov
> Date (v1): Thu, 25 Oct 2001 17:11:21 GMT (13kb)
> Date (revised v2): Tue, 23 Apr 2002 18:24:28 GMT (15kb)
>
> The basis problem in many-worlds theories
> Author: Henry P. Stapp, (Lawrence Berkeley National Laboratory)
> Comments: This extended version is to be published in The Canadian Journal of Physics
> Report-no: LBNL-48917-Rev
>
> It is emphasized that a many-worlds interpretation of quantum theory exists only to the extent that the associated basis problem is solved. The core basis problem is that the robust enduring states specified by environmental decoherence effects are essentially Gaussian wave packets that form continua of non-orthogonal states. Hence they are not a discrete set of orthogonal basis states to which finite probabilities can be assigned by the usual rules. The natural way to get an orthogonal basis without going outside the Schroedinger dynamics is to use the eigenstates of the reduced density matrix, and this idea is the basis of some recent attempts by many-worlds proponents to solve the basis problem. But these eigenstates do not enjoy the locality and quasi-classicality properties of the states defined by environmental decoherence effects, and hence are not satisfactory preferred basis states. The basis problem needs to be addressed and resolved before a many-worlds-type interpretation can be said to exist.

24-04-02 *The Program* (to B. W. Schumacher)

Schumacherism 6: *Next-to-last week of classes, and things are really humming. Hope all is well with you and yours.*

By the way, I'm gonna post a slightly weird paper on `quant-ph` tomorrow or the next day. [See "The Anti-Växjö Interpretation of Quantum Mechanics," `quant-ph/0204146`.] If you've got a few moments between the bars of your hum, maybe have a look at it. (It's easy reading; no equations.) Especially the parts titled Preskill and Wootters. Just to see the kind of strange friends you hang out with. (Tell me whether you'll disown me if I go through with this!)

25-04-02 *Two Nonorthogonal Pure States* (to P. Grangier)

I apologize for taking so very long to reply to your note. It's just that I've had a million things tugging at me from all directions.

Your reply to me was thoughtful, and requires no less of a thoughtful reply in return. And I'm finally writing you now because I think I've done that to my satisfaction. It just so happens to take the form of a paper which will appear on `quant-ph` tomorrow (`quant-ph/0204146`). I will attach a copy. I hope you will read it, especially the last two sections (concerned with Preskill and Wootters). At heart, I think there is no doubt that you and I are both realists. The only place we disagree is where we hope to see a hint of reality in quantum mechanics. I think in that paper I paint the clearest picture yet of the direction I am seeking. I sincerely hope you're reading it will delete some of the mystery of my ways to you.

Grangierisme 5: *Just a question: you are strongly insisting on gambling, but aren't you not surprised that a (pure) quantum state allows one to predict many things with probability ONE? This is a fairly strange behaviour for a "belief, state of belief, judgment, opinion" etc, that is usually far less efficient . . .*

Indeed this is quite an interesting question. And it is one I have thought quite a lot about in the last year. There was a time when I would have been surprised, but I am over that now. I say there is no right and true quantum state for a system, even when two distinct observers ascribe two distinct pure states to it (whether they are commuting or not). I talk about this in great detail in my web document "Quantum States: What the Hell Are They?" and eventually I'll be putting a lot of those arguments into a proper paper. Nevertheless, I think our discussion could benefit from your reading those pages. The link to my webpage is below. The places to look in particular are pages 19–23, 35–38, 42–48, 49–50, 53–54, 55–64, 72–75, 79–88. And then there are many more places beyond that in the same document on the same subject. In general, I think the explanations get better and more convincing on the later rather than the earlier pages. (So you might read it backwards.) However, it is certainly true that I tried to be clear in my writing throughout. So, if you end up not agreeing with me, I hope you will still find the effort entertaining.

25-04-02 *Two Nonorthogonal Pure States, 2* (to P. Grangier)

Grangierisme 6: *Exchanges of emails are hard to read, I prefer more concise formulations . . .*

You ought to know by now that my emails are more soliloquies than conversations! I might play off a couple of lines from my correspondents, but then they're almost solely just me spouting my mouth off. The technique is to read them as if they were independent papers.

Grangierisme 7: *By the way, may be you have seen* `quant-ph/0203131` *which is as short as I could do (3 pages only . . .). You will find . . .*

I have indeed read it, twice now.

25-04-02 *Short Thoughtful Reply* (to C. H. Bennett)

Thanks for the picture of the skunk cabbage. I've always wanted one. I'm sorry I wasn't able to reply to you earlier, but with all the students visiting, etc., I've had a gazillion things going on at once.

Let me give you a very short reply, for what it's worth.

Bennettism 10: *My main wonder about your beliefs is why do you find it so important to emphasize the subjectivity of quantum states, . . . What difference does it make in any case?*

I am just trying to do what scientists always try to do: understand how things "hang together." I.e., build a (satisfying) picture of the world that has nothing to do with my personal qualities. It just so happens, that my favorite problem happens to be different from your favorite problem.

Bennettism 11: *Do you think Katie really exists, or is she just a mathematical symbol for a set of bets you would [be] willing to make?*

Of course I think there is a sense in which Katie exists (i.e., some large remnant of Katie as she is now would be here even if I were killed tomorrow). I just make a distinction between all the stories I might write about her in my samizdats and whatever it is that she *is* in herself. What I don't understand about you is why you find that such a foreign concept.

In particular, I don't think I could ever write a sentence like this:

Bennettism 12: *My main wonder about your beliefs is why do you find it so important to emphasize the subjectivity of quantum states, but not other kinds of information, such as the dinner you just ate, your shoes, or other people?*

I don't think I've ever thought of the dinner I just ate as the information I just ate. Presumably there is something substantial to broccoli independently of my subjective judgments about how it tastes. Information, as motivated by Shannon, has something to do with the concept of surprise. If I believe strongly that broccoli tastes bad, then I will be surprised if I find that I actually like it. In that sense I will find that I have gained a lot of information when my subjective judgment makes a transition from its old value (Yuck!) to its new value (Mmm!). But that has nothing to do with broccoli as it is completely independently of me.

Even if I thought of a quantum state as an objective rather than a subjective quantity, I still don't think that I could ever talk as you did above. There is a difference between "systems" and "properties." And there is something in your language that seems to blur the distinction.

To make this concrete, take a classical description of a pendulum's motion in terms of phase space. I would never call the phase space point (x, p) "the particle." The particle is what *carries* the property (x, p). Within classical physics, both the particle and its property might as well be assumed to exist even when there are no physicists about. But let me ask the same thing about the Liouville distribution for a *single* instance of the particle. Can the single particle be said to "carry" a Liouville distribution in the same way it "carries" the coordinates (x, p)? I think you would be hard pressed to say that it does. For if I were to delete the physicist who is ignorant of the phase-space point the particle actually possesses, then I would delete the Liouville distribution. But I would not delete the value (x, p)—whichever one it is—that the particle can be safely assumed to have.

And that is all I am striving for in quantum physics. To figure out what properties we might safely ascribe quantum systems even when there are no physicists about. I think there are awfully good reasons for thinking that "the" quantum state is not such an "objective" thing. And thus the quantum state carries more of an analogy to the Liouville distribution than to the phase space point. However, that does not mean that I think our beloved theory gives us no hint of what the properties are that I can safely treat as objective. It is just that, among them, I do not see the quantum state.

Bennettism 13: *When you say a quantum state is just a set of bets you would be willing to make, what is the ontological status of you the bettor? Are you just a collection of bets some other bettor would be willing to make?*

Don't blur the distinction between the system and the state!

There, that's my short reply. As I say, I wish I could have replied to you earlier, but I had so many things tying me up. I wonder if I could make a birthday wish of you? Since my birthday was the 21st, would you give me this much of a present? Just read the parts of the paper I posted for `quant-ph` tomorrow, to do with Preskill and Wootters. I'll send it to you shortly. It's not long reading, and it's not hard reading. (Certainly no harder than a New Yorker.) And just give me two binary digits of satisfaction: After reading those passages, would you 1) say that you still do not understand my views of Katie, and 2) does it still look like a tower of turtles to you?

With enduring friendship (and a picture of skunk cabbage hanging on my wall),

25-04-02 *King Broccoli* (to C. H. Bennett)

Bennettism 14: *I relish the taste of broccoli (especially broccoli rabe) and brussels sprouts and cabbage, and I assume you don't.*

Actually, broccoli is my favorite vegetable of all time. Well, more particularly, broccoli as my mother—Texan through and through—makes it: overcooked, with a lot of butter and a lot of salt.

I loved your reply, but I don't think you answered either of my two questions.

Charlie's Preply

> Happy birthday. I read your last 3 sections, including the Acknowledgement [of `quant-ph/0204146`]. I knew when I wrote you my passionate and polemical words, in reply to your passionate and polemical words, that we were talking past each other. Basically I think the state vector is more like the point in phase space than like the Liouville probability density, and I think you think the reverse, but the big lesson we should draw from this is that it doesn't matter enough to get so passionate about. Passion distracts from doing science which I think is much more fun. For example I was recently reading your excellent paper with Caves and Schack about the quantum de Finetti theorem, which I became very interested in lately in connection with the quantum reverse Shannon conjecture, and after a while I was able to filter out the polemical remarks as effortlessly as all the ads for get-rich-quick I get in my email and enjoy the science almost as much as if I they had been absent, or had been ones I agreed with.

To return briefly to unimportant matters, I think all three—quantum state, classical state, and classical probability density—can be viewed either as autonomous realities or as bets someone is willing to make. Despite the strong feelings one may have, it's a fool's errand to try to prove that something is or isn't real. The scientific content of any quantum theorem or algorithm, such as de Finetti or teleportation, can be mapped almost effortlessly from one interpretation to another, by devices such as my favorite, the Church of the Larger Hilbert Space—with its unexplained preferred basis—or your favorite, the humble but undefined classical observer, and all we are left with to prefer one interpretation over another is its good or bad flavor, which ultimately is determined by our genetic makeup or early influences of our respective pedophiles. I relish the taste of broccoli (especially broccoli rabe) and brussels sprouts and cabbage, and I assume you don't. I conjecture that there is a correlation between love of broccoli and sympathy for the Everett interpretation. David Deutsch doesn't count, because he apparently doesn't much like any sort of food. Unlike the interpretations, this is a scientific question, susceptible to experimental proof or refutation. While we're at it, how do you feel about the Korean pickle known as kim chee?

25-04-02 *Quant-ph Number* (to C. M. Caves)

The `quant-ph` information is below. Of course I'd like to be cited at the second sentence in your abstract—"Whereas Hilbert space itself is an abstract construction, the number of dimensions available to a system is a physical quantity that requires physical resources."—but you can't do that. And, unfortunately, I don't find such a nice crisp statement with the same thought anywhere else in the paper. You know that I still don't buy into this business that, "A Hilbert space gets a physical interpretation … through privileged observables." And I don't really want to be associated with that thought. (From my way of thinking, I think it is safer to say that a Hilbert space gets its interpretation from the outside, not from the inside. I.e., From the subjective judgments an agent uses to give meaning to the measurement "clicks" he finds.)

25-04-02 *Guilt* (to U. Mohrhoff)

I am writing at the present moment because I have not been able to shake the feeling of guilt. To my shame, I still have your letter of 9/9/2001 sitting (with about seven others from other long-neglected correspondents) in the inbox of my email program. It stares at me every day and calls out for a reply. And one of these days, I'm going to do just that.

But in the mean time, as I say, I feel overwhelmingly guilty. This is because tomorrow morning another one of my foundational papers will hit the airwaves (`quant-ph`), and I know that I will be caught red-handed in having NOT neglected the subject. Also, next week I am going to put a rather large extension of my NATO paper on `quant-ph` too.

I can give you little in the way of compensation for my bad behavior, other than to let you know what I am feeling. Also, maybe I can let you know that I acknowledge your input in the larger paper (which will appear in the paper next week).

Acknowledgments

I thank Carl Caves, Greg Comer, David Mermin, and Rüdiger Schack for the years of correspondence that led to this view, Adán Cabello, Asher Peres, and Arkady Plotnitsky for their help in compiling the dramatis personae of the Introduction, and Andrei

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26-04-02 *Objective QM?* (to P. Grangier)

Let me answer your easy questions first:

Grangierisme 8: *PS In your paper you strongly "recommend" the work by Lucien Hardy. I have two comments:*

- *it surprises me that you adhere with the "relative frequency" approach to probabilities that is used by Lucien (his first axiom). I certainly agree with it, but I thought you would not.*
- *Lucien is trying to make QM look like a new probability theory. In my opinion, this cannot work because I am convinced that there is an inherent "geometric" (in Wigner's sense) content to QM. My views on this are explained in the* `quant-ph` *preprints.*

It is good that you picked up on that. Because those are two things that I dislike the most about Lucien's paper. I explain this in great detail on pages 147 and 159–166 in the document I wrote you about yesterday. [See 19-02-02 note "Re-Tackle" and 06-03-02 note "Poetry on Concrete" to L. Hardy.] (These are both examples of letters that need absolutely no background—other than having read Hardy's paper itself—in order to be read. So, do give it a shot.) However, that does not take away from the mathematical structure Hardy has set his focus on. And that is what I think is important, and worth wider recognition.

So yes:

1. quantum probabilities, like all probabilities, I take to be gambling commitments, and
2. I view quantum mechanics as plain old probability theory plus some further restrictions, NOT a generalization of classical theory. (Those further restrictions are what—I claim—carry the ontological content of the theory.)

And in both these opinions I contradict Hardy's paper. But I would rather accentuate the positive in his efforts for the moment.

26-04-02 *Transformation Rules* (to A. Peres)

Let me quote the piece of the paper you were concerned about:

> But the contexts are set by the structure of the Positive Operator-Valued Measures: one experimental context, one POVM. The glue that pastes the POVMs together into a unified Hilbert space is Gleason's "noncontextuality assumption": where two POVMs overlap, the probability assignments for those outcomes must not depend upon the context. Putting those two ideas together, one derives the structure of the quantum state.

What I was referring to with the point about the overlap is that one can derive the quantum probability law purely from the following simple assumption: there exists a function f from positive operators to the unit interval, such that the value of f sums to one over all positive-semidefinite resolutions of the identity. With this assumption (and nothing else, like continuity or differentiability), one gets that there must exist a density operator ρ such that $f(E) = \mathrm{tr}(\rho E)$. This is the modification to Gleason's theorem that I describe in `quant-ph/0106166` (section 4). So, it's not a completely trivial result of the standard probability rule; instead one can take it as an assumption and get the standard rule back out as a little gift.

Anyway, with this linear rule, comes for free the idea that probabilities transform linearly when one changes from one (sufficiently informative) POVM to another (sufficiently informative) POVM. And it is the transformations from "context" to "context" that Andrei has been making such a big deal about.

I hope that clarified a little bit.

03-05-02 *Accepting Quantum Mechanics—The Short of It* (to B. C. van Fraassen)

I'm feeling horribly guilty because I promised you a note over two weeks ago and everything—just everything—has conspired against me getting it constructed. I'm sorry. But still I want to write you something before I meet you next week. Let me try to do what I can in the next hour and then call it quits until Monday: The details of what I am about to say will be in a paper that I plan to have finished and can bring with me then.

van Fraassenism 1: *Now when it comes to theories that give us probabilities, whether absolute or conditional, I'll agree with scientific realists that literally read they say that there are objective probabilities in nature. But accepting such a theory does not involve believing that. Rather it involves appointing the theory as an 'expert' for guidance of our subjective probabilities concerning observable events. The metaphor of 'expert' is cashed out (as by Haim Gaifman) as follows. Suppose that I appoint Peter as my expert on snuffboxes. That means for my subjective probability P and Peter's subjective probability q the constraint:*

$$P\Big(A \,|\, q(A) = x\Big) = x$$

with generalizations of this to intervals, odds, conditional probabilities, for statements A that are about snuff boxes.

Thus the issue of whether there are objective probabilities in nature or whether to believe in them is finessed: there are only the theory's probabilistic <u>*pronouncements*</u> *accepted as input and my own subjective probabilities.*

That is clearly not how you are approaching it overall. But perhaps there are connections? I'd like to know how the QM probabilities are fed into your subjective probability as a whole – I wonder if it will not be similar. After all, even if a quantum state is read as a compendium of probabilities, and you say something like "this material is in quantum state such and such", your own subjective probability function has a domain much larger than facts pertaining to this material.

Yes, you are right, I don't like (ultimate) experts, and I don't think quantum mechanics has any more need of them than weather forecasting, say. In fact, I think any attempt to hold on to objective probabilities—even in the finessed form that you talk about, where there is a higher authority in whose judgment we place our faith—will only get in the way of our finding the deeper heart of quantum mechanics. Thus, I hold fast to the idea that there is no right and true quantum state EVER—just as de Finetti held fast to the idea that there is no right and true probability distribution EVER—and I take it as the very definition of my foundations program to see what is left behind. From my view, the theory does not pronounce probabilities; it only pronounces what we ought to be doing with them once we have set them just as subjectively as the next guy. (And to make the issue as pointed as possible, I even mean this for pure quantum states.)

What does it mean to accept quantum mechanics then? The imagery I am starting to build looks something like this.

If one generalizes the notion of quantum measurement to the one that has become essential in quantum information theory—namely to the positive operator valued measure (POVM)—then, for each quantum system, one can contemplate a *single*, fiducial quantum measurement for which the probabilities of its outcomes completely specify the system's quantum state. That is to say, whenever I write down a subjective probability distribution $p(h)$, for the outcomes h of such a fiducial measurement, I completely specify a quantum state ρ (pure or mixed). Imagine now that that fiducial measurement device is tucked away safely in some vault in Paris at the International Bureau of Weights and Measures.

A quantum state can be viewed as very literally nothing more than my subjective judgment for what would happen if I were to ever bring my quantum system up to that standard measurement device. What now can one say about a real-world measurement device, like one that we might have here at Bell Labs? Well, bringing my quantum system up to it will generally evoke a click of some sort that I might label d. Using all that I believe of the device, all that I might believe of Lucent's technical prowess, etc., etc., I would be a bad subjectivist if I didn't allow the click to update my beliefs about what would happen if I were to approach the fiducial device. Thus I end up with some updated probability distribution $p(h|d)$. (I.e., some updated quantum state.)

In what sense is this subjectivism connected to Bayesianism? This in part is what this paper of mine is about that I'll give you a copy of Monday. One can show that the usual story of quantum collapse can be viewed as the conjunction of two things: 1) Bayesian conditionalization, and 2) a final rotation of the axes of the probability simplex for the fiducial measurement. That is to say, quantum collapse in this description is only a pretty damned mild generalization of the Reverend Bayes.

But, again, what does it mean to accept quantum mechanics? It is this. If one studies the properties of these kinds of fiducial measurements, one finds that for no initial quantum state (in the usual Hilbert space picture) and no outcome is it ever the case that $p(h) = 1$. That is, when one accepts quantum mechanics, one eschews certainty for the outcomes of a fiducial quantum measurement. In fact, the set of allowed distributions $p(h)$ forms a convex set that is strictly contained within the probability simplex (i.e., the set of all imaginable probability distributions over an appropriate number of outcomes).

Thus, accepting quantum mechanics is not accepting the existence of an expert, but—in large part—accepting the two ingredients above:

1. voluntarily accepting a restricted range for one's beliefs $p(h)$
2. accepting a slightly modified form of Bayesian conditionalization for updating one's beliefs (i.e., standard Bayes + rotation)

The NEED for 1) a restricted range and 2) a minor modification of Bayes, is where I say we should be looking if we want to be looking for the "meaning" of quantum mechanics. What is it about the world as we view it that compels us to accept those two ingredients? That I see as the important question. And the ever more convoluted moves I see from some of our friends who want to hold on to a nonsubjectivist view of the quantum state, I see as a waste of good brain power.

That is to say, I agree with you in that, "Be a realist if you want to be." But I add to it with respect to the interpretation of quantum mechanics, "Don't do it for those parts of the theory where it is not productive to do so." If you're looking for a little realism in quantum theory, fine, but then look for it in a more clever place than in the state vector.

Anyway, that's my present take.

And I lied: that took me an hour and 35 minutes. I hope it's a little clearer at least for the extra time. See you Monday!

PS. You wrote:

van Fraassenism 2: *The two articles of yours that we took up in our discussion group in the fall were clearly only the beginning, and you have now taken the program much farther.*

Can you tell me which two articles you're referring to? That would give me a clearer vision of which views I've changed since your reading and which views I need to be careful not to let be propagated in your mind.

05-05-02 *Wigner and Clones* (to A. Peres)

I just skimmed your new paper. Near your sentence, "Why wasn't that theorem discovered fifty years earlier?" I think you ought to cite Wigner as a case example. Below is a little review of his paper I published at `quickreviews.org`.

The original volume might be hard for you to get hold of, but the paper can also be found in Wigner's later collection *Symmetries and Reflections*, page 200. The spot where he just misses getting the no-cloning theorem—i.e., the spot where he actually gets it wrong—is in the second paragraph after the paragraph containing Eq. (5). (In the S&R version of the paper, it's at the very bottom of page 205.) He writes, "Let us denote the n vectors which represent living organisms by v^k ... Then every linear combination of the v^k will also represent a living state."

Here's the story Bill Wootters told me, when I first told him about the Wigner paper in 1998:

Woottersism 5: *Thanks a lot for your note. I find it amazing that Wigner didn't quite realize that cloning was fundamentally impossible. Frankly, I'm still not impressed with the cloning paper Wojtek and I wrote. I remember asking [Professor X] for his opinion of a draft of that paper, and he said, in a very friendly way, that it would be a tough paper to referee, because on the one hand lots of people already knew the result, but on the other hand it may not have ever been written down.*

Review of:
Eugene P. Wigner, "The Probability of the Existence of a Self-Reproducing Unit,"
in *The Logic of Personal Knowledge: Essays Presented to Michael Polanyi on his Seventieth Birthday* (Routledge & Kegan Paul, London, 1961), pp. 231–238.

The no-cloning theorem first discussed by Wootters and Zurek and (independently) by Dieks is now understood to be a significant part of the foundation of quantum information theory. But have you ever explained it to another physicist and received a reaction of the form, "Is that it? That's the big deal everyone is talking about?" I have. And it's no wonder: the issue of no-cloning boils down to almost an immediate consequence of unitarity—inner products cannot decrease. In fact, Wigner's famous theorem on symmetries even shows that the group of time-continuous, inner-product preserving maps on Hilbert space is strictly equivalent to the unitary group. Therefore, it comes as quite a shock to see that Wigner himself just missed the no-cloning theorem! In this paper, Wigner tackles the question, "How probable is life?" He does this by identifying the issue of self-reproduction with the existence of the types of maps required for the cloning of quantum states. He doesn't tackle the question of cloning for a completely *unknown* quantum state head on, but instead analyses the "fraction" of unitary operators on a tensor-product Hilbert space that can lead to a cloning transformation for at least some states. Nevertheless, he states quite clearly that an arbitrary linear superposition of clonable states ought also to be clonable. But this, of course, cannot be.

I think this paper is quite interesting from the historical point of view of our field: it gives us an appreciation of the beauty and simplicity of that little theorem in a way that simply learning about it cannot provide. It gets at the heart of something deep in very present physical terms, terms that even a great mind like Wigner's missed.

07-05-02 *This Tape Will Self Destruct* (to G. Plunk & N. D. Mermin)

I don't know that I've ever told either of you how I started down this slippery slope of quantum foundations studies. It probably started officially in about 1985, when I took an undergrad research course with John Wheeler. He assigned ME a problem in computational general relativity. On the other hand, he assigned THE HONORS STUDENT who also took the course—I wasn't an honors student—the project of "deriving quantum mechanics." I was so jealous! I guess I never worked out that frustration. (I think the honors student dropped out of physics.)

So, now it's my turn with Gabe. And guess what I'm thinking about assigning him?

The problem is about fixing up (in a outlandishly Fuchsian way) the death and destruction I have tried to heap upon the Brun, Finkelstein, Mermin paper. That is, if you can't say a state is right and true, what can you say? The note below to Bas van Fraassen explains what I mean by all this in lay terms. [See note to van Fraassen dated 03-05-02, "Accepting Quantum Mechanics — The Short of It."]

The particular research project I have in mind for Gabe is to get a handle on the possible shapes for the convex set I describe below, as I range over all possible "standard quantum measurements." We'll start with single qubits. In particular, what I would like to know is what are the invariant properties of all these convex sets? (Read below and this question will start to make sense.) Could it be the volume? Something more esoteric? We can start with numeric work, and, if we get lucky, do something analytic.

Thus you see, what I want to say is, "When an agent accepts quantum mechanics, what he is doing is accepting a convex region on the probability simplex with such and such characteristics." It would be nice if the characteristic had a simple characterization.

This tape will self destruct in five minutes. Tell me whether you think this is a problem that could turn into a good senior thesis for a Cornell undergrad. I think it would.

07-05-02 *Failed Jokes* (to G. Plunk & N. D. Mermin)

I wrote:

> This tape will self destruct in five minutes. Tell me whether you think this is a problem that could turn into a good senior thesis for a Cornell undergrad. I think it would.

But of course, the title would have made more sense if I would have first said, "This is your mission if you accept it."

08-05-02 *Updated Version* (to B. C. van Fraassen, J. Butterfield, J. Bub, T. A. Brun, and D. Wallace)

Dear friends …

to whom I gave an early version of my paper "Quantum Mechanics as Quantum Information (and only a little more)". I astonished myself yesterday by having the time to clean it up to the point of publication. In particular, I think the introduction goes more smoothly now and I've made some different choices of words throughout that I think clarify things.

So, if you haven't looked at the version I gave you yet—and I know you haven't, with all the activity of the meeting—I'd encourage you to go to the real thing (when you have some time for sleepy reading). It'll be posted on quant-ph as of Thursday morning. Here's the link: `http://xxx.lanl.gov/abs/quant-ph/0205039`.

(And for Bas, in particular, you'll be able to find a PDF version there too; so you shouldn't have trouble printing.)

08-05-02 *The Big Eye* (to H. J. Bernstein)

Check out my new paper. It's titled "Quantum Mechanics as Quantum Information (and only a little more)" and you can find it at my webpage (link below). It'll also appear on `quant-ph` tomorrow.

I give you and your book with Mike Fortun a little advertisement at the beginning of Section 10.1 and in the Appendix, Point 21.

08-05-02 *The QMP* (to H. M. Wiseman)

Thanks for the letter, and particularly, thanks for skimming the paper. I chose a provocative title for the paper in an attempt to draw in the crowd, but the content of the two letters to Preskill and Wootters was pretty serious for me. What I'm looking for, in particular, is a way to make sense of science from a point of view that says at the same time, "Don't even think the terms in your theory ARE or CAN BE a reflection of reality. For the universe is big, and your head is small. And maybe reality is not static and unchanging—and thus describable in any finite terms, like in terms of GR and QM—anyway."

That's a sweeping statement, but I think it may be one of the two great lessons of quantum mechanics. (I'll keep my opinions secret on the other great lesson.) Anyway in particular, the way I see it, we as a community should be working as hard as we can to STOP trying to see the wavefunction as anything of a reflection of nature. Wavefunctions live in our heads. They live and die in our heads. And when they do their changing, they do that in our heads too. That's the point of view I'm trying to run through with as much consistency as possible. I.e., how can I hold to it, and still have quantum mechanical practice be what we all think it is? How can I hold to it, and not have everything that we say of nature be just a dream?

I'm sorry I've delayed so long in replying to you, but I wanted to do it right, and that required that I finish up a paper so that I could point you toward it. The paper's finished now, and I posted it on `quant-ph` this morning; it'll appear tomorrow. But if you wake up before `quant-ph` does, you can also get it at my webpage (link below). The title is, "Quantum Mechanics as Quantum Information (and only a little more)." The parts that are particularly relevant to our discussion are Section 4.2 and all of Section 6.

Wisemanism 4: *He implies that the same problems apply in classical theory. I disagree. The problem is that noncommutative algebra does not apply to the information in our brains. ... I'm not saying that it is impossible to do that, but it is an extremely difficult problem which he dismisses.*

Let me give an analogy that I think is apt. I would say Bohr's great genius in developing his model of the hydrogen atom lay solely in one little move: In dismissing the research program his predecessors laid out before him. That is, in dismissing the idea that the atom's spectrum required a mechanical explanation. Beyond that, the rest of his work was just details. Likewise, I think it is the case here.

I think that wavefunction collapse simply calls out for no explanation at all. And that is because I see it as nothing but a variety (and, in a way, an extension) of Bayesian conditionalization. To that extent, the problem—or, by my view, the nonproblem—was already there long before quantum mechanics ever showed up on the scene. Imagine that physical theory really were like the Newtonians wanted it to be: nice and deterministic, Laplace's demon and all that. Now consider a weatherman immersed in the world. He wakes up in the morning, thinks about all the weather readings he has taken the previous days, complains about the fact that his computer can't do as much number-crunching as he would like it to do, scratches his butt, decides whether he is feeling optimistic or pessimistic (that, of course, might depend upon how his girlfriend has been treating him lately), and so on and so on. He churns that all into a big mental pot, and finally comes up with a set of numbers $p(h)$ to describe his beliefs about what the weather will be doing tomorrow. In fact, at the same time, he'll probably come up with a whole set of numbers $p(h, d)$ to describe not only the weather tomorrow, but also the weather today.

Now, suppose he goes and has a look at the weather today and finds that it is d. Then, using Bayes' rule, he will update his belief for tomorrow from $p(h)$ to $p(h|d)$. It's a gut-wrenching, horribly discontinuous transition. But does it call out for an explanation? And if it does, does it call out for an explanation that has anything to do with the system the weatherman is modeling, i.e., the earth's atmosphere? For after all, in this world, when the weatherman looks out the window, one value of d was true and always remains true; one value of h was true and always remains true. The only discontinuous change is in his beliefs ... and those beliefs are presumably a property of his head.

So, yes, I think Duvenhage is absolutely right on this point. It's a point I've been trying to make for years—it's what Asher and I were up to in our *Physics Today* article—but of course sometimes it takes a long time to get the expression right. And I think Duvenhage did a particularly nice job on this particular score.

Now you and I both know that there's something more going on in quantum mechanics than there is for the weatherman. It's just that I don't think that "something more" is localized in the issue of "collapse." As far as the discontinuous change of belief goes—if you ask me—that happens both quantumly and classically, and in fact has nothing to do with any particular scientific theory. It is pre-science; it is simply a part of one's living and changing his beliefs in response to stimuli from the world outside himself.

By my present thought, part of the "something more" that goes on in quantum mechanics (as opposed to simple Bayesianism) is that we no longer have the right to assume that the things we do in our laboratories to change our beliefs (i.e., quantum measurements) leave the world unscathed in the process. I.e., we ought to be taking into account that when the world stimulates us, we stimulate it back in the process. But how do we take it into account in our description? My answer is that there's only one place to put it in a formalism, and that is in a further change in BELIEF up and beyond that dictated by Bayes' rule.

Thus, quantum collapse deviates from Bayes' rule, but not because it has anything to with something going on outside our heads. It deviates from Bayes' rule because the subject matter we are talking about when we are doing quantum mechanics (i.e., quantum systems) have an implicit action-reaction principle that Bayes, Cox, Ramsey, and de Finetti overlooked when they first worked out the calculus of belief change—i.e., Bayes' rule.

Now, you say things like,

Wisemanism 5 : *The problem is that noncommutative algebra does not apply to the information in our brains. . . . If we could believe that non-commutative algebra did apply to our knowledge then of course that would solve the quantum measurement problem. But how can we reconcile that with our experience?*

but, by my present view, using words like that is a red herring. (And Duvenhage is much more guilty of it than you.) The way I would say it now—please read the sections in the new paper I told you about—is that any kind of noncommutativity in quantum mechanics is just an artifact of a certain representation. The usual representation is a useful one to be sure—I could hardly calculate anything without it—but for the present issue I think it detracts from the clearer understanding we can hope to obtain if we'll just suppress it. (The "it" meaning the usual representation.) In fact, I think we're only going to get that understanding by exploring quantum mechanics as 1) a restriction on the space of probabilities (the probability simplex), with 2) a conditionalization rule that goes just a bit beyond Bayesian conditionalization.

And all this causes me to reject it when you say,

Wisemanism 6 : *The QMP is to find (i) a cut, and (ii) a way to bridge the cut, between the quantum systems that do have non-commutative information (in his terminology) and classical systems that do not.*

That is not the quantum measurement problem for me. People, experimentalists, scientific agents, have information or lack information when they are trying to talk about something outside themselves. Systems are just systems, and when they are treated as such, I would say the concept of information has nothing to do with them. Information is something I have or lack about a system. For instance, when I am speaking about you—thinking of you as a physical system—I lack quite a bit of information in the sense that your behavior could surprise me at any moment. But that means nothing about any kind of "information" intrinsic to you.

So, when you say what you said above IS THE QUANTUM MEASUREMENT PROBLEM, I would say that that is a problem that comes from trying to think of the quantum state, or the algebra of observables, etc., as literal properties of the things you are describing . . . and failing to recognize that the quantum state is a property of the observer and not the system. The quantum state is the full compendium of gambling commitments you would be willing to make about what the system will cause a measuring device to do.

What I myself see as the quantum measurement problem is to give compelling reasons for the two items I listed above. By compelling, I mean in the terms I lay out in the paper. When we can finally do that, then we will finally understand what properties we are really assuming for the "reality" of a quantum system.

Anyway, I hope that helps explain my position. I'll attach another piece of correspondence below with Bas van Fraassen that takes a different tack on some of the same issues. Maybe that'll help supplement the paper. I hope you'll get a chance to take a look at it. By the way, I quote one of your papers in Footnote 33.

Thanks for giving me the opportunity for trying to say these things a little more clearly. And certainly feel free to question or comment on anything that still doesn't make sense. (Anything that doesn't kill me might just make me stronger!) Hey, I'm coming to Brisbane May 25 to June 17. Do you think we'll get chance to talk sometime during then?

09-05-02 *Physics 7 Months Ago, Today* (to A. Peres)

Asherism 21: *I received* Physics Today *of October 2001 (I have not received September, because they took a cheap mailing service via India, and almost nothing is being retransmitted — a well known swindle). There is a gloomy article on Lucent. What is really the situation, seven months later?*

I'm sorry I haven't had a chance to write you before now; I have a backlog of email that is a mile high.

When I joined Lucent there were about 120,000 employees. Now, there are 50,000 … with Wall Street rumors running that Lucent will have to cut 5,000 to 10,000 more beyond that to return to profitability. So, things are a bit gloomy. We are now encouraged to try to bring in military grant money, etc. I doubt Bell Labs will ever be again what it was in the glory days, but so far it has been a safe haven for my nonsense. We shall see how long it will last.

Today Petra left. The group will miss her indeed. Frank Verstraete left last Friday. It was a very good month we all had together.

Petra told me today that she has decided not to return to Israel immediately. I suspect that was sad news to you.

Tomorrow Kiki and the family and I make the long drive to Ithaca for David Mermin's retirement party. Bill Wootters, Charlie Bennett, Gilles Brassard, Abner Shimony, and Lucien Hardy will be there among many others that I don't know. (Well, I've heard of some like Michael Berry.) In any case I'm sure looking forward to seeing the ones I do know! I will lecture on Sections 4.2 and 6.1 of my new paper, `quant-ph/0205039`.

10-05-02 *Go Ask Alice* (to G. L. Comer)

Well the days have come and gone, and now the week is just about over. I finally finished up my extension of the NATO paper posted on the LANL archive the other day. (I thank you in it, by the way. Have a look at `quant-ph/0205039`.) And the last of my visiting students, left yesterday. So I guess I'm getting ready for some kind of denouement in the coming week. [. . .]

Your note has had me singing that old Jefferson Airplane tune in my head all morning. (I got up a little before 5:00.) Torturous old man. I did really like that line of yours "Dualism is a Degeneracy." It strikes a chord in me with respect to my efforts to overthrow the idea that a scientific theory is (potentially) a mirror image of nature. I.e., it is not even potentially.

But I guess I have a hard intellectual time with the idea you express with: "[L]anguage is necessarily limited; beautiful, but just crude enough that my BElongINGs can never be completely shared." You can blame it on the Richard Rorty in me. You presume that there is a "person on the inside" that goes deeper than what can be built from language. That if we were able to conceptually strip all the rest of the world surrounding Greg Comer away, there'd still be something left. It's a long story, but I guess I don't buy that. The self is just a local "center of narrative gravity," Rorty put it. See his "Ethics without Principles" in his book *Philosophy and Social Hope*.

10-05-02 *Compatibility* (to D. Poulin)

Thanks for pointing me to your paper; in fact, I had already noticed it. Caves, Schack, and I are long overdue in our finishing up our criticism of the BFM paper. The manuscript made it up to 12 pages, and was essentially complete. But then I started to complain about "strong coherence," as opposed to the normal Dutch book argument. Anyway, hopefully we'll settle our differences and finish up the project in the late part of May when we all meet in Australia. Regardless of that, though, I certainly do find your measure of compatibility interesting.

14-05-02 *No BC's Role* (to R. Schack)

Thanks for the flurry of letters. They provoke a lot of thoughts in me and I am grateful.

I'm not sure how I'm going to reply to you, maybe just randomly (as the details occur to me).

Schackcosm 59: *In my discussions with classical cryptographers, I am often forced to concede that QKD is really quite limited in scope. You either assume an unjammable channel (which I believe cannot be assumed if you use the internet for communicating) or you use classical authentication, which means you share some initial key. Given these limitations, I am not convinced that QKD deserves such an exalted status as suggested by the Bub quote above.*

Has anybody taken the Brassard/Fuchs speculation any further?

Good point, of course. But I never meant for those two "axioms" to be read so literally. Perhaps my best expression of the idea is captured in a letter to Jeff Bub, 10 December 2000. It starts at page 100 of the old samizdat. The main point is that I see information-disturbance as the key idea, along with the commitment that information can never be completely locked away. Read those passages; I think they'll clear some of this up.

That said, precise versions of the no-bit-commitment ideas are coming to the top of my head again for their foundational value. Namely, as part of the extra assumptions that might get us to proper density operators in the "Wither Entanglement?" entanglement section (rather than simply linear operators). The thing that really powers the no-bit-commitment theorem is the Hughston-Jozsa-Wootters result that localized measurements on one system of two (described by a bipartite pure state) can "induce" any decomposition one wants for the other system's density operator. It turns out that the "pure states" of those other wacky operators in my bipartite-Gleason construction don't necessarily have this property. So, it looks like an assumption of such a nature might get me a little closer to the goal. (Though—even if I found it completely acceptable, and I'm not sure I do—it still wouldn't get me all the way to the goal.) But all of this is a long story, and maybe it'd be easier to talk about at a chalkboard.

14-05-02 *More Toenails* (to R. Schack)

Schackcosm 60: *Why are you so harsh on entanglement? In my quantum information lectures, I postulated the tensor product structure, as a natural formalism to deal with local operations on several particles. Entanglement is then derived, an unexpected consequence of the tensor product* *structure. You make this line of reasoning more compelling by showing that there is really no alternative to the tensor product structure. Far from being withered, entanglement emerges invigorated from your analysis.*

The main point is that entanglement can be thought of as secondary to the structure of quantum observables on localized systems. To that extent, one realizes that one can focus on the structure of simple observables in one's foundational efforts and forget about entanglement. In other words, it seems to me entanglement is not, as Schrödinger said, "the characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought." It is derived and secondary.

Now what is the structure of observables? The thing I try to argue in the paper is that a measurement is *anything* that gives rise to *any* convex decomposition of a one's original density operator. In that sense, measurement is nothing more than an arbitrary application of Bayes' rule.

Entanglement thus arises from the more basic idea of Bayesian conditionalization in conjunction with the idea that the allowed probabilities for a standard quantum measurement device do not explore the whole probability simplex.

Let me put two notes below that might clarify where I'm trying to go with this. I'd like to think that they add nothing beyond the paper, but it has been my experience that I can just never say enough. (I know what you're thinking: "You've got it backwards. If you'd just say it all in fewer words! That's what you really need.") [See notes to van Fraassen and Wiseman, dated 3 May 2002 and 8 May 2002, respectively.]

14-05-02 *Emphasis De-emphasis* (to R. Schack)

Schackcosm 61: *I am not sure I like your emphasis: You describe what I think is the exciting part as a "further readjustment of the posterior state".*

Yeah, I think I agree with that. There's no doubt that that's the most exciting part for me. That extra adjustment strikes me as capturing our beliefs about how we are stimulating the system (rather than how it is stimulating us). And to that extent, I would like to emphasize it. However, I guess I chose the tack I did because I wanted people to stop thinking of quantum collapse as something so different from "mere conditionalization."

Schackcosm 62: *Personally I like the concept of a compendium of probabilities better than your bureau, but it is very interesting to see how far one can get with the Bayes' rule alone.*

Again, here, it is a point of emphasis. The message I am trying to get across is that the structure of observables comes from Bayes' rule. They (measurements) are not defined independently of it. The SQMD struck me as an effective way to badger that point. "Compendium of probabilities" really stands for "compendium of ways of applying Bayes' rule." That is, I think it builds a stronger case for the idea that it is Bayes' rule all the way down when it comes to quantum measurement. The theory gives us no overt means to identify the objective thing that goes on behind a quantum measurement outcome. The only grounding we have is to *declare* a point somewhere off in the distance for which we will do no further updating. And that is the role the SQMD plays.

14-05-02 *Deletions and Their Obverses* (to R. Schack)

Schackcosm 63: *I think that taking William James and Darwinism seriously means to acknowledge that quantum mechanics is most likely to be superseded one day by a theory with even more* *cash value. This new theory may not have any of the features that we regard as the core of quantum mechanics. How then is Darwinism or pragmatism going to tell us anything about the foundations of quantum mechanics?*

You're right, I think you are definitely missing what I was hoping to express. But that probably just means I didn't express it so effectively.

No, I did not mean that Darwinism and pragmatism tell us something about quantum foundations. Just the opposite. I see quantum mechanics as giving us a great hint that there is still something deeper. Quantum mechanics is the first rip in the old fabric that told us our place in the world is a nullity. That is to say, I think quantum mechanics only gives us even better arguments for pragmatism.

Schackcosm 64 : *It looks to me as if your desire to find the objective core of quantum mechanics is against the spirit of both Darwinism and pragmatism.*

I think the core of the theory—I don't think I ever used the words "objective core"—is just our best guess of what we cannot change with our present level of skills. It is our best attempt to imagine what it would be like if we were not here. Obversely, when we have ferreted the core out, we will have a quantitative indication of how much of the world we can hope to control (given our present skills, present evolutionary level, etc.)

My desire for delimiting the core is expressed particularly in the passages below (taken from the paper). Maybe this makes no sense without A) watching the movie *It's a Wonderful Life*, and B) reading the "Sentiment of Rationality." Have you done either of these things? Probably not.

Your point certainly makes it clear to me that my paper is not self-contained! (The movie is an American classic and I think a large fraction of Americans have probably seen it; but James is another story.)

14-05-02 *Imaginations* (to R. Schack)

Continuing again . . .

Schackcosm 65 : *Actually, I found the part where you mention the selection of traits for your daughter outright disturbing, without any compelling connection to the discussion on quantum mechanics.*

Yes, me too. Because read in the wrong way—and maybe there was no other way to read it—it surely evoked images of Nietzsche (at the least) and Nazi Germany (at the worst).

But on the other hand, I don't know how to draw a meaningful distinction between our tools and ourselves. (Remember my point about the prosthetic hand in the paper?) Should we stop pursuing medical research because it goes against the grain of nature? Should we stop pursuing genetic techniques for controlling AIDS? Should we stop falling in love based on an attraction to our partners' complementary (positive) traits to our own? Traits that we would like to see (even if subliminally) appear in our children?

I guess I say no, no, no. Instead we need to cultivate at the same time a respect for everything in its time. Children of Down's Syndrome, say, deserve respect, not deletion. The Nazi experiment with nature was an atrocity. But, at the same time, I would say we cannot stop pursuing progress in genetic engineering.

The point you make is a deep one. I have no solution. Only an intuition of fear and promise, both at the same time.

If you get a chance, read Richard Rorty's book *Philosophy and Social Hope*. It's all about fear and promise and quite easy to read.

14-05-02 *More Still* (to R. Schack)

Schackcosm 66: *Everything you write suggests that you want more than just the theory with the highest cash value: you are looking for some form of absolute truth, something that transcends looking for the theory that makes the most accurate predictions.*

Yes, I guess so. I am looking for an indication that the world can be moved. I am looking for an indication that the only law in physics is the law that there are no laws. I am looking for an indication that the world is still writable.

I think if you want to call those things the pursuit of an absolute truth, you can. But they're only absolute in a pretty negative sense.

See, I told you you provoked a lot of thoughts in me.

14-05-02 *Qubit and Teleportation Are Words* (to C. H. Bennett, W. K. Wootters, N. D. Mermin, A. Peres, J. A. Smolin and others)

I doubt I will be of any use in constructing a short dictionary definition for the word teleportation, but let me try to explain my difficulty with the word "property" with regards to both quantum states and quantum entanglement. I do this with a little trepidation, but on the other hand, you're the ones who brought me into this discussion and I feel I ought to say something.

The trouble I have with the word "property" has to do with one of the main points Charlie brought up in his talk this weekend. What instantaneously and physically changes about Bob's system when Alice performs a measurement on hers? Charlie told us *nothing* and I agree with that. But then, I look in the *American Heritage Dictionary* and find:

> **property**:
> 1) Something owned; a possession. . . .
> 4) a. A characteristic trait or peculiarity, especially one serving to define or describe its possessor. b. A characteristic attribute possessed by all members of a class. See synonyms at **quality**.
> 5) A special capability or power; a virtue.

If you think of a quantum state as a property owned by the system of which it is about, then you—Charlie Bennett in particular—are obliged to continue propagating this thing you told us was a misconception. At the completion of Alice's measurement, there is a new quantum state for Bob's system. If the quantum state is interpreted as a "special capability or power" for the system at Bob's end, then you cannot get around the conception that Alice's twiddle caused a change to something localized way over there.

And that, it seems to me is dangerous business. What I am saying has nothing to do with hidden variables. It just has to do with the word "property". The trouble only has to do with the idea of a quantum state as a kind of feature possessed by a quantum system.

If you want to think of the quantum state as a property of something, it seems to me the best you can do is speak of it as a property of Alice's head (or Bob's head, or whoever's). For, the quantum state represents the *predictions* she can make about measurements upon the system in question. Similarly I could say all the same things about entanglement.

To my view, toying with the idea that a quantum state is a property, is to toy with a kind of pantheism or anthropomorphism that my materialist mind won't tolerate. Do all rocks have souls? You'd laugh at that, but it seems to me that's about what you're attempting to do in thinking of the quantum state as something possessed by the system itself. "That rock judges his chances of reacting to the measurement device to be such and such!" What could be more anthropocentric than that?

Bennettism 15: *"Properties" can reasonably be taken to be much broader than hidden variables, and may include all sorts of conditional and post-selected behaviors, eg "how a system would behave if I measured its Z spin component, after having watched my favorite horse lose at the race track."*

But, as I see it, conditional properties fare no better than the nonrelational type in this regard. At the completion of Alice's measurement, there is a new quantum state for Bob's system, and thinking of it as a conditional property in Charlie's sense still means something physical changed at Bob's end. This is because the system did not have that particular (conditional) property before Alice's action.

The main point is this: Whatever a property for a quantum system is, it should not be something changeable by someone's twiddles far, far away. For instance, take the dimensionality posited for a quantum system by the ascription of a Hilbert space to it. I think this is a perfectly good candidate for a property of a quantum system; it's one I would endorse. For, once set, there is nothing Alice can do at a far away location to change it.

So there, quantum systems do have properties—or at least I'm willing to bet they do. It is just that the properties do not include among them "the" quantum state ... and it is the quantum state that is transferred in the process of teleportation.

What I find miraculous about teleportation is that Victor (the guy who ascribes the original, unknown state) can transfer his predictions from one physical system to another at the cost of only two bits of physical action on the target system. In that sense, it is Victor's description that is teleported from one system to the other with almost nothing whatsoever traveling in between. But that'll never make it into a dictionary.

Of course, this is an ongoing debate between Charlie and me. I'll paste below one piece of our correspondence that has to do with the present conversation—it itself focused on the issue of properties. [See note to Bennett dated, 25 April 2002.]

By the way, I agree with Bill that the word "object" is better than "system." *American Heritage* writes this in its first two definitions for the word:

> **object**:
> 1) A material thing.
> 2) A focus of attention, feeling, thought, or action.

Asher's Reply 1, "Quantum Teleportation Defined: Nothing Happens at Bob's End"

> When the quantum teleportation process was conceived, we had no clear understanding of what was going on, and this may still be true today. I learnt a lot from Chris when we wrote our "Opinion" essay in *Physics Today*, March 2000, pp. 70, 71. Here is the passage about quantum teleportation, mostly due to Chris:
>
> > The peculiar nature of a quantum state as representing information is strikingly illustrated by the quantum teleportation process.[4] In order to teleport a quantum state from one photon to another, the sender (Alice) and the

receiver (Bob) need to divide between them a pair of photons in a standard entangled state. The experiment begins when Alice receives another photon whose polarization state is unknown to her but known to a third-party preparer. She performs a measurement on her two photons-one from the original, entangled pair and the other in a state unknown to her-and then sends Bob a classical message of only two bits, instructing him how to reproduce that unknown state on his photon. This economy of transmission appears remarkable, because to completely specify the state of a photon, namely one point in the Poincaré sphere, we need an infinity of bits. However, this complete specification is not what is transferred. The two bits of classical information serve only to convert the preparer's information, from a description of the original photon to a description of the one in Bob's possession. The communication resource used up for doing that is the correlated pair that was shared by Alice and Bob.

The crucial point is in the last five lines: Alice and Bob know nothing of the states of their particles (the one that the preparer gave to Alice, and the one that Bob finally has). To make the process simpler let the two bits be sent by Alice to the preparer, next to her, not to Bob, far away. Nothing happens at Bob's end. Only the preparer knows instantaneously what is the state that Bob can verify, if and when he receives the relevant information (same two bits).

How do you explain that to non-believers?

Asher's Reply 2, "Proposed Definitions: Qubit, Entanglement, Quantum Teleportation"

I tried to find definitions that are scientifically correct and do not depart too much from the style of the *American Heritage Dictionary*, so that they may be acceptable to them.

qubit − quantum bit

The smallest component in a computer designed to manipulate or store information through effects predicted by quantum physics. Unlike bits in classical systems, a quantum bit has more than two possible states labeled 0 and 1. It can also be in a combination of 0 and 1 according to the superposition principle, or even have no definite state at all, if it is entangled with other quantum bits.

entanglement

A quantum mechanical situation where a composite system has a definite state, but none of its constituents has a definite state. Moreover, if separate measurements are performed on these constituents, the results show strong correlations.

quantum teleportation

The instantaneous transference of our knowledge of the properties of a quantum object to those of another distant object, without physical contact with the latter.

This is not quite the same as our original paper, because here Bob (if he exists - he is not needed) does not perform a Pauli rotation to have an identical state. Anyway, if the state of his particle is publicly known, then the required Pauli rotation will eventually be known in the whereabouts of the second object.

Asher's Reply 3, "Instantaneous Remote State Determination"

Since Charles protested my use of the term "instantaneous" here is a paragraph of my paper "Classical interventions in quantum systems. II. Relativistic invariance." PRA **61** (2000) 022117:

> A seemingly paradoxical way of presenting these results is to ask the following naive question: suppose that Alice finds that $\sigma_z = 1$ while Bob does nothing. When does the state of Bob's particle, far away, become the one for which $\sigma_z = -1$ with certainty? Though this question is meaningless, it has a definite answer: Bob's particle state changes instantaneously. In which Lorentz frame is this instantaneous? In *any* frame! Whatever frame is chosen for defining simultaneity, the experimentally observable result is the same, owing to `\Eq{etcr}`. This does not violate relativity because relativity is built in that equation, as will now be shown in a formal way.

Indeed when Alice finds her result she definitely knows Bob's *potential* result if and when he performed or will perform his measurement.

14-05-02 *Chris's World* (to J. A. Smolin, C. H. Bennett, N. D. Mermin and others)

Good to see you in the morass.

Chris's world: It's a funny place with all these fancy words like "ascription" and "posit" to remind us that there's a head in the background of every quantum state, but there's no instantaneous action at a distance there—no one would have ever thought there might be. I know the language drives Charlie bonkers, and it probably drives you bonkers too. But mostly the complaints just remind me of what I used to hear in my hometown in Texas when the seatbelt law was first enacted. "Why that Majatek boy was thrown clean from the car! Not a scratch on him. If he'd have had a seatbelt on, his head would have been nothin' but mush now; we'd be at his funeral today. Damned politicians puttin' their noses into places where they ain't got no business."

So you see I view the language as a safety measure—one that I think will allow us to drive farther, longer, and ultimately faster. But first you've got to learn how to drive with a belt on.

John Smolinism 1: *One is naturally forced into Chris's world of saying all that changes is what people predict about the state, but that's a property properly defined as above.*

Chris would never say this. When I write down a quantum state, I think of it as my judgment or prediction for which of one or another measurement outcome will turn up. I don't predict things "about the state." I don't know what the TRUE state is or could be, and thus my ignorance cannot be about it. In contrast, I would say the ignorance is always about further measurement outcomes. Or a pleasing picture is that one can ground the ignorance with respect to a single device sitting in the National Bureau of Standards if one wishes. See Sections 4.2 and 6.1 of the fat paper I put on `quant-ph` last week (`quant-ph/0205039`).

That said, I also see the suppression of another crucial issue here—one that also flies in the face of the word property, especially with your emphasis on DEFINE in definition 4. If you will allow me to call a mixed density operator a quantum state, then I know even you will agree that there is no unique quantum state for a system. Thus it cannot be a property. So, I think you'll be left with being only willing to call a pure state a property. So be it. (I wouldn't do that, but I'll let you do that for the time being.) But now, let's go back to Alice and Bob. By your account, first Bob's system has no property, then Alice measures her system, and—Zing!—now Bob's system does have a property. I.e., first it has no quantum state, then it does. How does the system know it ought to have that property if there's no action at a distance? Alternatively, if it doesn't know it, why call it a property of the system?

It is because of that conundrum that it seems to me to be more fruitful to just give up thinking of the quantum state as a property of the system it targets.

I think a good analogy can be found in *classical* information theory. A homework problem in a textbook gives you a discrete memoryless channel by specifying the transition probabilities $p(y|x)$. Then it asks that you calculate the channel's capacity. One goes through all the work and gets a number. From that, one starts to get the feeling that the capacity can be an objective property for a real physical channel. Why else would it take so much work to calculate something if it weren't real? But it can't be real in any absolute sense. For, with respect to a Laplacean demon, there is never any noise in the channel at all; he can predict which bits will be flipped and which won't. The point is, the capacity is only objective WITH RESPECT TO a *subjective* judgment $p(y|x)$. Similarly, I would say with all quantum states. Just because a textbook says calculate such and such a property—the entanglement of formation, the distillable entanglement, or whatnot—of a quantum state, one finds that one gets into the same habit of thinking those properties have no subjective component.

You can do so, but then it's your burden to explain to young school children and journalists—who think of a property as something like the color red, a ball either has it or doesn't—what changes in Bob's system when Alice performs a measurement.

14-05-02 *Quick Answer about Quantum de Finetti* (to K. Jacobs)

Jacobsism 1: *Quick question about the Quantum de Finetti representation. The expansion for an exchangeble $\rho^{(N)}$ is unique — but does N have to be greater than or equal to 2 for this to be true?*

The de Finetti representation is something that applies to a *sequence* of density operators $\rho^{(N)}$ living on larger and larger Hilbert spaces $H^{\otimes N}$. The theorem says there is a unique decomposition of the form $\int P(\rho)\rho^{\otimes N}d\rho$. But of course, that does not negate the fact that for each N on can find many ways to decompose the mixed state. It is just that if one finds an alternative way of expressing the density operator at level N, that alternative expression will not work for level $N+1$. The case $N=1$ is especially relevant in that regard. There are a continuum of ways to decompose $\rho^{(1)}$, but only some of those ways will work to get you up to $\rho^{(2)}$, and even fewer will get up to $\rho^{(3)}$, etc. In the limit, only one of the original decompositions will work.

15-05-02 *And Only a Little More* (to W. K. Wootters)

I'm just taking care of some loose ends left from the meeting in Ithaca. While we were there, I had hoped to put a copy of my new paper into your hand. But every time I thought about it, I thought it would be awkward for you to have to carry it since you weren't carrying a briefcase. Then at our final encounter, just before your leaving, I didn't have a copy with me!

Anyway, here's the link in case you're interested:

> Quantum Mechanics as Quantum Information (and only a little more)
> `http://xxx.lanl.gov/abs/quant-ph/0205039`

Of course, feedback, both good and bad is always welcome. What I am striving for is consistency, and it's pretty hard to see lapses in that without an outside eye.

By the way, mention that philosopher's name again that you've been taken with. I'll look her/him up.

Bill's Reply

The book I mentioned is *Nature Likes to Hide*, by Shimon Malin, a physicist at Colgate University. The book is in three parts: (i) an introduction to the ideas of quantum mechanics (the book is for a very general audience), (ii) application of some ideas of A. N. Whitehead to the interpretation of quantum mechanics, (iii) presentation of a worldview based on quantum mechanics and ideas of Plotinus. I wouldn't follow Malin all the way, but there's something about part (ii) that struck me as exciting and worth exploring further. I had already been reading Whitehead and other writers inspired by him (especially Hartshorne), as well as C. S. Peirce, who inspired both Whitehead and Hartshorne.

Whitehead argued that a materialistic philosophy could never adequately account for mind. So in his view, the basic elements of the world are not material objects but are more like moments of awareness: they are more like events than persisting objects, and they always have both an interior and an exterior aspect. There is some similarity with Wheeler's view that the basic elements are quantum events, except that with Whitehead there is more of a balance between the interior and exterior aspects of each basic element. Wheeler thinks of quantum events merely as objective events, seen only from the outside, as it were. Whitehead says that at every level of being, there is always an interior view. At the higher levels, this interior view is what we call consciousness. At lower levels, it's not consciousness but it exists nonetheless. So consider a photon encountering a beam splitter, with a detector in each of the two possible paths. The firing of one of these detectors is an event that can be viewed objectively, but according to Malin (interpreting Whitehead), it can also be seen from the inside: the freedom to choose one detector or the other can be thought of as a kind of proto-experience. It's too primitive to be called either experience or consciousness, but it has some quality in common with consciousness (whereas a purely material object would not have this quality).

I think the above paragraph should give you a sense of the direction Malin is going. There are a lot of things about Whitehead's view, and Malin's view, that resonate with my own ideas. Of course I like the Wheelerian idea of taking quantum events as basic, and I like this question: Where does a quantum event reside? For example, where was the decision made to make one of those two detectors fire? It wasn't made at the location of the detector that fired, because then how would the other detector have known not to fire? And we know very well that the decision wasn't made at the beam splitter. Pretty clearly, the decision wasn't made at any particular place in spacetime. So I don't think a quantum event resides primarily in spacetime. A quantum event is more primitive than spacetime, as Wheeler has said many times. What I like about Whitehead and Malin is that they emphasize a commitment to consciousness as something central to existence and something that one has to have in mind even when talking about the most basic elements of existence.

15-05-02 *Not Even Vague Thoughts* (to R. W. Spekkens)

Spekkensism 9: *I've also been thinking that the analogy between the possibility of "steering" and the impossibility of cloning suggests that there is something, like the task of steering but different, that is analogous to broadcasting and which would truly separate the quantum from the classical. So far I've only got vague ideas. Have you given any thought to this?*

I like this line of thought you're pursuing, but no I haven't thought about it any more than the little I did in Princeton. I guess the first question is, what does entanglement buy you as far as steering is concerned? For, even with unentangled bipartite (mixed) states, Alice can steer Bob into noncommuting possibilities. So the line is not drawn at commutivity.

You might be able to prove something like this: Suppose Alice and Bob possess an unentangled mixed state, but one for which no decomposition exists in terms of commuting states on Bob's side. There can be no device on Bob's end that will make a tripartite state $AB'B$ for which the AB and AB' states are identical copies of the original. But then, what does that have to do with steering?

In any case, you know I hate the word steering. What one needs for No-QBC is simply that Alice be able to predict what Bob will find if he performs any valid measurement in the protocol. It has nothing to do with steering anything.

15-05-02 *Marburg, Strasbourg, Blunderburg!* (to A. Peres)

Asherism 22: *I started to read your QM as QI, a few pages each evening, when I am too tired to do anything productive, but not yet ready to go to sleep. Will this appear in the Växjö proceedings? Two misprints in the table page 2:*

> 1973 Marbourg → Marburg
> 1974 Strasbourg is in France, not in Germany (it was in Germany 1940-44 and before that 1871-1918, but it is in France since 1945).

Thanks for catching those mistakes. [See 01-04-02 note "Last Little Push" to A. Peres, N. D. Mermin, A. Cabello, H. J. Folse, and A. Plotnitsky.] I'm a little ashamed that I made them. And I am flattered that you are reading the paper; thank you for that independently.

Yes, an older version of the paper will appear in the Växjö proceedings. The version on `quant-ph` has 25 or 30 spots where the language has been changed somewhat. I have contemplated trying to send the final, final version to Andrei. But I don't know that it is worth my while. With Andrei's whirlwind methods, I suspect it is too late for me to send him revisions in any case. Of course, the only version people will ever read—if they read that much—will be the `quant-ph` version.

I just downloaded your no-cloning history to see how you took into account Wigner's paper. I was a little surprised when I didn't see you mention him. I would guess that you made that decision because you did not muddy Wigner's name. Still, though, I think his blunder only adds to the importance of the theorem (despite its mathematical triviality), and I wish the readers could have seen that at the same time that you defend your decision on FLASH.

Kiki and I arrived back home from Ithaca late Sunday evening, with a hobbling car. (It is our Honda mini-van, only one year old.) First Kiki had a crash Saturday morning that tore deeply into the left-hand fender and bumper. No one was hurt in the accident, only our pocketbook and insurance rate. But then Monday, while we were bookshopping, someone decided to even out the car's aesthetic for us: Somehow they hit the front of the car and tore the front bumper off. It was a hit and run, and no one in the neighborhood saw it happen.

The meeting in Ithaca was quite nice, with quantum informationists, quasi-crystalists, bioligists, magazine editors, science policy advisors, etc. I think the best thing that happened to me was that I met Michael Berry for the first time. He introduced himself as I was talking to Philip Pearle. He said, "Oh, you're Chris Fuchs?" I thought, "Michael Berry knows of Chris Fuchs?" It was quite a surprise to me. Anyway, he proceeded to tell me how much he liked our Physics Today piece, and how he quotes from it to his students. During the meeting, I also gave him copies of my two Växjö pieces and he gave me some good feedback on both. Sometimes, I need confidence builders like that.

15-05-02 *Berry, Etc.* (to A. Peres)

Asherism 23: *Michael Berry must know of you because you were offered a position in his department. Here is a frequent visitor to Israel because his wife has her parents here.*

I didn't quote Wigner. Why kick a dead horse? However I added a quote to Jozsa's recent `quant-ph`*, which is really very nice. It's Bill who called my attention to it.*

I accept your decision. But still I contend that you missed a good opportunity. The point is not to kick a dead horse, but to glorify a living theorem. Wigner was one of the greatest minds in physics this century—nothing can take that away—but yet he came to within an inch of the theorem and then missed it. I think that signifies something interesting psychologically. Perhaps it also signifies what a radical departure quantum mechanics is from classical lines of thought.

15-05-02 *Bayes, POVMs, Reality* (to A. Shimony)

It was good talking to you this weekend. If you wouldn't mind committing your story of meeting de Finetti to email, I will see to it that it is archived forever in one of my samizdats. (See my webpage; link below.)

Also, if you could send me your mailing address, I will send you copies of my two new papers on Bayesianism, POVMs, and good candidates for quantum reality. Alternatively, if you are accustomed to downloading things from the `quant-ph` archive, here are the links:

1. "Quantum Mechanics as Quantum Information (and only a little more),"
`quant-ph/0205039`

2. "The Anti-Växjö Interpretation of Quantum Mechanics,"
`quant-ph/0204146`

I wrote them both in an attempt to be entertaining. I hope you find them so.

Abner's Reply

> Here is what I recall of my conversation with de Finetti. In 1971 the 3rd International Congress of Logic, Methodology, and Philosophy of Science was held in Bucharest (where I had the dubious pleasure of meeting Ceauşescu in the receiving line at the Palace of Ministers; what an ugly hard face he had.) De Finetti was there, and I believe that I introduced myself, saying that I had some questions. The only question I recall was why he didn't use the strong version of coherence. He said that he was aware of the option of using it rather than the weak version, but he didn't like the consequence of the substitution: namely that $C(h/e) = 1$ only if e entails h. This consequence is part of my

(2') in Sect. 5 of my "Coherence and the Axioms of Confirmation", p. 136 in vol. I of my *Search for a Naturalistic World View*. I don't recall the details of his objection to this principle, but I think he said that it would cause trouble if one had a nondenumerable set of mutually exclusive possible hypothesis, as in the case of probability on continua. I vaguely recall agreeing with him that there would be a problem, because I discuss the problem on pp. 137–140, op. cit. I vaguely recall saying (or maybe just thinking) that epistemic probability doesn't apply well to nondenumerable sets of hypotheses, but thinking that the propensity interpretation of probability, usable in stat. mechanics, could properly deal with nondenumerable sets of possible outcomes. De Finetti surely would not have liked this discrimination, since he believed that the only clear sense of probability was epistemic, and in particular personalist. In my later paper, "Scientific Inference", op. cit. I suggest pragmatically reasonable strategies for dividing the entire set of hypotheses into as denumerable set, or even a finite set, by properly lumping subsets of hypotheses. This strategy seems to me in the right direction, partly because it is part of a program taking the Bayesian formalism as only a framework, which has to be supplemented by pragmatic and by a posteriori considerations.

16-05-02 *King Broccoli, 2* (to J. A. Smolin, C. H. Bennett, N. D. Mermin and others)

I'll close with this statement. But then after your rebuttal—if you care to make one—we should probably take this offline. I suspect no one cares to explore the issue further (not even you and not even me). But you said something so nicely, I thought I should emphasize it.

John Smolinism 2:

> [CAF wrote:] [F]irst Bob's system has no property, then Alice measures her system, and—Zing!—now Bob's system does have a property. I.e., first it has no quantum state, then it does. How does the system know it ought to have that property if there's no action at a distance? Alternatively, if it doesn't know it, why call it a property of the system?

It always had the property that IF Alice measured one thing, then it would behave as state ϕ. Conditional properties like that do, of course, imply a sort of action at a distance, but what's so bad about that? It's not the sort of violate-the-speed-of-light action at a distance that we should be concerned about. In the end, it means just what you want it to, except for the word property. To use a classical example, suppose I have a box that comes from the factory with either a red ball or a blue ball in it. Surely one can say that a property of the box is that it has either a red ball or a blue ball in it—that's the entire definition of the box. Now if I call up the factory and they tell me what color the ball actually is, never touching the box, does the box change? Was I wrong to call that other thing a property? But everyone understands what's going on. Entanglement doesn't really bring in anything new here. (Alternatively you could formulate it like Charlie sometimes does and say well, it was ALWAYS a red ball, but the measurement result travels backwards though time and fixes things up so it is ok to say "always," but I'm sure you won't care for that).

You said that perfectly, and, of course, I especially liked the concession that this choice of words does entail a kind of uninteresting action-at-a-distance … but action-at-a-distance nevertheless. Maybe my point is just, how is the non-quantum-practitioner supposed to know where to draw the line between the uninteresting and the interesting versions of the effect? Between the science and the science-fiction? How will he ever be able to shake the nagging feeling that we ought to be able to harness the uninteresting version and turn it into the interesting one? (For that matter, how will the quantum practitioner? Nicolas Gisin comes to mind.)

About the particulars of your box example, I think the common man would be hard pressed to take the textbook definition of a problem—like the one you describe above—as a property possessed by a physical system. How can the box containing the red ball know I've embedded it in a problem where the possible colors are {red, blue}, or instead in a problem where they are {red, blue, green}? People think of physical properties as the things physical systems carry around with them independently of the rest of the world. You may say that this is a limiting conception of the word "property," but I'm pretty sure it's the conception most people use. They would call the set {red, blue} a property of the problem you've defined, not of the system. Within classical physics, they would say the ball has whatever properties it really has (say, red OR blue) ... and it is the physicist's task to figure out which of the two it is.

The standard retort you and Charlie give me is that I am doing nothing more than encumbering the language by saying "a quantum state is ascribed to a system" rather than saying "a quantum state is possessed by a system." But, come on, the word count is the same. It is not that I am encumbering the language; it is just that I am beating on a prejudice you don't want to let go of. Or maybe to be more conciliatory, it is that you cannot imagine that this kind of language could ever be useful, whereas you think there are loads of examples where your own language has led to triumph. But I think Charlie's talk the other day about the public's perception of quantum teleportation as a kind of instantaneous action-at-a-distance (in the science-fiction sense) is a case in point.

Things are only interesting or noninteresting with respect to a context. I think there is a sense in which quantum teleportation is less interesting with respect to the conception that a quantum state captures a state of knowledge rather than a property possessed. What could be less interesting to say than that, "Quantum teleportation is the transference of one's predictions about one object onto another object that has never interacted with the first"? Maybe it's only this that's keeping you in the old bounds. Is it that if you keep a little bit of the science-fiction imagery alive, it'll help fuel the physics?

"In any case, none of this matters for doing physics," you say. But I think it does in the long run. (Certainly, you've got to concede that there's something that fuels me—and it hurts to think that you might think it is nothing more than irrationality.) When Charlie sent me the picture of a skunk cabbage in reply to these very issues, I found myself thinking of King Broccoli. The story goes that one day, by divine providence, it came to King Broccoli that broccoli, the vegetable, his namesake, actually tastes good. Good in a way that hitherto only gods and angels had known. Every child who had ever said, "Yuck, I don't like broccoli; it tastes awful!" was simply wrong ... or at least that's what the king realized. King Broccoli, being the head of state, decided to do something about it. Henceforward, all gardens in the kingdom should have a patch devoted solely to broccoli. It really wasn't much of a burden on the national product (except, perhaps, for the psychiatrists who had to treat all the movie stars who had never felt fulfilled in their broccoli experience). But think of the diversity of vegetables the kingdom might have raised if its citizens hadn't been encumbered with the king's notion that broccoli had an objective, but never verifiable, taste?

Moral? Maybe there's none. But it is a documented fact that the Kingdom of Broccoli eventually fell and was replaced by a liberal democracy (where the ideals rather than the particulars have an objective status).

Everyone in this mailing list knows by now—though Charlie and John seem to keep forgetting this, or maybe they've never let it sink in—that I think quantum mechanics is just the hint of something much deeper, some fantastic physics yet to come. But I also don't think we will ever stumble upon that physics until we truly get rid of our classical prejudices: seeing quantum states as "properties"—it seems to me—is one of these.

Signing off,

Chris

My own disclaimer: Though I implied above that quantum teleportation becomes less interesting within a subjective conception of the quantum state, I think effects like quantum cryptography become *more* interesting from this view. So there is a tradeoff.

16-05-02 *Words* (to C. H. Bennett)

Bennettism 16: *Sorry. Point well taken. You are a perpetual stimulus to me, if not always in the ways you hope, and I would miss it terribly if you stopped. I should be more grateful. Like the other day, when John was discussing your automotive metaphor, you inspired me to think that all the cautions you would have us take against quantum misconceptions are—for me, if I did them—like driving a car with the parking brake on all the time.*

Then it seems to me you should at least be consistent in your behavior. I interpreted your talk Sunday as a genuine concern for the perceptions the masses hold about some of our favorite quantum effects. Do you have a concern, or do you not? Is it that the world really has some kind of instantaneous action at a distance—like John's note yesterday supported—and we're just not allowed to say the phrase in polite company, or does it not? If the world does not, then so be it. But if it does, why should we try to so hard to delete the phrase from polite conversation? The main point I always wonder is how well you really have these issues worked out in your own head. If you care about misconceptions, then care about them—I say in this slightly grumbling state—and if you don't, then don't. But I have trouble understanding your mix of halfheartisms.

Charlie's Reply

> I think the world does not have instantaneous action at a distance, and it's important to find ways of speaking that do not encourage the frequent misconception that entanglement provides a means of faster than light communication. I am less interested, and think you should be less interested, in what seem to me to be hair-splitting arguments about "properties" or "changes in one's state of knowledge" that merely reflect different ways of talking about situations in which we entirely agree about the predictions of outcomes for any experiment. That seems a matter of aesthetics only, and the protections you find so reassuring I find merely annoying, like a parking brake, since they complicate my language and don't prevent me from making any wrong predictions, just what you would call wrong or fuzzy interpretations. I would say that if a way of thinking such as my own does not lead to wrong predictions, then it is not fuzzy in any serious way. Or to put in another way, the distinctions you would have me make because they seem so important to you are in fact less real than the wave function you don't want me to believe in.
>
> Talking with you first thing in the morning is way better than coffee.

16-05-02 *Exterior/Interior* (to W. K. Wootters)

Woottersism 6 : *Whitehead argued that a materialistic philosophy could never adequately account for mind. So in his view, the basic elements of the world are not material objects but are more like moments of awareness: they are more like events than persisting objects, and they always have both an interior and an exterior aspect. There is some similarity with Wheeler's view that the basic elements are quantum events, except that with Whitehead there is more of a balance between the interior and exterior aspects of each basic element. Wheeler thinks of quantum events merely as objective events, seen only from the outside, as it were. Whitehead says that at every level of being, there is always an interior view. At the higher levels, this interior view is what we call consciousness. At lower levels, it's not consciousness but it exists nonetheless.*

Of course I like the Wheelerian idea of taking quantum events as basic, and I like this question: Where does a quantum event reside?

There is also a similarity to Schopenhauer. He called the interior stuff the "will." Moreover, Schopenhauer also placed the will outside of space and time. But maybe that's where the similarity ends; I don't know enough about either philosopher to say. Let me place below two little paragraphs from my samizdat in which I had written about Schopenhauer. [See the note to Landahl, dated 10-07-01, titled "Replies on a Preskillian Meeting" and the note to Waskan, dated 07-08-01, titled "Kiki, James, and Dewey."] So I think there must be some significant overlap in what you and I are searching for.

But there are some differences:

> What I like about Whitehead and Malin is that they emphasize a commitment to consciousness as something central to existence and something that one has to have in mind even when talking about the most basic elements of existence.

though I'm not committedly against this. So, now thinking back on the long letter I wrote you, "It's a Wonderful Life" it dawns on me that you might have had trouble—at the very least!—accepting the part where I said something like "I eschew idealism in all forms."

I will try to come up to speed on Whitehead not too far in the future.

17-05-02 *Spekkens Summary* (to R. Pike)

Is this the sort of thing you're looking for?

> Robert Spekkens' research plan is to determine what sort of information-theoretic security can be achieved for two-party cryptographic tasks using quantum protocols. Two-party cryptographic tasks are those that are implemented between a pair of spatially separated parties that do not trust one another (like bit-commitment, coin-tossing, and remote gambling). These are some of the so-called "post-cold-war" applications of cryptography that may have commercial significance. Information-theoretic security is the security that can be achieved regardless of the technological capabilities of the adversary (including the capability to build a quantum computer). In other words, it is the security that can be guaranteed by virtue of the laws of physics alone. In addition to being of obvious foundational significance to cryptography, this research addresses the practical question of the extent to which quantum cryptographic protocols can take the place of classical protocols if the security of the latter are compromised (for instance, by quantum computers).

Spekkens is well-positioned to carry through with this research plan, since he has already demonstrated an ability to make original and significant contributions at the forefront of the field. In fact, some of his papers have defined the very field. The plan is to support Spekkens through a DIMACS postdoctoral fellowship—assuming he obtains it—which would pay for both his salary and benefits. The only burden to Lucent we foresee is the donation of some office space. He would most likely stay here through the tenure of the fellowship, which may be up to two years.

17-05-02 *No Nasty* (to T. Rudolph)

I won't be nasty; I enjoyed your note.

Your thinking has a lot of the flavor of the paper: A. Peres and W. H. Zurek, "Is Quantum Theory Universally Valid?," Am. J. Phys. **50**, 807 (1982). I'm not sure if you're aware of it. Asher once told me that he himself still likes the paper a lot, but Zurek basically disavows it now.

Rudolphism 3 : *what I'm trying to understand is the physics analogue of Turing's construction – what is it that I, a regular physicist, am doing in my interactions with the world and my construction of theories to explain those interactions?*

I like this question a lot, actually. Below is some of my own attempt to ask the same thing. In this regard, I suggest you read Richard Rorty's book, *Philosophy and Social Hope*. I think if I were to read it again, I would realize how much of an influence it's had on my own thoughts. In fact, my Anti-Växjö paper may be little more than a condensation of it.

I guess if you were to ask me now—in my present state of mind—I would say we are doing more than constructing theories. We are constructing the world (in part). See other note below. But beyond that, I don't know how to say more presently.

Keep thinking about your question! Don't listen to Spekkens.

By the way, I won't be around for essentially a month: Today, I leave for Texas until Thursday. Then the Saturday after that I'm off to Brisbane for a 3 weeks. I return to Bell Labs June 17. (I'm still looking for volunteers on the fence.)

Terry's Preply, "Musings on what the heck it is we're doing ..."

I'm trying to get a handle on what it is we (human) physicists actually take axiomatically (although implicitly) to be true before constructing our theories. From this, if Chris' many pages of papers are even an epsilon amount toward the truth, I should be able to get some glimmering about why quantum mechanics is the way it is – that is, if it really is only my way of describing my 'pushing on the world' to paraphrase Chris. [This latter statement is of course is NOT something that ALL physicists take to be true, so it can't be part of my list!]

So here's a brief synopsis of 3 things that I feel go, in some sense axiomatically, into any (rational) physicist's theories:

1. **Spacetime Invariance**

   We all believe, after some fashion, that an experiment can be replicated. Whenever we do so we implicitly neglect certain things that are not EXACTLY the same about the second experiment. The things we neglect generally vary from one situation to the next – we use our intuition about whether they're important or not and this can be dangerous. However, in all physics experiments, we presume that the fact the second experiment must take place at a different point in spacetime from the first (otherwise it WOULD be the first experiment) is not important with regards to its testing of our theory etc.

(Wild speculation aside: Since space/time translation invariance of a Lagrangian give, by Noether's theorem, conservation of momentum/energy, it would be interesting to conjecture that conservation of momentum/energy is a necessary consequence of a fundamental philosophical belief we have about the scientific method, and not a "truly objective" physical principle whatever the heck that is.)

2. **Universality of Physics**

   We often say blasé things along the lines of the universe is defined to be all we can observe. I think we all believe that physics, in principle, should be able to describe everything in the universe, that is, everything in the "natural" and not "super-natural" so to speak. If so then we must believe that it describes US as physical objects too. Anthropocentricity has failed us as an axiom too many times for anyone to rationally hold to it.

3. **Free Will**

   The whole study of science is meaningless without free will. I personally believe I have free will. By point 2, this means I must accept the possibility that other objects in the universe have free will – and since I assume you all claim that you do too I'll accept you as candidates for those other free will objects (you could be computers programmed to claim it, but not actually have it, for all I can really tell!)

What do I gain from all this? At the moment the only thing I can say is this. If I accept these three points, then I must conclude that any truly universal (complete?) physical theory we construct must be non-deterministic. Once there are two objects A, B in the universe that object A accepts have free will, then object A cannot rationally devise a deterministic theory that includes object B. So either we give up on Point 2 or we give up on 3, or we accept non-determinism as a inevitable consequence of performing physics as we currently know it. [Note that I am NOT saying the oft stated converse, which I think is completely pithy (and thus probably due to Penrose), namely that the free will we have will one day be found to be because of the uncertainty principle.]

Those are my thoughts in a nutshell. They have evolved from thinking a lot about the following:

A Physics Turing Machine

Turing was trying to capture the whole process of a mathematician's thinking. A mathematicians pen and paper were simply an extension of his mind Turing said. The symbols he drew came from a countable set he argued, and from arguments along philosophical line about the process of performing mathematical calculations came the Turing machine. Today we relegate this machine to the status of a Pentium. However what I'm trying to understand is the physics analogue of Turing's construction - what is it that I, a regular physicist, am doing in my interactions with the world and my construction of theories to explain those interactions? What sort of theories can object A devise about object A? [To call something which performs arbitrary unitary transforms a "universal quantum turing machine" is repulsive.]

So, what should I be adding or subtracting from 1–3 above?

And if you dont agree with something I've said don't be mean and nasty in your replies! Yes, I mean you too Fuchs.

17-05-02 *Dueling Banjos* (to W. K. Wootters)

Woottersism 7: *Well, it's true that there's a strong idealistic current running through me. Here's an interesting paragraph from Hartshorne about realism and idealism:*

> It appears, then, that the idealistic interpretation of reality as essentially relative to or consisting of mind, experience, awareness, that is, psychicalistic idealism, is entirely compatible with a realistic view of the independence of the particular object and the dependence of the particular subject, in each subject-object situation. It may also be urged that we need the word "realism" to refer to the mere thesis that every act of knowledge must be derivative from a known which is not derivative from that act. Thus the practice of contrasting "idealism" and "realism" as though they were contradictories, is of doubtful convenience. "Realistic idealism," or "realistic subjectivism," has a reasonable and consistent meaning.

Thanks for the Hartshorne quote. I had forgotten that I had sort of agreed with that a bit. Here's the way Martin Gardner put it in his essay "Why I Am Not a Solipsist":

> In this book I use the term "realism" in the broad sense of a belief in the reality of something (the nature of which we leave in limbo) that is behind the phaneron, and which generates the phaneron and its weird regularities. This something is independent of human minds in the sense that it existed before there were human minds, and would exist if the human race vanished. I am not here concerned with realism as a view opposed to idealism, or realism in the Platonic sense of a view opposed to nominalism or conceptualism. As I shall use the word it is clear that even Berkeley and Royce were realists. The term of contrast is not "idealism" but "subjectivism."

(The phaneron, by the way, was Peirce's term for "the world of our experience—the totality of all we see, hear, taste, touch, feel, and smell.")

Thus, in making the transition from the first paragraph to the first sentence of the second paragraph in the excerpt from my old letter below, I was making a non sequitur.

Let me ask you this about your "idealistic current." Does it run counter to what my first paragraph below says and what Gardner says above? I guess that's the main point for me.

I suppose if I were to start to label things, then this thing I was telling you about the other day—"the sexual interpretation of quantum mechanics (SIQM)"—would be a kind of dualistic theory. I said it metaphorically this way: When things bang together, something is created that is greater than the sum of the parts. Or again: When things—that's the materialistic aspect—bang together, something is created—that's the mentalistic aspect, for it is like an act of the will or a decision. But that's just a thought that's hitting me at 4:00 in the morning. (So trust it less than even the usual things that come out of my mouth.) I hadn't thought about it in this way before, and I'm not sure I want to continue to thinking about it this way. In general, I don't like dualisms. (Though even saying that is paradoxical; for I think I like "pluralisms" in the sense of James.) The excerpt from an old letter far below gives a slightly longer introduction to the idea of the SIQM.

Bill's Reply

I agree at least with the letter of Martin Gardiner's paragraph, because I don't think there's anything particularly special about *human minds.*

But I disagree with the spirit of your sentence:

> Yes there is certainly a kind of realism working in the back of my mind, if what you mean by "realism" is that one can imagine a world which never gives rise to man or sentience of any kind.

Let me explain. I think the sort of world envisioned by classical physics is in fact impossible. If we really understood what it takes to make a world exist *really*, and not just on paper, I think we would see that one needs the subject-object relation in order to hold things together. This is not to say that one needs dualism. In Whitehead's system, everything is both subject and object, depending on the point of view. There is no dualistic separation into two kinds of entity. But everything is related to something else as subject, and everything is also related to something else as object. In classical physics, there is no such relation. As Schrödinger points out, from the very beginning we eliminate the very notion of an experiencing subject. I agree with Schrödinger that this is too extreme an abstraction. In making this abstraction, we have removed something essential from our view of the world.

I know you will say that your realism is not the same as that of classical physics. But in what way, ultimately, is it not the same, other than by the absence of determinism? I think indeterminism is crucial, but I think one needs the further step of restoring the subject-object relation as fundamental.

I've heard some people argue as follows. "There are many conceivable physical worlds, because one can imagine all sorts of different physical laws, expressed as mathematical equations. Now, the mathematics certainly exists on its own. It doesn't need physical realization. But since the mathematics exists, and since the mathematical laws are the essence of the physical world that they would describe, then all these physical worlds must *actually exist.* They exist because the mathematics that describes them exists."

One is led to this conclusion (which I think is a wrong conclusion) because there really is hardly any difference between (i) an actually existing material world that follows definite laws, and (ii) a mathematical description of such a world. There's nothing important in the "actually existing" world that's not already present in its description. What difference does "existence" make in this case? What does existence mean in this case?

I would like to think that my view avoids the line of reasoning that takes the mathematical description as the essence of the world. As John Wheeler says, a set of mathematical laws will not "fly" by itself. I think the necessary added ingredient is something like *experience.* And that's what I find in Whitehead's view.

17-05-02 *Slide Show* (to N. D. Mermin & C. H. Bennett)

Boy you got me into a stink, didn't you, by getting me into that word debate! If you hadn't done that, I might have had some time to answer your other questions this week.

Below for your continued amusement, I'll include a side conversation I had with Charlie. Maybe this debate is at least edifying in some ways. Charlie sees me as hair-splitting; I see Charlie as being half-hearted and inconsistent.

I just read these words in a Martin Gardner article:

A third aspect of aesthetic theory that bores me even more are all those tiresome disputes, in book after book, about whether aesthetic values are subjective or objective. Here the situation is not quite the same as that of truth. In previous chapters I have argued that the least confusing way to talk about truth is to assume that the world and its structure are not mind-dependent. But beauty, so far as humanity is concerned (we will not consider what beauty may mean to birds or apes, to creatures on other planets, or to gods), obviously requires a human mind. Where is the red of an apple? As I have said, it is in the mind if by red we mean the sensation of red. It is on the apple if by red we mean the structure of a surface that reflects a pattern of visible light which causes a mental sensation of red.

I see no difference between this antique quibble and the question of whether beauty (however defined) is a property of an art object or a sensation in a brain. If by beauty you mean the pleasure aroused by a beautiful object, of course it is subjective. If by beauty you mean the structure of an object capable of arousing aesthetic pleasure, then the beauty is a part of the object. Or you may prefer a third approach and ground beauty in the combined dynamic structure produced by the interaction of an object and a mind. It is all such a weary waste of words. The last approach is the one taken by John Dewey in his influential book Art as Experience. Although I found fault with Dewey's attempt to redefine truth in pragmatic terms, I find his approach to aesthetics (essentially the same, by the way, as Aristotle's) a sensible way of speaking. Again, it is not a question of Dewey being right or wrong. It is a question of the most useful way to talk about aesthetic values.

And I find that I agree with most every word of this (except for the part about the Jamesian-Deweyan theory of truth). But the issue at stake with quantum mechanics goes much deeper than this, and it annoys me to no end that our friend Charlie lumps me in with the art critics.

If you have any words of wisdom that could take a little fire out of our relations, I'd love to hear them. Maybe I'll CC this note to Charlie too.

By the way, this was not intended to be the subject of this note. I wrote instead to tell you that I have now posted the slides from my talk at your party on my website. Maybe you'll enjoy seeing the second half, if not hearing it.

Charlie,

Bennettism 17: *Surely you must have something more sensible to say than some of us who have spoken.*

It would have been more neutral to say, "than those of us who have spoken." If I were trying to read between the lines, I might be tempted to write a note just like this one. But I'll refrain from reading between the lines.

Chris

Dear Chris,

Sorry. Point well taken. You are a perpetual stimulus to me, if not always in the ways you hope, and I would miss it terribly if you stopped. I should be more grateful. Like the other day, when John was discussing your automotive metaphor, you inspired me to think that all the cautions you would have us take against quantum misconceptions are—for me, if I did them—like driving a car with the parking brake on all the time.

-CHB

Charlie,

Bennettism 18: *Like the other day, when John was discussing your automotive metaphor, you inspired me to think that all the cautions you would have us take against quantum misconceptions are—for me, if I did them—like driving a car with the parking brake on all the time.*

Then it seems to me you should at least be consistent in your behavior. I interpreted your talk Sunday as a genuine concern for the perceptions the masses hold about some of our favorite quantum effects. Do you have a concern, or do you not? Is it that the world really has some kind of instantaneous action at a distance—like John's note yesterday supported—and we're just not allowed to say the phrase in polite company, or does it not? If the world does not, then so be it. But if it does, why should we try to so hard to delete the phrase from polite conversation? The main point I always wonder is how well you really have these issues worked out in your own head. If you care about misconceptions, then care about them—I say in this slightly grumbling state—and if you don't, then don't. But I have trouble understanding your mix of halfheartisms.

Chris

Dear Chris,

I think the world does not have instantaneous action at a distance, and it's important to find ways of speaking that do not encourage the frequent misconception that entanglement provides a means of faster than light communication. I am less interested, and think you should be less interested, in what seem to me to be hair-splitting arguments about "properties" or "changes in one's state of knowledge" that merely reflect different ways of talking about situations in which we entirely agree about the predictions of outcomes for any experiment. That seems a matter of aesthetics only, and the protections you find so reassuring I find merely annoying, like a parking brake, since they complicate my language and don't prevent me from making any wrong predictions, just what you would call wrong or fuzzy interpretations. I would say that if a way of thinking such as my own does not lead to wrong predictions, then it is not fuzzy in any serious way. Or to put in another way, the distinctions you would have me make because they seem so important to you are in fact less real than the wave function you don't want me to believe in.

Talking with you first thing in the morning is way better than coffee.

-CHB

17-05-02 *More Balking* (to R. Schack)

Schackcosm 67:

[CAF wrote:]

> Rüdiger said, "I am still convinced, despite your severe scolding, that a priori, without looking at the consequences, strong and weak coherence are similarly compelling axioms."

I don't see how you can make this distinction. It seems to me, axioms are only compelling or not compelling insofar as their consequences.

Then why would there be a difference between postulating the Kolmogorov axioms and postulating coherence? Why would we bother with the Dutch book arguments? Why do you bother looking for an information-theoretical reason for the quantum axioms?

It seems to me that the compelling reason to accept the axiom of (ordinary) coherence is that "whatever the consequences are, I certainly do not want to violate coherence, because I don't want to hand over money". It seems to me that a discussion of whether strong coherence is a compelling axiom has to be carried out at this level.

But that is not what is at issue. Strong coherence and regular coherence are *two different theories*, and a theory's worth can only be assessed by looking at the whole thing.

It seems to me a very simple issue. In the theory of strong coherence, there is a kind of normative behavior for all agents that regular coherence does not have. The theory says, in effect, "Thou shalt not tell a lie." If an agent writes down $p = 1$, then in the rigid world of strong coherence, I can trust his statement to be a reflection of his true inner belief. The world of regular coherence does not have that.

You can say that one theory is just more of the same with respect to the old theory. But that is to only look at one aspect of the problem. In another aspect of it, the two theories become qualitatively different.

There is a sense in which my mother-in-law is just a faster version of Kiki at times. That is, she's just more of the same when it comes to cooking, artistry, and a couple of other aspects. But I fell in love with Kiki, not my mother-in-law. This is because when I move past a few isolated features of my mother-in-law, I discover she is qualitatively different from the woman I love.

How can you reject that as a reasonable line of thought?

If you thought at the outset that probabilities could be proper properties of things—like Shimony does—then you might indeed accept strong coherence. For strong coherence grounds the very meaning of $p = 1$. A "$p = 1$"–statement is a TRUE statement. But if you start out from a strongly subjectivist stance on probability, then the only leg you have to stand on is the kind of argument you and Carl give: It is just more of the same of regular coherence; it is just slightly more sensible or cautious betting behavior. But I counter that by saying, carried through consistently and without exception, strong coherence makes it nigh on impossible to embed one's particular bets about particular events in the larger framework of all bets and all events.

It is an issue worthy of debate, I agree, whether my reason is a decent reason to reject strong coherence. (I think it is, clearly.) But it's not fair to say in distinguishing horses and zebras, we should all close our eyes and focus on tactile differences, eschewing all the visual information around us.

17-05-02 *The Divinity that Breathes Life* (to W. K. Wootters)

Woottersism 8: *I know you will say that your realism is not the same as that of classical physics. But in what way, ultimately, is it not the same, other than by the absence of determinism? I think indeterminism is crucial, but I think one needs the further step of restoring the subject-object relation as fundamental.*

To the extent that I could write something like the passages below, I don't think so. [See note to Renes, dated 20 November 2001.] Or, say, to the extent that I find myself liking this little quote, "I forbade any simulacrum in the temples because the divinity that breathes life into nature cannot be represented," I don't think so. I think there's more than simple indeterminism in my forming view; there is something lower level than determinism and indeterminism both.

But what I am not settled on is whether the "divinity that breathes life" is the subject-object relation. I will think hard about your point, and I will think harder about the impression my writings give.

18-05-02 *QM as QI, cont'd* (to A. Peres)

Thanks for the support. As I wrote Lou Hand, an accelerator physicist at Cornell, who wrote me something nice after my talk there, "My fragile self-confidence needs that sort of thing every once in a while."

Kiki, Emma, Katie, and I arrived in Texas late last night. We will visit my mother until Wednesday morning. It is her first time to meet Katie.

I hope Aviva and the rest of the family are doing well.

Asher's Preply

I read a few more pages of QM as QI, and I really enjoy it very much. [See C. A. Fuchs, `quant-ph/0205039`.] A few remarks:

- [21] Rosen also told me that I was a solipsist (about 40 years ago).
- Bureau International des Poids et Mesures is in Sèvres (not far from Paris), not in Paris itself.
- [27] is a bad reference for POVM. Their explanations and formulas are plainly wrong. I wrote it to them and they told me this will be corrected in a future edition.
- page 9: I loved "one can immediately write down a new state for the distant system". See the circular I just sent to Bennett et al.
- I loved footnotes 14–16.

More later.

20-05-02 *Denying Free Will* (to T. Rudolph)

Rudolphism 4: *This concept of free will bugs the hell out of me – in a sense when you say "I think we're constructing the world (in part)" I think any physicist would have to agree with you (in part) or else they'd end up denying free will.*

I know a lot of physicists who deny free will; Charlie Bennett and John Preskill come to mind.

Rudolphism 5: *Do you think there are more than the three "implicit assumptions" I mentioned?*

Yes and No.

20-05-02 *Dizzy in Texas* (to J. A. Smolin)

John Smolinism 3: *Now you guys are trying too hard not to think like Chris. Nothing is magic about the "instantaneous" first stage of teleportation, only knowledge is being changed. If I have two boxes, one with a ball in it and one without and send one far away, when I open the remaining one I instantly know if the ball is in the other one. Big deal. When I measure something, I find something out. Wow!*

I shouldn't let your note encourage me. (The sober side of myself won't.) But there is a way to make the sum content of all quantum measurements look exactly like your analogy above, even in a formal way. The mathematics may interest you, even if not the philosophy. You can find it in Sections 4.2 and 6.1 of the paper I put on `quant-ph` last week (`quant-ph/0205039`). Or you can see it sketched quickly in the Merminfest talk posted at my website.

The trick is to represent the quantum state as a probability distribution with respect to a fixed, fiducial informationally complete POVM. That is, one imagines a "standard quantum measurement device" sitting in the National Bureau of Standards beside the standard meter and the standard kilogram, and the quantum state captures nothing more than one's judgment for how the device will react if one were to throw one's quantum system into it. With that picture in mind, all a regular, everyday measurement does is update one's judgment concerning the outcomes of the standard measurement. The twist that comes with quantum mechanics (over regular probability theory) is that the update rule is Bayes' rule plus a little more ... it's not just Bayes' rule full stop.

24-05-02 *Snail Mail* (to S. L. Braunstein)

Tell me about some good places to go in Brisbane. Carl, Rüdiger, and I are converging there to work on this Rev. Mod. Phys. paper on Bayesianism in QM we're trying to write up. I have a small fear that the details of the paper may strain our relationship to the point of divorce, but let us hope for the best.

25-05-02 *Fun with Feyerabend* (to M. A. Nielsen)

I'm just putting some notes into my computer before I set off for travel again. At your recommendation, I've paid a little more attention to Paul Feyerabend. The quotations below summarize a little of what I see as likable and usable in his philosophy.

See you tomorrow.

From: P. Feyerabend, *Conquest of Abundance: A Tale of Abstraction versus the Richness of Being*, edited by B. Terpstra (U. Chicago Press, Chicago, 1999).

> *Humans as Sculptors of Reality.* According to the first assumption, our ways of thinking and speaking are products of idiosyncratic historical developments. Common sense and science both conceal this situation. For example, they say (second assumption) that atoms existed long before they were found. This explains why the projection received a response, but overlooks that vastly different projections did not remain unanswered.
>
> A better way of telling the story is the following. Scientists, being equipped with a complex organism and embedded in constantly changing physical and social surroundings, used ideas and actions (and, much later, equipment up to and including industrial

complexes such as CERN) to *manufacture*, first, metaphysical atoms, then, crude physical atoms, and, finally, complex systems of elementary particles out of a material that did not contain these elements but could be shaped into them. Scientists, according to this account, are sculptors of reality—but sculptors in a special sense. They not merely *act causally* upon the world (though they do that, too, and they have to if they want to "discover" new entities); they also *create semantic conditions* engendering strong inferences from known effects to novel projections and, conversely, from the projections to testable effects. We have here the same dichotomy of descriptions which Bohr introduced in his analysis of the case of Einstein, Podolsky and Rosen. Every individual, group, and culture tries to arrive at an equilibrium between the entities it posits and leading beliefs, needs, expectations, and ways of arguing. The separability assumption arises in special cases (traditions, cultures); it is not a condition (to be) satisfied by all, and it certainly is not a sound basis for epistemology. Altogether, the dichotomy subjective/objective and the corresponding dichotomy between descriptions and constructions are much too naive to guide our ideas about the nature and the implications of knowledge claims.

I do not assert that any combined causal-semantic action will lead to a well-articulated and livable world. The material humans (and, for that matter, also dogs and monkeys) face must be approached in the right way. It *offers resistance*; some constructions (some incipient cultures—cargo cults, for example) find no point of attack in it and simply collapse. On the other hand, *this material is more pliable than is commonly assumed*. Molding it in one way (history of technology leading up to a technologically streamlined environment and large research cities such as CERN), we get elementary particles; proceeding in another, we get a nature that is alive and full of Gods. Even the "discovery" of America, which I used to support the separability assumption, allowed some leeway, as is shown by Edmondo O'Gorman's fascinating study, *The Invention of America*. Science certainly is not the only source of reliable ontological information.

It is important to read these statements in the right way. They are not the sketch of a new theory of knowledge which explains the relation between humans and the world and provides a philosophical grounding for whatever discoveries are being made. Taking the historical character of knowledge seriously means rejecting any such attempt. We can describe the results we have obtained (though the description will always be fatally incomplete), we can comment on some similarities and differences that have come to our attention, and we can even try to explain what we found in the course of a particular approach "from the inside," i.e., using the practical and conceptual means provided by the approach (the theory of evolution, evolutionary epistemology, and modern cosmology belong in this category). We can tell many interesting *stories*. We cannot explain, however, how the chosen approach is related to the world and why it is successful, in terms of the world. This would mean knowing the results of all possible approaches or, what amounts to the same, we would know the history of the world before the world has come to an end.

And yet we cannot do without scientific know-how. Our world has been transformed by the material, spiritual, and intellectual impact of science and science-based technologies. Its reaction to the transformation (and a strange reaction it is!) is that we are stuck in a scientific environment. We need scientists, engineers, scientifically inclined philosophers, sociologists, etc., to deal with the consequences. My point is that these consequences are not grounded in an "objective" nature, but come from a complicated interplay between an unknown and relatively pliable material and researchers who affect and are affected and changed by the material which, after all, is the material from which they have been shaped. It is not therefore easier to remove the results. The "subjective" side of knowledge, being inextricably intertwined with its material manifestations, cannot be just blown away. Far from merely stating what is already there, it created conditions of existence, a world corresponding to these conditions and a life that is adapted to this world; all three now support or "establish" the conjectures that led to them. Still, a look at history shows that this world is not a static world populated by thinking (and publishing) ants who, crawling all over its crevices, gradually discover its features without affecting them in anyway. It is a dynamical and multifaceted Being which influences and reflects the activity of its explorers. It was once full of Gods; it then became a drab material world; and it can be changed again, if its inhabitants have the determination, the intelligence, and the heart to take the necessary steps.

and

> Pauli's views have much in common with the general picture that emerged from Aristotle's principle. In this picture we start with a world (which I shall call the primal world, or Being) which behaves in its own way and not necessarily in accordance with any one of the laws that have been discovered by scientists. (Here we still have an element of realism.) Humans are part of the primal world, not detached aliens, and they are subjected to its whims: Being can send scientists on a wild-goose chase—for centuries. On the other hand, it permits partial independence ... and it provides some of those acting independently (not all of them!) with *manifest worlds* they can expand, explore, and survive in (manifest worlds are in many respects like ecological niches). Inhabitants of a particular manifest world often identify it with Being. They thereby turn local problems into cosmic disasters. But the manifest worlds themselves demonstrate their fragmentary character; they harbor events which should not be there and which are classified away with some embarrassment (example: the separation of the arts and the sciences). The transition from one manifest world to another cannot be described in either except by excising large regions originally thought to be real—a good case for applying the notion of complementarity. Bell's request that a fundamental theory should not contain any reference to observation is satisfied, but trivially so. Being as it is, independently of any kind of approach, can never be known, which means that really fundamental theories don't exist.

25-05-02 *Becoming William James* (to G. L. Comer)

A thought struck me today as I was flying from Newark to LA reading Lloyd Morris's book *William James: The Message of a Modern Mind.* It is this: I think there's nothing I presently want more than to become a modern (quantum) incarnation of William James.

Let me leave you with a lovely quote from the end of Chapter 2.

> He conceived the individual's life, and all social progress, as a form of perpetual experiment. But he did not preach reckless faith. The "will to believe" for which he argued is best defined as courage weighted with responsibility. Contingency signifies that no precaution can absolutely eliminate all hazard of shipwreck. The individual must take everything into account that may tell against his success, and make every possible provision to minimize disaster in the event of his failure. But having done so,

he must act. And in this circumstance, James preached the right of the individual to indulge his personal faith at his personal risk. The part of wisdom would always be to believe what is in the line of one's needs, for only by such belief is the need fulfilled. Over a wide area of existence, possibilities and not finished facts are the realities with which we have actively to deal. So James argued, and pointed out that "as the essence of courage is to stake one's life on a possibility, so the essence of faith is to believe that the possibility exists." But his doctrine subordinated faith to action, for the real utility of faith is to make action genuinely dynamic. "These, then, are my last words to you," he told a group of Harvard students. "Be not afraid of life. Believe that life *is* worth living, and your belief will help create the fact."

Now I'm off to Brisbane. For my own self, I believe it is worth becoming William James.

27-05-02 *Late, but Never Too Late?* (to K. Svozil)

I hope you will excuse my horribly late reply to your letter of 4/19/02. I have just gone through a month of hell of traveling almost constantly, and I have gotten pathetically behind in all my correspondence. (Just check with Johann Summhammer there; he is also in my queue! I'm hoping to get to him later in the day, if not tomorrow.)

Thanks for the continued interest in having me around for the quantum structures meeting. Regrettably, I think I'm going to have to bow out of the possibility of coming. Presently I'm in Australia, separated from my wife and children for a month, and this trip is making me realize that I shouldn't take on any further travel than the stuff I'm already committed to for the summer. It's a shame really, because I am getting ever more involved in IQSA kinds of ideas, and it would be a great opportunity for me to learn a lot more about what is already "out there" mathematically … just waiting for me to plaster over with some words of Copenhagenish flavor. Beyond that though, I would love to have a chance to express some of my point of view to that audience. I think my talk at the quantum structures session at the AMS meeting in Atlanta two months ago or so went exceedingly well in that regard, and I found myself really enjoying conversations with Dave Foulis, for instance, afterwards.

I finally had a chance to read your paper "What Could Be More Practical than a Good Interpretation?," by the way. There are certainly large parts of it I agree with—I don't know if that will shock you or not. However, there was more to my paper with Peres than just the title! I do get a little shocked when I find people reading the title of the paper as its sum content. Here's the way I put it to Philippe Grangier a few months ago:

> **Grangierisme 9:** *By the way I also disagree with your point of view that "Quantum Theory Needs No 'Interpretation'," Phys. Today* **53***(3), 70 (2000). The fact that a physical theory ALWAYS needs an interpretation is in my opinion a central difference between physics and mathematics.*
>
> You won't find a disagreement with me here. The title and closing sentence of that paper were meant to be tongue-in-cheek plays on something Rudolf Peierls once said: "The Copenhagen interpretation *is* quantum mechanics." The whole paper is very definitely about an interpretation, and why one does not need to go any further than it to make sense of quantum mechanics as it stands. My paper `quant-ph/0106166` and the large (more personal) collection `quant-ph/105039` is about going the next step, i.e., what to do once we have established the belief that quantum states are states of knowledge.

When we do finally dig up an ontology underneath quantum mechanics, I'm quite sure it will be an interesting one!

And here was the way I put it to Paul Benioff a year before that:

> **Benioffism 1:** *To me that is an interpretation of QM. Interpretations are what give otherwise empty theories their meaning.*
>
> You're quite right about that. What Asher and I wrote about is indeed a kind of interpretation of the quantum mechanical formalism. The title and the ending words of the article were more for attention-getting than anything else. Also, though, the words were meant to be a small slap in the face to some of the extremes people have gone to (like Everett worlds, Bohm trajectories, and Ghirardi-Rimini-Weber stochastic collapses) just to hold on to a philosophic view that came around long before quantum mechanics was ever heard of. (Talk about people being set in their ways!)

I think the best renditions of my present views can be found in my new papers, `quant-ph/0205039` and `quant-ph/0204146`. I certainly would appreciate any comments you have on those. Especially I would love for you to articulate the weak points you see in the ideas. It is an evolving point of view. And what I want out of the effort is just what you suggest in your paper. I want new calculations, new effects to go search for, new mathematics, and *really* a new world view in total.

Will by chance you be at either the Oviedo, Spain meeting or the QCMC meeting at MIT this summer? Maybe we could talk then?

28-05-02 *Anti-Anti-Växjination* (to J. Summhammer)

Thank you for the beautiful long note of 4/29/02. Please allow to apologize for taking so long to reply. I've been almost completely incapacitated in my email efforts for a few weeks now by travels, family vacations, company duties, etc. Certainly I appreciate the efforts you take to read my things, and definitely your questions and comments are the best products of that!

Let me comment on a couple of your points.

Summhammerism 7: *But this 'core of the theory' is always tentative. New information, a wider frame of thought, can change it. And yet it is hard to deny that it captures something about that which has been observed. It is like clouds in the sky. For some time they do look like an animal, or the face of a witch, and anyone with eyes will agree, and a few minutes later they are gone. The immutable part here, as well as in physical theories, does seem to be the rules of thought. By the term 'core of the theory' you seem to want to say that they contain a timeless truth. But I have difficulties believing that a physical theory could ever achieve the degree of timeless truth as exhibited, for instance, by mathematical theorems, which are particular expressions of the rules of thought.*

Not quite. I would not say that this thing "the core of a theory" contains a timeless truth. Indeed I tried to be careful to squash that idea when I wrote:

> **Woottersism 9:** *But you obviously also want to say that our theories tell us* something *about reality, even if they are not descriptions of reality.*

I hope you can glean from all the above that I do indeed believe our theories tell us something about reality. But that something is much like what the elephant tells us about reality. Its presence tells us something about the accumulated selective pressures that have arisen up to the present date. A theory to some extent is a statement of history. It is also a statement of our limitations with respect to all the pressures yet seen, or—more carefully—a statement of our limitations with respect to our imaginations for classifying all that we've yet seen. (I for instance, cannot jump off the leaning tower of Pisa unprotected and hope to live; you, for instance, cannot get into your car and hope to push on the accelerator until you are traveling beyond the speed of light.) Finally, to the extent that we the theory users are part of nature, the theory also tells us something about nature in that way.

Thus even the core of the theory is as historical and contingent as the elephant. And just as the elephant could disappear from all historical record, so could the theory and, with it, its core.

Summhammerism 8: *I did not understand what you meant with the last paragraph on page 16 ('However, we would never haven gotten to this stage ....') Sounds as if you could envision that we could set the laws according to our wishes. Reminded me of an old view of evolution: The will (the basic entity) wanted a hand for this particular species, and so it came about ... Sometimes I like this idea, because it permits to look for other patterns and correlations in the history of life than 'mindless' Darwinism.*

Nor did I understand it really. But, yes, I suppose I'm imagining that we might have some control in shaping the "laws" of physics. However—and this is important—that control should be no stronger than the kind of control we might have for shaping a species with genetic engineering. The species has to be able to survive on its own after being produced in a trial. If it can't survive on its own, then one would be loath to call the monstrosity so produced a species to begin with. And so too with what I imagine for this malleable universe.

Did you see my larger paper `quant-ph/0205039`? There, I tried to incorporate some of these ideas in a more technical way. Also, I've now got a lot of supporting material posted at my website (link below).

Johann's Preply, "Anti Växjö"

Just read your Anti-Växjö paper (`quant-ph/0204146`). Despite its 17 pages, the assurance that it is free of equations was incentive to download it.

I like your distinction of the two aspects of reality (p.15): Its rawest form as the 'surprise of the sensations', and the 'core of the theory'. I would also see it this way.

The 'core of the theory' is an abstraction arrived at by applying rules of thought (a kind of immutable Platonic reality). But this 'core of the theory' is always tentative. New information, a wider frame of thought, can change it. And yet it is hard to deny that it captures something about that which has been observed. It is like clouds in the sky. For some time they do look like an animal, or the face of a witch, and anyone with eyes will agree, and a few minutes later they are gone. The immutable part here, as well as in physical theories, does seem to be the rules of thought. By the term 'core of the theory' you seem to want to say that they contain a timeless truth. But I have difficulties believing that a physical theory could ever achieve the degree of timeless truth as exhibited, for instance, by mathematical theorems, which are particular expressions of the rules of thought.

In physics, we are still haunted by the remnants of a naive picture of reality. We have thrown out the classical view, but still tend to think that our theories retain a kind of one-to-one link with 'the world as such'. We do no longer say we describe how something is, but that we describe what we know about that something. Thereby we still stipulate the existence of that something. But I think any statement about distinctions in the world, that should exist independent of us, is premature. We can say "The world is," but we cannot say "The world is such and such" and mean it to apply independent of our categorizations.

Take gravity, for instance. Invented by great minds, to be sure. But a statement that the universe contains masses which interact by gravity, better expressed as curvatures of a manifold in which all the masses are supposed to be located, is so obviously a human way of categorization of observations, that any alien would coil up in laughter, if we wanted to sell this as a deep truth (my human way of categorizing his/her/its reaction, further, that aliens would be individual lumps like us, further, that they would be interested in science at all — a consequence of that particular brain disease, which leads to an extreme slow down between observing and acting and manifests itself as language, etc.).

The human categorizations leading to gravity:

- spatial representation of sensory input (3-space as our mode of representation, the concept of distance as an unquestioned primitive)
- the separation of the sensory input into distinct 'things' according to some invariance principles
- mass (the 'cause' of muscular strain when lifting and moving around an everyday 'thing')
- force (the muscular strain itself)

and probably some more.

Nevertheless I grant the following: We will never get out of our categorizations. And as these are ultimately not our own inventions, the fact that *these* categorizations lead to *these* laws may — in combination — be seen as containing a higher 'truth'.

I did not understand what you meant with the last paragraph on page 16 ('However, we would never have gotten to this stage ....') Sounds as if you could envision that we could set the laws according to our wishes. Reminded me of an old view of evolution: The will (the basic entity) wanted a hand for this particular species, and so it came about ... Sometimes I like this idea, because it permits to look for other patterns and correlations in the history of life than 'mindless' Darwinism.

28-05-02 *Little Toes and a World of Experience* (to W. K. Wootters)

I'm finally down in Australia, with a little time to think. I'll be down here for three weeks. Kiki and the kids are in Munich, visiting Kiki's mom and dad.

If you don't mind, I'd like to ask you a couple of questions about your last letter.

Woottersism 10: *I think the sort of world envisioned by classical physics is in fact impossible. If we really understood what it takes to make a world exist* really*, and not just on paper, I think we would see that one needs the subject-object relation in order to hold things together. This is not to say that one needs dualism. In Whitehead's system, everything is both subject and object, depending* *on the point of view. There is no dualistic separation into two kinds of entity. But everything is related to something else as subject, and everything is also related to something else as object. In classical physics, there is no such relation. As Schrödinger points out, from the very beginning we eliminate the very notion of an experiencing subject. I agree with Schrödinger that this is too extreme an abstraction. In making this abstraction, we have removed something essential from our view of the world.*

I suppose I too have a gut feeling that your first sentence above is on the right track, but I wonder if you have an actual argument in mind for supporting the case? Also let me ask you this. Would you say the same thing about a world governed by quantum mechanics? Or do you think the quantum world differs from the classical world in this respect? Maybe, in a refinement of this, let me ask the same thing not about the quantum world in general, but the quantum world in the vision of the many-worlders, say David Deutsch and Charlie Bennett. The way I understand what they envision for quantum mechanics, it has never struck me as so very different from what I would call classical physics. (It is just now that the world as a whole has this thing called a "state" and it is it that goes along according to some mathematically precise law.)

Woottersism 11: *I would like to think that my view avoids the line of reasoning that takes the mathematical description as the essence of the world. As John Wheeler says, a set of mathematical laws will not "fly" by itself. I think the necessary added ingredient is something like* experience*. And that's what I find in Whitehead's view.*

Would you flesh out this thing you call the "subject-object relation" a little more? What do you mean by it? Let us focus on a simpler system than one usually thinks of as a sentient being. Say a rock or the little toe on my right foot. When we think of these systems in their capacities as subjects, rather than objects, what is it that defines those capacities? What are their characteristic traits?

When I think of a person as a subject, I think of him carrying around sets of probability distributions for this and that. That is, I think of a subject as something that can carry beliefs. However, I guess, I have a hard time thinking of a rock or my toe as carrying around beliefs. I also think of a subject as something that can play an active role in shaping other parts of the world because of those beliefs.

However, maybe you mean something completely different when you're thinking of a rock as a subject.

Beyond that, let me ask about this word "experience." What do you mean by that? I hope to try out one of Whitehead's shorter books soon but I haven't gotten to that yet. However, I did read another small book on James on my flight over and it got me into some territory that I haven't yet seen of his. Namely, his stuff from *A Pluralistic Universe* and *Radical Empiricism*, neither of which I've read yet. The book I read was *William James: The Message of a Modern Mind*, by Lloyd Morris. It's only 91 pages, and I found it an engaging little thing (at least for giving me a quick overview of all of James's views, even if not the arguments he used for getting there). Anyway, Chapter 5 was titled "A World of Pure Experience" and what he described there seemed to have some overlap with what you expressed in your last note. Morris wrote:

> "My thesis," James declared, "is that if we start with the supposition that there is only one primal stuff or material in the world, a stuff of which everything is composed, and if we call that stuff 'pure experience,' then knowing can easily be explained as a particular sort of relation towards one another into which portions of pure experience may enter.

> The relation itself is a part of pure experience; one of its terms becomes the subject or bearer of the knowledge, the 'knower,' the other becomes the object known."
>
> This doctrine is essentially monistic. But it is radically unlike the monistic doctrines of either idealism or materialism, which respectively affirm that mind and matter are the ultimate substance of reality. Pure experience is neither mind nor matter, but is the ground of both. In itself it is, as James asserted, neutral.

Morris also said that this had some feed-in to Whitehead's later thought, but he didn't elaborate. So I'm guessing there will be some overlap between this and what you're thinking about. But still I'm having trouble understanding what all this might mean, especially since I'm having trouble envisioning the mental life of my toe.

Now the other day I said the idea of a random outcome in a quantum measurement might be viewed as having a mentalistic aspect. From the outside, it looks like a random occurrence; however, from the inside one might think it looks like a "decision." But right now, I'm wondering what even I really mean by this.

Thinking of you from way down under.

28-05-02 *Australiocentrism* (to M. J. Donald)

Thanks a million for the letter of 4/27/02. Please let me apologize for not replying before now. I'm about a month behind in my email due to excessive travels, company business, etc. Just yesterday I arrived in Australia for a three-week stay without the family. So I'm hoping to finally get caught up a little!

Anyway, you flatter me by reading my drivel and taking the time to comment. But I wish you didn't think the universe is doing nothing.

One question:

Donaldism 1 : *I'm an idealist because in the course of making that explanation I'm prepared to throw away any ontological presuppositions.*

How do you define idealism? And how would you contrast your flavor of idealism with this little thing Martin Gardner says in his essay, "Why I Am Not a Solipsist":

> In this book I use the term "realism" in the broad sense of a belief in the reality of something (the nature of which we leave in limbo) that is behind the phaneron, and which generates the phaneron and its weird regularities. This something is independent of human minds in the sense that it existed before there were human minds, and would exist if the human race vanished. I am not here concerned with realism as a view opposed to idealism, or realism in the Platonic sense of a view opposed to nominalism or conceptualism. As I shall use the word it is clear that even Berkeley and Royce were realists. The term of contrast is not "idealism" but "subjectivism."

(The phaneron, by the way, was Peirce's term for "the world of our experience—the totality of all we see, hear, taste, touch, feel, and smell.")

And how would you contrast it to this thing Charles Hartshorne says:

> It appears, then, that the idealistic interpretation of reality as essentially relative to or consisting of mind, experience, awareness, that is, psychicalistic idealism, is entirely compatible with a realistic view of the independence of the particular object and the dependence of the particular subject, in each subject-object situation. It may also be

urged that we need the word "realism" to refer to the mere thesis that every act of knowledge must be derivative from a known which is not derivative from that act. Thus the practice of contrasting "idealism" and "realism" as though they were contradictories, is of doubtful convenience. "Realistic idealism," or "realistic subjectivism," has a reasonable and consistent meaning.

28-05-02 *A Comment on One of Your Comments* (to C. M. Caves)

Cavesism 71 :

> [CAF wrote:] But there's really more to the story. They can always come to agreement, indeed—regardless of how disparate their initial opinions—if they are willing to make an essentially infinite expenditure toward laboratory technique. That is to say, the only thing that will give assured agreement in all cases is a set of Kraus operators all of rank-one. Jacobs and I called those infinite strength measurements: the idea being that they are hard to actually do. In more real-world measurements, where the operators are never really rank-one, coming to final agreement will generally require some initial agreement. Whence the point in my August 7 letter to Mermin.

My puzzlement here is related to that above. How can it be that rank-one's lead to "assured agreement" when we can never be sure what measurement was made nor what rule to use for the post-measurement state?

You're getting off track with this in the same way that you were yesterday in our walk near the lake. The issue is solely this in all issues of "coming to agreement." When can some amount of previous agreement between two agents lead to an increased amount of agreement after an act of consulting the world?

In the view I'm trying to promote, a measurement and a state-change rule (i.e., a POVM and an associated conditional quantum operation) is a subjective judgment. When you say something like "we can never be sure what a measurement was," in my way of thinking it is exactly like saying "we can never be sure what the true quantum state was." To the extent that the latter is a non sequitur for me, your phrase is a non sequitur too. When two experimentalists walk into the same laboratory, one carries in with him a subjective judgment about the POVM and quantum operation of the measurement device, and the other may carry in a completely different conception. But both of them know what they themselves think. For neither of them is there a "true" quantum measurement working in the background.

Suppose they lay their beliefs on the table. The question is this, what must the characteristics of those beliefs be so that, upon the completion of a measurement, they will ascribe the same quantum state to the post-measurement system regardless of what each initially said about the system to be measured? The only claim is that it is sufficient that their separate POVM + operations be 1) identical *and* 2) consist of rank-1 elements.

I found myself laughing last night, by the way. After all your preaching to me about the ineffectiveness of email for conveying one's thoughts, you nevertheless wrote this note while we were in the same room!

Rüdiger, we can't wait until you get here!

28-05-02 *Strong Coherence?* (to M. C. Galavotti & E. Regazzini)

My colleagues, Caves and Schack, and I are once again having an argument about the foundations of probability. In particular, one of the things we are getting hung up on is the issue of requiring A) "strong coherence" as introduced by Shimony and Kemeny, or instead B) "normal de Finetti coherence" in Dutch-book arguments. I hope that you are aware of these distinctions.

Caves and Schack are attracted to strong consistency as a normative criterion because it leads to certain results that they would like to see when we apply the idea to quantum mechanical betting situations. I, however, am opposed to the criterion for just the opposite reasons. What I would like to ask of both of you is might you point me to some of the literature where the issue of "strong coherence" versus "regular coherence" is discussed?

I am aware of de Finetti's own rejection of strong coherence from his book. However, in particular, I am wondering if there are any independent arguments (i.e., other than de Finetti's, which concerns an infinite limit and probabilities on a non-discrete space) for rejecting strong coherence? Could you send me references if there are any?

Thank you so much for your help, and I hope to hear from you soon.

29-05-02 *I Think She'll Know* (to N. D. Mermin)

Remember what the dormouse said; feed your head.

You can tell I'm pretty darned behind in my email. I'm in Australia now, finally with a little time to think. Kiki and the kids are in Munich. Caves, Schack, and I are down here for three weeks doing Bayesiany things.

Merminition 90: *I don't see what your teleportation example (pages 11, 12) adds to ordinary EPR. Aren't all the issues exactly the same if Alice "in her laboratory prepares" the single qubit in (1) that she possesses by an appropriate measurement (to be sure, she can't control which outcome she'll get, but that doesn't seem to be central to your point, or is it?) after which she and only she knows what the outcome of the corresponding yes-no measurement on Bob's qubit will be.*

Yeah, I'd agree that it doesn't add a heck of a lot to the old argument. Mostly I wanted to say something about teleportation: Namely that if Einstein had known about it, then he might have used it to the same devious purposes he did with his old argument.

But still, I guess there were a couple of features of this version that I thought made it a bit cleaner than the old Einstein thing. 1) With the supplementation of *only* two bits of physical action on the part of Bob (i.e., one of four possible unitaries), Alice can put Bob's system into any state she wishes. So, in essence (i.e. up to two bits), there's nothing even random and uncontrollable about the process. 2) In the case of teleportation, even examining the measurement device before and after the measurement will tell you nothing about the posterior state for Bob's system that Alice ends up with. Very literally, only Alice ends up doing some updating. If it's she and only she, why not call the state her knowledge?

29-05-02 *That Damned von Neumann* (to N. D. Mermin)

Now let me try to answer more adequately the question you asked after my talk in Ithaca. The point is simply this: Suppose I tell you that I've got a device that measures a standard observable H. How do you know that you should accept my claim? Let's say you do this: You simply give me a supply of a gazillion nonidentical states you've prepared (anyway you wish) in your laboratory.

I'll perform my measurement on each of them and report the results I found. If you find that I'm giving you back outcomes with the (conditional) statistics you expect, then you'll have some warrant for believing I'm really performing the measurement I claim.

Now let me ask you this. Suppose you confirm my measurement to your satisfaction by that method. Do you now have warrant to say anything about the post-measurement state for each of the systems you gave me? The answer is "no" of course. The point is, you need to know more about the particulars of the device.

Now, von Neumann said that for an "ideal" device the post-measurement states for the systems will be eigenstates of the observable. But I claim that is an arbitrary notion of ideal, and Kraus's theory of "effects and operations" backs that up in a kind of technical way. The Kraus theory says that the state change can always be thought of as a collapse in the von Neumann sense PLUS a trace-preserving completely positive map.

Now, the standard quip that is made is that the CPM part of this is just extra noise that didn't need to be there. But again, I claim this statement is arbitrary. Here is a simple counter example. Suppose I perform the measurement H on half of an entangled pair. Then (via the entanglement), I can always think of this measurement as *really* a measurement of some sort on the other half. In fact I can think of it as simply the observable that is the transpose of the original one ... only performed on the second system rather than the first. What could be a more minimally disturbing way of measuring H^{T} on the second system than that? But does von Neumann's collapse postulate hold for this kind of measurement? Try it, and you'll find that it doesn't. The only state changes this kind of measurement can produce are of the pure-refinement kind.

Von Neumann brainwashed the generations in a needless way. Even something so simple and "ideal" as a standard photon detector does not follow the postulate. When there's a click, the photons are absorbed from the mode and the field is now in the $|0\rangle$ state.

I hope that helps make up for my lack of lucidity at your celebration.

29-05-02 *Notes from the Web* (to C. M. Caves & R. Schack)

[[**Note:** This is a quote I thought worthy of recording the time. Unfortunately I did not record its source.]]

> Note that traditional conditionalisation is a special case of Jeffrey conditionalisation. It is important that Jeffrey stressed that this rule of the kinematics of rational belief is all that is needed. It is never needed to give up propositions that are assigned probability one. The assumption is that only logical tautologies have probability one and only contradictions probability zero. This view is defended by means of the condition called strict coherence. A probability function obeys strict coherence if it is coherent and there is no set of bets consistent with it such that the better might lose, and cannot win. Jeffrey, among others, has argued that by assuming strict coherence it follows that no contingent proposition should have probability one, because no rational agent would bet his head on the truth of any contingent proposition.

29-05-02 *Strict Coherence?* (to B. Skyrms)

I doubt you remember me, but we met between 1996 and 1998, when I was a postdoc at Caltech and you had a visiting position there.

I dawned on me today that you might be able to help me in my plight described below. [See 28-05-02 note titled "Strong Coherence?" to M. C. Galavotti and E. Regazzini.] Is there any literature that you could send me to that expresses a dissatisfaction with "strong coherence"? I'd be very grateful for any pointers you could give.

29-05-02 *More Strict Coherence* (to B. Skyrms)

Skyrmsism 1: *Sure, I remember you. I think that the usual response to Shimony is just that strict coherence isn't a plausible requirement in the usual way of doing probability theory with continuous mathematics. Don't remember any good references, though.*

That's too bad. What I'm really looking for is anything that expresses a dissatisfaction with strict coherence with respect to discrete event spaces. It turns out that enforcing strict versus normal coherence can make a pretty drastic difference for some interpretive problems in (even finite dimensional) quantum mechanics. Unfortunately, with respect to the attractiveness of the conclusion, my coauthors and I have opposite opinions. Thus, I am inclined to require only normal coherence for the agents in our game; whereas they are inclined to require strict coherence.

The main point, even in the classical case, is that strict coherence requires that an agent ascribe probability 1 to an event if and only if he believes the event is a certainty. Whereas under the assumption of normal de Finettian coherence, a probability-1 assignment cannot be used to conclude a belief of certainty on the agent's part. He might be assigning probability 1, not because it reflects his true beliefs, but because it is advantageous for other purposes. That is, with respect to certainty, strict coherence compels an agent to never "tell a lie."

I think that goes too far as concerns a foundation for "rational behavior." Whereas normal coherence appears to me to strike a sweet spot.

In any case, what I am looking for is some confirmation of my troubles in the published literature ... to help me build a case for the inevitable battles I foresee with my coauthors.

By the way, you might be interested to know that we have been putting a substantial effort into interpreting quantum probabilities as personalist probabilities. Let me recommend four of our papers to you (along with the web links to get them) in case you're interested. [`quant-ph/0205039`, `quant-ph/0204146`, `quant-ph/0104088`, `quant-ph/0106133`]

As you climb from bottom to top in this list, you'll find us moving closer and closer to a personalist position. Also, there is a lot of supporting information posted at my website, link below.

31-05-02 *Poor Young Duvenhage* (to H. M. Wiseman)

Thanks yourself for your long note in reply to my long note! You can see I'm working with a much larger lag time than you in my emails. Anyway I'm in Australia now, and slowly working off the jetlag and becoming more productive as the days go by.

Let me say a few words on your last note.

Wisemanism 7: *Freedom taken, thank you. And I think you will be stronger if you don't try to prop up your position using another paper which I think goes fundamentally against your views in a number of places.*

If you believe that, you misconstrue my purposes for citing Duvenhage. My citation was to give attention to and encourage this young researcher. Of course, I don't agree with a lot of the paper;

but that is beside the point for me. There is one thing I certainly agree with and I think he said it particularly cleanly.

You can find where I said this outright to Duvenhage himself by looking at pages 167 and 168 of my web samizdat, "Quantum States: What the Hell Are They?" (It's in a 28 March 2002 letter; I hope you'll read it.) There you will find me expressing some of the same misgivings as you, though with less detail. As I told him, I would rather encourage the similarities at the moment than the contrasts.

Duvenhage's paper will appear in *Foundations of Physics*, and I think that is a good thing. Having the paper out in publication-land just encourages someone to write a comment on its deficiencies. Indeed, I would love for that to go further and get a discussion stirring. For, what might get more people to pay attention to the potential of a Bayesian approach to QM than a good stirring discussion? Moreover, if people start to pay attention to the potential of the approach, they might just get to work on filling its other (I would say, more real) deficiencies!

So, there. I can even imagine an eloquent writer to start the ball rolling ... (OK, a hint: His initials are HMW.)

Now, let me go back to the one thing I said "I certainly agree with."

Wisemanism 8: *But here is the difference between classical and quantal.*

In the classical theory the belief p of the weatherman could be given a precise formulation in terms of a set of classical variables (presumably related to his [brain]), and that belief would actually evolve (for a "true h" etc.) in a deterministic way from $p(h)$ to $p(h|d)$ as he looks out the window. That is, the belief of the weatherman can be treated ON THE SAME BASIS as the objects of that belief (i.e. the physical world). That is not to say it MUST be treated on the same basis, but it CAN be, and there is no NEED to have Bayes thm in the foundations of the theory.

In the quantum theory, our belief cannot be treated on the same basis as the physical world. We use a state matrix to encapsulate our belief about the world: our expectation for the results of a fiducial measurement if you like. In the absence of information gathering, this state matrix evolves in some well-defined way. But if we try to treat our belief on the same basis, as a function of physical brain variables, we run into the QMP.

I think this pinpoints in a pretty terse way the root of our disagreement. To say what you said is to 1) accept a kind of reductionism that I no longer think is healthy, and 2) to posit a strong faith that something *can* be done that, in actual fact, has *never* been done.

Concerning 1) I apologize for using the phrase "those beliefs are presumably a property of his head," for it evokes an imagery I would rather not have in this discussion. Perhaps it would have been better for me to use the word "possession" rather than "property."

Here's a belief I presently possess: With probability greater than .9, there will be another suicide bombing in Israel in the next month. Try to put that into physical terms. What can the word "suicide" mean in classical mechanical terms (or even quantum mechanical terms)? Or to make it look just a little more physical, here is another belief: With probability greater than .99, I will see at least one car today. But why didn't I say a lump of atoms with this characteristic, that characteristic, and the other characteristic? The point is: The world independent of man does not, and cannot, know that that lump of atoms signifies a car. My beliefs—at their starting points—are always about things denoted at such a level of *practical* existence.

Thus I would claim, concerning 2), that it is nothing but a religious faith to suppose that one can derive the form of Bayes' conditionalization rule from a mechanistic physics. It has never been done before, and I would venture to guess that it will never be done.

But still, let me suppose that it could be done after all—it's just that it hasn't been done yet. What would that do? In agreement with (a small part of) Duvenhage's paper, I would say we're left with one conclusion: We should call the difficulties so found the "classical measurement problem." For in classical physics we wouldn't know where the observer begins and the world ends … just as we don't presently know it with quantum mechanics. Nor as I said above, would we know how to get a Bayesian collapse in the beliefs of the observer (even once he has been identified).

So, with regards to this particular aspect of things, I hold firm with my assessment of Duvenhage. If there is a distinction between a classical and quantum conception of nature, it is somewhere else than in the updating of one's beliefs. I claim the same difficulties are either absent from both conceptions, or present in both conceptions.

But … maybe I say this all for nothing. For you also wrote this:

Wisemanism 9: *Duvenhage is quite specific about what he is saying. He says "the Heisenberg cut is therefore no more problematic in quantum mechanics than in classical mechanics". This is true if you are a committed Bayesian, because then the cut is necessarily there from the beginning of your conception of the world.*

and that seems to express that my previous quotation of you was directed more toward Mr. D than me.

In any case,

Wisemanism 10: *I accept that. This is your solution to the QMP. You say "there is no QMP", but isn't that the same as their saying "I've solved the QMP"? You don't have to answer that.*

I would say it doesn't solve anything; it just shifts the terms. For, where it dismisses old problems, it creates new problems. And it's the problems that keep us all young.

31-05-02 *'Reality'* (to M. J. Donald)

Thanks for the clarification on your form of idealism. I also had a nice time reading the FAQ on your webpage. I'm whiling away a little time in Australia at the moment, visiting Nielsen and trying to write some papers with Caves and Schack.

Donaldism 2: *If the "current wavefunction" is just the best description of our knowledge of the system, then what are we made of?*

Something that is not the wavefunction, but for which, once we have accepted its existence, compels us to a structure of reasoning and belief revision whose form is identical to what we once thought of as "quantum mechanics." My best shots so far at saying this in a clear way can be found in Sections 4.2 and 6 of the new paper `quant-ph/0205039`. Or you can see the same thing in pictorial form by looking up the talk "Where's a Good Weatherman When You Need One?" at my webpage.

And so the sparring match goes on …

01-06-02 *Finally, My Slides* (to R. J. Greechie)

I've been meaning to write you for a while. At the AMS meeting in Atlanta, you asked me if I could mail you a copy of my slides from the talk I gave there. Well, I still can't do that easily (I'm in Australia for three weeks), but you can download my slides from my website now. The link for it is below. I was hoping to get them printed out in color to mail to you before I left for Australia, but time ran out on me. I hope this will do as a substitute.

Also let me advertise some papers that outline what I would like to see happen in quantum foundations. The top paper in the list below (0205039) makes my strongest statement of that. All of them, I think you'll find to be pretty easy reading.

- "Quantum Mechanics as Quantum Information (and only a little more)"
  `http://xxx.lanl.gov/abs/quant-ph/0205039`
- "The Anti-Vaxjo Interpretation of Quantum Mechanics"
  `http://xxx.lanl.gov/abs/quant-ph/0204146`
- "Unknown Quantum States: The Quantum de Finetti Representation"
  `http://xxx.lanl.gov/abs/quant-ph/0104088`
- "Quantum Probabilities as Bayesian Probabilities"
  `http://xxx.lanl.gov/abs/quant-ph/0106133`

Unfortunately, I can't come to the IQSA meeting in Vienna this summer; time just became too short for me. But I get the feeling that there is a large mass of results I could use from the quantum logic community to further my program. "Results which are," as I wrote Karl Svozil recently, "just waiting for me to plaster over them with some words of a Copenhagenish flavor."

01-06-02 *Vienna and Amherst* (to D. J. Foulis)

I just wrote a letter to Dick Greechie giving links to some of my papers and talks. Rather than dreaming up some new things to say, let me just paste that letter into this one for your use. [See 01-06-02 note "Finally, My Slides" to R. Greechie.] If you can download it, I especially hope you enjoy my paper "Quantum Mechanics as Quantum Information (and only a little more)" … and don't find it too offensive!

I'm so happy to have met you in Georgia. You really helped build my confidence that this method of attack I keep trying to push the community into might just go somewhere. From time to time, I am in Amherst visiting Charlie Bennett (he's got a weekend home in Wendell) or Herb Bernstein at Hampshire College. It'd be great if I could swing by and have some further talks with you.

01-06-02 *Wheeler Compendium* (to K. W. Ford)

Lucien Hardy gave me your name as a contact. The reason I am writing you is that I am working to put together a rather large compendium of references and quotations from the literature with the title, "The Activating Observer: Resource Material for a Paulian–Wheelerish Conception of Nature." The compendium presently stands at 439 references and fills 107 printed pages. I expect it to be significantly larger than that when it is finished. As you can guess from the title, the thoughts of John Wheeler play a significant role in the project. In fact they started me down the course; John has long been one of my heroes.

The reason I am writing you is that I wonder if I could ask your help in making sure that I have the document as complete as possible. Also I would like to ask you if you have any ideas of where I might publish the thing once it is completed. I had originally planned to submit it to the journal *Studies in History and Philosophy of Modern Physics* but now I am starting to fear that the document is becoming much too long for that.

If you would like me to email you a copy of the manuscript-under-construction, I can do that. (Either as a raw REVTEX file, a DVI file, or a PostScript file.) I would certainly welcome any help you could give.

About myself—in case you worry about my legitimacy—I am a research physicist at Bell Labs working in the field of quantum information and computing, am 37 years old, and have had post-doctoral positions at Caltech and Los Alamos. My full CV can be found at my website (there is a link to it below). Also, you can read about me in the nice foreword David Mermin wrote for my samizdat *Notes on a Paulian Idea* published on the Los Alamos archive. Here is a link: `http://xxx.lanl.gov/abs/quant-ph/0105039`. In general, you can read my views on quantum mechanics and how it fits in with some of John Wheeler's larger questions by either consulting my website or perusing the Los Alamos `quant-ph` archive.

Anyway, I put all this information so that you won't think I am just another crackpot who has become smitten with John's words. It is a serious project.

I hope I hear from you soon.

01-06-02 *Back from the Grave* (to G. Brassard)

Good to hear from you. Our letters just crossed paths.

Brassardisme 5: *But can you tell me what's the homunculus fallacy?*

Sounds dirty, doesn't it? I think it's the same fallacy as the camera obscura fallacy.

01-06-02 *High Dispute* (to P. Grangier)

I hope you will forgive me for not replying to your letters for so long. The volume of email I have been getting lately has started to become something I am not equipped for. Sometimes I just crack under the pressure, and thus my silence. But at the moment, I've got a little time away from home: I am in Australia, visiting Nielsen and trying to write some papers with Caves and Schack. Beyond that, my wife, kids, and Bell Labs have all been left behind! So I am hoping for a productive three weeks.

Grangierisme 10: *If we speak about "objectivity", we have first to agree about what it is. To keep it simple, I stick with the "naive" view that if I do (or if a student does) a measurement in the lab, this is an objective process: this "did happen", and the fact that you don't know the result of the measurement will not change it.*

As far as I can tell, we do NOT disagree on this. So, it kind of annoys me that you keep bringing it up. Instead, I would say our disagreement lies right here: For some reason YOU think it is NECESSARY to uphold the idea that a pure quantum state is an objective property (of something in nature) in order for your sentence above to come about. Whereas I do not.

I know that I can function just fine with subjective quantum states, even with subjective pure quantum states. You will find no logical inconsistency in me; and I doubt I can find any in you. At the level of our squabble, it is to a large extent a matter of taste. However, you and I both know that matters of taste can lead to matters of fact with regards to the questions we will seek of nature. Your taste will lead you one way; my taste will lead me another. Only history will tell which of us will have had the more productive view. Only history will tell which one of us ended up asking the most interesting questions of nature. (Recently I tried to capture this in a little story to Charlie Bennett and John Smolin about the pleasures of broccoli. I will paste the story at the end of this note.)

Here is the way I put the whole point in a message to Matthew Donald just a minute ago. At the present time, I just do not know how to say it any more cleanly than this:

> If the "current wavefunction" is just the best description of our knowledge of the system, then what are we made of?
>
> Something that is not the wavefunction, but for which, once we have accepted its existence, compels us to a structure of reasoning and belief revision whose form is identical to what we once thought of as "quantum mechanics." My best shots so far at saying this in a clear way can be found in Sections 4.2 and 6 of the new paper `quant-ph/0205039`. Or you can see the same thing in pictorial form by looking up the talk "Where's a Good Weatherman When You Need One?" at my webpage.

The sections I recommended to him, in their focus on the Bureau International des Poids et Mesures, have direct implication on you. So I have a secret dream that you will read them ... and finally, finally something will click in your head, and you will say, "You know, Chris is not being so unreasonable after all."

Grangierisme 11: *it is quite enough that a complete (=self-defining) set of physical properties can be predicted with certainty. The crucial point is that there is no "ignorance" left (you must admit that each time you write that a pure state has zero entropy).*

It is fine to note that some observables can be predicted with certainty when a state is a pure state, but that does not delete the fact that there is plenty of ignorance left for other observables. One can even quantify that ignorance nicely. See equations (77) through (82) in my `quant-ph/0205039`. (But the result goes back to Wootters.) The von Neumann entropy—which is what you are thinking of in your statement—simply captures the BEST CASE predictability. So what?

Grangierisme 12: *PS In your paper you strongly "recommend" the work by Lucien Hardy. I have two comments:*

- *it surprises me that you adhere with the "relative frequency" approach to probabilities that is used by Lucien (his first axiom). I certainly agree with it, but I thought you would not.*
- *Lucien is trying to make QM look like a new probability theory.*

If you look at my paper, you will note that I said:

> Beyond that, let me recommend four other articles. The first two are the most technically important for the enterprise I promote in my other contribution to this volume: Namely, to secure a transfer from our present abstract, axiomatic formulation of quantum mechanics to a more physically meaningful one. I think some elements in Lucien Hardy's papers *almost* carry us to the brink of that. In his work, I think the right emphasis is finally being placed on the right mathematical structures. ...
>
> I should point out, however, that in all four of the above references, I think significant improvements could be made by adopting a sufficiently Bayesian stance toward the use and meaning of probability.

I said what I meant. (Just as the experimentalist should strive to perform his measurements accurately; the good theorist should strive to read his friends' papers carefully.) The interesting part of Hardy's papers, in my view, is the mathematics. Saying that constitutes an endorsement of neither 1) the relative-frequency interpretation of probability, nor of 2) Hardy's desire to generalize probability theory to a larger structure. Hardy and I, indeed, have had extensive discussions on this.

Grangierisme 13 : *Let us simply admit that local realism is dead, but that physical realism can do quite well without it. Why is that so difficult to accept?*

One more time: I do not deny realism. Moreover, just as you, I am happy with the death of local realism (if what one means by that is "hidden variable realism"). However, I simply do not find your proposed solution to the whole shebang of quantum interpretation problems to be as compelling as you find it.

Measurement clicks alone do not specify post-measurement quantum states. Rather, measurement clicks PLUS prior quantum states (for one's description of the measurement apparatus) do. Thus, where you think a measurement outcome prepares a unique quantum state, I say it has a subjective component. But you say, my measurement device is calibrated. And I say with respect to what? And on and on we could go ad infinitum. The Gordian knot of the state's subjectivity simply cannot be cut by your assuring me that I ought to think otherwise.

Grangierisme 14 : *admit that there is a "reality" attached to the pair of particles, but that there is no "reality" attached to each particle.*

This I see as an arbitrary move. Just as one man's unipartite system is half of another man's bipartite system, one man's bipartite system is half of another man's quadripartite system. And so it goes.

Instead, I would say *all* systems have a reality attached to them. It is simply that that reality has *nothing to do* with the quantum states one ascribes to those systems. A system has a Hilbert space, and a Hilbert space has a dimension that does not depend upon which state is "alive" within it. That dimension, I would offer you, can be treated as a reality for a quantum system. But there I stop, whereas you want to go further (by supposing a reality to some nonlocal quantum state).

Grangierisme 15 : *the "reality" attached to the pair makes no problem with Lorentz invariance, because it was created when the two particles interacted, and it simply follows them if they move very far away. The same conclusion apply to more fancy schemes like entanglement swapping, that requires classical communications to effectively prepare the remote entangled state.*

In the usual sense of what one means by the word "interacted," this—I would say—is just wrong. Suppose Alice and Bob possess two particles that interacted in the past so that they are now entangled (by your way of speaking). Further suppose Carol and Ted possess two particles that interacted in the past so that they are entangled. Maybe even make both states—the AB state and the CT state—to be singlets.

Suppose finally that Alice and Bob have never before seen Carol and Ted. A conclusion one can draw from this is that the A and C particles have never interacted in the past. And the B and T particles have never interacted in the past either.

Let now Alice and Carol meet by chance and perform a Bell measurement on their two particles. If Alice shares all her knowledge of the original AB interaction with Carol, and Carol shares all her knowledge of the original CT interaction with Alice, then they will both be warranted to update their assessments of the BT system. They each will immediately ascribe a pure Bell state (one of four possibilities to that system). With *respect* to Alice and Carol, the BT system will now be in a Bell state, even though B and T have never interacted in the past.

And this has nothing to do with Alice and Carol sending the information about their measurement outcomes to Bob and Ted. I'll say it one last time: *with respect to* Alice and Carol, the BT system will be in a Bell state after the measurement.

You see, you get hung up because you want to think of entanglement as a real objective property of two systems, not merely a property of an observer's judgment about those systems. Beyond that, your desire for Lorentz invariance makes you want to think of entanglement as only a consequence of local interaction. But that just goes too far, as the above example shows. Entanglement, just like quantum states, can arise from measurements in the distance.

You can call that a "contextually objective" affair if you wish, i.e., that the entanglement between B and T arises only with respect to the context set by A and C. But then I say—as I've said before—why not just call the state ascribed to the BT system Alice and Carol's information about it? Moreover, by calling it information, you will find that you will stop forgetting the prior (subjective) information that was crucial for defining the posteriori states in the first place.

By the way, I have read your FAQ posted on `quant-ph`; I have not ignored it.

Grangierisme 16 : *Bell's inequalities do not hold!!!! . . . if a measurement if performed on one side, ABSOLUTELY NOTHING happens on the other side.*

Well, at least we can agree on this. But look how many of your words I had to delete from the paragraph below to get us there:

Grangierisme 17 : *- since there is no "reality" attached to each particle, Bell's inequalities do not hold !!!! All the job is done by the fact that the individual particles have no quantum state, or no other property whatsoever that would decide on the result of the measurement. Then "action at a distance" simply vanishes: if a measurement if performed on one side, absolutely nothing happens on the other side.*

And we most certainly agree on the following:

Grangierisme 18 : *What next? QM is a fantastic theory that can only stimulate one question: why is it working so well? Then we may notice that QM was invented 75 years ago in a somehow anarchic way, as an attempt to understand atomic spectra. But we may speculate the following: QM is actually the answer to a question that was never clearly formulated. We have the answer, what about finding the question?*

Even if we do not see eye to eye in the secular world, we seem to dream of the same heaven.

PS. Don't forget the story below.

> King Broccoli
>
> "In any case, none of this matters for doing physics," you say. But I think it does in the long run. (Certainly, you've got to concede that there's something that fuels me—and it hurts to think that you might think it is nothing more than irrationality.) When Charlie sent me the picture of a skunk cabbage in reply to these very issues, I found myself thinking of King Broccoli. The story goes that one day, by divine providence, it came to King Broccoli that broccoli, the vegetable, his namesake, actually tastes good. Good in a way that hitherto only gods and angels had known. Every child who had

ever said, “Yuck, I don’t like broccoli; it tastes awful!” was simply wrong ... or at least that’s what the king realized. King Broccoli, being the head of state, decided to do something about it. Henceforward, all gardens in the kingdom should have a patch devoted solely to broccoli. It really wasn’t much of a burden on the national product (except, perhaps, for the psychiatrists who had to treat all the movie stars who had never felt fulfilled in their broccoli experience). But think of the diversity of vegetables the kingdom might have raised if its citizens hadn’t been encumbered with the king’s notion that broccoli had an objective, but never verifiable, taste?

Moral? Maybe there is none. But it is a documented fact that the Kingdom of Broccoli eventually fell and was replaced by a liberal democracy (where the ideals set forth in the constitution, rather than the particulars laws of any given day, have an objective status).

01-06-02 *Postmodernity* (to N. D. Mermin)

Ever since you wrote me this,

Merminition 91: *I liked the first half (the anti-Final Theory) part of your sermon to Preskill very much. You really could become the darling of the postmodernists if you put your mind to it.*

I’ve been wondering what the heck a postmodernist really is. Despite my occasional mention of Derrida and Baudrillard and the like, I’ve never really read any of their essays. The language barrier was always just too big.

In fact, I had never even read Sokal’s parody. Well anyway, I’ve started the process of remedying that now. Yesterday, I finished the book *The Sokal Hoax: The Sham that Shook the Academy*. It is a collection of maybe 50 or 60 articles and news reports that arose in the aftermath of Sokal’s paper. Here is the entry I put for Sokal’s article in my upcoming Cerro Grande II, the compendium “The Activating Observer: Resource Material for a Paulian–Wheelerish Conception of Nature.”

> A. D. Sokal, “Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity,” Social Text **Spring/Summer 1996**, 217–252 (1996). This parody article contains a wealth of references.

There appear to be some good references there, some of which I have read I *know* that I like!

Anyway, reading the book was an eye-opening experience. Now that I know the perils, it should be interesting to look back 20 years from now and see how successfully or unsuccessfully I managed to navigate the waters.

By the way, I just noticed a mystical coincidence: The big samizdat last year was assigned the number `quant-ph/0105039`; whereas my fire and brimstone “Quantum Mechanics as Quantum Information” from this year was assigned the number `quant-ph/0205039`. Pretty cool.

Did I tell you that I’m in Brisbane now? I’ve been here for a week, and will be here for two more. Kiki and the kids are in Munich until June 18.

01-06-02 *Smaller than the Circle* (to G. Plunk)

The more I chew on what your graphs indicate, the more I like it. “Accepting quantum mechanics is, in part, merely accepting that one’s judgments for an SQMD ought to live within an ellipsoidal volume smaller than X. And X is the volume of the largest sphere that can fit within the simplex.”

The statement will get simpler and simpler as we think about it more, *if it is true*.

The question is, how can we ferret out its truth. I hope you downloaded the thing from Caves's webpage. I think there's one calculation you should be able to do pretty easily for the SICPOVMs. Namely, it shouldn't be hard to calculate the *average* distance (or distance squared) of a pure state (thought of as a probability distribution) from the center of the simplex. Just use the methods from:

- R. Jozsa, D. Robb, and W. K. Wootters, "Lower Bound for Accessible Information in Quantum Mechanics," Phys. Rev. A **49**, 668–677 (1994).

However, maybe it'll even be easier than that. Maybe you can just use the abstract properties that define a SICPOVM to show that the distance of a pure state from the origin is invariant. In fact, thinking about this in my head, it might just be a piece of cake. But I'm probably missing something. You never can tell until you sit down with a piece of paper and a pencil.

02-06-02 *Lamb* (to G. Brassard)

Brassardisme 6: *Is this the Lamb of Lamb shift? [Willis E. Lamb, Jr. and Jagdish Mehra (ed.), The Interpretation of Quantum Mechanics, ISBN 1-58949-005-3] Do you think it should be interesting?*

Yep, that's THE Lamb. Willis Lamb. But he is quite old (over 90) and a bit off his rocker now. I hear that he comes out a bit against quantum teleportation there. I was at a talk of his about 2.5 years ago when he said, quote, "quantum teleportation is beneath contempt," lumping it in the same set of things he was already putting down, like Bohmian mechanics and such. He didn't at all explain why he said that.

Buy it at your own risk.

02-06-02 *Too Silly?* (to M. A. Nielsen)

Nielsenism 2: *Can you send me a title and abstract for your talk this Thursday?*

I just plucked one off my webpage. If it sounds too silly to you, I can write something a little more staid and proper. Your choice.

> **The Oyster and the Quantum**
>
> I say no interpretation of quantum mechanics is worth its salt unless it raises as many technical questions as it answers philosophical ones. In this talk, I hope to convey the essence of a salty, if not downright briny, point of view about quantum theory: The deepest truth of quantum information and computing is that our world is a world wildly sensitive to the touch. When we irritate it in the right way, the result is a pearl. The speculation is that this sensitivity alone gives rise to the whole show, with the quantum calculus portraying the best shot we can take at making predictions in such a world. True to form, I ask more questions than I know how to answer. However, along the way, I give a variant of Gleason's theorem that works even for rational and two-dimensional Hilbert spaces, give another variant of Gleason's theorem that gives rise to the tensor-product rule for combining quantum systems, and finally derive a new form for expressing how quantum states change upon the action of a measurement.

02-06-02 *Spheres, Spheres, Spheres* (to G. Plunk)

Yeah, I did get your Saturday email, but it came after I wrote the note to you. (That's the danger of writing a note before logging in to a server.)

So, I'd say, try again. Today, I'm hoping to work on my project with Petra and Rüdiger. In your expansion of the state ρ in terms of the sicpovm, did you a) take into account that $\mathrm{tr}\,\rho = 1$, and b) take into account that $\mathrm{tr}\,\rho^2 = 1$. I think you mentioned one of them, but I don't know if you mentioned both. Also, did you make sure to tabulate the distance from the center of the simplex. I remember you mentioning something along those lines, but I got a little confused as to whether you thought you could ignore it.

It'll work out; I'm starting to have faith.

Gabe's Preply, "More on Spheres"

> So get this. I woke up this morning to head off to the lab because I'm itching to get back to this sphere problem (more on that later) and I run into Mark the neighbor who reminds me about the party tonite. Funny, I thought the party was tomorrow night. Then I start driving and there isn't any traffic—lucky break I think. But when I find an empty lot at Bell Labs it all finally clicks and I realize that it isn't Friday but Saturday! Anyway, the moral of this story is that Steven van Enk isn't here to help me with my problem so I'm gonna ask you.
>
> I feel it is almost definite that we should see spheres in the simplex for SICPOVMs. I have a simple plan to prove it too. Start with Caves' form: POVM $= \{(1/D)\Pi_\alpha\}$. Where Π_α are projection operators. The symmetric part comes in by requiring the inner product between these Π_α to be all equal to some number, $1/(D+1)$. OK, so now we have this set of D^2 lin. ind. operators which form this operator basis as Caves says. So the idea is that we want to decompose a general pure state, ρ, in terms of this operator basis. So I write
>
> $$\rho = \sum a_i \Pi_i \;,$$
>
> where the a_i all add up to one since ρ is a density matrix.
>
> But to make these rho pure we put an additional restriction on the a_i by demanding $\mathrm{tr}\,\rho^2 = 1$. This gives a little relationship for the a_i. So we take this arbitrary pure state ρ and the POVM and calculate the D^2 probabilities. These give the components of vector which reaches into the simplex to a point on the sphere. Take the magnitude of this probability vector and substitute the relationship for the a_i and I think what *should* happen is that the a_i drop out and we get a quantity that is just dependent on the dimension D. (Now this isn't the radius of the sphere since we didn't project the probability vector into the simplex but this isn't a problem since the component perpendicular to the simplex is constant so just adds in a constant when we calculate the magnitude of the probability vector. It's just as easy to first subtract the point $(1/D^2, 1/D^2, \ldots, 1/D^2)$ which is the center of the sphere and work forward from there.) So with a probability vector with constant magnitude we have proved our sphere (see previous parenthetical note).
>
> So that's it. But I think I goofed somewhere. Because I worked it out a hundred thousand times and it doesn't quite work. I've got residual a_i that won't die. So I tried it for the case that I've already done (rebits) and know for certain it should work … and it doesn't. What's wrong? Something with my construction of ρ? Some additional restriction on the a_i?

I guess I'm gonna head home.

Gabe's Reply, "Soccer Balls in the Probability Simplex"

Did you see Brazil play Turkey? It was a fun match. The world cup games must be at reasonable times in Australia. Here you have to stay up till 4AM or settle for a replay the next day.

We finally found those spheres. Steven showed me something that I'd missed. It becomes embarrassingly easy after that. The SICPOVM gives rise to a sphere in the probability simplex. Its radius squared is:

$$R^2 = \frac{D-1}{D(D+1)}$$

Right. So we'd like the volumes for all ICPOVMs to be ellipsoidal and less than the volume of the sphere. I'll see what can be done in that direction. I have some ideas.

Oh and I'll do out the qubit too, I've been neglecting it in favor of looking for these spheres.

02-06-02 *Postmodern Rags* (to A. W. Harrow)

Here are four representatives of what it is that Carl and Rüdiger and a I are trying to get at:

- "Quantum Mechanics as Quantum Information (and only a little more)"
`http://xxx.lanl.gov/abs/quant-ph/0205039`
- "The Anti-Växjö Interpretation of Quantum Mechanics"
`http://xxx.lanl.gov/abs/quant-ph/0204146`
- "Unknown Quantum States: The Quantum de Finetti Representation"
`http://xxx.lanl.gov/abs/quant-ph/0104088`
- "Quantum Probabilities as Bayesian Probabilities"
`http://xxx.lanl.gov/abs/quant-ph/0106133`

The thing that comes closest to a postmodern rag is my Anti-Växjö paper. (That's the one I thought you might enjoy reading for the literary value.) The top paper, however, "QM as QI," is my present pride and joy.

Below is a passage from my samizdat about Derrida and quantum mechanics. The samizdat, by the way, is `http://xxx.lanl.gov/abs/quant-ph/0105039`

By the time you see this note, I'll be sitting next to you in 161.

From a 09 June 1997 note to G. L. Comer, "Dictionaries and Their Problems"

How are you my friend? It's been so long since I've written you anything of substance, I almost wonder if I can still remember how! Lately, I've once again taken to reading about Bohr's (and the other founding father's) thoughts on the epistemological and ontological lessons of quantum mechanics. I suppose part of my reason for getting back to these things is just a general tiredness of looking at equations; maybe it's a form of procrastination—papers need writing, papers need revising, papers need refereeing, talks need preparing . . . and I'm getting a little tired of it all.

In any case, the exercise is having its own payoff. Maybe I'll share a little with you. Remember I told you that Mermin suggested that Derrida's mumblings shouldn't be written off? I guess I'm starting to think he was right (though I have to admit that I haven't yet read any of Derrida's own writings, only commentaries). It seems that the focal point of Derrida's thought centers around none other than your "problem of the dictionary"! Let me try to give you something of a flavor of how these things might be connected to the quantum. My starting point has been an excellent essay by John Honner titled, "Description and Deconstruction: Niels Bohr and Modern Philosophy" (found in *Niels Bohr and Contemporary Philosophy*, edited by Jan Faye and Henry J. Folse (Kluwer, Dordrecht, 1994), pp. 141–151). I hope you enjoy the quotes:

> Derrida undermines the notion that words and signs can capture present experience: our tracing of experience always discloses a supplement, a 'difference'. This attack is equivalent to a subversion of the notion of strong objectivity and correspondence theories of truth. For the deconstructionist, the foundations for knowledge are never securely laid: words do not correspond exactly to the world. "Presence" can never present itself to a present consciousness, and hence experience is always and already constituted as a text. [I use 'text' loosely here, of course, meaning any collection of signs—discourse, mathematical equations, pictures, poems, prose, drama, hand-waving—used to trace and express insight and experience.] A text is a collection of signs and any sign presumes a presence which it represents, but the sign is not the same as that which it represents. In signifying our awareness of a presence a move is made from the presence to sign. By the word 'presence' Derrida is indicating something like substance, essence, or object, but he rejects such 'totalising' categories as these, for such terms assume more about the presence than perhaps we are entitled to assume. The term may 'trace' the presence, but a remainder is always left over.
>
> Speaking and writing are, according to Derrida, 'linear' activities which lock us into space and time. "The great rationalisms of the seventeenth century", as Derrida describes them, fall into the trap of objectivity and neglect the timelessness of self-presence. The linearity of the words limits the conditions for the use of language: "If words and concepts receive meaning only in sequences of differences, one can justify one's language, and one's choice of terms, only within a topic [an orientation in space] and an historical strategy." Here we have a curious serendipity. Our usage of words is tied, arguably, to the reidentifiability of particular objects, which itself implies those bastions of classical physics, the conservation of position and momentum and an absolute space-time framework. And it was precisely these bastions that Bohr attacked. As I have argued elsewhere, Bohr's fundamental arguments entail a provocative hint at a link between the given character of ordinary language and a deterministic-mechanistic view of the workings of nature. For Bohr, classical physics is the inexorable result of the use of language based on the identification of experienced material particulars; or, vice versa, the use of language based on identification of experienced particulars will ultimately lead to a sense of the persisting presence and movement of material object in space and time, and hence to principles of conservation, causal change, and continuous space-time frameworks.

03-06-02 *I Think She'll Know, 2* (to N. D. Mermin)

Merminition 92: *Kurt Gottfried got me back to thinking about the old "derivations of probability" dating back to Hartle in the 1960's and going through the Sidney Coleman application to many worlds. Turns out Jeffrey Goldstone did something on it and there's a nice paper by Gutmann (*`quant-ph/9506016`*). To remind you, in the modern version one's only probabilistic assumption is that if* $\mathrm{tr}(\rho E) = 1$ *then* E *must happen. Combining that with some highbrow analysis of the nonseparable hilbert space formed by infinitely many copies of a system with itself, one derives all the usual probabilistic rules.*

As I remember you were quite scornful of this approach, saying that they were sneaking probability into the story without admitting it. Was this because as a good Bayesian you regard probability 1 as no different from any other probability — merely the current best guess. (Our conversation about Coleman took place before we had our arguments about the difference between probability 1 and "has to happen".) Or did you have some other leakage of probability into the argument in mind?

Yeah, I've still got loads of issues with that approach. But maybe just let me mention the simplest one again: $\mathrm{tr}(\rho E) = 1$ certainly should not be taken to imply "must happen" in an infinite setting. Take the converse. Consider an infinite sequence of coin tosses. Each individual outcome string has probability strictly zero, yet one of them does happen.

Shifting the problem to sets of measure 1 doesn't help either. For lots of inequivalent sets have measure 1. What principle of nature sets one out as important?

It's all ad hockery that these guys are up to . . . shined up with some high-powered mathematics so that it looks important.

04-06-02 *Random Questions* (to G. Plunk)

At least we can probably bank on this much (subject to proof). The volume of the maximal quantum state space is bounded above by the volume of the sphere with radius given by the number you sent me earlier. Here's a question. How does that volume compare to the volume of the simplex? It looks to me like it becomes a smaller fraction of the simplex volume, but I'm not sure.

Gabe's Reply, "Random Answers"

> I think we have easily that non-pure states will not touch the sphere. See the attached files (the `.txt` file is TEX output from Mathematica).[31]
> — — — — — — —
>
> The SICPOVM is defined using projection operators Π_i which satisfy
>
> $$\frac{1}{D}\sum_i \Pi_i = I$$
>
> and
>
> $$\mathrm{tr}(\Pi_i \Pi_j) = \frac{1}{D+1}\ .$$

[31] A key thing to note here is that Gabe presents an expression for ρ in terms of the SIC outcome probabilities. Somehow I missed this, perhaps not being able to open the files he had sent me at the time. Thus, the surprise in my 18-06-02 note "SIC POVMs" to C. M. Caves; apparently I still did not understand Gabe in the previous day's discussion. Just discovering this now, sadly I have never before given Gabe credit for this result.

The SICPOVM is thus $\{E_i\}$ with $E_i = \frac{1}{D}\Pi_i$.

Write an arbitrary state ρ as in terms of the operator basis $\{\Pi_i\}$:

$$\rho = \sum_i a_i \Pi_i \quad \text{with} \quad \operatorname{tr}\rho = 1 \implies \sum_i a_i = 1\ .$$

The probability of outcome α is

$$\begin{aligned} p(\alpha) &= \operatorname{tr}\rho E_\alpha \sum_i a_i \\ &= \frac{1}{D}\left(\frac{1}{D+1}\sum_i a_i + \frac{D}{D+1}a_\alpha\right) = \frac{1}{D}\left(\frac{1}{D+1} + \frac{D}{D+1}a_\alpha\right)\ . \end{aligned}$$

We can find the center of the sphere by calculating the probabilities with $\rho = \frac{I}{D}$. We get that all probabilities are $\frac{1}{D^2}$. Subtract this from $p(\alpha)$ to get the radius vector:

$$r(\alpha) = \frac{1}{D}\left(\frac{D}{D+1}a_\alpha - \frac{1}{D(D+1)}\right)$$

$$r^2(\alpha) = \frac{1}{D^2}\left(\frac{1}{D^2(D+1)^2} - \frac{2a_\alpha}{(D+1)^2} + a_\alpha^2\frac{D^2}{(D+1)^2}\right)\ .$$

Now sum over α to get the radius squared

$$R^2 = \sum_\alpha r^2(\alpha) = \frac{1}{D^2}\left(\frac{D^2}{(D+1)^2}\sum_\alpha a_\alpha^2 - \frac{1}{(D+1)^2}\right)\ .$$

Now we can learn about the quantity $\sum_\alpha a_\alpha^2$ by taking the trace of ρ^2:

$$\operatorname{tr}\rho^2 = \frac{1}{D+1}\sum_\alpha a_\alpha^2 + \frac{1}{D+1}\ .$$

Define $F = \sum_\alpha a_\alpha^2$ and note that $\operatorname{tr}\rho^2 \le 1$. And we get $F \le 1$, with equality if and only if ρ is a pure state. Plugging into R^2 ...

$$R^2 = \frac{1}{D^2}\left(\frac{D^2}{(D+1)^2}F - \frac{1}{(D+1)^2}\right)\ .$$

For pure states

$$R^2 = \frac{D-1}{D^2(D+1)}\ .$$

Otherwise it's less.

04-06-02 *Morning Headaches* (to G. Plunk)

I've got a huge headache this morning ... but you are my aspirin.

Plunkism 1: *It seems a reasonable hope that we could get that the surface created by the pure states is closed. Then we would have that the entire sphere is generated by the pure states (right?).*

The more I think about it, the less likely I think a miracle will happen here. The problem is that normalized pure states have a dimensionality of $D-1$, whereas the full set of states has dimensionality D^2. ... Though in saying that I'm a little comparing apples and oranges. (Because the pure states don't form a subspace in the space of operators.) So, it's worth further thought, but I fear ...

Maybe a better way to say it is that to parameterize the set of pure states you only need $2D-2$ real parameters (i.e., D complex numbers minus normalization), whereas to parameterize the full set of states you need D^2-1. When $D=2$, that means you get 2 and 3, respectively. So that means one can think of one set of parameters living in the sphere and the other on its surface. But, when $D>2$ that clean connection goes away.

Plunkism 2: *I found the n-Sphere paper. You said you'd like a copy–would you like me to mail it? Will Lucent do this for me?*

No, I just meant to make me a copy while you were making yours and hold onto it.

04-06-02 *Big File Coming* (to K. W. Ford)

Thanks for the encouraging email. In the next note, I'll send you the compendium as it stands. I'll send it both as a LaTeX file (REVTeX actually) and as a PostScript file. I'll attach both to the same email. If you have any trouble opening or reading these files or getting them to compile, let me know and I'll try to find a solution.

As you'll be able to tell, I'm quite in mid-project with this. A lot of material still needs to be added (essentially all the entries marked with a ¶), but also the introduction still has a long ways to go. In particular, with regard to you, you'll notice very few quotations in the Wheeler area. But that is only because I haven't yet gotten them out of the papers and into my scanner. Most of those papers I read more than 10 years ago, long before I started this project. Anyway, I'll be fixing that shortly, i.e., as soon as I get back to New Jersey.

Fordism 1: *Or I could send you a copy of his bibliography (59 pages) for you to eyeball.*

That would be great![32] I appreciate the help. If you can send it electronically, just send it now. If, however, you only have it on paper, please send it to my Bell Labs address below. Beyond that, maybe one of the best ways you could help me would be in helping me obtain copies of some of the papers I haven't been able to find. For instance,

> J. A. Wheeler, "Delayed-Choice Experiments and the Bohr-Einstein Dialogue," in *American Philosophical Society and the Royal Society: Papers Read at a Meeting, June 5, 1980*, (American Philosophical Society, Philadelphia, 1980), pp. 9–40.

seems to be a crucial paper. And there are some ones beyond that.

Fordism 2: *I really have no good ideas about a place to publish it. Maybe it will have to end up on the Web with references to it published in journals and newsletters.*

[32] NOTE: I would have worked to include the bibliography Ken sent me, but using it as a seed, Terry M. Christensen built a significantly more thorough bibliography for his PhD thesis, *John Archibald Wheeler: A Study of Mentoring in Modern Physics*. The bibliography section of the thesis has been conveniently posted by Baruch Garcia here: `http://jawarchive.files.wordpress.com/2012/03/bibliography.pdf`.

Yeah, that's certainly an option. I guess I was half hoping there might be some room in the Wheelerfest volume you're associated with, and if so, that'd give me the incentive to get this finished right away. But, in truth, the paper is already pretty large, and I expect it to maybe double in size by the time I'm finished. So, it's probably way too large for anything like that.

Fordism 3: *I'm sorry that John Wheeler himself has slowed down so much that he is probably no longer able to absorb your work. There was a time when it would have meant a lot to him and when he no doubt would have loved to enter into a colloquy with you.*

How is John's health? Both physical and mental? I haven't talked to him since 1994. I had been thinking about visiting him this summer if was still going in to his office in Princeton. I read an article in the *New York Times* that said he was still going in a couple of times a week. And I thought, if his health is that good, then it might be good to establish contact now that I am in the neighborhood.

OK, the files will come in the next mail, after I go have a cup of coffee.

05-06-02 *Touching the Bound* (to G. Plunk)

I guess the simplest and main question on my mind at the moment is: How does volume of the "state sphere" (i.e., the sphere within the simplex that *surrounds* the full set of quantum states) scale as a fraction of the volume of the full simplex? So, calculate the volume of the sphere in arbitrary dimension, and calculate the volume of the simplex, and take their ratio.

I suspect the volume tends to zero, but it'd be nice to get a formal expression of that.

This would give a formal expression to the idea that, as the size of a system grows, one accepts in quantum mechanics that one can know less and less about it.

06-06-02 *Good News and Not-So-Good News* (to G. Plunk)

Plunkism 3: *In higher dimensions my results say that the n-sphere's volume divided by the n-simplex's volume becomes unbounded(!). This means (a) the n-sphere of required radius does not fit inside the n-simplex and consequently (b) the states trace out only part of the sphere–at most the part that intersects the n-simplex.*

Yeah, that means something's got to be wrong. The sphere, by definition, has to be contained within the (regular) simplex. So, I guess there are two options. Either your original calculation of the radius is wrong (make sure you calculate it from the center of the simplex). Or the formulas you've found for the volumes are off or misapplied.

I presented this picture of quantum mechanics in a talk yesterday at U. Queensland. It went over well, especially with some of the postdocs. I told them also about your explorations. Next week I do the same at Griffiths University.

Main question: Are you having fun with this problem?

Gabe's Preply

> Some good news. I was just thinking a little about the parametrization of pure states in terms of the SICPOVM elements. In the analysis I sent you I decompose using D^2 coefficients. The conditions on the coefficients are that they sum to 1 and the squares sum to 1. In "coefficient space" this is the intersection of a plane and a sphere which

gives a sphere of dimension $D^2 - 1$ which requires $D^2 - 2$ parameters to describe. This is exactly the dimensionality we need for describing the sphere in the simplex. Pure states have dimensionality $D^2 - 2$ and the full set of states has dimensionality $D^2 - 1$.[33] It's even simpler. Creating the "radius" vector in the simplex amounts to a shift and rescaling of the "coefficient vector" (look at $r(\alpha)$ in the proof I sent). The coefficient vectors form a sphere so the radius vectors do also. It looks like we have the *complete* sphere after all. A similar analysis fills the interior of the sphere using the mixed states.

Finding the volume ratio is rough going. For the qubit, my results for the ratio are exactly as you'd expect for a sphere inscribed in a tetrahedron. In higher dimensions my results say that the n-sphere's volume divided by the n-simplex's volume becomes unbounded(!). This means (a) the n-sphere of required radius does not fit inside the n-simplex and consequently (b) the states trace out only part of the sphere—at most the part that intersects the n-simplex. That can't be right (especially given what I said above). But I'm using textbook formulae for the volume of regular n-simplices and n-spheres. I'll attach a Mathematica file of what I've done.

07-06-02 *Spheres and Simplexes on a Saturday Morning* (to G. Plunk)

Plunkism 4: *I'm dumb. I forgot completely about positivity. We don't have the whole sphere, you were right all along. At most we have the intersection of the sphere with the simplex. Maybe that's exactly what we have. Is it obvious? Does the fact that the probabilities are contained in the simplex guarantee that a given ρ is a valid positive density matrix?*

You're not dumb. I know you're smarter than me. But goddamit, start thinking about the sphere from the center of the simplex! That is the one that is of interest. Not the one centered on the origin of the coordinate system. That sphere—the one from the center of the simplex—is automatically contained *within* the simplex.[34]

In other words, the set of valid quantum states will be a subset of that sphere. A proper subset. Nevertheless, even though the sphere's volume will only be an upper bound on the volume of the true set of quantum states, we can still glean some interesting information by studying the sphere. If the ratio of sphere's volume to the simplex's volume goes to zero as the dimension grows—as I suspect—then it will follow that the more interesting ratio (namely that of the volume of the valid quantum states to the simplex) will go to zero too. It'd just be a nice fact to know for sure.

Plunkism 5: *My principle hope is to come out of this with a deeper understanding of quantum mechanics.*

I hope so too. Have you given my paper `quant-ph/0205039` a shot? If you have any questions, I'd be glad to answer. Or why don't you pick up the book *Pragmatism* by William James or the book *Philosophy and Social Hope* by Richard Rorty off my desk? I think, with respect to quantum mechanics, they contain a lot more physics than you might believe.

11-06-02 *Who Is asdf? Who Is jkl?* (to G. Plunk)

Plunkism 6: *So what I'm trying to say is that I've been doing this … and the sphere is still too big. Here's how I resolve the discrepancy:*

---

[33] This sentence and the one previous to it are incorrect, as the pure states have dimensionality $2D - 2$; however I preserve it for historical accuracy.

[34] Clearly, I was the "dumb one" because this is not true except in $D = 2$.

Hmm. This is intriguing. But I have difficulty seeing how it could happen. I guess strictly speaking, what you have really proved is that all the pure states are equidistant from the center of the simplex. Period. And indeed, there is no requirement that the sphere those points sit on need be strictly contained within the simplex. However, I find it difficult to see how that could happen. It seems to me that that would require that the pure states form a set of disconnected sets (one for each element of the SICPOVM). But we know the pure states form a connected set.

I'll be glad when I'm back to there, so I can have a chance to see these things more directly. I get back very late Sunday night. I'll probably spend the night with my friend Jeff and then come back to the house Monday morning. Would you mind hanging out there until then, so that I can get a ride to the repair shop to pick up my mini-van etc.?

Gabe's Preply, "asdf likes jkl;"

> [CAF said:] "goddamit, start thinking about the sphere from the center of the simplex!"

So what I'm trying to say is that I've been doing this ... and the sphere is still too big. Here's how I resolve the discrepancy:

By taking linear combinations of the POVM elements (the construction of ρ in the proof I sent) and requiring trace and trace of the square to be one, we don't necessarily get valid states–they aren't necessarily positive operators though they are hermitian and normalized by construction. I've been banking on these things being actual states. It turns out that some of them give probabilities outside the simplex (i.e. negative probabilities). So those ones definitely can't be states.

The question (which I tried to pose in the last email) is if we take the points on the sphere that are inside of the simplex can we say that all of them correspond to actual, valid states? Most of them certainly. If *all*, then I can attempt calculate the volume of the intersection of the sphere and the simplex and get your bound ....

This is a strange result–the sphere-simplex intersection as the quantal probability space. But I've been so careful checking the math that I think it's right. Just in case you don't believe me (which is understandable given my vacillations) I went ahead and calculated the radius of the largest sphere which could fit in the simplex (the inscribed sphere). Its volume divided by the simplex volume goes to zero as expected.

Gabe's Reply, "Spheres with Holes"

> [CAF said:] "It seems to me that that would require that the pure states form a set of disconnected sets (one for each element of the SICPOVM). But we know the pure states form a connected set.

The picture I have in my head is a connected region with holes. The vertices and edges of the simplex always are further from the center of the simplex than the sphere's radius. Here the sphere is contained in the simplex. It is close to the center of the faces of the simplex (set one probability to zero to get a face) that the sphere emerges from the simplex ($D > 2$). D^2 faces give D^2 holes. That the pure states are a connected set follows from this picture I think.

11-06-02 *Radical Probabilism* (to A. W. Harrow)

http://www.princeton.edu/∼bayesway/

11-06-02 *Act II – Griffith University* (to A. W. Harrow)

1. Tomorrow at 11 seems fine for a seminar, although I am yet to get a room booked. Could you send me a title and abstract, please?

2. Another good question I meant to ask, is how would you translate the following into subjective language? In quantum control, optimal feedback control is based upon using one's knowledge of the system (i.e. its quantum state). However due to practical processing power limitations (shades of the quantum computer problem), one often cannot compute the true quantum state conditioned on the measurement results quickly enough. Instead, one computes an approximation to this, a best-estimate given the limited resources available, and uses that as a basis for feedback control. By simulation, one can determine on average how well this does compared to feedback based on the true state.

3. On the "let's call the whole thing off" theme, if you used some Greek letter other than ρ, it could work really well. For example, "you say beeta, I say bayta, you say eeta and I say ayta ..." Or for pure states it could work: "you say psigh, I say psee, you say phigh, I say phee ...".

12-06-02 *Receipt* (to M. J. Donald)

I got your long note; thanks. I'm going to mull over it for a while before replying. But I will reply.

Donaldism 3: *I also haven't commented much on the points I agree with in the paper, although there are a few of these!*

It would be nice to see what these are. In ways, I'm more interested in where we agree than in where we disagree. A good discussion needs some solid ground somewhere.

Beyond that, it would please me to no end to learn that you might have found a thought or two that you could use in the thing. But if you didn't, you didn't. It's a horrible feeling to think I might be writing nothing for nothing.

Each day, I tell my daughters that they can change the world. I tell them that they can change it to the core. But I never tell them that they can believe anything they want. There is a difference. And you don't see it.

The summary at the end of your note troubles me to no end.

Donaldism 4: *Your know-nothing ism, like de Finetti's irrationalism (Gillies, "Philosophical Theories of Probability", page 86), have the dangers of Bohr's writings on which I would agree with Beller (Physics Today, September 1998, pages 29–34). In particular, by leaving far too much in vagueness, incoherence, and pious hope, you give the religously-minded the official endorsement of the physics establishment that they may believe anything they want, instead of, by example, instructing them that they can believe anything they want as long as it is rational, coherent, tentative, revisable, and compatible with the evidence (and therefore contrary to naive expectations, because if quantum theory, or indeed science in general, tells us anything it is that the world is not how we would have imagined it before we investigated); and they accept that they may be completely wrong.*

I will certainly return.

12-06-02 *Bell Ineqs as Limitations on State Distinguishability* (to A. Peres)

I'm in Australia at the moment, but it won't be too long before I'm able to go home! I'm getting quite homesick. Kiki and the girls are in Munich; they arrive in New Jersey one day after me.

Asherism 24: *I spent two days in Geneva. The first was mostly business, related to Helle's final PhD exam (a public lecture) and the second was relaxing and touring the city. My preceding visit was in 1979, for three weeks, when I was the guest of John Bell.*

Helle's work was on Bell ineqs and cryptography, and relations between the two (you probably remember Gisin's insistence on this point). In an informal discussion, Helle asked me whether Bell ineqs could be reformulated in terms of limitations on state distinguishability. My answer was "QM as QI: there is no God but Chris Fuchs, and Danny Terno is His prophet." Can you be more specific?

I wish I could say more about that! But unfortunately my mind runs dry right now.

16-06-02 *Weddings and Connecting Flights* (to A. Peres)

Thanks for the pleasant note keeping me up-to-date on everyone. We are indeed like one big family. Sam actually invited me to his wedding! But Israel is a bit out of my reach at the moment.

Actually, I'm not home yet. At the moment I am in the American Airlines Lounge in Los Angeles. Still six more hours of flight to go. Kiki and the girls return to New Jersey from Munich on the 18th.

An interesting coincidence happened to me on the flight from Brisbane to Los Angeles. It stopped for an hour in Auckland, New Zealand. To my surprise, one of the passengers who entered there was Andrew Doherty, one of Hideo Mabuchi's postdocs. Moreover, his seat was directly behind mine. He had been in New Zealand on vacation, visiting his family, and was now going back to Caltech. We had some pleasant conversations about the latest results in entanglement.

Take care of yourself.

17-06-02 *Jones Stuff* (to G. Plunk)

1. K. R. W. Jones, *Quantum Inference and the Optimal Determination of Quantum States.* PhD thesis, University of Bristol, Bristol, England, 1989.

2. K. R. W. Jones, "Entropy of random quantum states," *Journal of Physics A*, vol. 23(23), pp. L1247–L1250, 1990.

3. K. R. W. Jones, "Principles of quantum inference," *Annals of Physics*, vol. 207(1), pp. 140–170, 1991.

4. K. R. W. Jones, "Quantum limits to information about states for finite dimensional Hilbert space," *Journal of Physics A*, vol. 24, pp. 121–130, 1991.

5. K. R. W. Jones, "Towards a proof of two conjectures from quantum inference concerning quantum limits to knowledge of states," *Journal of Physics A*, vol. 24(8), pp. L415–L419, 1991.

6. K. R. W. Jones, "Riemann-Liouville fractional integration and reduced distributions on hyperspheres," *Journal of Physics A*, vol. 24, pp. 1237–1244, 1991.

7. K. R. W. Jones, "Fractional integration and uniform densities in quantum mechanics," in *Recent Advances in Fractional Calculus* (R. N. Kalia, ed.), (Sauk Rapids, MN), pp. 203–218, Global Publishing, 1993.

8. K. R. W. Jones, "Fundamental limits upon the measurement of state vectors," *Physical Review A*, vol. 50(5), pp. 3682–3699, 1994.

17-06-02 *The Dreams Stuff Is Made Of* (to T. Rudolph)

Rudolphism 6: *This would be a good title for one of your papers: "Quantum mechanics: The dreams stuff is made of".*

I like the title. Now we just have to figure out a content for it.

18-06-02 *SIC POVMs* (to C. M. Caves)

That result is rather neat. Thanks for sending it. Funny coincidence: at the end of the day yesterday, Gabe and I parted after asking whether there was a simple way to express a quantum state in terms of the SICPOVM probabilities.[35] But I didn't work on the problem after getting home. Instead, I woke up this morning and find that you've sent me the answer! Maybe I'll try the experiment again tonight ...

Carl's Preply

Here's a nice property of SIC POVMs. Let

$$p_\alpha = \mathrm{tr}(\rho E_\alpha)$$

be the measurement probabilities for a SIC POVM

$$E_\alpha = \Pi_\alpha / D\ .$$

Then

$$\rho = \sum_\alpha \left((D+1)p_\alpha - \frac{1}{D} \right) \Pi_\alpha$$

The complete story is in the attached TEX file updating my little document on SIC POVMs.

18-06-02 *SIC POVMs* (to G. Plunk)

Have a look at the document below that I just got from Caves. [See 18-06-02 note "SIC POVMs" to C. M. Caves.][36] The result is rather neat and relates to our conversation at the end of the day yesterday. I'll attach a PS file of the account in case you can't TEX yet. It might be a good exercise to see if you can find a more elementary derivation of the fact.

[35] See 04-06-02 note "Random Questions" to G. Plunk. Gabe had actually already solved this problem, and I was somehow not aware of it.

[36] See 04-06-02 note "Random Questions" to G. Plunk. Gabe had actually already solved this problem, and I was somehow not aware of it.

18-06-02 *Horrible!* (to T. Rudolph)

As I said, horrible (title, that is):

> INSTITUTE FOR QUANTUM INFORMATION SEMINAR
>
> Tuesday, June 18: IQI Seminar
> Quantum steering
> Frank Verstraete, KU Leuven, Belgium
> 3:00 p.m., 74 Jorgensen
>
> ABSTRACT: `http://www.iqi.caltech.edu/seminar_abstracts.html#verstraete`
>
> Immediately following the seminar, refreshments will be served in 156 Jorgensen.
>
> Directions to Caltech:
> `http://www.admissions.caltech.edu/visiting/directions.htm#drive`
> Campus map: `http://www.caltech.edu/map/` (Jorgensen is bldg. #80.)

19-06-02 *Classical Essence* (to E. Chisolm)

Thanks for your letter. You ask, "What is the essence of classical physics?" I agree that this is something worth thinking about. I don't have an answer.

Maybe the closest thing I can give you at the moment is to send you to a few passages in my samizdat, `quant-ph/0105039`. First have a look at the passage titled "Genesis and the Quantum" on pages 85–86, and then have a look at the note "The Oyster and the Quantum" on pages 100–103. Finally, have a look at the note "Reality and the Symbol" on pages 233–234.

They will kind of lead you down the path of where I think classical mechanics fits into the scheme of things. As I say, I agree with you that it is worthwhile to try to characterize the essence of classical physics. However, in possible opposition to you, I think trying to do that in isolation, independently of the quantum problem, won't lead to the greatest insight. The difficulty as I see it is that quantum mechanics strikes me much more as a radical departure from classical physics than just a shift in "calculational recipes." For instance, take Newtonian gravity. I would argue that finding its essence gives one little insight into the essence of general relativity ... even though Newton's gravity is one of the limiting cases of general relativity. The underlying ontologies of the two theories are just so very different. Similarly I am thinking of classical versus quantum physics.

Eric's Preply

> I recently read your e-print "Quantum Mechanics as Quantum Information (and only a little more)", and I like your suggestion that our understanding of QM right now is in a state similar to that of physicists trying to understand length contraction et al. before special relativity. I also like your idea (hope?) that there are a few simple physical statements from which the formalism of QM emerges as naturally as special relativity does from Einstein's two postulates, and I want to ask you a few questions related to that idea.
>
> Let me describe how I think about the overall structure of theories in physics. I see classical mechanics and quantum mechanics, understood in their broadest senses to include the corresponding field theories, as basically recipe books: If you want to make a theory of the electromagnetic field, or projectile motion over the surface of the earth, or of the gravitational field, then you come up with a Lagrangian or Hamiltonian and

then either classical or quantum mechanics provides you with a basic set of rules to follow (use the Euler-Lagrange equations in one case or Schrödinger's equation in the other, states are in either a phase space or a Hilbert space, etc.) to get a fully predictive theory. Each one is an outline, so to speak, that tells you how to use a Lagrangian or Hamiltonian to describe the behavior of a system. On the other hand, another question most theories must answer is how they incorporate the principle of relativity, and this can be done in two ways: Galilean invariance or Lorentz invariance. Thus, for example, we can formulate a theory of free body motion classically or quantum mechanically and with Galilei or special relativity. (This is a huge oversimplification, I know; many theories aren't strictly classical or quantum, not every theory has a Lagrangian, for some theories the principle of relativity isn't included at all, etc., but this is a good scheme to think about.)

In this scheme, quantum mechanics and special relativity are not naturally parallel; instead, special relativity is parallel to Galilean relativity and quantum mechanics to classical mechanics. That leads me to ask this question: We know the simple, clear physical statements that lead to special relativity, and we know the same for Galilean relativity (I believe replacing Einstein's postulate about the speed of light with something like "velocities add" would do it); how about classical mechanics? Now an answer to this question would be interesting; surely it would help us understand what to look for as the "essence" of quantum mechanics if we could clearly state the essence of its classical partner. I honestly have no idea what simple physical ideas could serve as the foundation of classical mechanics (broadly understood, including theories like classical electromagnetism and general relativity). Do you?

19-06-02 *Experience* (to W. K. Wootters)

Woottersism 12: *I'm writing just to let you know that I have not ignored or forgotten the excellent questions you've asked. I've been busy with other things, and I still need to do some serious thinking about your questions (i.e., about my earlier statements). I promise a response before long!*

That's no problem, but certainly I'd like to hear your answers. In fact, I'm just back from Australia and Kiki and the kids are back from a month in Munich, so I'll probably be taking a couple of days off from the intellectual life.

Nevertheless, I've picked up my first Whitehead essays to read. Maybe I'll have a chance of better understanding your terminology this way. They're from a book titled *Philosophers of Process: Bergson, Peirce, James, ..., Dewey, ..., Whitehead.* Given my fascination with some of the ideas of James and Dewey, I think that indicates I may have some of the groundwork already within me. We'll see.

20-06-02 *Superadditivity* (to S. L. Braunstein)

Kiki and I got back from almost a month away from home yesterday (in Munich and Brisbane, respectively), and we found the invitation to your wedding in our mail pile. Many congratulations to you and Netta! Kiki and I wish you a long and happy marriage. I wrote this to another friend some time ago—in this hurry to write you something tonight, let me just repeat the words: "Marriage is about stability, happiness, and the kind of leisure that leads to true productivity (which only two people propped against each other can ever hope to negotiate)."

It's hard to imagine you being more productive, Sam, but I suspect we'll see a kind of brilliance from you that we've never seen before.

20-06-02 *Physics without Math* (to U. Mohrhoff)

You may not believe it, but I have been struggling on and off for several months to make some comments on your papers. The writing in each of them is locally beautiful, and it is for that reason to no small extent that I have kept coming back to give them another shot. But tonight, with too many burdens upon me, I have decided I must admit defeat and try to shake this feeling of guilt I have placed upon myself.

Life is short, and one has to make decisions on how to spend it. It is a gamble, it is always a gamble, but if one wants to do something of lasting value, one just has to proceed with dogged determination. I can see in your writings that you are doing that. But you have got to understand, I am doing it too. I may be damned for taking the route I am taking, but I feel I will only be able to know that with hindsight. My game is one of consistency: I want to push the subjective interpretation of the quantum state harder than anyone has ever pushed it before. I do that because I have a gut feeling that this will rattle something loose from quantum mechanics. Something that is wonderful and new. That little nugget—whatever it turns out to be—strikes me as something our community can hope to attain in the near future, and something that may even be revolutionary in a Kuhnian sense.

From your view, this strategy certainly looks like a stupid move. I understand that. I guess what I am writing is that I am willing to live with what you must be thinking of me. I really am sorry that I have not been able to say something sensible and useful about your papers.

I don't quite understand why I have had such difficulty reading your papers. They draw me in and then they push me back out. This may be the most flippant thing I can say, and may reveal a certain artlessness in me, but over and over I find myself asking, where's the meat? Where's the mathematics? Where's something explicit I can point to and either find a logical flaw in or—with greater desire—find some logical satisfaction. In your 14 papers (289 pages) on `quant-ph`, I count only 58 displayed equations. I lose orientation very quickly with a nonmathematical structure like that. Beyond this, in all honesty, for the same reason I find myself losing faith in your program. The gap between the size of your claims and the number of your equations is enormous. My intuition is that that is a symptom of a deeper trouble.

A couple of times, for instance, when you spoke of deriving the quantum probability rule, you wrote, "To find the quantum counterpart to $\mathcal{P}$, all we have to do is make room for nontrivial probabilities, and the obvious way to do this is to replace the subsets of a phase space by the subspaces of a vector space, or by the corresponding projectors." Well, it is not obvious to me; none of these things are obvious to me. (I struggle everyday to see where the vector-space structure of quantum mechanics might come from.) But I could give countless instances beyond that. One of my greatest complaints about Bohr is how he spoke over and over of how the quantum formalism is forced upon us by complementarity. But I never could see that he gave the slightest shred of evidence for that. Instead, I only saw him attempt to demonstrate that his point of view was consistent with the structure of quantum mechanics already given in its abstract axioms. I am sorry to say, but I fear the same for you.

You may indeed have a novel view that allows you to fulfill all your Vedantic wishes, just as I may have a view that will allow me to fulfill all my Jamesian (pluralistic) ones, but I would say we both need more. We both need to know what is it that allows us to have no other view. That is, to be convincing, we need a hard-core derivation of the quantum formalism. I see myself and the community I am trying to stir up as moving in that direction. On that count, however, your papers leave me cold, and I am making the judgment to leave the issue now.

I hope that explains somewhat my obstinacy. I think it probable that I have hurt your feelings, and I apologize for that; I don't wish any harm upon you. It really only boils down to, as I said, that life is short, and we all have to make decisions.

21-06-02 *Semi-Classical Mail* (to K. W. Ford)

I just mailed off my "Activating Observer" document to you. Hopefully it'll reach you in a couple or few days. You called the method "classic mail", but I hope the document is so thoroughly infused with the quantum it would be hard to think of the package as anything but semi-classical ... at the very least!

21-06-02 *Holevo Paper, Etc.* (to G. Plunk)

Well, I guess I don't have the Holevo paper any more after all. Here it is, in case you want to dig it up and make copies for us:

- A. S. Holevo, "Statistical decision theory for quantum systems," *Journal of Multivariate Analysis*, vol. 3, pp. 337–394, 1973.

It contains the most general result.

However, I also remembered a derivation of the result for the more limited case of rank-1 POVMs, as I was tossing and turning in bed. The paper below lays it out essentially, and is a classic in any case. Would you mind copying that for us:

- L. P. Hughston, R. Jozsa, and W. K. Wootters, "A complete classification of quantum ensembles having a given density matrix," *Physics Letters A*, vol. 183, pp. 14–18, 1993.

To get the result we need, we just consider the case where the density operator is proportional to the identity.

23-06-02 *Voice Recognition* (to C. M. Caves & R. Schack)

At the moment, I'm taking a few minutes from a frustrating morning to do a little web-surfing for fun. Reading about new voice-recognition technologies, I ran across the following. Apropos to our present situation, wouldn't you say?

> To understand the problem of speech recognition, one must merely compare written and spoken conversations. In a spoken conversation, we have hundreds of inflections that allow us to "read" further meaning into the words that are actually spoken. Those inflections are instructed by the intelligences at both ends of the conversation, by culture, by environment, by timing, by context, and a myriad other factors. When we write, it often takes many more words to make sure that the meaning is clear. Many times, a two or three sentence conversation can clear up a month's correspondence over some difficult point.

24-06-02 *Points* (to H. M. Wiseman)

You see, I'm catching up on my email finally. Thanks for the Nash pointer and the Aussie WWII statistics.

Wisemanism 11: *4. re. the processing power limitations on quantum states. You could have a look at my paper "Bayesian feedback versus Markovian feedback in a two-level atom" (Phys Rev A 2002, or* `quant-ph`*) to see what language I actually did use (I can't remember). But the point is that the state an experimenter computes on the fly to do feedback control is not necessarily less pure than the "true" state which the experimenter does not know. But the experimenter still wants to know how much his state is likely to be "in error" from the "true state". I guess so the experimenter armed with this knowledge would have a better state estimate, by accounting for the error by making the state more mixed. But in any case, how can you call the "true state" a state of belief or whatever if there is no one who believes it? It is what the experimenter's state of knowledge should be, given what they know. That is, it is their state of knowledge, even if they are too dumb to know it. This is another reason you may use to avoid the phrase "state of knowledge", but the challenge is still how to describe this "true state" in that case, without circumlocuting intolerably.*

What you are asking for is the experimenter to compare his pragmatic gambling commitments under the real world constraints he is living under at a given moment, to the beliefs or commitments he would possess under more ideal conditions (for example, if he had limitless computing power). Just as there is no "true" quantum state, independent of the agent—i.e., for two agents, there might be two quantum states—the same may be the case for a single agent. I like your examples because they help draw that out. I can contemplate how I should bet believing what I believe now and knowing that my feeble mind cannot analyze the full implications of those beliefs, or I can contemplate how I should bet believing what I believe now and imagining a supercomputer that draws out the proper conclusions from those beliefs.

In all cases, whatever the final products of all calculations and all approximations one writes down, those are one's gambling commitments. And *with respect to* that situation, that is one's "quantum state" for a system.

24-06-02 *The World is Under Construction* (to H. M. Wiseman)

Wisemanism 12: *Do you believe that events in the world really are random? Or do you believe they only appear to be random? In the first case, doesn't that mean that you have to believe in objective probabilities? ...*

Or are you saying that the real world is unanalysable, unthinkable even? Everything we say should be couched in terms of gambling commitments. First, that seems to be a cop-out, giving up on any understanding of the Universe. Second, it can't explain anything in the Darwinian way you mentioned, except Dutch-book consistency. It can't explain why it is "bad" to hold a gambling commitment based on the idea that all world cup soccer balls contain bombs that have a 50% chance to blow up every time a goal is scored ... You cannot say anything about animals that would have been likely to have gone extinct because of poor (but consistent) gambling commitments, because that is a statement using the concept of objective probabilities. You cannot <u>explain</u> *anything that is not strictly deterministic without using objective probabilities, it seems to me.*

I trust you understand my motives. I wouldn't bother discussing this with you if I didn't think your ideas were potentially revelational. What does not kill you makes you stronger.

For a couple of days I have been thinking about how to reply to the questions in your 6/14/02 email 'reality', but this morning I found myself significantly revising the response I had started to build. In particular, I decided to hardly reply to your questions at all! This may be a little bit annoying to you, but I think it will benefit our longer term discussion.

Of the three options you gave me for answering your questions (I only quoted the last one of the three above), I suppose if I were forced to choose one, I would align myself with the one you called a "cop-out." However, from my point of view, the language you use builds about the ugliest picture it can for where this effort is going. Indeed, you miss the very point, the very beauty, of the "cop-out." So, what I'd like to do is set that right—right here and right now—before we go much further. As advertised, in that way, I will not reply straightforwardly to your questions.

You see, the very starting point for most of my latest thoughts—the thing I think quantum mechanics gives us the deepest and most thorough hint of—is that there is no such thing as THE universe in any completed and waiting-to-be-discovered sense. The thought I am *testing out* is that the universe as a whole is still under construction. And when I say this, I am not thinking of just bits and pieces of it; I am thinking of the whole shebang, all the way to the roots. Nothing is completed. Not just the playhouse Kiki is building for Emma and Katie, or the evolutionary track of the human species, but even the "very laws" of physics. The idea is that they too are building up in precisely the way—and ever in the same danger of falling down as—individual organic species. That is to say, it's Darwinism all the way down.

So when you ask me if I am "saying that the real world is unanalysable, unthinkable even," the answer in a way is "yes." For it is blatantly impossible to analyze to the last detail the characteristics of a world that has not even been dreamt up (even in its own mind's eye).

But how can I impress this upon you, or even make it seem reasonable as a direction for research? That is a tough call. For, like with beer or single-malt Scotch, it is surely an acquired taste that builds only slowly and with the right company. Of course, I could just send you back to my paper `quant-ph/0204146` and ask you to take it very seriously. But this morning it dawned on me to maybe spend a little time with my scanner to try to IV some thoughts straight into your bloodstream.

At the moment, I can think of no better introductions to the line of thought I'd like to expose you to than three articles by Richard Rorty: "A World without Substances or Essences," "Truth without Correspondence to Reality," and "Thomas Kuhn, Rocks, and the Laws of Physics."[37] (Read them in that order, if you read them.) All three papers can be found in his collection of essays, *Philosophy and Social Hope*. If you absorb these, I think you'll understand completely what I'm up to, and why I so dislike the negative connotations you associate with the radical-Bayesian way of viewing the quantum state. Of course, it may not turn your head the way it turns mine, but at least you'll know where I'm coming from, and from what pool of enthusiasm I derive my strength to eschew the "golden nuggets" of *mere* quantum cosmology, *mere* Bohmianism, and *mere* "dreams of a final theory." The world as I see it is a much bigger place than those stories can tell. And the interpretational issues at the core of quantum mechanics strike me as our first rigorous indication that there is something more to this idea than simply the hopes and desires of an enthusiast.

[37]WARNING: Just because I say I can think of no better introductions to these ideas, it does not mean I endorse every statement in these papers; I may not endorse even half of them. However, I think these papers go in the right direction, even if they go too far ... and even if their arguments are far too weak. But I choose the papers I do because they are easy reading, with beautiful writing, and I suspect these thoughts are so foreign to you that if you can find any sense in *some of them*, then it may be a good start for a dialogue. Moreover, I continue to stress that the best justification yet to pursue this direction of thought—and this is something Rorty does not know—is quantum mechanics itself. So, rather than being the final words on things, these are just the beginning words on things.

For now, let me give you a flavor of the thoughts in these papers, and then leave you on your own in the case that you would like to pursue this further. The following quotes come from "Truth without Correspondence to Reality."

> In this essay I shall focus on Whitman's phrase 'counts ... for her justification and success ... almost entirely upon the future'. As I see it, the link between Whitmanesque Americanism and pragmatist philosophy—both classical and 'neo-'—is a willingness to refer all questions of ultimate justification to the future, to the substance of things hoped for. If there is anything distinctive about pragmatism it is that it substitutes the notion of a better human future for the notions of 'reality', 'reason' and 'nature'. One may say of pragmatism what Novalis said of Romanticism, that it is 'the apotheosis of the future'.
>
> As I read Dewey, what he somewhat awkwardly called 'a new metaphysic of man's relation to nature', was a generalization of the moral of Darwinian biology. The only justification of a mutation, biological or cultural, is its contribution to the existence of a more complex and interesting species somewhere in the future. Justification is always justification from the point of view of the survivors, the victors; there is no point of view more exalted than theirs to assume. This is the truth in the ideas that might makes right and that justice is the interest of the stronger. But these ideas are misleading when they are construed metaphysically, as an assertion that the present status quo, or the victorious side in some current war, stand in some privileged relation to the way things really are. So 'metaphysic' was an unfortunate word to use in describing this generalized Darwinism which is democracy. For that word is associated with an attempt to replace appearance by reality.
>
> Pragmatists—both classical and 'neo-'—do not believe that there is a way things really are. So they want to replace the appearance-reality distinction by that between descriptions of the world and of ourselves which are less useful and those which are more useful. When the question 'useful for what?' is pressed, they have nothing to say except 'useful to create a better future'. When they are asked, 'Better by what criterion?', they have no detailed answer, any more than the first mammals could specify in what respects they were better than the dying dinosaurs. Pragmatists can only say something as vague as: Better in the sense of containing more of what we consider good and less of what we consider bad. When asked, 'And what exactly do you consider good?', pragmatists can only say, with Whitman, 'variety and freedom', or, with Dewey, 'growth'. 'Growth itself,' Dewey said, 'is the only moral end.'
>
> They are limited to such fuzzy and unhelpful answers because what they hope is not that the future will conform to a plan, will fulfil an immanent teleology, but rather that the future will astonish and exhilarate. Just as fans of the avant garde go to art galleries wanting to be astonished rather than hoping to have any particular expectation fulfilled, so the finite and anthropomorphic deity celebrated by James, and later by A. N. Whitehead and Charles Hartshorne, hopes to be surprised and delighted by the latest product of evolution, both biological and cultural. Asking for pragmatism's blueprint of the future is like asking Whitman to sketch what lies at the end of that illimitable democratic vista. The vista, not the endpoint, matters.
>
> So if Whitman and Dewey have anything interesting in common, it is their principled and deliberate fuzziness. For principled fuzziness is the American way of doing what Heidegger called 'getting beyond metaphysics'. As Heidegger uses it, 'metaphysics' is the search for something clear and distinct, something fully present. That means

something that does not trail off into an indefinite future ...

and

So far I have been trying to give an overview of Dewey's place in the intellectual scheme of things by saying something about his relation to Emerson, Whitman, Kant, Hegel and Marx. Now I want to become a bit more technical, and to offer an interpretation of the most famous pragmatist doctrine—the pragmatist theory of truth. I want to show how this doctrine fits into a more general programme: that of replacing Greek and Kantian dualisms between permanent structure and transitory content with the distinction between the past and the future. I shall try to show how the things which James and Dewey said about truth were a way of replacing the task of justifying past custom and tradition by reference to unchanging structure with the task of replacing an unsatisfactory present with a more satisfactory future, thus replacing certainty with hope. This replacement would, they thought, amount to Americanizing philosophy. For they agreed with Whitman that America is the country which counts for its 'reason and justification' upon the future, and *only* upon the future.

Truth is what is supposed to distinguish knowledge from well-grounded opinion—from justified belief. But if the true is, as James said, 'the name of whatever proves itself to be good in the way of belief, and good, too, for definite, assignable, reasons', then it is not clear in what respects a true belief is supposed to differ from one which is merely justified. So pragmatists are often said to confuse truth, which is absolute and eternal, with justification, which is transitory because relative to an audience.

Pragmatists have responded to this criticism in two principal ways. Some, like Peirce, James and Putnam, have said that we can retain an absolute sense of 'true' by identifying it with 'justification in the ideal situation'—the situation which Peirce called 'the end of inquiry'. Others, like Dewey (and, I have argued, Davidson), have suggested that there is little to be said about truth, and that philosophers should explicitly and self-consciously *confine* themselves to justification, to what Dewey called 'warranted assertibility'.

I prefer the latter strategy. Despite the efforts of Putnam and Habermas to clarify the notion of 'ideal epistemic situation', that notion seems to me no more useful than that of 'correspondence to reality', or any of the other notions which philosophers have used to provide an interesting gloss on the word 'true'. Furthermore, I think that any 'absoluteness' which is supposedly ensured by appeal to such notions is equally well ensured if, with Davidson, we insist that human belief cannot swing free of the nonhuman environment and that, as Davidson insists, most of our beliefs (most of *anybody's* beliefs) must be true. For this insistence gives us everything we wanted to get from 'realism' without invoking the slogan that 'the real and the true are "independent of our beliefs" '—a slogan which, Davidson rightly says, it is futile either to accept or to reject.

Davidson's claim that a truth theory for a natural language is nothing more or less than an empirical explanation of the causal relations which hold between features of the environment and the holding true of sentences, seems to me all the guarantee we need that we are, always and everywhere, 'in touch with the world'. If we have such a guarantee, then we have all the insurance we need against 'relativism' and 'arbitrariness'. For Davidson tells us that we can never be more arbitrary than the world lets us be. So even if there is no Way the World Is, even if there is no such thing as 'the intrinsic nature of reality', there are still causal pressures. These pressures will be described in different ways at different times and for different purposes, but they are pressures none the less.

The claim that 'pragmatism is unable to account for the absoluteness of truth' confuses two demands: the demand that we explain the relation between the world and our claims to have true beliefs and the specifically epistemological demand either for present certainty or for a path guaranteed to lead to certainty, if only in the infinitely distant future. The first demand is traditionally met by saying that our beliefs are made true by the world, and that they correspond to the way things are. Davidson denies both claims. He and Dewey agree that we should give up the idea that knowledge is an attempt to *represent* reality. Rather, we should view inquiry as a way of using reality. So the relation between our truth claims and the rest of the world is causal rather than representational. It causes us to hold beliefs, and we continue to hold the beliefs which prove to be reliable guides to getting what we want. Goodman is right to say that there is no one Way the World Is, and so no one way it is to be accurately represented. But there are lots of ways to act so as to realize human hopes of happiness. The attainment of such happiness is not something distinct from the attainment of justified belief; rather, the latter is a special case of the former.

Pragmatists realize that this way of thinking about knowledge and truth makes certainty unlikely. But they think that the quest for certainty—even as a long-term goal—is an attempt to escape from the world. So they interpret the usual hostile reactions to their treatment of truth as an expression of resentment, resentment at being deprived of something which earlier philosophers had mistakenly promised. Dewey urges that the quest for certainty be replaced with the demand for imagination—that philosophy should stop trying to provide reassurance and instead encourage what Emerson called 'self-reliance'. To encourage self-reliance, in this sense, is to encourage the willingness to turn one's back both on the past and on the attempt of 'the classical philosophy of Europe' to ground the past in the eternal. It is to attempt Emersonian self-creation on a communal scale. To say that one should replace knowledge by hope is to say much the same thing: that one should stop worrying about whether what one believes is well grounded and start worrying about whether one has been imaginative enough to think up interesting alternatives to one's present beliefs. As West says, 'For Emerson, the goal of activity is not simply domination, but also provocation; the telos of movement and flux is not solely mastery, but also stimulation.'

and

It may seem strange to say that there is no connection between justification and truth. This is because we are inclined to say that truth is the aim of inquiry. But I think we pragmatists must grasp the nettle and say that this claim is either empty or false. Inquiry and justification have lots of mutual aims, but they do not have an overarching aim called truth. Inquiry and justification are activities we language-users cannot help engaging in; we do not need a goal called 'truth' to help us do so, any more than our digestive organs need a goal called health to set them to work. Language-users can no more help justifying their beliefs and desires to one another than stomachs can help grinding up foodstuff. The agenda for our digestive organs is set by the particular foodstuffs being processed, and the agenda for our justifying activity is provided by the diverse beliefs and desires we encounter in our fellow language-users. There would only be a 'higher' aim of inquiry called 'truth' if there were such a thing as *ultimate* justification—justification before God, or before the tribunal of reason, as opposed to any merely finite human audience.

But, given a Darwinian picture of the world, there can be no such tribunal. For such a tribunal would have to envisage all the alternatives to a given belief, and know everything that was relevant to criticism of every such alternative. Such a tribunal would have to have what Putnam calls a 'God's eye view'—a view which took in not only every feature of the world as described in a given set of terms, but that feature under every other possible description as well. For if it did not, there would remain the possibility that it was as fallible as the tribunal which sat in judgment on Galileo, a tribunal which we condemn for having required justification of new beliefs in old terms. If Darwin is right, we can no more make sense of the idea of such a tribunal than we can make sense of the idea that biological evolution has an aim. Biological evolution produces ever new species, and cultural evolution produces ever new audiences, but there is no such thing as the species which evolution has in view, nor any such thing as the 'aim of inquiry'.

To sum up, my reply to the claim that pragmatists confuse truth and justification is to turn this charge against those who make it. They are the ones who are confused, because they think of truth as something towards which we are moving, something we get closer to the more justification we have. By contrast, pragmatists think that there are a lot of detailed things to be said about justification to any given audience, but nothing to be said about justification in general. That is why there is nothing general to be said about the nature or limits of human knowledge, nor anything to be said about a connection between justification and truth. There is nothing to be said on the latter subject not because truth is atemporal and justification temporal, but because *the* **only** *point in contrasting the true with the merely justified is to contrast a possible future with the actual present.*

I don't have to tell you that I find these ideas tremendously exciting. It is not that nature is hidden from us. It is that it is not all there yet and never will be; 'nature' is being hammered out as we speak. And just like with a good democracy, we all have a nonnegligible input into giving it shape. That is the idea I am <u>*testing*</u> for consistency and utility. On the chance that it will lead somewhere, it seems to me, worth the gamble.

24-06-02 *Five Fuchsian Reasons for Rejecting a Line of Thought* (to H. M. Wiseman)

Wisemanism 13: *Yes, perhaps. But there is another challenge for you. If I understand Chris' latest to me, his response is to dismiss as impoverished the HEP's view of the world, rather than to try to incorporate it.*

I thought you might enjoy this. It is a list Rüdiger distributed during our last days in Brisbane.

Five Fuchsian Reasons for Rejecting a Line of Thought:

1. This question is not part of the foundations of quantum mechanics . . .
2. . . . and has no answer within quantum mechanics.
3. The question is ill-defined anyway . . .
4. . . . and, if formulated properly, would have a trivial answer.

5. And should it have a nontrivial aspect, working on it is a waste of time that really should be spent on proper foundational issues.

25-06-02 *Of Interest* (to G. Plunk)

This may be of some interest to us; I don't know.

> `quant-ph/0206169` [abs, ps, pdf, other]:
> Title: How to mix a density matrix
> Authors: Ingemar Bengtsson, Asa Ericsson
> Comments: 13 pages, 3 figures
>
> A given density matrix may be represented in many ways as a mixture of pure states. We show how any density matrix may be realized as a uniform ensemble. It has been conjectured that one may realize all probability distributions that are majorized by the vector of eigenvalues of the density matrix. We show that this is not true, but a marginally weaker statement may still be true. (70kb)

26-06-02 *The One Boolean Algebra Approach* (to I. Pitowsky)

I might as well let you know: I was one of the referees on your proposal to the ISF to study a Bayesian approach to quantum mechanics. Of course I was flattered by the prominent position you gave some of my papers, but, no matter, it is a good proposal. I wrote about the best report I could; now, all we can do is sit back and keep our fingers crossed that the other referees agree with me.

Let me make one further comment. I too used to think that the PBA approach was the way to go if one wanted to build up a theory along quantum logical lines. But now, I'm not so convinced of it. That is because I am starting to think that quantum mechanics is more analogous to the epistemological theory Richard Jeffrey calls "radical probabilism" than anything else. From that view, there are "probabilities all the way down" with one never getting hold of the truth values of *any* propositions. Rüdiger Schack and I just discovered a wealth of material on Jeffrey's webpage `http://www.princeton.edu/~bayesway/`.

In any case, I think what this leads to is that we ought to be focusing much more on characterizing quantum mechanics solely in terms of the "logic" of POVMs than anything else—these being the structures analogous to what crops up in Jeffrey's "probability kinematics." Thus, if one is looking to characterize PBAs, the best task might be to focus on the kinds of PBAs that POVMs generate, rather than the ones of Kochen and Specker based solely on standard measurements. (This may or may not have some connection to what people are calling "effect algebras" but I don't know.)

Beyond that, I am now of the mind that all one really ever needs for understanding quantum mechanics is a *single* Boolean algebra that is kept safely in the background (solely) for reference. The rest of the theory (and indeed all real-world measurement) is about probability kinematics with respect to that algebra. See Sections 4.2, 6, and 6.1 of my paper `quant-ph/0205039` for details.

Take care, and I'm so happy you find this approach interesting.

27-06-02 *Compatibility Never Ends* (to D. Poulin)

I apologize for taking so long to get back to you. My email box has just been running over lately, and on top of that I've been traveling a lot (just back from Australia, actually).

Poulinism 1: *About your approach: as far as I can see, the main difference (aside from the "vocabulary") between your derivation of the BFM compatibility criterion and theirs is that you are aware that it follows from* strong consistency*.*

No, I think the difference between us and them runs deeper than that. Let me try to express the point in a way that maybe I haven't used before. Concerning the BFM criterion: When BFM call it "necessary and sufficient" and when CFS call it "necessary and sufficient," we mean two very different things. For BFM the "necessary" part is enforced by the supposed existence of a super-observer, Zeno. That is, they mean that their criterion is "necessary" because they suppose a super-observer, Zeno, must exist. (See the final section of their paper.) What we (i.e., CFS) do instead is define a notion of "compatibility" based on the classical notion of "strong consistency" and show that the BFM condition is necessary and sufficient with respect to that. Beyond that, however, we leave the issue open.

That is, with regard to the question, "MUST the quantum states of two observers be compatible?," we make no statement in the paper. That is because, from our view—or at least mine in particular—there is nothing in nature that enforces that states MUST BE compatible. This is quite important if one wants to get a consistent Bayesian view off the ground. (I try to say all this in a lengthier and maybe more complete way in my samizdat *Quantum States: What the Hell Are They?*, pages 116–118, in a note titled "The Spirit of Gandhi." Actually, I think I say it better there; so have a look at that.)

Now, if there is nothing to enforce compatibility in the BFM sense, what would happen if two observers are incompatible with respect to this criterion? That's a fair and decent question, and I think where all the excitement begins. What the W criterion shows is that the two observers can just shake off their incompatibility and go forward (if they are in a congenial mood). That is, they can come to future agreement, not simply by pooling their prior beliefs, but by making an active intervention upon the system their states are about (i.e., by making a measurement). I view this as an extremely interesting property of the quantum world: it allows possibilities for going forward that the classical world does not.

On the other hand, if two observers are not in a congenial mood (i.e., will not perform a measurement of the W variety), then a "crisis" can ensue. What the full implications of that are, we don't know yet. But it may be even more exciting still.

Poulinism 2: *Maybe honesty can be related to strong consistency?*

Yes, you are right, and this is an important point. I discuss this at length in a note titled "The Commitments" on pages 133–138, and again on pages 192–193 in a note titled "More Balking." The upshot is, yes, strong consistency is deeply tied to honesty. The next question is, must one enforce honesty in a Bayesian approach to things? Your note implies that you tend to think, "yes." The pages I refer you to show that I tend to think, "no."

Poulinism 3: *Finally, while you relate the BFM condition to your ES, PP, W, and W', it is fundamentally different in spirit. This distinction is related to what we refer to as "type of knowledge". In our vocabulary, the BFM criterion is appropriate to compare knowledge of the quantum while the other are suited for quantum knowledge (just like quantum fidelity is). To me,* *the most convincing argument is the one of two nonorthogonal pure states: the "measurement criteria" will grant that they are compatible while, as we expressed it "if two observers claim to have complete knowledge of a system, their descriptions had better agree completely." Comparing states of knowledge and measurement outcomes predicted from these states are two different things. We have discussed a similar subtlety by comparing knowledge described by a density matrix and one described by a preparation: these are two different questions which deserve different answers although physically, they cannot be distinguished.*

It's a question of perspective. If one goes the full Bayesian route (which I am starting to do), then it is much better to call a quantum state a "state of belief or judgment," and not a "state of knowledge." For then, one finds that one is never inclined to say "their descriptions had better agree completely." There is nothing to enforce that, except possibly Darwinism. (I'm serious about that statement.) That is to say, when the world rears its head, someone with a firm belief might be wiped out, but in a Bayesian approach (where probabilistic statements always depend upon *subjective* priors), there is nothing to enforce compatibility beforehand. A misjudgment can only be declared a misjudgment after the fact, not before the fact (which in the quantum setting is created by the process of measurement).

27-06-02 *Probabilism All the Way Up* (to H. M. Wiseman)

Wisemanism 14: *Second, my wife, Nadine, wants to know why Kiki was building a playhouse while you indulge yourself in philosophy.*

I'd like to think we both do what we do best. But I suspect there's no philosopher out there who would say I'm doing good philosophy. (Kiki's artistry on the other hand is always a good hit.)

Wisemanism 15: *Now, more seriously, you say that my language "builds about the ugliest picture it can for where this effort is going". As I keep saying, I mean to be provocative. I hope it drives you to new heights in building a beautiful picture in response. Honestly I do see the beauty in your program. And I think the more extreme it becomes, the more beautiful it becomes. I am very interested to see where it ends up.*

Thanks for the compliment. And, indeed, your correspondence does drive me to new heights (of something). But now I worry that I offended you with my phrase "ugliest picture." It probably came off that way, but it wasn't meant to be an emotional statement or a point about you personally. If some emotion did slip into it, it most likely refers to a conversation I had with Harvey Brown, circa September 11 of last year. Harvey kept saying that I wanted to "doom" nature to being "ineffable." But that language carries such a negative connotation. It carries the idea that there is something there that we can never, or should never, attempt to speak of. So, when you said something similar in print, it gave me the opportunity to try to reply in print. (As you know, I try to have my thoughts recorded so I can refer people to them. One of the original ideas was that it would save me time that way; so far, that aspect of it hasn't worked out.) Anyway, as I made clear, I want to combat that with all my strength. In particular, the way that I am thinking about it, it is not a bad thing that there are some things beyond description in nature. Instead, it is just a statement that there are more things to come; it is a way of leaving room for something new.

Wisemanism 16: *As it happens, I don't have much of a taste for beer or single-malt Scotch. Also as it happens, I was reading a critique of Richard Rorty the very morning before I got your* *letter. Otherwise I never would have heard of him. It was a 1997 article by Alex Callinicos "Postmodernism: a critical diagnosis". The most interesting criticism in there was to say that Rorty "presumes what he needs to establish, namely that science and philosophy can be assimilated into literature. ... It is ... very hard in practice when trying to explain why one theory can be said to be more useful than another to avoid at least tacitly appealing to the idea that it captures how things are better than its rival does."*

Perhaps this is one aspect of Rorty you disagree with. But I wonder about your saying that quantum mechanics is the best justification for Rorty's philosophy, as if quantum mechanics is something you accept to be real, an "intrinsic nature of reality", the very idea of which Rorty explicitly rejects.

First, just a technical point. The philosophies I am most attracted to at present are those of James and Dewey and what James says about F. C. S. Schiller (but I haven't read Schiller himself yet). Rorty has donned himself to be the spokesman of those guys—and I don't mind that because he writes so nicely—but his writings also have a good admixture of the postmodernist ideas (of Foucault, Derrida, etc.) thrown into them to boot. This business about science not being more trustworthy or real than literary criticism presently strikes me as going too far.

But to Callinicos' point (give me the reference, by the way, and I'll read it)—"It is ... very hard in practice when trying to explain why one theory can be said to be more useful than another to avoid at least tacitly appealing to the idea that it captures how things are better than its rival does."—I would just reply, "Darwinism." And then, if that didn't sink in, I'd say, "Darwinism." The point is, from this conception, there is very little to say beyond that. Were elephants written into the blueprints of the universe? From the Darwinistic conception, they were not. Yet, the species fills a niche and has had a stability of at least a few million years worth. There is a sense in which an elephant, like a theory, is a "true" component in a description of the world. But that "trueness" only has a finite lifetime, and is largely a result of a conspiracy of things beyond its command (selection pressures). To put it another way, in contrast to Callinicos, the elephant doesn't "capture how things are better than its rival does" in any absolute sense—only in a transitory sense—but that doesn't take away from the functional value of the elephant today. So too, I am trying to imagine with theories.

Henry Folse, by the way, wrote me that there is something of a tradition with this evolutionary idea (beyond Rorty). So I've got a big reading list ahead of me: He tells me Toulmin, Kuhn, Kitcher, and van Fraassen.

Now, to quantum mechanics. You find something contradictory about my liking both quantum mechanics and Rorty. Here is the way I would put it. Presently at least, I am not inclined to accept quantum mechanics "to be real, an 'intrinsic nature of reality'," except insofar as, or to the extent that, it is a "law of thought," much like simple (Bayesian) probability theory. Instead, I view quantum mechanics to be the first *rigorous* hint we have that there might actually be something to James's vision.

I've already told you the history of this, haven't I? I gave a talk in 1999 at Cambridge on the quantum de Finetti theorem, after which Matthew Donald came up to me and bellowed, "You're an American pragmatist!" I didn't know what that meant really, but I kept the thought in the back of my head; I figured one day, I'd figure out what he meant. As it goes, that happened on July 21 of last year. I came across this book of Martin Gardner's of which one of the chapters was titled, "Why I Am Not a Pragmatist." (Part of the story is recorded on page 15 of my little samizdat in a note titled "The Reality of Wives." You might read it for a little laugh.) As I read it, it was like a flash of enlightenment. For every reason Gardner gave for not being a pragmatist, I thought about quantum mechanics and realized that indeed I was one. Donald was right after all; I am an

American pragmatist. And my further study of pragmatism has borne that out to a T.

My point of departure, unlike James's, was not abstract philosophy. It was simply trying to make sense of quantum mechanics, where I think the most reasonable and simplest conclusion one can draw from the Kochen-Specker results and the Bell inequality violations is, as Asher Peres says, "unperformed measurements have no outcomes." The measurement provokes the "truth value" into existence; it doesn't exist beforehand. Now, go off and read about James's and Dewey's theory of truth and you'll find almost exactly the same idea (just without the rigor of quantum mechanics). And similarly with lots of other pieces of the philosophy.

So, I view quantum mechanics as the hint of something much deeper. But the full story is not yet told. That is, quantum mechanics strikes me as being to our community what the Galapagos Islands were to Darwin—just a hint of something bigger.

Wisemanism 17: *You and Rorty I guess would agree that "dreams of a final theory" will never be more than dreams. I guess that idea does not worry me as much as it would some physicists, but it does seem like a defeat. But perhaps that just says something of my personality. How much of a role does personality play in one's preferred philosophy?*

Your question is a good one, and one I worry about a lot. Where your knee-jerk reaction is defeat, mine is one of unlimited possibilities and newfound freedom. On a similar issue, James put it like this:

> The history of philosophy is to a great extent that of a certain clash of human temperaments. Undignified as such a treatment may seem to some of my colleagues, I shall have to take account of this clash and explain a good many of the divergencies of philosophies by it. Of whatever temperament a professional philosopher is, he tries, when philosophizing, to sink the fact of his temperament. Temperament is no conventionally recognized reason, so he urges impersonal reasons only for his conclusions. Yet his temperament really gives him a stronger bias than any of his more strictly objective premises. It loads the evidence for him one way or the other, making a more sentimental or more hard-hearted view of the universe, just as this fact or that principle would. He *trusts* his temperament. Wanting a universe that suits it, he believes in any representation of the universe that does suit it. He feels men of opposite temper to be out of key with the world's character, and in his heart considers them incompetent and 'not in it,' in the philosophic business, even though they may far excel him in dialectical ability.
>
> Yet in the forum he can make no claim, on the bare ground of his temperament, to superior discernment or authority. There arises thus a certain insincerity in our philosophic discussions: the potentest of all our premises is never mentioned. I am sure it would contribute to clearness if in these lectures we should break this rule and mention it, and I accordingly feel free to do so.

But I think the disparity between our views is in better shape than that. I think you're only seeing the program "physics is the ability to win a bet" as a defeat because—even if you don't know it—you're working within a kind of Kantian mindset. That the universe is already formed and there; that there is an "a priori." Anything that can't be said about the universe is then most surely a loss or limitation. But, I think once you see that what the pragmatist is trying to get at is not that, maybe your heart will change. Physics as the ability to win a bet will strike you as something immensely positive. Physics is like that because reality is still forming, and the Darwinistic component (along with the "non-detachedness" of the observer in quantum mechanics) indicates that it may be somewhat malleable. From that point of view, to have "dreams of a final theory" is almost like admitting defeat.

But given what you've said, maybe you're already starting to feel some of this. And that tickles me immensely.

Wisemanism 18: *To conclude, I can (or rather could) accept a lot, or even all, of what you and Rorty are exploring. But I am still not emotionally or intellectually compelled to do so. And I am really not sure whether I want to be compelled in one direction, or whether I want to be able to contain conflicting philosophies. I have this idea that there is an incoherence at the heart of things. Irreconcilable levels of description. Profound truths being the opposite of other profound truths. Incompleteness theorems. That sort of stuff.*

Understood. I know that there's nothing worse than an evangelist knocking at your door on a Saturday morning. Feel free to not reply to this note at all. In the mean time, I'll try to do my best to do what I really ought to be doing: proving theorems, simplifying the quantum axioms, trying to find real-world physics problems for which this view is the most powerful way to tackle it, etc., etc. One thing physicists never deny is a better method for solving a problem.

28-06-02 *To Believe to Know* (to D. Poulin)

Poulinism 4: *Otherwise, you get incompatible statements with which Bayesian theory cannot deal (like computing $p(x|y)$ when y is assigned probability 0).*

It's starting to sound like we mean two different things by Bayesian theory. See, `http://www.princeton.edu/~bayesway/KC.tex.pdf`, slide #12 (on page 13 actually). More seriously, see: `http://cepa.newschool.edu/het/essays/uncert/subjective.htm`, especially the parts on de Finetti and Ramsey.

Poulinism 5: *I think that most of the discussion is based around this distinction: "state of knowledge" and "state of belief". I would say that states of knowledge must be (BFM) compatible while states of belief can be incompatible. How are these two things defined? Well I would say that a state of knowledge is built with the help of an initial state on which everybody agrees, the postulates of quantum mechanics, and "public actions".*

I will agree to your definition of "state of knowledge." But, backtracking from that, an initial state upon which everyone agrees? If one is taking a subjectivist approach (or what I had been calling a "Bayesian approach") to interpreting the quantum state, there is nothing in nature to enforce an initial prior agreement. God does not come down from on high and say to all the agents (i.e., all the observers), "Your starting point shall be the quantum state Ψ." Everyone is left to fend for himself.

That is to say, in the language of the second webpage I sent you to above, in a world where the quantum state is not presupposed to be objective—as had been Mermin's goal when he started this exercise—there is nothing to enforce the "Harsanyi doctrine."

28-06-02 *The Harsanyi Doctrine* (to C. M. Caves & R. Schack)

I just happened to run across the following discussion:

> Finally we should mention that one aspect of Keynes's (1921) propositions has re-emerged in modern economics via the so-called "Harsanyi Doctrine"—also known as the "common prior" assumption (e.g. Harsanyi, 1968). Effectively, this states that if agents all have the same knowledge, then they ought to have the same subjective probability assignments. This assertion, of course, is nowhere implied in subjective probability theory of either the Ramsey / de Finetti or intuitionist camps. The Harsanyi doctrine is largely an outcome of information theory and lies in the background of rational expectations theory—both of which have a rather ambiguous relationship with uncertainty theory anyway. For obvious reasons, information theory cannot embrace subjective probability too closely: its entire purpose is, after all, to set out [an] objective, deterministic relationship between "information" or "knowledge" and agents' choices. This makes it necessary to filter out the personal peculiarities which are permitted in subjective probability theory.

28-06-02 *Who Would Have Thunk?* (to N. D. Mermin)

Who would have guessed that all we were debating in our ill-fated correspondence on the BFM criterion was simply the validity (or maybe the jurisdiction) of the "Harsanyi Doctrine"? Not me! But that's what I found out this morning quite by accident.

If you want to read the story leading up to this, download my mini-samizdat "Quantum States: What the Hell Are They?" and look at the notes to Poulin on pages 215 and 219. I now find this quite interesting actually. Harsanyi won the Nobel prize with Nash, and there seems to be a bit of literature on his doctrine.

By the way, you never responded to (the main content of) my December 10 note "The Spirit of Gandhi." Here's your chance before I close the doors on this chapter of my life. (I.e., I'm going to bind up and close this mini-samizdat by the end of the day. The next one is going to be titled, "Darwinism All the Way Down.")

29-06-02 *Old Boole* (to I. Pitowsky)

Pitowskyism 1: *Anyway, the Bayesian Quantum Probability paper is much better than my research proposal (which was written with some haste, but this is a different story). I'll send you a copy when it's in a reasonable shape. I use projection operators, but have no prejudice against POVM's, only I'm not clear how to use the gambling language with them (we talked a bit about this problem in the Växjö conference).*

You play exactly the same game you did in your proposal, except now that a complete set of mutually exclusive events are identified with the elements in a POVM. I.e., *stop* thinking of POVM elements as non-mutually-exclusive events. (Well, I don't know that you haven't already abandoned that line, but much of the community tends to call them "unsharp" measurements and such. And that just creates the wrong imagery. They are sharp as anything else from this view.) These elements correspond to measurement "clicks" and are thus mutually exclusive. From my standpoint, the "clicks" are the closest we can come to grasping a "truth value." And thus that is the formal role they take on.

Pitowskyism 2: *The slogan "probability all the way down" is great. One should even make a sharp distinction between "probability equals unity" and truth.*

Yes. And that is what the better part of my samizdat "Quantum States: What the Hell Are They?" posted at my website (link below) is about. You might enjoy thumbing through it. In my own view, it leads one along the lines of something like a Jamesian theory of truth … but hopefully with the opportunity to make it more rigorous now.

29-06-02 *Incompatible Beginnings* (to D. Poulin)

Poulinism 6: *Maybe requiring a single description on which everyone agrees is a bit too strong, but something has to be imposed.*

The whole point of the research program I am building up, and the point of most of the 220 pages in *Quantum States: What the Hell Are They?*, and much of the point of my paper `quant-ph/0205039`, is that I don't buy that. Remember the line in the Pink Floyd song, "Mother's going to put all her fears into you."? The point is, none of us are born with "rational" views. We start out in life with whatever our community pumps into us. The mark of "rationality" instead is how we change our views in the light of evidence, and how we gamble based on what we believe. That is what Dutch-book coherence is about, for instance.

So, when I say "agents are left to fend for themselves," I mean it. Every now and then agents start out compatible in one sense or another and then they stay that way, indeed … perhaps with ever more similarities between their distributions as data flows in. But that is not the norm. Instead most of the time, there is some incompatibility in our world views lurking in the background. We can live with that as long as nothing comes to light to challenge our views. But when the opportunity arises, there will be a "crisis," as Rüdiger Schack likes to say.

At that point, as you say, Bayesian theory can't handle things. But the view I am shooting for is that Darwinism can. That is, if you have a firm belief (i.e., a probability 1 ascription) for something, by Dutch-book rules you are willing to bet your whole bank account on the event. If the event doesn't occur, then you have lost your whole bank account, and in a way your life.

The program, as I see it, is to see how rigorous and fruitful on can make that line of thought.

01-07-02 *Nose Jobs* (to T. Rudolph)

Rudolphism 7: *Yah – finished Rorty, I'm actually rereading a bit of the stuff near the beginning and the section on Weinberg/Kuhn etc. I'm not entirely satisfied with the way he answered Weinberg … not that Weinberg's statements were that great, which is the reason I think Rorty could have been stronger.*

Agreed. (Clearly there was some emotion behind it that got out of hand.) But funny, on my own second reading, I liked the reply better.

I posted some stuff in that connection in the form of some letters to Howard Wiseman. You might be interested. They can be found near the end of my "Quantum States: What the Hell Are They?" on my webpage.

One thing I need to understand better (and not just trust Rorty on) is Donald Davidson's theory of truth. But my reading list is a mile high.

05-07-02 *The Physics of Floyd* (to M. J. Donald)

I said I would be back; I apologize for the delay.

Donaldism 5 : *Your know-nothing ism, like de Finetti's irrationalism (Gillies, "Philosophical Theories of Probability", page 86), have the dangers of Bohr's writings on which I would agree with Beller (Physics Today, September 1998, pages 29 - 34). In particular, by leaving far too much in vagueness, incoherence, and pious hope, you give the religiously-minded the official endorsement of the physics establishment that they may believe anything they want, instead of, by example, instructing them that they can believe anything they want as long as it is rational, coherent, tentative, revisable, and compatible with the evidence (and therefore contrary to naive expectations, because if quantum theory, or indeed science in general, tells us anything it is that the world is not how we would have imagined it before we investigated); and they accept that they may be completely wrong.*

In saying this, you demonstrate that you have read Gillies page 86. I'm glad I know that. But, as I tried to convey before, I think you show a deep misconstrual of de Finetti's program (or radical subjectivism, or radical probabilism, or whatever you want to call it, even irrationalism). I never tell my girls that they can believe anything they want. I always try to be as impartial as possible, but in the end, I'm sure all I really teach them is to believe what *I* believe and what the community around me believes. That's where they take their start in life.

And so it is with all of us. Pink Floyd recorded a great lesson with the words, "Mother's going to put all her fears into you." De Finetti's theory of probability is simply a stark recognition of this fact and a stark recognition that there is no getting around it. The point is, none of us are born with "rational" views, and a particular opinion at any one moment is *neither* rational or irrational. The mark of "rationality" is instead in how we change our views in the light of evidence, and how we gamble based on what we believe. This is what "coherence" and Bayesian updating are about. When Mother has put such a bad opinion into us that rationality cannot realign it enough for us to survive, then we won't survive, and that opinion will not propagate.

It is not "anything goes" here any more than it is for the species in the animal kingdom (Darwin). If you see in my writings a hint that anything goes, then it seems to me you are fighting a battle with someone else and hoping I will fill his shoes. The mathematical side of "radical subjectivism" is an attempt to quantify the ideas above and to show their consistency and utility. It is what the 586 pages of Bernardo and Smith's book *Bayesian Theory* is about; example after example that rationality is not in the prior, but in the conditionalization and in the coherence.

Where you think you need a law-driven reality to explain the success of our scientific activity, I think all I need is (something along the lines of) de Finetti and Darwinism. The most important point of distinction between our views is in how we are willing to mark the "badness" of an opinion/belief/judgment. I say it is only possible to mark a judgment as a misjudgment—in any objective way—after the fact, after the event to which it was directed has either occurred or not occurred. Whereas you want to think there is something that defines the badness or goodness of a judgment before the event.

This is the great gulf between us. I say, "unperformed measurements have no outcomes." "The universe is in the making." You say, if I'd just step back far enough—until I'm outside of the universe, actually—I'd see that the universe "is." It's already there. It's already complete and waiting for me to draw a precise picture of its smooth surface. And in the dimples of that smooth surface, I would see which of my actions are right and which of my actions are wrong long before they take place. Indeed, in that timeless ground state, I would see that everything I'm talking about is folly.

There's no denying that there is something about the former picture that piques the lascivious side of my character. But the problem is deeper and more openly communicable than that. A couple of times in your notes, you have said something to the effect,

Donaldism 6 : *I do hold as an article of faith that there is a true view, although I accept that we may not ever know it, let alone ever know that we know it.*

If we may never know it, or even know that we know it, then I cannot see that it plays any normative role whatever in helping us get along in life. Its explanation of the phenomena about us is little more than a hollow promise. The argument is a minor modification of J. S. Mill's argument against substance:

> If there be such a *substratum*, suppose it at this instant miraculously annihilated, and let the sensations continue to occur in the same order, and how would the *substratum* be missed? By what signs should we be able to discover that its existence had terminated? Should we not have as much reason to believe that it still existed as we now have? And if we should not then be warranted in believing it, how can we be so now?

Just change "substratum" to "immutable law."

There, I've said my piece. And I've probably said too much in too condescending a way: For that I apologize (almost). But I wanted to make a point and make it forcefully. My purpose is not to give fuel to the religious fire-burners. It is to change our conception of how the law-like stability we see around us arises, in a way that does justice to what I view as the great lesson of quantum mechanics. Quantum mechanics, that is, as viewed from inside the world, where we actually live, instead of from some Everettic fantasyland (where 1957 stands out as the most atypical year ever, the year man first saw the universe in its entirety and confirmed what the rationalists had suspected for so long: its image is so bleak as to fit within the mind of a single man). The mark of pious hope is in the belief of miracles: I don't believe in miracles or miraculous years.

Is all that I've said vague? Of course it is. But that is why this is a research program rather than a completed product. There is so immensely much to do. Carl Caves instilled in me long ago that the good physicist is the one who poses a problem he can hope to just solve in the span of his lifetime. That's the way I see this program. With sustained determination, some creativity, and the help of a lot of friends, I see it as actually going through (and carrying us to a vista that is almost unimaginable now).

Some of the best help we could probably get would be the mind of Matthew Donald. But that might require a midlife crisis! (And I would hate to think of what that might do to your wife and children.)

Now let me address some of your other comments.

Donaldism 7 : *Actually, it was great fun writing: I've always wanted to be allowed to say "Nope", but I've never got away with it before.*

Are you saying that you find it easier to let your hair down around me than with your Cambridgean/Oxfordian contemporaries? If so, then I will reinstate the image of the romantic and mustachioed revolutionary just for you. What the whole movement needs is a little letting down of the hair; glad to be of service.

Donaldism 8 : *page 9: "the final state itself for B cannot be viewed as more than a reflection of some tricky combination of one's initial information and the knowledge gained through the measurement." To what extent then is the "final state for B" actually "for B" and to what extent is it "for" the observer?*

similarly, on page 12, in the last paragraph of section 3, can we say that the system "has" the state $|\psi\rangle$, or should we say that for Alice the system has the state $|\psi\rangle$?

A later point in your note indicates that you know I would say "for the observer" here. Indeed that is better language. Sometimes it's just hard to make the right rhetorical judgment; sorry if I caused you confusion. My intention instead was to put one foot into the enemy camp, and try to fight on their terms. That is to say, my intention was to draw an absurdity from associating a quantum state with a system rather than with an observer's judgment.

Donaldism 9: *footnote 12: which view gives the most coherent, consistent, plausible explanation of the most phenomena? which view is true?????*

I exasperate you, don't I? The point is, I have my reasons for refraining from that word, and I tried to explain some of them above.

Donaldism 10: *page 22: I'm not sure what to conclude from section 4.2. This is all that comes to mind:*

Somewhere in the classical division of the BIPM is a die testing machine. It is used to throw a die sufficiently many times to provide a reasonable estimate of the probability of each face. It shares with its quantum analogue the features that in general the probability of any face is never zero, and that many trials are needed to provide useful statistics.

If it were a good classical die testing machine—that is, if it really did the same thing each and every time—then the toss would always come out exactly the same. That is to say, there are no good classical die testing machines.

Donaldism 11: *page 24: Are POVM's really "the structure of our potential interventions"? All of our potential interventions? Our most important interventions? The structure of a two-year old child's interventions? Or are they just one particular "very carefully contrived way by which we can sometimes manage to see fairly directly into part of [reality's] deep structure".*

The way I would say it now is this. Our interventions are whatever they are. Even a two-year old's. The concept of our interventions having a structure is something we lay on top of them. To that extent, the use of POVMs as a formal descriptive device is a normative one. I would say the same thing for our general use of probability theory. That is to say, we don't use the probability calculus in making all our decisions, but we ought to. Bernardo and Smith put it this way:

> What is the nature and scope of Bayesian Statistics within this spectrum of activity?
>
> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."

Similarly I would say of POVMs. If you want to do your best reasoning concerning the consequences of what you've just done (or what you are about to do) then you should use the calculus of quantum mechanics. And that says lay a template down over what you've done (or what you are about to do) that happens to have the shape of a POVM.

Donaldism 12: *page 34: "Quantum measurement is nothing more, and nothing less, than a refinement and a readjustment of one's initial state of belief." But it is something more. It is also the result of a physical process.*

No, I would say, the result of the physical process is something else. It is a creation of sorts. The structure of quantum mechanics, I would say, has something to do with what I can say about that creation ... thus my refinement and readjustment. But concerning the real stuff of the world, the theory hardly goes there. (And that's what I try to say, for better or for worse, in my `quant-ph/0204146`.)

Donaldism 13: *The idea in many-worlds interpretations is to drop von Neumann's process 1 (collapse of the wavefunction). In section 6, it looks as if you're intending to drop process 2 (unitary time propagation). But, the A_{di} in (63), or the Π_d in (65), should be derived from the physics of the measurement; no doubt using environmental decoherence at some point.*

Yes, I do intend to drop process 2 as being something more fundamental than process 1. I keep looking for ways to do that that I find completely convincing. From my view, process 2 is just a special case of process 1, where the POVM is a one-element set. The word "should," you should realize, is little more than a cultural statement ... from a cult that I do not buy into. The phrase "no doubt," on the other hand is a statement of faith. The reasons for your use of that phrase I will leave as an exercise in self-reflection.

Donaldism 14: *Probability theory is only a law of thought in as far as it describes how one should deal with new information. (vN process 1). But physics should also be able to describe how one acquires new information. (vN process 2).*

Ditto.

Donaldism 15: *It is the physical laws and initial conditions which make an agent's beliefs about the forms and the probabilities of possible future experiences either right or wrong given his past experiences.*

I told you that was the gulf between us.

Donaldism 16: *The rational assignment of a POVM to a given experimental device is not a matter of free choice.*

I claim it is; at least initially so. The rationality lies purely in coherence and updating, not in particular assignments. But clearly it's going to take a lot of inculcation into my culture to convince you of that. I know it won't help, but I'll throw this out for evidence's sake that I take your skepticism seriously: Just as there is a de Finetti theorem to make sense of "unknown states," there is a de Finetti theorem to make sense of "unknown measurements." (This is a result Schack, Scudo, and I will be posting soon.)

Donaldism 17: *The strict quantum Bayesian of the Fuchsian persuasion, however, faces the quagmire that physical laws are as subjective as probabilities, so that there is nowhere to start (not even with nothing!).*

Darwinism. Darwinism. Darwinism. Darwinism. It's presently the best nothing I've ever known.

Donaldism 18: *page 51: Hilbert space dimension, of the small integer type you are referring to, is itself only an effective concept, which depends on not looking too closely at the systems studied. For example, D=2 arises when you consider a photon which may hit one of two detectors, and ignore the full (infinite-dimensional) photon-detector space. Therefore D also is state and agent and context dependent and subjective.*

Absolutely. But it is of the harmless type that fixing a classical phase space is: and that's the point. Any classical (real-world) pendulum has more than one degree of freedom, but once the context is set, once the approximation is made, the human interests that set that context and made that approximation can be safely forgotten. This is what I envision for Hilbert-space dimension (but not the quantum state).

Let me thank you also for the other uplifting note you sent me. Despite my own combative reactions (as witnessed above), I do find my conversations with you valuable. And in ways, you are partially responsible for the creation of this monster. (In fact, I recently set to paper the story about your involvement. In case you're interested, I'll narrow the search for you: it's somewhere between page 202 and 221 of the mini-samizdat "Quantum States: What the Hell are They?" posted on my webpage. If you wade through that, you'll get the full impact of the story. By the way, I'll be shutting the doors to that samizdat soon. I'll equip it with an index, etc., like I did with the last one, but I won't post it on `quant-ph` this time, only my webpage. After that, I intend to start constructing a new one titled "Darwinism All the Way Down." The present letter to you will be my first entry.)

Finally, let me end with a long quote that I pulled from Richard Jeffrey's paper, "Reading Probabilismo." You inspired me to copy it into my machine.

> There is a most instructive contrast between de Finetti's "irrationalist" probabilism and Carnap's rationalist positivism.
>
> For Carnap, probabilism was a fallback position from what one might call dogmatic rationalism, i.e., the view that scientific forecasts and universal hypotheses ought to be logically deducible from *Protokollsätze*. Probability theory was to replace deductive logic; dogmatic rationalism would give way to probabilistic rationalism. Rationalism itself was seen as essential to empiricism. The probability $c(h,e)$ of a scientific forecast or hypothesis h relative to a sentence e that reports the totality of one's empirical evidence needed to be independent of who one might be, provided only that one were ideally rational: qua scientist, an ideally rational being would be individuated only by e, the report of that being's total individual experience to date. If c as well as e could vary from scientist to scientist then $c(h,e)$ would represent a subjective judgment. The only scientific basis on which the $c(h,e)$ values might vary must appear in the second argument-place, not in the function c itself, i.e., such a basis must be empirical. Logical empiricism was wedded to that sort of rationalism. *That* sort of rationalism—not the rationalism of the bogeyman who thinks he can predict the future by pure reason, or can prove that space must be euclidian, a priori.
>
> De Finetti's probabilism was "irrationalist" in denying the possibility of an intelligible split between reason, represented by the conditional probability function c, and experience, represented by the total observation-report e. "Anti-rationalist" would have been a less provocative term. But if de Finetti's probabilism was anti-rationalist it was anti-empiricist as well on the same showing, and "anti-empiricist" has the same ring

of madness that "irrationalist" has. The suggestion is that an anti-empiricist opposes observation and experiment, just as an irrationalist is against being reasonable and thinking things through. What's rejected, though, is neither experience nor reason, but the dichotomy underlying logical empiricism, according to which the two can be cleanly separated—say, into e and c. de Finetti rejects as untenable the basic dogma of logical empiricism: the dualism between reason and experience.

05-07-02 *Coherence Everlasting* (to H. M. Wiseman)

Wisemanism 19: *The comment by van Enk and Fuchs on Ref. [1] can be understood as an explicit calculation showing (part of) how a laser beam, without an absolute phase, can function as a clock; how the phase information can be distributed and how there is no harm in regarding the phase as real. This is essentially the same point originally made by Molmer, that laser phase is a "convenient fiction".*

No. Our point was not even remotely the same as Klaus's. Klaus was concerned with an intracavity mode, where I would say there is no reason whatsoever for preferring one decomposition of the density operator over another. This contrasts with the multi-mode system we were concerned with. With respect to that *tensor-product structure*, there is a preferred decomposition, and it involves coherent states. If the modes are believed to be exchangeable, as they standardly are with continuous-wave laser light, then the de Finetti theorem dictates that there is a unique decomposition of the big multi-mode density operator into a probabilistic mixture of pure states, each of which preserves the exchangeability property. Yes, it is a convenient fiction, just like when anytime one has a density-operator description of a system, it is PURELY a fiction to furthermore think of that state as composed of a deeper (more real) set of pure states.

The point of the expansions of a density operator—as I tried to make clear in my lecture at Griffith—is not to talk about "what is there," but rather to tell something about the potential measurements one can perform on a system, and what those measurements will teach us. I.e., what predictive value they will give. Measurements correspond to decompositions. Full stop. There is nothing deeper than that. What is significant about the de Finetti expansion of a laser beam is that it tells you that IF you can measure the phase of one mode, THEN the remainder of the modes will be describable via a simple, pure product state. If one were to think of the multi-mode case as a mixture due to an original mixture of intracavity photon-number states, one would have to work like hell to see that. If that is not a reason to prefer one valid decomposition over another, I don't know what is. One thing is for sure though: It does not endorse the *reality* of any decomposition, number-state, coherent-state, or otherwise.

07-07-02 *And Continuing Consternation* (to H. M. Wiseman)

Wisemanism 20: *Actually I don't think that is so important, if you mean the multimode versus single mode thing. I hope you don't mean they should have written down a statistical mixture, because then I would really disagree with you.*

Yes, that is exactly what I meant. Sargent, Scully, and Lamb, in their Chapter 17, do a perfectly fine calculation, and the result is a mixed-state density operator. It represents the laser operator's state of knowledge about his laser field, given some standardly agreed upon information. To think there is a purer state of affairs—i.e., to think that there is an "unknown state" over and above his mundane description—is to go against the grain of everything I (and Caves and Schack, and Peres, etc.) have been striving for in the interpretational game. Those pure states that lie underneath a density operator are pure artifice from my point of view.

Wisemanism 21: *Well you certainly do not make that clear in your paper. On my reading, you are accepting Rudolph and Sanders' idea that there is such a thing as a real coherent state. That before you measure it, the laser is in a statistical mixture, and after you measure it, it isn't. I am saying there is no need to ever write down a statistical mixture for the laser. The laser can be its own clock, and hence (ignoring phase diffusion) it is in a coherent state.*

Then, you carry a lot of baggage into your reading. (Perhaps as I carried a lot of baggage into my reading of your comment.) A few notes ago, you wrote the following:

Wisemanism 22: *After looking at it with Carl and Rüdiger, I take back what I said to Chris about the paper having sneaked in objective probabilities — I was misled by reading "probability" as "objective probability".*

I would suggest you are doing something similar with my paper with Steven. The idea that "that before you measure it, the laser is in a statistical mixture, and after you measure it, it isn't" is now a foreign concept to me. And it was already a foreign concept by the time Steven and I wrote those two papers.

I just re-read both papers, the PRL and the QIC version, and though they are written in a more neutral way than you usually see from me—Steven did the bulk of the writing—I still find none of the thoughts you thought you saw there.

Wisemanism 23: *An electronic oscillator is no more real than an optical oscillator.*

Agreed. The issue in my mind is one of a chain of inference. Given the previous ascription of a multi-mode mixed state, what pinging of the quantum world (i.e., what measurements) will allow one to refine that ascription to a pure state for some of the remaining (unmeasured) modes? We think we know how to ping the electronic oscillator, and we draw a chain of inference. That's all. But without the electronic oscillator as an intermediary, we are stuck with the mixed state ascription. (Notice, I keep using the word ascription, not description.)

08-07-02 *Short First Reply* (to H. M. Wiseman)

Wisemanism 24: *Hey, I think I do understand the quantum de Finetti theorem. If you have an infinite sequence of systems you are happy to swap around in any way then it is always possible to represent your state of knowledge as being AS IF there is an unknown quantum state common to all of them. It's not that complicated. That's not to say of course that I could reproduce your proof, so to avoid being offended, I could assume that being able to do that is what you mean by "strong understanding". On the other hand, your statement could hardly have been said unless you thought I had demonstrated in the past a weak understanding of the issues (e.g. my not understanding what is so terrible about the idea of an unknown preparation procedure) then my defence would be that I believe I am in quite good company.*

Yes, it is the latter (i.e., being in "quite good company") that I meant. So, I really only mean the points made in the introduction of that paper.

Wisemanism 25: *For an optical experiment the laser is the clock, and therefore it is in a pure coherent state.*

Our agreement lasts for the first part of this sentence. Where we part, is in the "therefore."

Wisemanism 26: *All quantum states are pure artifice, full stop. They are in the observer's head — what can be more artificial. I thought that's what you taught me. If I say a laser is in a pure coherent state, how can you say I'm wrong?*

True enough. My point is only one about consistency. If one *accepts* the standard derivation, say of Sargent, Scully, and Lamb, Chap. 17, then one has NO RIGHT TO GO FURTHER and say, "well, there's really a coherent state there after all, and I just don't know it." One is simply not being internally consistent. If, on the other hand, one wants to believe that the field should be described by a pure coherent state, that is fine by me, but then one should know what that state is (otherwise, to my post-1999 ears, they are speaking nonsense).

Wisemanism 27: *What other possible meaning can be taken from this quote: "This measurement would create an optical coherent state from a standard laser source for the first time."*

> **create** (krê-ât') verb, transitive
> created, creating, creates
> 1. To cause to exist; bring into being.
> 2. To give rise to; produce.
> 3. To invest with an office or title; appoint.
> 4. To produce through artistic or imaginative effort.

13-07-02 *The Fabric of Sexuality* (to A. Plotnitsky)

Plotnitskyism 3: *As I was reading your Växjö piece, indeed already as I started to read it at Cornell, one impression it made on me was a bit peculiar. The very concept and terminology of "quantum states" appeared to me (in part because of an appeal to a "state") to be a remnant of a more conventional ontological view, and your argument seemed to have difficulty to adjust the concept to the view advanced by you in your article and elsewhere. Almost immediately, however, I recalled the discussion in "Quantum States," where you speak, I think rightly, of the quantum state as a state of belief rather than knowledge, although "state of expectation" or "probabilistic estimates" may even be more precise. (I agree that "knowledge" is better suited to refer to the outcomes of measurements already performed, even though, at least in my view, both knowledge and expectation concerns only what is manifest in measuring instruments, never anything pertaining to quantum objects themselves.)*

Yes, I see your point and I agree with it. It is a remnant. But so far, I've had no stroke of inspiration for a better term.

Plotnitskyism 4: *It seems to me, however, that, rather than of QUANTUM STATES, it would be more accurate to speak of the STATE VECTORS (the ket-vectors defined by the psi-function in a corresponding Hilbert space) as representing or reflecting the state of expectation. I do not think one could speak of states vectors themselves as states of expectation, but only as a mathematical device representing information related to our expectations concerning the outcomes of certain possible experiments or events. One might in fact call them "EXPECTATION VECTORS."*

And actually this one doesn't do it for me either. It's hard to pinpoint why, but I guess mostly I would be searching for a more philosophically evocative term. Secondary to that, you use the word "vector," and it is crucial to my point of view that there is nothing more fundamental about a pure state (i.e., a state vector) than a general density operator. Thus, perhaps, "expectation operator" might be more to my taste, but along the point I just made, I really would like something more evocative than that.

Right now, "belief" still does the most for me—because I am so taken with William James's essay "The Sentiment of Rationality"—quantum states are, in part, those things that he talks about there—but I agree with you that the terminology ought to go, once something better comes along.

Plotnitskyism 5: *With these qualifications in mind, however, "quantum state" may be a possible conception and perhaps even a necessary one (especially in the EPR-type situations), insofar as it indicates an independent existence of "quantum objects." That includes their capacity to enter interactions with our measuring instruments or other classical and quantum entities with which they can interact, and various occasions of what you would call "Zinc!" that may arise as a result.*

Here I think I disagree with you: the *ascription* of a Hilbert space (and with it a dimension) already strikes me as up to the job. Thus, in a sense, the Hilbert space—but none of the vectors in it—is the proper mode for expressing an individuality or independent existence.

And by the way, it's "Zing!" dammit. (Howard Barnum wrote a paper recently, where he made slight fun of the term, and called his version of it "Voom!") "It don't mean a thing, if it ain't got that zing."

Plotnitskyism 6: *Now (this is perhaps a tad less banal) it appears to me that, at least on this view, it is in this and only in this that one could speak of an UNKNOWN quantum state (the locution one sometimes find in, among others, your writings). This "state," however, could, on this view, never be represented by a state vector, which indeed reflects the state of expectation, or by any other mathematics.*

I believe I understand where you are going here. But precisely because of the last sentence, I would be reluctant to call what you are talking about a state. The way I imagine it, quantum systems have little to no properties in and of themselves ... and thus no "states." To the extent that they have any properties at all, they are born in a sexual act, so to speak. What is most real of a quantum system is what comes out of two of them going bump in the night.

Right now, I'm about two hours into my flight toward Madrid. As you can see, I've been filling my time writing a little email. But also I am forcing myself during this trip to finally read David Deutsch's book *The Fabric of Reality*. I figure I ought to do this since I might get a chance to talk to him at the QCMC meeting at MIT. Before setting off to write you, I finished the first chapter, and have been finding it just as detestable as I had suspected. God save me this week if I get all the way through the thing.

13-07-02 *Yestopher* (to A. Plotnitsky)

Plotnitskyism 7: *I would only reiterate that all our knowledge (already obtained in measurements) or predictive estimates only concern classical effects of the interactions between quantum objects and measuring instruments upon the latter, but not quantum objects themselves, although these effects would not of course be possible without the existence of quantum objects. I am not saying "reality," since there is no concept of reality I know or can think of applicable to such "objects,"* *while it seems to be possible to think of existence as a capacity to produce effects, in a classical-like (as in classical physics) or in a nonclassical fashion (as in quantum mechanics).*

Yes, I definitely like this statement. But let me think more about the rest of your letter (for maybe a couple of days) before commenting further. It might prod me to finally write down some thoughts about a divergence that I think is starting to develop between Bohr and me.

BTW, you don't mean "orthogonal" in this passage:

Plotnitskyism 8 : *Indeed, once we perform a momentum measurement on the first system we completely cut ourselves from any possibility of making prediction concerning the [position] measurements on the second system. I suppose this point is linked in some ways to Mermin's observation to the effect that once a given system is [in] a given state (in the sense of psi-function) it cannot be in the state orthogonal to it. This language does not altogether satisfy me, however, since, as I said in my previous email, ket-vectors in a Hilbert space do not describe actual physical states. One could say (in a more cumbersome but more accurate way) that the possibilities of predictions defined by orthogonal state vectors (expectation vectors) are always mutually exclusive, which is of course to say, rigorously complementarity in Bohr's sense, since they also entail mutually exclusive experimental arrangements.*

Position and momentum eigenstates are not really orthogonal in the sense that Mermin is speaking of here. Rather they are "complementary" (as Zeilinger would say) or "mutually unbiased" (as Bill Wootters would say). That is, two vectors in a D-dimensional space are "mutually unbiased" when their inner product is $1/\sqrt{D}$ in magnitude. It just so happens with position and momentum eigenstates that $D = \infty$, (and thus they are strictly speaking orthogonal), but that is a secondary concept or property. Mermin's trouble with orthogonality arises, even in a D-dimensional space, when the inner product is zero.

29-07-02 *20 More Minutes* (to B. W. Schumacher)

I was intrigued by your comment of, "If you would have just had about 20 more minutes, I might have been convinced by your talk." Seriously, could I get you to tabulate your most serious objections to a Bayesian approach to quantum mechanics? It would certainly help to orient me and the other boys: sometimes it hard to see the biggest leaks in a ship from the inside. We need this kind of criticism if the point of view is going to hope to survive.

On another subject, I'm afraid you haven't lifted your pen yet to write up your doubting-Everett argument. The original deadline has just about slipped past, and I really would like to have your paper. There are some stories that only you can tell. What can I do to help you, or help keep you on course? Seriously; I'll be at your service if you need me. (And if that doesn't work, remember the guilt thing. Once upon a time you said, "I owe you this paper.")

Ben's Reply

> Oh, I guess my basic worry is that the wave function is a bit too peculiar to be an "a priori" object — probabilities seem inescapable, but who ordered the quantum? So a program of thinking of quantum mechanics as a rational calculus of inferences seems to be lacking some ingredient. Wish I knew what it was. (Is it the Zing?) But until I have an inkling, the project seems a little doubtful.
>
> I wish you could see just how much I'm hanging my head in abject shame. (It makes it hard to type.) I'm leaving for sabbatical tomorrow, and will take two weeks to drive

across the country. I'll be in Los Alamos for a day next week. Anyway, I'm taking my Everett stuff, with the intention of trying to write the danged thing on the way. I'll be in touch and let you know how it goes. I will *not* be on e-mail every day.

I suppose my basic problem is that I feel such a paper should be couched as a massive and authoritative review of the conceptual issues in the Everett interpretation. This of course is madness. I will ignore this instinct and write only what I really have to say.

It was amazingly good to see you at MIT and I am really looking forward to visiting Montréal.

29-07-02 *Complimentarity* (to A. Plotnitsky)

OK, the title is probably a bad pun. But I thought I might tell you something nice I heard Asher Peres say of you while we were in Spain. "That Plotnitsky is a wise man." He was quite impressed by your understanding of Bohr.

Here's another piece of complimentarity: While I was in Cambridge last week, I picked up a copy of your book *Reconfigurations*. It's in beautiful shape and I got it for a great bargain, only $11.75.

How much longer will you be in NYC? I might drop in one day soon if you've got some free time.

29-07-02 *Coordinate Systems, 1* (to W. K. Wootters)

This is a small note to let you know that after a little thought I decided I really liked your question at the end of my talk Wednesday. Setting the standard quantum measurement device is more like setting a coordinate system than merely setting a length or time scale. Now I'm not sure how to take that any further, but I'll keep you up to date as the thoughts arise.

Did you and Robin make any progress with the SIC-POVM? If there's still some work left to be done, then Gabe and I will likely plunge back into the water as the week goes by.

30-07-02 *Coordinate Systems, 2* (to W. K. Wootters)

Woottersism 13: *PS. Have you seen the following concept in classical probability theory? Consider a probability distribution P over n variables. Now consider all the $(n-1)$-variable marginal distributions derived from P. Let $\tilde{P}$ be the n-variable distribution that agrees with P in all its marginals but otherwise maximizes the entropy. Then the difference in entropy between $\tilde{P}$ and P can be taken as a measure of the amount of information in P that is not in its marginals.*

Have you seen that idea anywhere? Noah Linden, Sandu Popescu and I are doing the same thing for quantum states, and I'm wondering what's been done classically along these lines. Thanks.

I meant to tell you, by the way, that that's a really beautiful idea. I don't think I have ever seen it before, so there's not much I can tell you. Presumably—in analogy to what you showed for the quantum case in your talk—in the bipartite setting it reduces to simply the standard mutual information. (Well of course it does; I started to see it as I wrote that sentence.) I'll ask around to some of the information theorists here.

By the way—now this is a completely different subject, but the memory was jogged by your question—here's a historical tidbit that might interest you. In the mid 1850s, George Boole was also interested in the marginals a posited joint probability distribution could produce. In fact he studied the inverse problem, given a set of potential marginals, under what conditions can there exist a joint distribution giving rise to them? Those conditions are now called Bell inequalities, but Boole had expressions for some of them even in the 1850s. Here's a great little article to read along those lines:

- I. Pitowsky, "George Boole's 'Conditions of Possible Experience' and the Quantum Puzzle," Brit. J. Phil. Sci. **45**, 95–125 (1994).

30-07-02 *Copenhagen Visit* (to F. Topsøe & P. Harremoës)

A friend and I will be visiting Copenhagen September 12–17, partially as a vacation, but partially for some work. (He will be there because of a conference Sept 8–12 on laser physics; and I will be there because of an obligation in Växjö, Sweden during the same time.) Also while in Copenhagen, I thought I might do a little work, either visiting you and/or doing a little historical digging at the Niels Bohr Institute.

Do either of you have any suggestions for how this friend and I might make the trip affordable but still pleasant? For instance, do either of your universities have any visitor housing? Or could you recommend any affordable, but pleasant, hotels in an interesting part of town (with cafés, bars, etc., nearby)?

We're just fielding suggestions at this point. Please tell me anything you can?

30-07-02 *20 More Minutes, 2* (to B. W. Schumacher)

Schumacherism 7: *I suppose my basic problem is that I feel such a paper should be couched as a massive and authoritative review of the conceptual issues in the Everett interpretation. This of course is madness. I will ignore this instinct and write only what I really have to say.*

Yes, that would be madness. Just think over and over, "Transcribe my talk. Transcribe my talk. Transcribe my talk. Capture the atmosphere of my talk." And when you need some filler, say something about information being relative, say something about what quantum information holds for quantum foundations studies. Say what you feel, and everything will be OK. If you need references, I'll dig them up.

Have a safe trip … but make it your best mental writing experience ever.

01-08-02 *Research Problem* (to M. Raginsky)

Maxim's Maxim 1: *I must say that I thoroughly enjoy reading your papers on the foundations of quantum theory. However, I came of late to take any "Quantum Theory as Information" manifesto with a grain of salt. Is it just me, or are people insisting on an information-theoretic revision of quantum theory simply because the field is so, shall we say, "trendy?" Certainly we may demand an "information-theoretic reason" for the fact, say, that a pure state of a quantum-mechanical system is a ray in a complex Hilbert space, and that is fine as far as that goes, but what about the good ol' classical information theory? Why doesn't anyone clamor for an "information-theoretic reason" for the mathematical formalism of classical mechanics, with its symplectic manifolds and Hamiltonian flows and the like? Perhaps the answer is this: the modern mathematical machinery of classical mechanics is, after all, a fancy way to express the belief in the ultimate determinism of the classical world. So, insofar as there is no "spooky" artifact of this formalism, no one could give a damn* *about any "information-theoretic reason." It is only when our macroscopic intuition gets blown to smithereens by the microworld (entanglement, anyone?), do we suddenly demand a paradigm overhaul. What I mean to say, so loquaciously, is that the "classical" (Shannon) information theory is far from being a physical theory the way quantum information theory is shaping out to be. Any thoughts on this?*

I promised you that I'd tell you what I said to Ulrich Mohrhoff when he said what you said (but in a meaner way). It's pasted below. [See 04-07-01 note "Carts and Horses" to U. Mohrhoff.]

But that aside, I think your question is a good one, and it leads immediately to a research problem that might interest you. Indeed I do think it is worthwhile to try to give information theoretic reasons for some aspects of classical physics—Hamiltonian flow being precisely one of them. Go to my samizdat, `quant-ph/0105039`, and have a look at the following notes:

- page 408, "Andrei"
- page 283, "End of Day"
- page 284, "Wigner"
- page 285, "contra-Koopman"
- page 165, "The Evolution of Thought"
- page 166, "On the Mark"
- page 167, "More Linearity"
- page 168, "Three References"

I think they explain what I'm talking about in pretty good detail.

It was good meeting you at MIT, and I'm looking forward to studying your channel papers if I ever get an ounce of time. If you have any good ideas along the lines above, I'd love to hear them.

01-08-02 *Manuscript* (to C. King)

Dear Math-Chris,

Kingism 1: *I have one observation about your multiplicativity problem: if you replace the "maximum eigenvalue" operation (aka the operator norm) by the entropy function (you have to normalize the states first, and put the normalization factors outside, so you have a weighted average of entropies) then the corresponding minimal entropy quantity is additive. Any interest?*

That sounds intriguing, but I can't quite visualize which expression you're talking about. If you can, just TEX in a quick equation. What would the interpretation of that quantity be (within the cluster of ideas that the paper is about)?

All the best,

Philo-Chris

01-08-02 *Notes* (to C. King)

Aha! Thanks for the notes. I don't think I had ever thought of the set of states as defining a doubly-stochastic channel before. So, at least I understand your notation. Very good. And neat result of yours. Does it lead to any insight on how to solve the original problem? And here's another question: Does $G(\Phi)$ define an interesting channel characteristic in general? Have you ever seen it before in that kind of context?

02-08-02 *Baby* (to C. H. Bennett & T. M. Bennett)

Beautiful pictures, but more importantly beautiful babies. Congratulations to everyone in the family. Busy days ahead. Charlie knows I get philosophical when I think about life and all the possibility it opens up in the universe—possibility that wasn't even there before. These babies are part of that. Wonderful creations. Please give our best to George and Martha.

05-08-02 *The Spirit that Breathes Life* (to G. L. Comer)

"The divinity that breathes life into nature cannot be represented."

Thanks for your note of August 1. It gave me food for thought; free will saved another life. Indeed, I think the divinity that breathes the whole of life into nature is just that: free will, the essence of making a choice.

I don't think I ever told you this, but 132 years ago, the realization that free will is key saved the life of another great man. It was William James, and the realization is attributed not only to saving his life—he had been in deep depression for some time, with the accounts saying he was on the brink of suicide—but in turning his philosophy and all his thought around. The full road to recovery took several years, but that moment was the starting point.

Let me paste in a passage from his diary for you.

> April 30, 1870
>
> I think that yesterday was a crisis in my life. I finished the first part of Renouvier's second "Essais" and see no reason why his definition of Free Will—"the sustaining of a thought because I choose to when I might have other thoughts"—need be the definition of an illusion. At any rate, I will assume for the present—until next year—that it is no illusion. My first act of free will shall be to believe in free will. For the remainder of the year, I will abstain from the mere speculation and contemplative *Grüblei* in which my nature takes most delight, and voluntarily cultivate the feeling of moral freedom, by reading books favorable to it, as well as by acting. After the first of January, my callow skin being somewhat fledged, I may perhaps return to metaphysical study and skepticism without danger to my powers of action. For the present then remember: care little for speculation; much for the form of my action; recollect that only when habits of order are formed can we advance to really interesting fields of action—and consequently accumulate grain on grain of willful choice like a very miser; never forgetting how one link dropped undoes an indefinite number. *Principiis obsta*—Today has furnished the exceptionally passionate initiative which Bain posits as needful for the acquisition of habits. I will see to the sequel. Not in maxims, not in *Anschauungen*, but in accumulated acts of thought lies salvation. *Passer outre*. Hitherto, when I have felt like taking a free initiative, like daring to act originally, without carefully waiting for contemplation of the

external world to determine all for me, suicide seemed the most manly form to put my daring into; now, I will go a step further with my will, not only act with it, but believe as well; believe in my individual reality and creative power. My belief, to be sure, can't be optimistic—but I will posit life (the real, the good) in the self-governing resistance of the ego to the world. Life shall [be built in] doing and suffering and creating.

06-08-02 *SHPMP Quantum Information Issue* (to the SHPMP participants)

Dear friends,

I have to open up this note with some sad news. Rob Clifton the editor-in-chief of *Studies in History and Philosophy of Modern Physics* and the main instigator of our special issue, died sometime late last week, succumbing to cancer. He was a young man in the prime of his life and leaves behind a wife and two young children. You can find out a little about Rob, his life, and his love for quantum mechanics at his webpage `http://www.pitt.edu/ rclifton/index.html`.

With that, I should get to the formal task of this note, our special issue on quantum information. The original deadline for all papers was to be August 1. Jeff Bub and I have talked to many of you privately about your progress, but especially in light of this tragedy, we think it is time to get serious. With that, we propose to extend the deadline to October 1, but to make that one hard and fast. It is important that we get the papers to the journal with plenty of time for refereeing and copy editing before the special issue's appearance.

I hope that you will all reconfirm your participation, and that we can count on you for a quality issue. Please let us know your status as soon as possible. Below, I list the proposed papers (forgive me if I didn't quite get your title right), and further below that I paste in the original invitation with details.

Best regards,

Chris (and Jeff)

Contents:

- H. Barnum
  "Quantum Information and Quantum Logic: Toward Mutual Illumination"
- C. H. Bennett and J. A. Smolin
  "Maxwell Demons, Bit Commitment, Lock Boxes, and Quantum Information"
- L. Hardy
  "Quantum Mechanics from N Reasonable Axioms"
- R. Jozsa
  "Quantum Computation and Quantum Foundations"
- N. D. Mermin
  "Philosophical Ruminations on Deutsch's Problem"
- I. Pitowsky
  "Betting on the Outcomes of Measurements: A Bayesian Theory of Quantum Probability"

- J. Preskill,
  "Defending Everett"

- B. Schumacher,
  "Doubting Everett"

07-08-02 *Trouble Man* (to N. D. Mermin)

Merminition 93: *I completely forgot about this and have no idea what I was thinking of when I sent you that title. Better count me out. I have too many other things I'm supposed to do in the next two months.*

Is there none of that college spirit left in you? No loyalty to the editorial board (of which you are a member)?

Honestly, is there really nothing you would find worthy to write up? I remember loving the last line in this paragraph from your "teaching" paper:

> There are nevertheless some who believe that all the amplitudes α_x have acquired the status of objective physical quantities, inaccessible though those quantities may be. Such people then wonder how that vast number of high-precision calculations (10^{30} different amplitudes if you have 100 Qbits) could all have been physically implemented. Those who ask such questions like to provide sensational but fundamentally silly answers involving vast numbers of parallel universes, invoking a point of view known as the *many worlds* interpretation of quantum mechanics. My own opinion is that, imaginative as this vision may appear, it is symptomatic of a lack of a much more subtle kind of imagination, which can grasp the exquisite distinction between quantum states and objective physical properties that quantum physics has forced upon us.

Surely, that sentence could be turned into a whole paper, at least under my pen.

I remember the first time I met you—in Montréal—I heard you say, "Quantum computation is the biggest sham in the industry. The government gives you all this money to crack codes, and what you're really doing is quantum foundations." Couldn't you find some way to flesh that out and say it in print? It would benefit you and it would benefit the community.

07-08-02 *Books and Notes* (to A. S. Holevo)

Thank you for the Humboldt book; I very much enjoy reading things like that. And I will surely distribute copies of your own book to Ruskai, Schumacher, Caves, etc. I will be seeing all of them in the month of October.

In sorting through your things yesterday, I put all of your handwritten notes into a single pile. I also placed all the papers by other people on which you had written some notes into the same pile. It really would be no problem for me to mail that to you. If you will just give me an appropriate address, I will do it.

By the way, it was a surprise to see that you had started a draft titled "A Remark on Conjecture of Bennett, Fuchs and Smolin." Too bad you were never able to complete it! Regardless of the result, with a title like that it would have certainly helped my career. Few people today associate me with that question, even though to my knowledge, Bennett and I were the first to ever bring it up (at a meeting in Torino in 1996).

08-08-02 *The Recalcitrant P's* (to A. Peres)

I almost titled this note "Two Recalcitrant Positivists," remembering something you had called us long ago. But then thinking of how I am more closely aligned with the pragmatists now (James, Dewey, Rorty, etc.) than with the positivists, I decided to be more careful. We're just two recalcitrant P's, and I'll leave it at that.

I'm glad to hear that you and Aviva are home safely.

09-08-02 *Conspiracy Theory* (to J. W. Nicholson)

By the way, have a look at my conspiracy theorist article:

`http://www.pcmag.com/article2/0,4149,440168,00.asp`.

09-08-02 *Conspiracy Theory, 2* (to J. W. Nicholson)

Nicholsonism 7: *Did you really say, "I'm sure that the government is already using quantum cryptography systems for real applications."??*

That line really scares me, because he put quotes around it. I can't imagine that I would have said it. But on the other hand, I'm sure reporters know how to cover their asses. I've been thinking about what to write this guy to lodge a complaint. Maybe it was really said in a context that took some of the edge off it? I don't know. I can imagine that I would have said "developing" ... but he bases the whole story on the word "using".

13-08-02 *Variola* (to R. W. Spekkens)

Spekkensism 10: *A quick question for you regarding POVMs. It seems to me that there is an interesting distinction between, on the one hand, POVMs that can be obtained by convex combinations of coarse-grainings or convolutions of PVMs, and, on the other, those that can only arise by restriction of a PVM on a larger Hilbert space. An example of a POVM in the latter category would be one with rank 1 elements numbering greater than the dimensionality of the Hilbert space. Has this distinction been noted and studied anywhere?*

Yes, this distinction was first noted by Holevo: A. S. Holevo, "Information-Theoretical Aspects of Quantum Measurement," *Problemy Peredachi Informatsii*, **9**(2), 31–42 (1973); also listed as A. S. Kholevo, *Problems of Information Transmission*, **9**, 110–118 (1973).

Beyond that, a most interesting distinction has to do with thinking of the POVMs as a convex set. The extreme points of that set have the character you mention, except that all the operators in the POVM must be linearly independent. You can read about that in: A. Fujiwara and H. Nagaoka, IEEE Trans. Inf. Theory **44**, 1071–1086 (1998).

14-08-02 *Empty Head* (to J. W. Nicholson)

I loved the Maureen Dowd "empty-headed" comment today. Just sayin.

By the way, I've thought a little bit about the ethics of killing cattle and the culture of the "compassionate cattle raiser/killer." Interesting issues with, shall we say, quantum implications. I'm honing the thoughts for a (thoughtful) conversation in the near future.

20-08-02 *Old Note to Rolf* (to C. H. Bennett)

Your remark last night about "Gilles's idea" to derive quantum mechanics took me by surprise. The surprise came from the way you continue to associate my efforts with a kind of religion, rather than with an honest effort to get at some different ways of looking at quantum mechanics (by deriving it from information theoretic structures, etc.)—i.e., you don't even see that this is what I am always talking about. I'd like to think some of my latest papers represent progress in that direction, and I guess I'd also like to think the pressures I put on the people around me (Hardy is a good example) have been part of the reason for their own good work in that direction. Religion without theorems, or a dream to prove them, is just religion. But religion that gives rise to some solid results in return is usually called science.

Anyway, thinking of the history of this, I came across a little passage I wrote to Rolf Landauer in July of 1998 which I thought was cute. I'll paste it below. It puts a lot of weight on your shoulders Atlas.

With my usual love, Chris

> The [speculated] information-processing limitation has to do with a funny property of the world we happen to live in. It is this: *my* information gathering about a given physical system will generally disturb *your* description and predictions for that self-same system. Nonetheless we, as communicating beings, must come to a consistent description of what we see and know about that physical system. This, I ever more firmly believe, is the essence of the quantum mechanical formalism. Quantum theory is nothing more than Bayesian-like reasoning in a world with such a funny property. (This I see as a large chunk of my personal research program: clarifying, delineating, and searching for holes in this point of view.) Perhaps ... to put it in a more amusing way ... what I am saying is that I wouldn't be surprised if the whole edifice of quantum theory couldn't be constructed from the singular fact that "quantum cryptography exists." That is to say, Hilbert spaces, the inner product rule, entanglement, and unitary evolution all from that clean, simple idea Bennett and Brassard were the first to make some currency of.

21-08-02 *Way in the Back* (to D. Gottesman)

Gottesmanism 1: *Yes, I was wondering if I should suggest you as a recruit. Some of the Perimeter recruiting is in Foundations of QM rather than quantum computing proper; I guess Lucien is the first real hire in that area.*

Thank you; I'm flattered. I guess I don't have to advertise to you the depth of my faith that a beautiful spark will eventually fly out of rubbing those two fields together.

Charlie Bennett and I are sitting at the back of this enormous room in the OpryLand Hotel [at the ARO conference in Nashville] acting like it's our office. He's reading science fiction from his laptop, and I'm writing email. There seems to be a speaker saying something from way up in the front.

27-08-02 *Dimensionality* (to P. J. Reynolds)

Reynoldsism 1: *Thanks for the e-mail. I love your web page! I'll have to make some time and look at those PowerPoint talks.*

Sorry for the long delay in my reply: Any man who says he loves my webpage certainly deserves a prompt, if not immediate, reply!! But I finagled my way into the ARO review meeting last week, and that ended up commandeering all my attention … I'm just recovering now.

Reynoldsism 2: *I justify the small amount still in my program as benefiting metrology, clocks, and time transfer. Is the Hilbert space "resource" different (versus "more fundamental than") entanglement? In other words, does it enable different things than the things we attribute to entanglement? If so, then (depending on what those things might be) we might have an "in" for you.*

What I am thinking of here is seeking ways to quantify the extent to which the dimensionality D of a quantum system's Hilbert space signifies that quantum system's "sensitivity to the touch." If you have a look at some of my more foundational writings, in fact, you'll see I'm starting to view this "sensitivity to the touch" as the most fundamental property there is for a quantum system—i.e., the most significant feature that sets quantum stuff apart from classical stuff is its potential to be perturbed. From this point of view, quantum entanglement is a secondary effect, and has more to do with how this sensitivity scales with the number of quantum systems than anything else. The more important question is what does D tell us, and what can we do with it?

With regard to your program, I can foresee at least a couple of directions with which to take this idea.

1) In an upcoming paper with Masahide Sasaki—I can supply the manuscript-under-construction if you'd like—we try to quantify the "quantumness of a Hilbert space" by a system's ability to sniff out whether it is being sent through a classical channel or a quantum channel. In fact, we view this ability as a Bureau of Weights and Measures kind of thing: Lucent Technologies says it is supplying you with a good quantum channel. How can you tell that it really is?

2) Good "sensitivity to the touch" signifies some potential for good antennas. Here I'm thinking of things like LIGO in particular (though in a more abstract setting). What does Hilbert-space dimensionality buy you with respect to the ability to distinguish weak signals? I've got some of this captured in my Caltech lecture notes "Viewing LIGO through Schwarz Colored Glasses," which I can send you. Also you can find some hints of the ideas in a paper by Childs, Preskill, and Renes, "Quantum Information and Precision Measurement" (`quant-ph/9904021`), of which I played some part.

Anyway, that's the sort of stuff I'm thinking about. If I were to put this down in a proposal I could beef it up pretty significantly, and hit the theme from still a couple more directions.

I hope that builds a little clearer picture for you.

30-08-02 *Transformations of This and That* (to G. L. Comer)

Thanks for the enjoyable note.

Comerism 2: *Why did this passage interest me? Because of the problem of the dictionary: How can one "understand" language? It seems to me that even when we native English speakers speak English, all we are doing is using rules of translation. Into what? I don't know. Interestingly enough, when I think of relativity, I see a big similarity. For me, relativity is not only about gravity as a manifestation of the curvature of spacetime, but also about how two observers can communicate the results of their experiments in such a way that they can agree that they have witnessed the "same" event. As an example, in order to determine the energy of the cosmic microwave background, one observer with unit four-velocity u_1^μ will say the energy is $u_1^\mu p_\mu$, where p_μ is the momentum of the radiation, but another observer with four-velocity u_2^μ will say the energy is $u_2^\mu p_\mu$. Even if the* *observers are at the same place, at the same time, when they make their measurement, they will in general not record the same energy because their motions will in general be different. So how can they ever agree on anything? Relativity tells them how to "translate" one measurement into the other so that a consistent description results. What is objective about the energy? Nothing. Its value depends on the motion of the observer. Where is the objectivity? Only in the rules of translation. The only objective thing to me seems to be the how one object is to be compared with another.*

Did you ever read my "Anti-Växjö Interpretation of Quantum Mechanics" paper? (If not, you can download it from my webpage; link below. It should be super-easy reading; no equations at all.) If I'm not mistaken I play up a similar point there, in the context of noncommuting observables.

But I kind of like something about your twist. Spacetime is the dictionary. And, like the dictionary, the "divinity that breathes life" into its connections is its user, the active agent. Without the agent it is a snake that bites its tail.

I'm in Los Alamos at the moment, being forced by the scenery to remember some of my own transformations. It has been two years since I've been here; the first time I've been back since the fire. The mountainside is still full of these toothpicks that used to be trees; I guess it'll be like that for at least 20 more years or so, fading slowly. Yesterday I drove to the street where our house (and all my material possessions) used to be. A new house is finally being constructed in its place. Funny, I found myself thinking mostly about the ground underneath it; I guess like a burial place.

05-09-02 *Copenhagen Visit* (to O. C. Ulfbeck)

My name is Chris Fuchs. We have a mutual friend in David Mermin, and he has suggested that I contact you. For a quick introduction to me and my work, you can read David's foreword to my pseudo-book posted on the Los Alamos archive; here is a quick link to it: `http://xxx.lanl.gov/abs/quant-ph/0105039`. Alternatively, for a technical introduction to the sorts of things on my mind, see my paper "Quantum Mechanics as Quantum Information (and only a little more)": `http://xxx.lanl.gov/abs/ quant-ph/0205039`. (It is a coincidence that the two papers have almost identical locator numbers.)

In any case, I will be visiting Copenhagen September 12 through 17, and I would very much like to talk to you and/or Prof. Bohr about your quantum-foundations paper on genuine fortuitousness. I would also like to spend some time in the Niels Bohr Archive if it is possible and I can figure out how to make an appointment there, etc. Would you like to get together?

If so, there is a chance I will be able to maintain email contact within Denmark; so perhaps I can be reached that way. Also, however, I will be staying at a guest room of the university, and should be findable by some means there. Here is the information I have on it: [...] Finally I should be reachable through my hosts Peter Harremoës and Flemming Topsøe: [...]

If you have the time, I would love to hear from you.

PS. I will also be giving two talks while in residence:

- 13/9 13.15-14.00 On Unknown Quantum States: The Quantum de Finetti Representation Theorem
- 16/9 9.15-10.00 Quantum Mechanics as Quantum Information (and only a little more)

I'm not sure where they will be yet and there is some possibility that the order will be reversed, but those are their dates and times. I would be flattered if you could attend either one; I will place the abstracts below.

Quantum Mechanics as Quantum Information (and only a little more)

I say no interpretation of quantum mechanics is worth its salt unless it raises as many technical questions as it answers philosophical ones. In this talk, I hope to convey the essence of a salty, if not downright briny, point of view about quantum theory: The deepest truth of quantum information and computing is that our world is wildly sensitive to the touch. When we irritate it in the right way, the result is a pearl. The speculation is that this sensitivity alone gives rise to the whole show, with the quantum calculus portraying the best shot we can take at making predictions in such a world. True to form, I ask more questions than I know how to answer. However, along the way, I give a variant of Gleason's theorem that works even for rational and two-dimensional Hilbert spaces, give another variant of Gleason's theorem that gives rise to the tensor-product rule for combining quantum systems, and finally derive a new form for expressing how quantum states change upon the action of a measurement.

On Unknown Quantum States: The Quantum de Finetti Representation Theorem

There is hardly a paper in the field of quantum information theory that does not make use of the idea of an "unknown quantum state." Unknown quantum states can be protected with quantum error correcting codes. They can be teleported. They can be used to check whether an eavesdropper is listening in on a communication channel. But what does the term "unknown state" mean? In this talk, I will make sense of the term in a way that breaks with the vernacular: an unknown quantum state can always be viewed as a known state—albeit a mixed state—on a larger "multi-trial" Hilbert space. The technical result is a quantum mechanical version of the de Finetti representation theorem for exchangeable sequences in probability theory. Interestingly, this theorem fails for real and quaternionic Hilbert spaces. The implications of this theorem for the point of view that quantum states represent states of knowledge, rather than states of nature, will be discussed.

05-09-02 *Everett Quote* (to N. D. Mermin)

I just want to make sure I have this quote archived. An easy way for me to do that is to send it to you. It comes from a footnote in Hugh Everett's original relative-state paper.

> Note added in proof. — In reply to a preprint of this article some correspondents have raised the question of the "transition from possible to actual," arguing that in "reality" there is—as our experience testifies—no such splitting of observer states, so that only one branch can ever actually exist. Since this point may occur to other readers the following is offered in explanation.
>
> The whole issue of the transition from "possible" to "actual" is taken care of in the theory in a very simple way—there is no such transition, nor is such a transition necessary for the theory to be in accord with our experience. From the viewpoint of the theory *all* elements of a superposition (all "branches") are "actual," none any more "real" than the rest. It is unnecessary to suppose that all but one are somehow destroyed, since all the separate elements of a superposition individually obey the wave equation with complete indifference to the presence or absence ("actuality" or not) of any other elements. This total lack of effect of one branch on another also implies that no observer will ever be aware of any "splitting" process.

08-09-02 *Commune '02* (to the Communards)

Quantum Foundations in the Light of Quantum Information II Montréal, Canada October 13, 2002 – November 2, 2002

Chers communards,

Gilles and I are making final plans for the October Montréal quantum foundations commune. I think the meeting promises to be a lot of fun and more importantly promises to be the site of some good physics.

Below is the latest tabulation of attendees. If your dates are still marked with an X or a ?, please make a final confirmation as soon as possible. Alternatively if you cannot ultimately attend the meeting, please let us know that too. (In the coming days, we hope to populate the remaining spots with friends in the waiting list. Recall, we have room for about 15 attendees at any one time, so that everyone will have a desk.)

Beyond that, the main thing we need from you now is to make your flight plans and living arrangements. (Because of the late date of this letter, this is something that should be taken care of rather urgently.) Concerning the former, please try to include a Saturday stay in your plans so as to keep the airline fees down. Concerning the latter, the procedure is the following. Have a look at the following webpage, and decide which option looks best for you: `http://www.crm.umontreal.ca/en/niveau2/index_vis_sug.html`. After that, write an email to Luc St-Pierre (`stpierre@CRM.Umontreal.Ca`) at CRM telling him which option you would like reserved and what your precise dates will be. Gilles and I would suggest the Chateau Versailles Hotel as the best option for most of you: it is the most pleasant hotel and only a short, straightforward bus ride from the university. If you prefer no busses, Hotel Terrasse Royale is a small walk from the university, but—be warned—it's not nearly as pleasant a place to live.

The format of the meeting will be relaxed and mainly devoted to work, conversation, and collaboration time. We will, however, schedule one to two talks per day; so everyone should come prepared for that eventuality. Also—and this is a key component to the meeting—we ask that everyone compile and share a list of concrete problems whose solution, they believe, will tell us something novel about quantum foundations.

Key:

`X` – dates proposed by Fuchs (based on request and/or guesswork)
`O` – dates confirmed by participant
`?` – additional dates participant is contemplating and/or requesting
`-` – means person is not in attendance on that date

(Saturdays and Sundays are signified by an `S`.)

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08-09-02 *Y'all Come* (to C. H. Bennett)

You will have noticed that I included you in the latest announcement for the Montreal Commune. I do hope you'll come for some of it, and I mean that from the bottom of my heart. I'm spending a week in Sweden presently, and I'm typing this letter from the hotel lobby where John Smolin, Lucien Hardy, and I had so many pleasant conversations at the last quantum foundations meeting I put together. John, as I've told you before, turned out to be one of the most useful and interesting participants of the meeting—that took me by surprise because I never imagined he would have any interest in this subject. Nothing could be better than for you to surprise me the same way!

Looking forward to your response.

08-09-02 *Information is Physical* (to A. Peres)

Asherism 25: *I am looking for references that will appear in my RMP with Danny. Ben Schumacher confirmed that he invented "qubit", and Gilles found the father of Alice and Bob (Blum, 1981) after I found Savitt 1982. I remember that Rolf Landauer used to say "information is physical" but I can't find a written record. Would you know one?*

I'm in Sweden with a bad connection, so I'll be brief.

The following comes from page 192 of my samizdat, `quant-ph/0105039`.

> **08 October 1998, to Rolf Landauer, "Info is Physical"**
>
> I have room in a very tight conference proceedings paper that I'm writing to make one citation to your phrase, "Information is Physical." Do you have a favorite paper that I should cite? Should I make the citation to your earliest mention of the phrase? Or should I make the citation to what you think is the clearest statement of it? Please

make the decision for me and send me the correct reference. My paper is going off to the editors tomorrow, so if you could send me a quick note that'd be great!

Rolf's Reply

The concept, but not that phrase, first appeared in R. L. *IEEE Spectrum* vol 4, issue #9, pgs. 105–109 (1967). (Like for *Physics Today*, the page numbering restarts with every issue.) The exact wording PROBABLY first showed up in my 1991 *Physics Today* paper. But it seems best to cite an early or a very recent paper. Two are on their way into print. One that is likely to appear within a few weeks: R. L. in *Feynman on Computation 2*, ed. by A. J. G. Hey, Addison Wesley, Reading (1998?). The title of that one "Information is Inevitably Physical". About 11 days ago at a session in Helsinki I complained to my audience: I have gotten a fair amount of acceptance for that phrase, but not for the message attached to it. But thanks for checking.

P.S. I'll mail both papers, even though they will arrive too late. But if you take an instant dislike to the one you used, you can scratch it at galley proof time.

09-09-02 *Information Carriers are Physical* (to A. Peres)

Asherism 26: *I could not understand your answer, but there is nothing urgent. What I would like is the exact wording, and a reference that ordinary readers can easily access (*Physics Today *is fine, or some conference).*

I was just demonstrating that my samizdat can be a resource for historical questions. The words below are Rolf's words himself:

> The concept, but not that phrase, first appeared in R. L. *IEEE Spectrum* vol 4, issue #9, pgs. 105–109 (1967). (Like for *Physics Today*, the page numbering restarts with every issue.) The exact wording PROBABLY first showed up in my 1991 *Physics Today* paper. But it seems best to cite an early or a very recent paper. Two are on their way into print. One that is likely to appear within a few weeks: R. L. in *Feynman on Computation 2*, ed. by A. J. G. Hey, Addison Wesley, Reading (1998?). The title of that one "Information is Inevitably Physical". About 11 days ago at a session in Helsinki I complained to my audience: I have gotten a fair amount of acceptance for that phrase, but not for the message attached to it. But thanks for checking.

Thus you should cite either the *IEEE Spectrum* and/or the *Physics Today* articles of his. I don't know the exact sentences in those papers; for that you should probably get Danny to go to the library.

By the way, I really wish Rolf had said "information carriers are physical" instead of "information is physical" ... but I never got a chance to tell him that while he was alive.

09-09-02 *Dates and Talks* (to W. K. Wootters)

I think, it is best to assume that people will be trickling in on the 13th, and the events will start on the morning of the 14th. Let me make an executive decision: Events will start on the morning of the 14th. There.

I would like to reserve dinner with you, or you and Ben, for Oct. 13. Do you think we could do that? (I sure wish you had the time to stay longer.) I'd really like to get a chance to corner you on some of these Whitehead things.

On top of that, though, would you be willing to give the first talk of the meeting? Well, the first technical talk; Gilles or I will probably give some procedural talk/s before that. I would be particularly pleased if you would talk about your "private world within entanglement" ideas. And beyond that, I'd also be pleased if you would add a smattering of discussion about the Whitehead stuff. Even if you don't feel that these things are science yet—maybe just ideas for ideas—I think it would still be immensely useful for setting a tone for the meeting.

Moreover, can you dream up any concrete problems to share that are motivated by these points of view? That's your challenge.

10-09-02 *Tentative Talk Schedule for Commune* (to the Communards)

Chers Communards,

Below is a tentative schedule for talks at the commune. Not having an official title from any of you yet—you can't be blamed, I haven't asked for one—I decided to make up some titles of my own. Mostly the titles are a little whimsical, but they do reflect what I've found interesting in my conversations with you. (Thus, I guess I have a secret wish that you'll talk about these things.) But do feel free to talk about anything you want. Please send me a better title as you get a chance.

Also, further below you'll find the latest tabulation of attendance. If I've gotten anything wrong please let me know.

Soon I will send more detailed instructions for how to get from the hotels to the meeting site.

Looking forward to seeing you all soon.

Quantum Foundations in the Light of Quantum Information II
Montréal, Canada
October 13, 2002 – November 3, 2002

<u>Monday, Oct 14</u>

a) Gilles Brassard and/or Chris Fuchs
"Opening Remarks"
"Moment of Silence for the Paris Commune 1871"
"The Spirit of the Commune"

b) Bill Wootters
"Speculative Physics from Speculative Philosophy"

<u>Tuesday, Oct 15</u>

a) Nicolas Gisin
"Why Correlation?"

b) Dominic Mayers
"The Foundational Significance of Bit-Commitment and Coin-Tossing No-Go Theorems"

<u>Wednesday, Oct 16</u>

a) Charles Bennett
"Why I Feel It Important to Always Defend Many Worlds in Private, but Never in Public"

b) Patrick Hayden
"A Paper I Wrote Long, Long Ago with David Deutsch – Pro and Con"

<u>Thursday, Oct 17</u>

a) Hans Briegel
"The Quantum-Foundational Significance of Measurement-Based Models of Quantum Computation"

b) David Poulin
"What Do You Mean 'Simulating a Quantum Computation?"'

<u>Friday, Oct 18</u>

a) Stefan Wolf
"Intrinsic Information and Classical Analogs to Entanglement"

b) Problem session, roundtable, festival of outlandish ideas.

<u>Monday, Oct 21</u>

a) Chris Fuchs, 10:30 AM
"The End of the World as We Know It: Doing Quantum States and Quantum Time Evolutions on a Simplex"

b) Gilles Brassard, 3:15 PM
"Quantum Computing without Entanglement"

<u>Tuesday, Oct 22</u>

a) Rob Spekkens, 10:30 AM "In Defense of the View that Quantum States Are States of Knowledge"

b) Adán Cabello, 3:30 PM
"All the Latest on Kochen-Specker, and Why It's Important"

<u>Wednesday, Oct 23</u>

a) Rüdiger Schack, 10:30 AM
"Exorzismus der objektiven Quantenoperationen"

b) Howard Barnum, 3:30 PM
"Quantum Information Processing and Quantum Logic, Toward Mutual Illumination, … or How I Became a (Closed, Convex, Pointed) Conehead"

<u>Thursday, Oct 24</u>

a) Jeffrey Bub, 10:00 AM
"Characterizing Quantum Theory in Terms of Information-Theoretic Constraints"

b) John Smolin, 1:35 PM
"Another Excuse to Say 'Lock Box'"

c) Ben Schumacher, 3:30 PM, departmental colloquium
"Reversible Computation and Demonic Thermodynamics"

Friday, Oct 25

a) Claude Crepeau, 10:30 AM
"Quantum Authentication and Codes Correcting more than N/4 Arbitrary Errors: A Possible Key to Quantum Foundations"

b) Ernesto Galvao, 3:35 PM
"What a Single Qubit Gives Us"

c) José, 4:20 PM
"A Result in Quaternionic Quantum Mechanics"

Monday, Oct 28

a) Lucien Hardy, 10:30 AM
"The Classical and Quantum State Change Rules are the Same"

b) Roberto Floreanini, 3:05 PM, just after coffee
"Entanglement and Complete Positivity in Open System Dynamics"

Tuesday, Oct 29

a) Carlton Caves, 10:30 AM
"The Point Fuchs Keeps Failing to Appreciate"

b) Marcus Appleby, 3:05 PM, just after coffee
"The Man Who Mistook His Wife for a Hat, or Nullification of the Nullification"

Wednesday, Oct 30

a) Chris Timpson, 10:30 AM
"Claude Shannon Was Smarter Than You Might Think: Quantum Mechanics Can't Stop His Information Measure"

b) Fotini Markopoulou, 3:30 PM
"What Can Quantum Information Do for Quantum Gravity and Quantum Cosmology, and What Can They Do for Quantum Information?"

Thursday, Oct 31

a) David Mermin, 10:00 AM
"Poetry without Poetata"

b) Ben Schumacher, 3:05 PM
"Checklist of What I DO NOT Find Compelling in any Quantum Interpretations To Date"

Friday, Nov 1

a) Problem session, roundtable, workshop of outlandish ideas.

10-09-02 *Don't Forget Your Problem Sets* (to the Communards)

Chers Communards,

This time I am writing briefly to remind you that Gilles and I would like you to dream up a set of concrete problems to share with everyone at the meeting. It would be so nice to see everyone playing, having fun, and perhaps a few collaborations spark up in the process. By my count, the problems from the last Montréal meeting led to at least four published papers.

To give you a feel for the sorts of things we are thinking of—the level of difficulty, etc.—below I place a write-up of the problems I myself presented at the last meeting. (Some of them are still unsolved.)

Let's make this a great meeting! It's mostly in your hands.

"Problems Based on Information-Disturbance Foundation Quest," 15 May 2000

I hope you've had a chance to think about the request Gilles and I made in our invitation letter: namely, to compile a list of concrete problems whose solutions might shed some light on the foundations of quantum theory. What we were thinking in particular is that no point of view about quantum foundations is worth its salt if, at this stage, it doesn't raise as many questions as it answers. Why should we buy into a point of view if it doesn't lead to more fun or, at the very least, something more concrete than a stale philosophical satisfaction?

With that in mind, I've decided to grease the gears a bit by giving you a preview of some of the problems motivated by my particular ish-ism. If you haven't yet created a set of your own problems (based on your ish-ism of course), I hope this will give you a flavor of what we were thinking when we made our request. Certainly the more varied the sets of problems everyone brings, the greater the chance we have for making some real progress!

The point of view I'm likely to represent at our meeting is, I think, best captured (though perhaps a little flamboyantly) by a manifesto I wrote a couple of years ago. Let me reproduce that here as an introduction and motivation to the problems that follow.

Genesis and the Quantum

> In the beginning God created the heaven and the earth. And the earth was without form, and void; and darkness was upon the face of the deep. And the Spirit of God moved upon the face of the waters. And God said, Let there be light: and there was light. And God saw the light, that it was good; and God divided the light from the darkness. And God called the light Day and the darkness he called Night. And the evening and the morning were the first day. ... [And so on through the next five days until finally ...] And God saw everything that he had made, and behold, it was very good. And there was evening and there was morning, a sixth day. Thus the heavens and the earth were finished, and all the host of them.

But in all the host of them, there was no science. The scientific world could not help but *still* be without form, and void. For science is a creation of man, a project not yet finished (and perhaps never finishable)—it is the expression of man's attempt to be less surprised by this God-given world with each succeeding day.

So, upon creation, the society of man set out to discover and form physical laws. Eventually an undeniable fact came to light: information gathering about the world is not without a cost. Our experimentation on the world is not without consequence. When *I* learn something about an object, *you* are forced to revise (toward the direction of more ignorance) what you could have said of it. It is a world so "sensitive to the touch" that—with that knowledge—one might have been tempted to turn the tables, to suspect a priori that there could be no science at all. Yet undeniably, distilled from the process of our comparing our notes with those of the larger community—each expressing a give and take of someone's information gain and someone else's consequent loss—we have been able to construct a scientific theory of much that we see. The world is volatile to our information gathering, but not so volatile that we have not been able to construct a successful theory of it. How else could we, "Be fruitful, and multiply, and replenish the earth, and subdue it?" The most basic, low-level piece of that understanding is quantum theory.

The *speculation* is that quantum theory is the unique expression of this happy circumstance: it is the best we can say in a world where *my* information gathering and *your* information loss go hand in hand.[38] It is an expression of the "laws of thought" best molded to our lot in life. What we cannot do anymore is suppose a physical theory that is a direct reflection of the mechanism underneath it all: that mechanism is hidden to the point of our not even being able to speculate about it (in a scientific way). We must instead find comfort in a physical theory that gives us the means for describing what we can *know* and how that *knowledge* can change (quantum states and unitary evolution). The task of physics has changed from aspiring to be a static portrait of "what is" to being "the ability to win a bet."

This speculation defines the larger part of my present research program.

A. Some Concrete Problems

Problem #1: Pre-Gleason, or Why Orthogonality?

Andrew Gleason's 1957 theorem is an extremely powerful result for the foundations of quantum theory. This is because it indicates the extent to which the Born probability rule and even the state-space structure of density operators are *dependent* upon the theory's other postulates. Quantum mechanics is a tighter package than one might have first thought.

The formal statement of the theorem runs as follows. Let $\mathcal{H}_d$ be a (complex or real) Hilbert space of dimension $d \geq 3$, and let $\mathcal{S}(\mathcal{H}_d)$ denote the set of one-dimensional projectors onto $\mathcal{H}_d$. We shall suppose that whatever a "quantum measurement" is, it always corresponds to some complete orthogonal subset of $\mathcal{S}(\mathcal{H}_d)$. Particularly, within each such orthogonal set, the individual projectors are the theoretical expressions for the possible outcomes of the measurement associated with it.

Assume now that it is the task of the theory to assign probabilities to the outcomes of all conceivable measurements. Suppose all that we know of the way it does this is the following: There exists a function

$$p : \mathcal{S}(\mathcal{H}_d) \longrightarrow [0,1] \tag{30}$$

such that

$$\sum_{i=1}^{d} p(\Pi_i) = 1 \tag{31}$$

whenever the projectors Π_i form a complete orthonormal set. It might seem a priori that there should be loads of functions p satisfying such a minimal set of properties.

[38] Why is that a happy circumstance? Because it implies in part that the book of Nature may not yet be a written product. "The world can be moved."

But there isn't. Gleason's result is that for any such p, there exists a density operator ρ such that

$$p(\Pi) = \mathrm{tr}(\rho\Pi) \ . \tag{32}$$

In words, Gleason's theorem derives the standard Born probability rule *and*, in the process, identifies the quantum state-space structure to be the density operators over $\mathcal{H}_d$. Moreover, he gets this from assumptions that are ostensibly much weaker than either of the end results. This theorem is quite remarkable in that it requires no further conditions on the class of allowed functions p beyond those already stated. In particular, there is not even an assumption of continuity on the functions p.

A question on my mind is to what extent, if any, does the structure of this theorem support an information-disturbance foundation for quantum mechanics? I think this might be fruitfully explored by thinking in the following way. The assumptions behind Gleason's theorem naturally split into two pieces. (A) The questions that can be asked of a quantum system *only* correspond to orthogonal projectors onto $\mathcal{H}_d$. A consequence of this is that there is no good notion of measuring two distinct questions simultaneously—that is, there is no good notion of an AND operation for two measurements. And (B), it is the task of physical theory to give probabilities for the outcomes of these questions, and we can say at least this much about the probabilities: They are *noncontextual* in the sense that, for a given outcome, it does not matter which physical question (i.e., which orthogonal set) we've associated it with. This is the content of the assumption that the probability rule is of the form of a "frame function" (a function satisfying Eqs. (30) and (31)).

It seems to me that the first assumption to some extent captures the idea that information gathering is invasive. If you gather some information and I gather some other information, there is no guarantee that we can put the two pieces of information into a consistent picture: my information gathering has disturbed the relevance of the information you've already gathered. The second assumption, however, appears to be more of the flavor that nevertheless such information gathering is not *too* invasive. For otherwise one might imagine the probabilities for a measurement's outcomes to depend upon the full specification of the orthogonal set used in its definition. The Born probability rule clearly has a much weaker dependence on the measurement than it might have had.

A question whose answer could bolster (or discourage) this point of view is the following. Why is the invasiveness of quantum measurement specifically captured by identifying measurement outcomes with orthogonal sets of projectors? Hilbert space has a lot of structure; why single out precisely the orthogonal projectors for defining the notion of measurement? To get a handle on this, we could try to see how it might have been otherwise.

As a wild example, consider an imaginary world where quantum measurements are not only associated with orthogonal projectors, but with the projectors onto *any* complete linearly independent set of vectors. This would be a notion of measurement that made use solely of the linear structure of $\mathcal{H}_d$, eschewing any concern for its inner product. What kinds of probability rules can arise for such a notion of measurement? In particular, can one have an interesting "noncontextual" probability rule in the spirit of Gleason's theorem? More precisely, what kind of functions p can satisfy Eqs. (30) and (31) but with the summation in the latter equation satisfied for any linearly independent set?

Well, it's not hard to see that the only noncontextual probability rule that works for all "measurements" of this kind would have to be the trivial probability assignment of $1/d$ for each outcome, no matter what the measurement. To give an example of how to see this, visualize three linearly independent unit vectors v_1, v_2, and v_3 in R^3 and imagine assigning them probabilities p_1, p_2, and p_3. Hold v_1 and v_2 fixed and rotate the third vector whichever way you wish. As long as it doesn't fall on the two lines spanned by v_1 and v_2, then the projector associated with it must always be assigned the same probability, namely p_3. Now do the same thing with vector v_2, holding v_1 and v_3 fixed. This will make almost all vectors on the unit sphere associated with projectors of probability p_2, proving that $p_2 = p_3$. Finally, one does the same trick with v_1, proving that $p_1 = p_2 = p_3 = 1/3$.

The lesson is simple: if every linearly independent complete set of vectors in $\mathcal{H}_d$ constituted a measurement, one could not hope to retain a noncontextual probability assignment for measurement outcomes without making the world an awfully dull place!

But maybe this version of the game is just too dumb. So, let's try to spice it up a bit by explicitly using the inner product structure of $\mathcal{H}_d$, but in a nonstandard way. Again consider R^3, the smallest Hilbert space on which Gleason's standard theorem can be proved. Suppose now that a "measurement" corresponds to any three vectors with a fixed angle relation between themselves. What I'm thinking of here is to start with three vectors v_1, v_2, and v_3 whose angles (moving around them cyclically) are α, β, and γ. Now rigidly rotate that structure in all possible ways to generate all possible measurements. Are there any interesting *noncontextual* probability rules—again in the spirit of Gleason—that one can associate with this notion of "measurement?"

Here I don't know the answer. But I do know of some special cases where one again gets only the trivial assignment $p_1 = p_2 = p_3 = 1/3$. For instance, take the cases where $\alpha = \beta = \gamma$ and α is such that if we rotate around v_1, v_2 will fall back upon itself after an odd number of "clicks"—what I mean by a click here is rotating v_2 into v_3 and so on ... click, click, click. What happens if we run through such a "clicking" process? Well, v_1 must be constantly associated with the same probability value p_1 by the assumption of noncontextuality. But then by that same assumption, as v_2 rotates into the old v_3, it must pick up the probability p_3. And so on it will fluctuate up and down: p_2, p_3, p_2, p_3 ... until it finally falls upon its original position. If this happens in an odd number of clicks, then it will have to be the case that $p_2 = p_3$ or the assumption of noncontextuality would be broken. Similarly we can see that the whole circle generated by rotating v_2 and v_3 around v_1 must be "colored" with the same probability value. Finally, run through the same process but by rotating about the vector v_2. This will generate a second circle that intersects with the first. From that and the assumption of noncontextuality, it follows that $p_1 = p_2 = p_3 = 1/3$ and this will be true of any triad by the very same argumentation.

Another case where one can see the same effect is in the single qubit Hilbert space C^2. There the game would be that any two vectors with a fixed angle α between them would constitute a measurement. Thinking about the Bloch-sphere representation of C^2, one can use the argument similar to the one above to see that whenever $\alpha \neq 90°$ the only possible noncontextual probability assignment is $p_1 = p_2 = 1/2$ for all possible measurements.

Conjecture 1: *In the case of R^3, whenever one of the angles α, β, or γ is not identically $90°$, then the only possible noncontextual probability assignment for measurements* *outcomes will be the trivial one* $p_1 = p_2 = p_3 = 1/3$.

How to tackle such a problem? I think it may not be too hard actually, especially if one assumes that the noncontextual probability assignments, whatever they are, must be continuous functions. The starting point would be to try to trace through Asher Peres's derivation of the standard Gleason theorem in his textbook. There, something will surely fail when one looks at the expansion of the proposed "frame functions" in terms of spherical harmonics. (I hope someone will bring Asher's book with them to the meeting: I would bring mine, but I don't have it anymore.)

What's to be learned from this problem? I'm not quite sure, but I think mostly it will help reinforce the idea that our standard notion of quantum measurement is not simply an arbitrary structure. It's there for a reason, a reason we still need to ferret out.

Problem #2: Wootters Revamped with POVMs.

Bill Wootters in his Ph.D. thesis explored an alternative derivation of the quantum probability rule. His work was based on the hope that it could be obtained via an extremization principle much in the spirit of the principle of least action in classical mechanics. (I believe he may talk about this very problem at the meeting.) The quantity extremized was the Shannon information a measurement reveals about a system's preparation, under the assumption that one has many copies of the system all with identical measurements.

Specifically the scenario was this. Consider a *real* Hilbert spaced of dimension d and a fixed orthogonal basis within that space. One imagines that one has possession of N copies of a quantum system with that Hilbert space, all with precisely the same quantum state $|\psi\rangle$. Which quantum state? One drawn randomly with respect to the unique unitarily invariant measure on the rays of $\mathcal{H}_d$, only one doesn't know which. The fixed orthogonal basis represents a measurement that one will perform on the separate copies in an attempt to ascertain the unknown preparation.

[**NOTE:** *Here, through the remainder of this problem set, the words were thrown together hurriedly after the fire.*]

Anyway, Bill's attempt of a derivation didn't really work so nicely for complex Hilbert spaces. The question here is, can we make it work after all, if we start thinking of POVMs as a primitive notion of measurement in its own right.

Specifically, the first thing we must ask is does there always exist an informationally complete set of rank-one POVM elements all of equal weighting on $\mathcal{H}_d$. And it might be even nicer if that set could taken to have precisely d^2 elements. That is, for each d, does there exist a set of d^2 projectors $|b\rangle\langle b|$ and a positive number g such that

$$g \sum_{b=1}^{d^2} |b\rangle\langle b| = I \ ? \tag{33}$$

We know that there does exist such a set if $d = 2$ or $d = 3$. When $d = 2$, just take any four states corresponding to the vertices of a regular tetrahedron on the Bloch sphere. For the case $d = 3$, Bill has explicitly worked out an example that perhaps he can remind us of. Also I remember some vague murmurings by Armin Uhlmann that "of course" they exist in all dimensions. But still, we should treat the existence in all d as an open question—I'm not sure how much of Armin's talk was statement of known fact, how much was conjecture, and how much was faith.

If such a set exists always, then we can ask of it precisely the same question that Bill did in his thesis. Assume we don't yet know the quantum probability law: we only know that there is some function f for which the probability p_b is given by

$$p_b = f(|\langle\psi|b\rangle|^2) \tag{34}$$

when the system's "unknown" preparation is $|\psi\rangle$. What function f extremizes the information we gain about $|\psi\rangle$ when we have only one copy of the system available? What function f extremizes the information when we have a very large number of copies available?

What does this have to do with my manifesto? Perhaps nothing. But I have always felt that Bill's attempt at derivation was missing something in that nowhere in it did it make use of the idea that quantum measurements are invasive beasts: it talked about information, but it didn't talk about disturbance. If Bill's derivation does turn out to work nicely by the addition of POVMs, then maybe that will be some motivation for me to rethink my ish-ism.

Problem #3: Post-Gleason, or Should I Think von Neumann Is Special?

For a long time, I have disliked the tyranny of thinking of von Neumann measurements as more fundamental than other POVMs. Here's a question that might break some of that orthodoxy.

Suppose such a set of informationally complete POVMs as described in the last problem exists. Let us think of the class of all "primitive" measurements on $\mathcal{H}_d$ as those that can be gotten from acting the unitary group on that set. For instance, for a single qubit, the primitive measurements would correspond to all possible regular tetrahedra draw on the Bloch sphere.

Let us now imagine a notion of "frame function" as in Problem #1 for these kinds of measurements.

Question 1: Is there a Gleason-like theorem for these structures? And in, particular does the extra freedom of having d^2 outcomes to play with simply the proof of Gleason's result?

Question 2: By use of the Church of the Larger Hilbert space, can we construct an arbitrary POVM with this notion of primitive measurement. That is, is there a kind of Neumark extension theorem for this notion of measurement?

Problem #4: Where Did Bayes Go?

From my point of view, quantum states are best interpreted as states of knowledge, not states of nature. Quantum mechanics is **mostly** a "law of thought" in that it provides a firm method of reasoning and making probabilistic estimates in light of the fundamental **physical** situation that the world is "sensitive to our touch."

With that in mind, I have to ask myself why doesn't wavefunction collapse look more like Bayes' rule for updating probabilities under the acquisition of new information. Recall Bayes' rule for when we acquire some data D about a hypothesis H:

$$P(H|D) = \frac{P(H)P(D|H)}{P(D)}\ . \tag{35}$$

On the other hand, when we perform an efficient POVM $\{E_b\}$ and find outcome b, we should update our quantum state ρ according to

$$\rho \longrightarrow \tilde{\rho}_b = \frac{1}{p_b} U_b E_b^{1/2} \rho E_b^{1/2} U_b^\dagger \;, \tag{36}$$

where U_b is some unitary operator and $p_b = \mathrm{tr}\rho E_b$.

Forgetting about the unitary U_b for the present discussion, notice the difference in expression of these two "collapse" rules. Bayes' rule involves states of knowledge alone: it is constructed solely of probabilities. Quantum collapse, on the other hand, appears to involve two distinct kinds of entities: density operators and POVMs. Can we put it into a form more reminiscent of Bayes' rule and perhaps learn something in the process.

Here's one way that I think might be fruitful. For each density operator ρ and each POVM $\{E_b\}$, we can construct a canonical decomposition or refinement of ρ: just multiply the equation $I = \sum E_b$ from the left and the right by $\rho^{1/2}$. We get,

$$\rho = \sum_b p_b \rho_b \;, \tag{37}$$

where

$$\rho_b = \frac{1}{p_b} \rho^{1/2} E_b \rho^{1/2} \;. \tag{38}$$

Note that with this, and just a little bit of algebra, we can rewrite the collapse rule (again forgetting about the U_b) to be

$$\tilde{\rho}_b = \rho^{-1/2} \left(\rho^{1/2} \sqrt{\rho^{-1/2} \rho_b \rho^{-1/2}} \rho^{1/2} \right)^2 \rho^{-1/2} \tag{39}$$

This expression is, I think, quite intriguing. This is because it turns that the quantity in the large parentheses above,

$$G(\rho_b, \rho) \equiv \rho^{1/2} \sqrt{\rho^{-1/2} \rho_b \rho^{-1/2}} \rho^{1/2} \tag{40}$$

has been characterized independently in the mathematical literature before. It appears to be the most natural generalization of the notion of "geometric mean" from positive numbers to positive operators. Here are some references:

1. W. Pusz and S. L. Woronowicz, "Functional Calculus for Sesquilinear Forms and the Purification Map," Rep. Math. Phys. **8**, 159–170 (1975).
2. T. Ando, "Concavity of Certain Maps on Positive Definite Matrices and Applications to Hadamard Products," Lin. Alg. App. **26**, 203–241 (1979).
3. F. Kubo and T. Ando, "Means of Positive Linear Operators," Mathematische Annalen **246**, 205–224 (1980).
4. M. Fiedler and V. Pták, "A New Positive Definite Geometric Mean of Two Positive Definite Matrices," Lin. Alg. App. **251**, 1–20 (1997).

Can someone bring these references? As you know, my copies don't exist anymore. (If you only have time to copy one, perhaps the last one is best ... as it gives a large summary of all things known. Jozsa may be most interested in the first reference by Pusz and Woronowicz. Kubo and Ando would be the third most useful.)

Something fun to do is to rewrite the classical Bayes' rule in a similar form as this revamped quantum rule. Indeed the standard geometric mean crops up in precisely this way. Now try to further the analogy if you can.

One possible fact I seem to recall is that the operator geometric mean can be characterized in the following way. Start with ρ_b and ρ and consider the following matrix, where X is also a positive semidefinite matrix:

$$\begin{pmatrix} \rho & X \\ X & \rho_b \end{pmatrix} \tag{41}$$

Then $X = G(\rho_b, \rho)$ is the matrix that maximizes the above matrix in the sense of making it as large as it can be in the standard matrix ordering sense.

Can we learn something about why the collapse rule takes the form it does from this exercise. (Perhaps Jeff Bub can give us an introduction to his other characterization of the Lüders collapse rule.)

B. Some Not-So Concrete Problems

Proto-Problem #1: Computing Power vs. Error Correctability.

We know (suspect) that quantum computing gives us a speed up over classical computing. But we also know that we have to strain slightly harder to get error correction and fault tolerance for it. Imagine now the set of all computational models (whatever that might mean). Within that set will be both classical computing and quantum computing, but also a lot of other things. Could it be the case that quantum computing hits some kind of happy medium, the one where the ratio of speed-up to error correction resources is best? (You can see that this is directly motivated by my parable: could the speed-up of quantum computing be due to this world's wonderful sensitivity to our touch?)

Proto-Problem #2: Entanglement Monogamy and Schrödinger's Insight.

Can we think of a way of viewing quantum entanglement as a secondary effect? The primary effect being information disturbance tradeoff. Let me just cut and paste an old email here. [See the note to Todd Brun, titled "Information Theoretic Entanglement," dated 8 June 1999, and the note to Howard Barnum, titled "It's All About Schmoz," dated 30 August 1999.]

Proto-Problem #3: Down with Beables, Up with Dingables.

Quantum logicians (and presumably Jeff Bub), like to think that quantum mechanics is about "what is," i.e., beables not observables (a phrase coined by Bell). The only way they can do that is by introducing a kind of logic about the "facts of the world" that is different from our usual logic of AND, OR, and NOT. I, on the other hand, like to think that that kind of change of logic is a strong indicator that we just can't get a notion of a "free-standing reality" within physics.

So the question on my mind is what kind of algebraic structure (if any) does my parable indicate/motivate for a purely algebraic approach. I've already said, I don't think AND makes any sense at all in such a world. Do the other notions from standard logic still make sense though?

I realize, I've left too much vague in this question. Talk to me at the meeting.

Proto-Problem #4: Intrinsic Characterization of Complete Positivity.

I really don't like the Church of the Larger Hilbert space. Let's see how far we can get toward CPMs and POVMs without ever having to assume it. Is there a physically motivated criterion for CPMs that makes no reference to the Church.

Proto-Problem #5: Doing Gleason with Algebraic Numbers.

Problem motivated by Bennett remark. Recently Pitowsky and Meyer have raised some interesting questions about Gleason's theorem when the number field of quantum mechanics is restricted to the rationals. It might be useful for our understanding of QM to ferret out what is essential and what is not in its formulation. Cabello and Peres completely discount Pitowsky and Meyer because their world has no superposition principle. But who cares? Well, maybe if Gleason still works in the minimal world with a superposition principle (i.e., algebraic number fields), we should think harder about the meaning of the "superposition principle."

Proto-Problem #6: Challenge to Everettistas.

- Would your interpretation still work if the number field of $\mathcal{H}_d$ were the reals instead of the complexes? If it were the rationals instead?
- Would your interpretation still work if the time evolution of the universe as a whole were nonlinear instead of unitary?
- Would your interpretation still work if the collapse rule of QM were anything different from the standard one?
- Would your interpretation still work if _______? [You fill in the blank, challenge yourself.]

I understand that I'm being belligerent, but I *suspect* your answers to each of these will be "yes." Cf. Any of David Lewis philosophical works on the modal logics and the plurality of worlds, or Max Tegmark's paper `gr-qc/9704009`, "Is the 'theory of everything' merely the ultimate ensemble theory?" The tentative conclusion I draw from this is that the Everettista has a contentless interpretation.

12-09-02 *The Copenhagen Interpretation* (to C. King)

I'm in Copenhagen at the moment and will be here until next Tuesday. I'm giving two talks in the Math department—one tomorrow and one Monday—and then I've got an appointment at the Niels Bohr Institute Monday afternoon. With some luck, there's a chance I'll get to talk to Niels Bohr's son Aage (Nobel prize sometime in the 1950s or 60s). He's recently turned his attention to quantum foundations. In any case, I'm definitely talking with his collaborator Ole Ulfbeck. If the Bohr thing comes through, I'll call this my best vacation ever!

17-09-02 *Wow!* (to G. Brassard)

Wow, read this introduction to Appleby's paper!!!! You've just got to tell me that we can have him at the meeting (and pay his way if need be)! (I feel like a kid in a toy store, screaming, "Daddy, daddy, I've just got to have that toy!")

Introduction

The stimulus for this talk came from a number of conversations with Chris Fuchs about his information-theoretic approach to the interpretation of quantum mechanics (include references).

Fuchs starts from the position that the quantum state simply is a probability distribution[39]: that, and nothing more whatever. He also takes a radically Bayesian approach to the interpretation of probability[40]. According to him the quantum state has a *purely* subjective significance: it simply represents one's expectations (or beliefs, or gambling commitments) regarding the outcome of a measurement. On these assumptions the collapse of the wave-function is no more remarkable than the collapse of a classical probability distribution when new information is acquired—no more remarkable than, for example, the probability of a coin landing heads being 1/2 before the toss, but either 1 or 0 afterwards.

Of course, this proposition, that the wave-function should not be interpreted as an objectively real entity, is the starting point of the Copenhagen Interpretation. So it might appear from the above that Fuchs is proposing nothing very new. However, I think that that would be wrong. It seems to me that Fuchs's programme is best described as an attempt to reinvigorate the Copenhagen Interpretation. To breathe new life into the old bones. The qualification is crucial. If Fuchs's hopes were fulfilled—indeed, if his hopes were only half-fulfilled—the consequences would, I think, be revolutionary.

The term "Copenhagen Interpretation" , as it has previously been understood, is not very sharply defined. To begin with, it isn't really a single interpretation at all. Rather it is a family of interpretations, held together by certain common features. Moreover, even if one focuses on a particular variant, one finds numerous obscurities. The writings of Bohr, in particular, are notoriously difficult to follow.

If I have understood him correctly Fuchs would argue that what is contained in the work of the Copenhagen school is, not so much a completed interpretation, more a project: a direction of thought which, if pursued with sufficient determination, might eventually *lead* to a satisfactory understanding of what quantum mechanics is really telling us. Unfortunately, the programme which they initiated, during the heroic age of the subject, has languished ever since.

Fuchs suggests that if we were really to follow through on the basic insights of the Copenhagen school it might have major implications, not just for our philosophical understanding, but on an empirical level. As he puts it

> I have this "madly optimistic" (Mermin called it) feeling that Bohrian-Paulian ideas will lead us to the next stage of physics. That is, that thinking about quantum foundations from their point of view will be the beginning of a new path, not the end of an old one. (samizdat, p. 173)

I find these ideas deeply interesting. I share Fuchs's feeling that the present situation in quantum foundations is one of "impasse". His programme excites in me the hope that this could be a way to break out of the sterile circle.

[39]Specifically: it is the probability distribution for the outcomes of an informationally complete (generalized) measurement.

[40]The interpretation of probability gives rise to almost as many conflicting views as does the interpretation of quantum mechanics. So saying that the wave-function simply is a probability distribution is not yet a complete answer. It is also necessary to say what a probability distribution is—which is not so easy (at least, it does not seem easy to me).

Before becoming acquainted with Fuchs's ideas I was definitely not an adherent of the Copenhagen philosophy[41]. It was not so much that I disagreed with the Copenhagen Interpretation. I could not get as far as that. I felt that it was not sufficiently clear for me to be able to tell whether I agreed or disagreed. In other words, I took the same view as Bell: the Copenhagen philosophy struck me as "unprofessionally vague and ambiguous" (*Speakable*, p. 173). The degree of vagueness and ambiguity is, in places, such that agreeing to these propositions would be like signing a blank cheque: you might be committing yourself to almost anything (I think Beller scores some palpable hits in this respect).

However, Fuchs's ideas have caused me to revise this assessment. It is not, let me hasten to say, that, where I formerly saw vagueness and ambiguity, I now see sharpness and rigorous exactitude. On the contrary: the Copenhagen Interpretation remains, in my view, as obscure as ever it was. And I continue to think that this means it cannot be seen, in the way Bohr and others wanted it to be seen, as the terminus of all intelligible thought on the subject. If, however, one looks at it from the standpoint Fuchs suggests, not as a *finish*, but as a *start*—not as a completed interpretation, but as a project—then the ideas of the Copenhagen School appear much more interesting. What I had not properly appreciated, before reading Fuchs, was the extent to which my rejection depended on the claim to complete finality. If one subtracts this claim—if, instead of regarding Bohr as the voice of unimpeachable authority[42], one merely sees him as someone groping toward ideas which he himself did not adequately comprehend—then I think one can easily acknowledge, without doing any damage to one's logical conscience, that there is much in Bohr's writings which is deeply suggestive. Seen in this light the obscurities in Bohr's account may appear, less as grounds for rejection, more as a challenge. One may be stimulated to think that, concealed among the obscurities, there could be the germs of something cogent, which it might be worth one's while to try to ferret out.

Reading Fuchs has not persuaded me of the basic correctness of the Copenhagen philosophy. It has not even persuaded me of the correctness of a kind of neo- or revitalized Copenhagenism. Such a statement of belief would, in the circumstances, be premature. In any case, it seems to me that, if history shows anything, it shows that we ought to be extremely cautious about making unreserved commitments at the level of basic concepts. Like Fuchs, I do not believe in final theories.

However, what I am persuaded of is that Fuchs has opened up an extremely interesting line of thought, which deserves to pursued very seriously. I do not know where his programme will lead. But even if it does not lead in precisely the direction he now envisages, I feel it promises to take us significantly further forward.

17-09-02 *No-Cloning* (to S. L. Braunstein)

Not only does he miss the full result, but there is a line in there where he actually gets it wrong. For the full story, see page 451 of my samizdat, `quant-ph/0105039`.

I'm in Heathrow at the moment, heading home from my first trip to Copenhagen. What a

[41]This is not to say that I preferred some other interpretation. I belonged to the school of the frankly perplexed, which does not pretend to know how quantum mechanics should be interpreted.

[42]Which, it would seem, is how he all too often was regarded, in the past. For a discussion of the "overpowering, almost disabling, impact of Bohr's authority", see Beller [add reference].

wonderful place! Copenhagen, that is.

17-09-02 *Wigner* (to S. L. Braunstein)

You still don't get it: Wigner actually gets it WRONG. There is a line in the paper where he says, (essentially) "all linear superpositions of living states must also be living states." That cannot be; that's the point of the no-cloning theorem. But he didn't even realize that he made a mistake.

19-09-02 *Chris's Blurb* (to R. Pike)

Steven van Enk is indeed a physicist. I don't know what I am: physicists tend to call me a mathematician, while mathematicians generally call me a physicist. Information theorists call me a philosopher. But I practice quantum information. Predominantly I have been interested in formalizing and quantifying the idea that information gathering causes a disturbance when it comes to quantum phenomena. That idea has steered me to several problems in the field, including: developing criteria for successful quantum teleportation, calculations in quantum channel capacity theory, work in quantifying quantum entanglement, and exploring novel notions of quantum nonlocality.

19-09-02 *Unsurprising Fact* (to N. D. Mermin)

Merminition 94: *Did you know that Bob Griffiths believes that within each and every one of his frameworks there is a "history that actually occurs"?*

Yes, I did. He says so quite explicitly in the *Scientific American* article (or was it *Physics Today*?) he wrote with Omnès. I talked with Omnès at length about it at the NATO meeting in Greece you sent me to a couple of years ago, picking particularly on this point.

I'm not sure whether I've told you this in the past, but I am convinced that the consistent historians do nothing beyond what (some interpretations) of Bohr already do. Focus for the moment on a SINGLE standard observable. If one considers that observable in isolation—i.e., without consideration of all other possible observables—then there is absolutely nothing to stop one from acting AS IF one of the values of the observable obtains and all the others don't. Where one runs into trouble (via Kochen-Specker, etc.) is if one tries to hold that view for all observables simultaneously. Now, what do the consistent historians do? Instead of playing the game above for a single standard observable they do it for a single so-called "consistent set of histories." But from my point of view, all a consistent set of histories is, is an arbitrarily singled-out kind of multi-indexed POVM. The point being, woop-ti-do! You give me *any* POVM, and there is nothing to stop me from acting *as if* one of its values obtain and all the others don't. I would only run into trouble (through KS, etc.) if I were to try to play this game for various noncommuting POVMs simultaneously. When Griffiths and company command that one cannot consider distinct sets of consistent histories simultaneously, all they are doing is what I could already have done with *any* POVM. There is nothing deep there.

19-09-02 *Black Sheep* (to A. Kent)

Kentism 4: *I think there's lots of interest to talk about in quantum foundations, and of course in quantum information, but I'm not at all sure that quantum information actually sheds any new light on the deeper problems of quantum foundations. Is this heresy tolerable?!?*

Tolerable, and in fact, maybe welcome. That can be your theme for the week and the subject of your talk. If you can defend it well, that'll surely lead to some good discussions.

20-09-02 *How's It Look?* (to R. Pike)

I just threw thing below together. I'm going to lunch now, but when I get back I'll put the finishing touches on (if there are finishing touches to put). Tell me what you think.

Employee Report on Activities and Accomplishments
Oct. 2001 – Sep. 2002

> This year has been another good one for science. I had eight articles going to press, all of them confirming the thread of thought that has become so important to me: the predominant structure of quantum information is automatically and deeply connected to the structure of Bayesian probability theory. Where the two structures differ is where one should look for the power of quantum information as an information-processing and communication resource. On the practical side, this has implications for understanding quantum information experiments performed with standard laser light, such as the quantum teleportation experiment performed at Caltech and more recently at the Australian National University.
>
> I consider two results my better ones of the year. The first had to do with the settling of a long drawn-out debate in the literature concerning the compatibility of quantum-state assignments made by different experimentalists with differing amounts or kinds of information about a single quantum phenomena. (This work is soon to appear in Physical Review A, co-authored with C. M. Caves and R. Schack.) The second had to do with the precise form of the quantum-state change rule under the gathering of experimental data. This phenomenon is normally mentioned in conjunction with the "von Neumann collapse postulate." Using a little-observed fact from linear algebra, I was able to express the collapse in a way that makes it look *almost* identical to standard probabilistic Bayesian conditionalization. This I believe sheds significant light on the process of quantum-state-change-through-measurement, and, consequently, as already alluded to, points toward the key ingredient in the power of quantum information processing. The evidence that the quantum information community also sees these efforts as shedding light can be found in the number of invitations to speak I have been given this year.
>
> I count having given 14 external invited talks this year. These talks have carried me to Japan, Ireland, Australia, Spain, Sweden, and Denmark, all at zero cost to Lucent. On the official-recognition side of things, I was awarded an (international) E. T. S. Walton Visitor Award by the Science Foundation Ireland, which would translate into a 20% increase in salary and substantial equipment and travel funds, if accepted. (Presently the award is being renegotiated into a small series of small visits.) I made three media appearances, via short interviews in *PC Magazine*, *PC Magazine Online*, and the book *The Best American Science Writing 2002*.
>
> Concerning general service to the physics community, I served as an Associate Editor for the Rinton Press journal *Quantum Information and Computation*, as a member of the Advisory Board for the International Center for Mathematical Modeling in Physics, Engineering and Cognitive Sciences at Växjö University, Sweden, and as a member of the Advisory and Award Committee for the Sixth International Conference on Quantum Communication, Measurement and Computing (QCMC'02). I co-organized an

international meeting *Workshop on Quantum Foundations in the Light of Quantum Information*, to be held at University of Montréal, October 13 – November 3 this year, co-edited a special issue of the journal *Studies in History and Philosophy of Modern Physics* devoted to quantum information and quantum computation, and mentored two visiting graduate students for DIMACS for six weeks this year.

As internal service goes, I mentored a summer intern for the SRP program, I gave two talks for Lucent customers, France Telecom and SBC, and attended the Army Research Office annual review of funding for quantum computation and quantum information to strengthen the ties between Bell Labs and government funding sources. Finally, I played a significant part in a lively visitor program of quantum information scientists of all walks to present their research to the Bell Labs community.

23-09-02 *Genuine Fortuitousness* (to H. J. Folse)

I haven't talked to you in a while. I was in Copenhagen last week, and I got a chance to meet with Ole Ulfbeck, who has recently written a quantum foundational paper with Aage Bohr titled "Genuine Fortuitousness: Where Did That Click Come From?" Have you seen it? If not, you can find it in Foundations of Physics **31**, 757–774 (2001).

In it, young Bohr makes a quite sharp turn around from his father's position. It's kind of a shame, and as I understand it all, not a system worked out to any great extent. Just wondering if you've got any opinions.

By the way, I noticed that you never submitted you Växjö paper to the `quant-ph` archive. I think you ought to do that since it is already written in LaTeX. If the concept frightens you, I could submit it for you; it's a rather easy procedure and would only take about 10 minutes of my time. (Though, of course it would be best if you did it, so as to retain control, etc.)

Henry's Reply

Right now Isidore is bearing down on New Orleans, so they've canceled classes and I have time to catch up on email. Sorry I didn't know you were in Copenhagen, I would be glad to provide an introduction to Jan Faye at Kobenhavns Universitetet, whose work you know. I've also been corresponding recently with Erik Rudinger who is teaching a course on Bohr, I presume at NBI, and who edited a lot of the Archives. He is a mine of Bohr information and I'd be glad to provide an introduction there as well.

Yes, I did have that Ulfbeck and A. Bohr article called to my attention. [...]

It definitely appears to me that this view rejects a big portion of Father Bohr's commitment to the reality of atoms and embraces an extreme anti-realist attitude which I, for one, do not attribute to Niels Bohr, though of course many others have indeed taken to be his view. But at the same time I do see a lot of bits of the father's ways of thinking poking through the mix. Niels constantly reflected on the theme of the limits of "space-time description" and in the period from 1913 to 1925 he wrote repeated bits and pieces with the title "Space-time Description" ("Rum-tids Beskrivelse," in Danish). In the end, i.e. by Como, he concluded it stood in a complementary relation to "the claims of causality." I think this period is very revealing precisely because there was then no formalism to reason from, and one had to think out experimental interactions (observations) physically, so to speak, without any mathematical algorithm to help (or worry about). Already in his 1913 model Niels *knew* that the electrons in an atom couldn't be considered as "real particles" in the classical sense. Talk about the electrons "orbiting" the nucleus was thus already then not to be taken in a classical realist sense. The discontinuity in change of state of atomic systems in an interaction between, as he put it, "radiation and matter" implied it was meaningless to ask whether the electron in one orbit before the "jump" was the same as the electron in a new "orbit" after the jump, and of course the attempt to trace the electron's spatio-temporal career through this interaction is explicitly forbidden. So Bohr concluded that talk about elementary particles as though they were the sorts of things that traced classical trajectories was used solely for its instrumental value in "interpreting" the interaction as a "measurement" determining some property of the "object" system. The interpretation of elder Bohr that I have defended is that he rejects regarding the citizens of the microdomain as realistically represented by "particle" or "wave" "pictures" (the "accustomed demands for visualizability"), but that he nonetheless regards such entities as *real* (not as instrumental fictions) because they are one of the interactors in the "indivisible" interaction with the observing instruments which provide the empirical evidence for the theory. Electrons, in short, are *real*, but they're not *really* particles (or waves, either).

Now U&B clearly reject the reality of particles in a more uncompromising way. I think this is a version of the class of interpretations you have categorized as "correlations without correlata," and they of course do refer to Mermin's article. (What did he think about U&B?) In philosophical jargon we would say it is a macrorealism combined with a micro anti-realism. "Clicks" are real; but electrons are just, as it were, "the set of all electron clicks." Of course they're able to dispense with any commitment to a microreality only by paying a very high price in insisting that the occurrence of such clicks is "genuinely fortuitous." Immediately after defining this phrase, they write: "What makes the click 'irrational' is seen to be that the variable, which manifests itself by the click, never enters spacetime." (p. 762) (The reference 'irrational' struck me because Father Bohr was fond of saying "the quantum postulate with its inherent irrationality" which a lot of people completely misunderstood in a positively perverse way.) Now this seems to me to be a very strange way of speaking; indeed, I get the feeling that their "genuine" fortuitousness, isn't so genuine after all.

We are supposed to undercut any problems with crossing the bridge from the formalism to the physics because "matrix variables" never "enter spacetime." However, when they "manifest themselves" in spacetime, they do so with good old fashioned classical single valued measured outcomes. It all sounds quite ghostly: if I am told "poltergeists never enter spacetime but they manifest themselves by making noises and making doors creak open" then I understand this to mean they are non-physical (out of spacetime) *causes* for physical (empirically observable) effects. Thus it seems to me that they're really just *saying* the "click" is "genuinely fortuitous" but at the same time talking about how it is "caused" by the "manifestation" of a "matrix variable" when it "enters" spacetime. Talk of "variables" of course originally was a way of talking about *properties* of macrophysical real entities which were functions of time. Now it seems that matrix variables are a way of talking about properties of outside-of-spacetime entities (aka "atomic systems" or "systems in quantum mechanical states"). If clicks are real, they are the "manifest*ed*," but if the manifest*ed* are real, that implies the reality of a manifest*or*, the thing being "manifest*ed*" when it "enters" spacetime. So what began as an anti-realist ontological minimalism (only click events are real) blossoms into a whole 'nother "reality" which is characterized by "matrix variables" in a way somehow analogous to the way old time classical reality was characterized by single valued variables. We see only the surface of the pond, but the bubbles and eddies on its surface are the manifestations of the doings of submerged beings beneath that spacetime surface. I expect U&B would reject that, and say that's only a way of speaking to say that "clicks" are such "manifestations," but the point is that it is damn hard to do physics when you stop talking about "causes," and the temptation always lurks to simply paper over the problem with an alternative locution. [Kant for example, forbad any application of the category of "causality" transcending the phenomenal, empirical world. But at the same time, in order to cling to realism and not cave into idealism, he wanted to see these phenomena as the "manifestations" of "things-in-themselves" not experienced but necessarily "posited" by the cognizing subject. He realized, of course, he couldn't say "noumena *cause* phenomena" so instead he started talking about the noumenon as the "ground" of the phenomenon. But everyone saw this as a transparent ploy to circumvent the strictures on "causality," and in point of fact in history philosophy did cave in to idealism.]

The same things happens when U&B talk about "associating" one set of "source clicks" with another set of "measurement clicks" in the case of alpha decay of uranium. (They also use expressions like "give rise to," etc.) The phenomenon of "alpha decay" manifested in our macro-surface-of-the-pond world of spacetime refers to, we are told, a certain distribution of clicks. With that phenomenon we *associate* another set of macro clicks which we refer to as the phenomenon of "a lump of pure uranium turning into half thorium after half-life t." But if "causes" is treated as "constant conjunction in time" we have in effect just said the uranium's transmutation into thorium is the cause of the alpha radiation. Moreover, we must still make use of particle pictures in designing the "clicker" as a detector of the sort of "particles" we are looking for, a fact which the elder Bohr certainly would have stressed in his talk about interaction. We can know of causal connections quite independently of knowing anything about the mechanism by which such connections are affected, or indeed if there be any mechanisms at all. We knew sexual intercourse causes babies long before we knew any biology. All U&B have really professed here seems to me to be that uranium decay causes emission of alpha particles, but we don't know the mechanism by which it is accomplished, and we do know whatever it might be it isn't a classical one capable of being predicted deterministically. Instead we speak of these processes using the language of "manifesting," "entering spacetime," "giving rise to," etc. So it seems, when all is said and done, quantum mysteries abide. If this be demystification, it seems more like demystification by *fiat*, rather than one earned by explaining the mysteries. Not that this does not happen in the history of physics.

You might be interested to learn that the biennial meeting of the Philosophy of Science Association this coming November features no less than two sections on Bohr and the Copenhagen Interpretation, one of which is chaired by Jeff Bub, whom you know, of course. The other is devoted to Mara Beller's inflammatory *Quantum Dialogues*. So it seems that interest in Bohr is heating up again. The program is at: `http://www.pitt.edu/~psa2002/`. It would no doubt be as weird to you as Växjö was to me, but it'd be fun if you could come.

To change subject abruptly: I obviously do not keep up with the `quant-ph` archive and am only dimly aware of it through your references. I would greatly appreciate your posting the paper there. What do you need me to do?

BTW, in connection with the Proceedings of the Växjö conference I got an email from Andrei in June saying that "Soon we sill be able to send you copies of this volume." I have never received any copy. Did they actually send them? Did I miss my copy?

Oh, also, did you ever get together with van Fraassen, and what happened if and when you did?

23-09-02 *Appleby Confirmed* (to G. Brassard)

So, Appleby is confirmed for the last week of the meeting. Thus, basically, the end of the meeting is full now. The issue now is what is going to happen at the front of it? It looks like we've got loads of room there. (Though I still wait to hear from you how many people we can really house.)

If I don't hear from you as the day passes, I'll probably be calling you!

23-09-02 *Your Future* (to R. W. Spekkens)

Spekkensism 11: *The more I think about quantum foundations, the more I become sure that you are on the right track. For a variety of reasons, I am now absolutely convinced that the quantum state represents information, and that we are on the verge of major advances in our understanding of quantum mechanics. So, what's the news on funding?*

You know, even if it weren't obvious that you were pulling my strings, I'd still want you as a postdoc. But reality is not completely shaped by wants. Tell me about your back-up plans. [. . .]

I'll be in Waterloo, Oct 2-8, giving talk at Perimeter Oct 4 and a talk at IQC Oct 7. Is there a chance I could visit with you then? Are you very far down the road? I'll probably have a lot of time on the weekend (the 6th in particular).

23-09-02 *Let Me Count the Ways . . .* (to R. W. Spekkens)

Spekkensism 12: *First of all, I am not simply pulling your strings. I am absolutely sincere about my conviction that the state vector is a representation of information. (I'm not about to follow you in your attitudes on realism though.)*

How many times (and how many ways) do I have to say I'm a realist?!?! I'm just a more subtle realist than most. (Read the anti-Växjö interpretation paper again.)

24-09-02 *Title and Abstract* (to D. Poulin)

Does this sound like the sort of thing you guys might want to hear? If not, I could probably put together a new talk on an information function Sasaki and I have been studying. But the present talk has the advantage for me that half of it has already been prepared!

> **Title:** Exchangeable Quantum States / Exchangeable Quantum Operations
>
> **Abstract:** There is hardly a paper in quantum information that does not make use of the idea of an unknown quantum state. Unknown quantum states are protected with quantum error correction, teleported, and used to check for quantum eavesdropping. But what does the term "unknown state" mean? In this talk, I focus on quantum-state tomography and make sense of the term in a way that breaks with the vernacular: An unknown quantum state can always be viewed as a known state—albeit a mixed state—on a larger multi-trial Hilbert space. The technical result is a quantum mechanical

version of the de Finetti representation theorem from classical probability. Interestingly, the theorem fails for real and quaternionic Hilbert spaces, and this teaches us something new about entanglement. Furthermore, a variation of the theorem applies to quantum operations, where it tells us something about the nature of complete positivity. The implications of both theorems for the point of view that quantum states represent states of knowledge, rather than states of nature, may be discussed briefly, time permitting.

24-09-02 *Carriers of the Torch* (to A. Peres)

Asherism 27: *Have you seen my* `quant-ph/0209114`*? It has no relation to* `quant-ph`*. I had tried to put it in* `hep-th`*, but the* `arXiv` *manager thought it was a joke and refused to publish it. After lengthy negotiations, with the help of Mermin, I was told to put it in* `quant-ph`*, whose readers know my name. If I am not concerned about my good reputation with* `quant-ph`*, I'll be able to cross reference it in* `hep-th` *(so I did, also* `gr-qc` *and* `astro-ph`*).*

I just had a read of it; thank you for pointing it out. I'll think about the point you make.

25-09-02 *Ulfbeck and Bohr* (to N. D. Mermin)

Merminition 95: *While I don't think they're [Ulfbeck and Bohr] foolish by any means, I was not terribly excited by what they had to say. If you were, by all means invite them.*

I'm finally getting a chance to clean up my mailbox. Sorry for the long absence.

After writing you the note referred to above, I got a chance to actually read the Ulfbeck/Bohr paper. Before that, I had only had discussions with Ulfbeck. Here's what really surprised me. In talking to him, I got the impression that he was giving me something of a précis of their views, and that I would find the heart and the details in their paper. But I didn't! In fact, as far as I could tell, I didn't find anything more in their paper that I didn't already find in our short conversation ... only said six times over in paper form. "The older view did not adequately account for the genuine fortuitousness of the measurement click."

I know I've complained about Father Bohr's lack of detail when asserting the origin of the quantum formalism, but I think they force my complaints to a new level.

There is, however, one idea in the paper that I am inclined to keep or, at least to me, seems worth trying to develop. I say this predominantly because of its William Jamesian feel. Here it is, deleting the words of theirs that I don't like or don't agree with,

> The click with its onset is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety, with all the complexity required for a break-through onto the spacetime scene. ... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

You see, from the Jamesian viewpoint of "radical pluralism," every piece of the universe, every crumb of its existence, is a unique entity unto itself. Here's a little quote in that direction from his essay "Abstractionism and 'Relativismus' ":

> Let me give the name of 'vicious abstractionism' to a way of using concepts which may be thus described: We conceive a concrete situation by singling out some salient or important feature in it, and classing it under that; then, instead of adding to its previous

> characters all the positive consequences which the new way of conceiving it may bring, we proceed to use our concept privatively; reducing the originally rich phenomenon to the naked suggestions of that name abstractly taken, treating it as a case of 'nothing but' that, concept, and acting as if all the other characters from out of which the concept is abstracted were expunged. Abstraction, functioning in this way, becomes a means of arrest far more than a means of advance in thought. It mutilates things; it creates difficulties and finds impossibilities; and more than half the trouble that metaphysicians and logicians give themselves over the paradoxes and dialectic puzzles of the universe may, I am convinced, be traced to this relatively simple source. *The viciously privative employment of abstract characters and class names* is, I am persuaded, one of the great original sins of the rationalistic mind.

I wish I could find a better quote than that—I have memories of reading the idea expressed in much greater detail and so much more eloquently—but this morning, try as I might, I can't find it.

So I'll end this little note with another note I wrote a few months ago—it carries the sentiment, if not the eloquence. [See note to Greg Comer titled "Music in the Musician," dated 23 April 2002.] It's pasted below. Maybe I should have titled the present article, "A Click is but a Click Not: it is so much more." For the same holds with "clicks" as with "atoms."

26-09-02 *More Ulfbeck and Bohr* (to H. J. Folse)

I hope things are going well for you in spite of Isidore. I see from the weather channel that it made an early-morning landfall in Louisiana.

Thanks for your long assessment of the Ulfbeck/Bohr paper. I enjoyed reading it, and think I agree with many (all, possibly) of the things you said. You asked what was Mermin's reaction to it. The only record I have (other than that he called their ideas "very Mohrhoffian," referring to Ulrich Mohrhoff) is contained in the note below. [See 25-09-02 note "Ulfbeck and Bohr" to N. D. Mermin and 23-04-02 note "Music in the Musician" to G. L. Comer.]

Unfortunately, I think it is going to be almost impossible for me to attend the Pittsburgh meeting. I'll be in Montreal Oct 13–Nov 3 running an extended workshop on "Quantum Foundations in the Light of Quantum Information"—I've got about 30 people coming on and off this time—and there are limits to how far I can push my family. But, boy, I sure would like to be at those Bohr sessions.

Henry's Reply

Folsesm 8: *Quoting Chris Fuchs:*

> There is, however, one idea in the paper that I am inclined to keep or, at least to me, seems worth trying to develop. I say this predominantly because of its William Jamesian feel. Here it is, deleting the words of theirs that I don't like or don't agree with,
>
> > "The click with its onset is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety, with all the complexity required for a break-through onto the spacetime scene. ... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution."

This part of Aage's view is consistent with his father's. In a recent discussion of this, Erik Rudinger gave me a bunch of quotes from Niels: As a collector of quotes, I thought you might like these:

> "In this connection, it is also essential to remember that all unambiguous information concerning atomic objects is derived from permanent marks -such as a spot on a photographic plate, caused by the impact of an electron- left on the bodies which define the experimental conditions. Far from involving any special intricacy, the irreversible amplification effects on which the recording of the presence of atomic objects rests rather remind us of the essential irreversibility inherent in the very concept of observation. The description of atomic phenomena has in these respects a perfectly objective character, in the sense that no explicit reference is made to any individual observer and that therefore, with proper regard to relativistic exigencies, no ambiguity is involved in the communication of information." — (Mid-Century, p. 310 – BCW 7, p. [390])
>
> "... the emphasis on permanent recordings under well-defined experimental conditions" — (Mid-Century, p. 313 – BCW 7, p. [393])
>
> "Moreover, the circumstance that all such observations involve processes of essentially irreversible character lends to each phenomenon just that inherent feature of completion which is demanded for its well-defined interpretation within the framework of quantum mechanics." — (Dialectica, p. 317 – BCW 7, p. [335])
>
> "It may also be added that it obviously can make no difference as regards observable effects obtainable by a definite experimental arrangement, whether our plans of constructing or handling the instruments are fixed beforehand or whether we prefer to postpone the completion of our planning until a later moment when the particle is already on its way from one instrument to another." — (Einstein Paper, p. 230 – BCW 7, p. [370])

27-09-02 *Email* (to R. W. Spekkens)

Concerning the epiphany, you tease me: I don't want to see the research fruits at the moment; I just want to see a psychological description of their originator. You've got to know by now, I find even the emotional aspects of quantum mechanics extremely interesting. Concerning my publishing your words publicly, you can always ask that a given thing you write not be repeated: Plenty of people do that with me, and I always abide.

27-09-02 *A Pearl!* (to A. Peres)

Asherism 28 : *Here is part of a letter I received from an old lady who was a pretty student in 1974 and forced me to learn Bell's theorem. She is now unemployed and has plenty of time to work on physics. She even intends to publish her first paper.*

> What makes me read papers is to make sure that I am original. I have to tell you that I covered quite a material. There were three things that I liked, I don't have enough

words. One is a paper of Chris Fuchs, a PEARL, another one is Mermin's "Ithaca Interpretation" and the third is Gordon Baym's book.

That made my day!

27-09-02 *Commune of the Incorrigibles* (to C. H. Bennett)

Communard Charles,

You're incorrigible. (And you should not read that as merely reflecting a fear of mine about your foundational fetish.) I've marked you for October 15 through 17. It's looking like we're going to have plenty of extra space the first week; I think I'm going to try to see if Reinhard Werner can come then.

If you'd like to stay in a hotel follow the instructions below, post haste. Alternatively, if you want to save Gilles some money, you can stay at the Rockledge Apartments with me: I've got an empty second room during that time. (But whichever way, let me know for accounting purposes.)

Communard Chris

28-09-02 *Better Editing* (to H. J. Folse)

That's it, I think. You should check over the result, making any further changes you want, and then send it back to me. I'll send it to `quant-ph` as time permits (though I'll be in Canada Oct 2–8, so it probably won't get done until after I'm back). [The final result was `http://arxiv.org/abs/quant-ph/0210075`.]

Concerning the content of the paper, I believe I absorbed that pretty well too. This time, however,—you know I've read all but one of your papers—I was struck more than ever for the need of a translation table to mediate between some of your choices (or Bohr's choices) of words and the more modern, more common quantumspeak that present-day practitioners of the field use. I hope to get a chance to write you about this in detail while I'm at the Montreal commune (Oct 13 – Nov 3). There is the seed of something good in your "ontological lesson" section, but I am afraid it will be obscured to most in our field because of the point above.

28-09-02 *Substances and Properties* (to H. J. Folse)

I'm back again. As I said, (hopefully) I'll write in more detail about the content of your paper when I'm in Montréal. In the meantime, let me forward you some letters I wrote contra taking the quantum state (in the usual sense of a $|\psi\rangle$) as a *property* of a quantum system. [See 25-04-02 note "Short Thoughtful Reply" to C. H. Bennett, 14-05-02 note "Qubit and Teleportation Are Words" to C. H. Bennett and others, 14-05-02 note "Chris's World" to J. A. Smolin and others, and 16-05-02 note "King Broccoli" to J. A. Smolin and others.] You made me think of this because of your talk of substances and properties in the "ontological lesson." The letters below were spurred by a discussion about the words "qubit," "entanglement," and "quantum teleportation" soon appearing in English dictionaries. (Charlie Bennett, in particular, was seeking better definitions than had been proposed by some lexicographer. He wanted something that didn't confuse the reader into thinking that quantum teleportation involves instantaneous action at a distance. *Yet*, he still wanted to call a quantum state a property. I found these two demands inconsistent.)

You might enjoy the letters, for what they're worth.

06-10-02 *Ed's Book* (to A. Plotnitsky)

Plotnitskyism 9: *I have a question. Rüdiger in his piece refers to Jaynes' 2003 book on probability. Would you recommended it? I'd like to revisit probability.*

In fact, I wouldn't be able to think of a better place for you to start. Jaynes' book—never finished before his death—is really the very best Bayesian-tilted book to start off with. (It doesn't go as radically Bayesian as I am now going, but that is a minor blemish in comparison to the good it does.)

I see, looking in Rüdiger's paper, that he has cited the soon to be published posthumous version. At the moment, you can nevertheless download it at the website: `http://bayes.wustl.edu/`.

Beyond that though, a good book to understand the more philosophical issues around Bayesianism, have a look at the Kyburg and Smokler book he cites in reference 1.

08-10-02 *FYI Comments on Recent Papers* (to M. B. Ruskai & B. J. Hiley)

Thank you both for sharing your correspondence with me. I very much enjoyed reading them on my flight from Toronto to Boston this morning. There are substantial issues here and it is pleasant to read about them in an evenhanded way.

I got a nice snicker in reading Basil's comment,

Hileyism 1: *In my own study of quantum theory I have often reached the stage when I am led to wonder why a particular author has come out so strongly for a certain point of view and against all others when there is no observational evidence one way or the other. My own long experience of being associated with Bohm's work has brought me directly and forcefully into contact with such questions. For example I have been greeted with the phrase "You're a Bohmian aren't you?" I would like to feel flattered by such a comment but it is often meant as a reference to someone who is regarded as belonging to the complement of sane!*

I certainly feel that social and cultural factors strongly influence these choices. As I have already remarked above they are very implicit and very difficult to bring to the surface, partially because it is universally held that such factors should not have any role to play. Yet why does the debate about the interpretations of quantum theory generate such deep emotions if these questions do not touch the very depths of what we believe lies at the heart of nature and even our very being? There are only two other areas where I have experienced the same passion and energy and that is in politics and religion.

thinking about the pleasant breakfast we had together in Växjö last year and the talk he gave soon after that: "Chris Fuchs bounded up to me this morning and said, 'Are you a Bohmian?!?"' Well, it's good to know a true Bohmian, and to know the distinctions between him and a Goldsteinian.

10-10-02 *Infinitesimal Comments* (to H. J. Folse)

Let me just make some infinitesimal comments on your comments of my comments on van Fraassen's comments.

Folsesm 9: *I interpret this to mean that the justification for your holding that subjective judgment is its success in predicting the POVM's which are themselves objective.*

Predicting the *outcomes* of POVMs, yes. (A POVM—i.e., a *set* of operators with a certain property—is the theoretical expression of a (physical) measurement.) The operators within the POVM express the possible outcomes of the measurement.

Folsesm 10 : *The updating is in a subjective belief, but the cause of the updating is something presumably as objective as a "click"?*

Yes.

Folsesm 11 : *Here it seems to me that the "about the world" part is the objective part. Consider two odds-makers for horse races who give different odds for the same races. Clearly one is rational to "accept" the odds made by the guy who, betting on those odds, wins the most money. So if somebody asks me why do I go by these odds, I answer because of the outcomes. It's all about what I'm justified in believing regarding how to bet. But then if I ask why does one guy succeed over the other, and I discover the successful one has a lot more information about the horses, jockeys, conditions, etc., isn't this "objective" and has nothing to do with what I believe?*

I'm shooting for a Darwinian conception of success here. Success is only defined with hindsight. There is no objective state of affairs that predetermines a "degree of success" for one's future gambles. In that sense, there are no objective probabilities, past gambling success or not.

10-10-02 *Your First Quant-ph* (to H. J. Folse)

OK, the dirty deed is done: You've now had your first `quant-ph` submission. I've checked it and everything appears to be OK; it downloads fine and looks just like it was supposed to.

You once wrote me this:

Folsesm 12 : *Actually I've had at the back of my mind the project of collecting a subset of these papers and trying to get them published as a book. If you do read thru them all, I'd appreciate your telling me what you think of the worthiness of such a project.*

I do indeed think that would be a worthy project; your papers certainly opened my eyes. I'll pledge this: If you ever decide to do it, if you're interested I'd volunteer to write a foreword to the book — I bet you couldn't find a more enthusiastic foreword-writer than me. In particular, I'd love nothing more than to see the quantum information community get a good healthy dose of Bohr. The extent to which I can contribute to making that happen is bound to help me get a little closer to heaven.

13-10-02 *Quantum Foundations Commune* (to L. Hardy and Perimeter Institute generally)

Partially because of the size of the auditorium we ended up with and partially because of our desire to keep the sparks flying, Gilles Brassard and I decided to selectively open the doors for attendance to our quantum foundations commune. Thus, if anyone at Perimeter is interested in audience participation and general discussions, they are more than welcome to come. There are no more official talk slots open at the moment, but that shouldn't stop anyone from giving an impromptu talk on the subject if they'd like to. (Unfortunately though, there are no more travel funds. And if there's any desk space in the offices, we'll probably dole it out on a rotating basis.)

If you wouldn't mind, please help spread the word by forwarding this note to anyone at Perimeter who might be interested. (Presently, I'm cc'ing the note to those of you at Perimeter who are in my address book.)

Below, I'll place four items of information:

a) The tentative talk schedule and attendance table.

b) Part of the original meeting announcement.

c) Instructions for how to obtain a hotel.

d) Directions to the meeting.

Please join us if you can.

26-10-02 *Classical Measurement* (to K. Jacobs)

Jacobsism 2: *You know that classical measurement can just be written as quantum measurements, so long as all the POVM elements and the density matrix commute. But do you know of a paper that actually states this (other than my paper on pooling knowledge). I would like to reference somebody other than myself.*

Sorry to take so long to reply to you. I've been tied up for two weeks with a workshop in Montreal that Gilles Brassard and I organized, and I've hardly gotten a spot of email done in that time.

I don't recall any papers that have discussed the issue explicitly, but I think it is implicit in my discussion of Section 6 of my paper:

- C. A. Fuchs, "Quantum Mechanics as Quantum Information (and only a little more)," in *Quantum Theory: Reconsideration of Foundations*, edited by A. Khrennikov (Växjö University Press, Växjö, Sweden, 2002), pp. 463–543. `quant-ph/0205039`.

Have a look at that and let me know if it's appropriate.

27-10-02 *A Crowded Placetime* (to G. L. Comer)

Thanks for sending your poem about the solitary place. I especially liked the penultimate paragraph.

> My choices are radioactive remedies
> That rot the roots, spin down the spirals,
> Snip the snares, and dampen the dark.
> Something much larger than the universe,
> With all that space and time and matter.

You make me want to quote William James again. This one comes from a letter in his collection (of letters):

> All I can tell you is the thought that with me outlasts all others, and onto which, like a rock, I find myself washed up when the waves of doubt are weltering over all the rest of the world; and that is the thought of my having a will, and of my belonging to a

brotherhood of men possessed of a capacity for pleasure and pains of different kinds. ... And if we have to give up all hope of seeing into the purposes of God, or to give up theoretically the idea of final causes, ... we can, by our will, make the enjoyment of our brothers stand us in the stead of a final cause ...

27-10-02 *Technical Question Minus* (to H. M. Wiseman)

Sorry to take so long to reply to you. I've been inundated with the duties of organizing this meeting in Montreal I'm involved in ("Quantum Foundations in the Light of Quantum Information II"). Luckily, however, I had to fly home this weekend for pumpkin picking, and so I've got a little time on the flights to get some email done.

Wisemanism 28: *He was preparing a section on q. tomography, but I was not happy with it as it did not address the question of how many different projective measurements are necessary to fully characterize a state. I thought this would be well known, but the answer does not seem to be in any tomography papers Gerard can find. My hypothesis is that the answer is $N+1$, where N is the Hilbert space dimension. Michael Nielsen suggested you would be the person to ask about where to find this result (or whatever the relevant result is), as there was a time when you cared about things less general than POVMs. Can you help?*

To my knowledge, the first guys to give that answer were Band and Park. Here are some references:

- W. Band and J. L. Park, "The empirical determination of quantum states," *Foundations of Physics*, vol. 1(2), pp. 133–144, 1970.
- J. L. Park and W. Band, "A general method of empirical state determination in quantum physics: Part I," *Foundations of Physics*, vol. 1(3), pp. 211–226, 1971.
- W. Band and J. L. Park, "A general method of empirical state determination in quantum physics: Part II," *Foundations of Physics*, vol. 1(4), pp. 339–357, 1971.
- W. Band and J. L. Park, "Quantum state determination: Quorum for a particle in one dimension," *American Journal of Physics*, vol. 47(2), pp. 188–191, 1979.

A paper showing that something goes wrong with tomography in real Hilbert-space quantum mechanics is:

- W. K. Wootters, "Local accessibility of quantum states," in *Complexity, Entropy and the Physics of Information* (W. H. Zurek, ed.), (Redwood City, CA), pp. 39–46, Addison-Wesley, 1990.

To my knowledge, the first people to talk about doing tomography with a single (minimal informationally complete POVM) were Caves, Schack, and I:

- C. M. Caves, C. A. Fuchs and R. Schack, "Unknown Quantum States: The Quantum de Finetti Representation," *Journal of Mathematical Physics*, vol. 43(9), pp. 4537–4559 (2002). [Reprinted in *Virtual Journal of Quantum Information* **2**(9).] `quant-ph/0104088`.

Wisemanism 29: *Now, moving swiftly into the metaphysical plane, given that you profess to believe in a real world (apart from your state of belief about it, which is a quantum state) ...*

No, my states of belief are in general captured by probability distributions. In some special cases, those probability distributions have the character of quantum states. As I'm presently toying with the idea, the world is a much more varied place than I would ever use quantum mechanics to describe in toto. It is just I think that quantum mechanics hints at something particularly deep about how we (as active agents) are wired to the world.

Wisemanism 30: *Another question: if your state of belief is your odds for future occurrences (on which you can either win or lose money), what is your state of belief about events after you are dead? You can't win or lose any money on them, so how can rational behaviour help? To put it another way, how do you justify having life assurance? I haven't read your tracts lately, so my point of view is [wandering?] all over the place again.*

I don't understand the question. But maybe that's just because I'm giving this reply a rush job.

Wisemanism 31: *A simpler question: have you read Carlo Rovelli's work "Relational Quantum Mechanics". He says quantum states are about information, but I'm not sure if you'd agree with his approach.*

Aha! You haven't actually read either of my papers `quant-ph/0205039` or `quant-ph/0106166`. I caught you. In both those papers, I wrote:

> "But what nonsense is this," you must be asking. "Where else could they start?" The main issue is this, and no one has said it more clearly than Carlo Rovelli. Where present-day quantum-foundation studies have stagnated in the stream of history is not so unlike where the physics of length contraction and time dilation stood before Einstein's 1905 paper on special relativity.

But, yes, though I really liked Rovelli's motivational sermon ... and even something of the flavor of his two principles, I think he floundered pretty badly when it came time to put some meat on the bones.

28-10-02 *Blather, Lather, and Rinse* (to J. Bub)

The following is a note I started to construct for you several weeks ago. Unfortunately, I never got a chance to finish it, but it seems appropriate to send you the note as it stands nevertheless ... if for nothing else as a starting point for this week's discussions. The note was to be titled "Blather, Lather, and Rinse." I had wanted to polish it better and add some more detail, but maybe this will get the ball rolling.

> Pilate was probably not the first to ask what truth is, and he was by no means the last. Those who ask it seek something deeper than disquotation, which was the valid residue of the correspondence theory of truth.
>
> — W. V. Quine

I apologize for taking so long to reply to your several letters, but I wanted to read Quine's little book *Pursuit of Truth* (revised edition) before doing so.

I want to defend my position that there is some good stuff in Marcus's manuscript. At the very least, there are things that I needed to read about and be exposed to. One man's blather is another man's rinse, I suppose. But I think it is more than that. There were three things that caught my attention.

a) The strategy of taking the Bohmian and Everettista's strong desire to find a "designatable reality term" within quantum theory to take as its anchor and turning that against them. The point he was trying to make could be more polished and intensified, but it was one that I found to be a keeper to be developed.

b) I found the discussion of the "primitive theories" that underlie our developed theories quite well done, and I think it is an important point that has yet to sink in to most of our mentalities. I just loved some of his lines in that section. Here was one that particularly struck me:

> My second point was that the function of a formal physical theory is to extend the primitive theory. And, indeed, modern physical theories extend it to a degree which may seem almost miraculous (which, to a Palaeolithic hunter, and perhaps even to an Elizabethan savant, might really have seemed miraculous). But, however striking the advance, we are never in a position actually to dispense with the primitive theory. No matter how sophisticated the fighter aircraft, the pilot still controls it in the time-honoured Palaeolithic manner, using hands and feet. Similarly here: if you want to confirm the theory, or to apply the theory, then you must have recourse to your Palaeolithic sensory organs, and your Palaeolithic sensory cortex. No matter how marvellous the theory, if you want to relate it to the world it is supposed to concern, you have to go through these Palaeolithic channels. Which means the theory can have no commerce with the world it is about except through the mediacy—the good offices, as it were—of the primitive theory. There is no escaping it: it's the way we're wired.

I think this is the proper way to understand (or at the very least build upon) what Bohr was talking about when he kept referring to the necessity of a classical description for making sense of quantum phenomena. You can see this mimicked (but in much less developed form) in my Samizdat: p. 260, "Foods for Thought [sic]," p. 464, "Always One Theory Behind," and in footnote 18 of my `quant-ph/0205039`.

In particular I found Marcus's discussion of this point extremely relevant to an ongoing debate I've been having with Mermin, Caves and Schack for some time. From where comes the judgment that a beamsplitter is a beamsplitter (and not some other kind of device)? Those fellows want to think that it somehow—in a way yet to be explained!—comes out of quantum theory itself. But I don't buy it. The path I'm traveling indicates that that judgment will never come out of quantum theory. The judgment that a beamsplitter is a beamsplitter is, in proper Bayesian terms, the assumption of a "prior". And where an ultimate prior comes from, Bayesian theory is silent. One must seek an answer from somewhere else, if from anywhere at all. In fact, I would say this is the ultimate meaning of my slogan "Quantum Mechanics is a Law of Thought" (at those times when I hold to it).

c) Finally, I rather liked the discussion of "reality as a logical requirement" at the very end of his draft. I mimic some loosely related thoughts at the beginning of Section 4 of my `quant-ph/0205039`, and flesh it out a little more fully in Section 5 of my `quant-ph/0204146`. The reason to draw a distinction between the "state space" and the "sample space" is not because the "sample space" captures the idea that there is a representative that is "really there" and we are just ignorant of it, but because it captures the idea of a spur on which we revise our beliefs.

The things in Marcus's draft that seemed to capture your attention were those (small) things that fall within the frame of usual (nonpragmatic) philosophical debate. I'm thinking here of your remark on disquotation. But I think he and I both have bigger bones to pick. The deeper issue is to do away altogether with a correspondence notion of truth, and to put into its place something akin to a pragmatic account (where "truth" is made by the process of "measurement intervention"). The old pragmatists, as I see it, had a glimpse of something that quantum mechanics gives us a much stronger reason to take seriously. [I was going to write much more here, but now no time.]

30-10-02 *Time to Make a Difference* (to R. W. Spekkens)

I've been thinking about it on and off all night, and I wonder if I can convince you to do a rush job on the paper you proposed writing (when we were talking in Waterloo). Namely, putting down in paper form the talk you gave here last week. What I'm thinking is that I would really love to have it in the special issue of *Studies in History and Philosophy of Modern Physics* that Jeff Bub and I are co-editing. We've already got more than enough stuff for the issue, but I'd like to squeeze you in . . . if I can convince you to take on the project.

Your talk struck a chord with me as being the right way to wham people over the head (if done right). And I'm now pretty sure you can polish it to that point. Moreover I think with this special issue we've got the right opportunity to be convincing to a lot of (the right) people, all at once.

So I hope you'll think seriously about this. If I can get a relatively final draft from you in one month—but not a week over that—then I think I'll be able to squeeze you in. But I've got to be absolutely hard about the deadline. I know you'll very likely have to seriously rearrange your schedule to pull this off, but I think this could be a grand opportunity for you (and for me vicariously) to make a difference in the community.

Let me list below the papers we've now got in hand to show you that you'll be in some respectable company.

- Howard Barnum, "Quantum Information Processing, Operational Quantum Logic, Convexity, and the Foundations of Physics"
- Charles Bennett, "Notes on Landauer's Principle, Reversible Computation, and Maxwell's Demon"
- Armond Duwell, "Quantum Information Does Not Exist"
- Itamar Pitowsky, "Betting on the Outcomes of Measurements: A Bayesian Theory of Quantum Probability"
- Chris Timpson, "On the Supposed Conceptual Inadequacy of the Shannon Information"
- David Wallace, "Quantum Probability and Decision Theory, Revisited"
- Andrew Steane, "A Quantum Computer Only Needs One Universe"

Talk to you later today or tomorrow.

02-11-02 *Technical Question Minus, 2* (to H. M. Wiseman)

Wisemanism 32: *Let me try again. It makes no sense to have a bet on an event that will happen after you are dead, as you can never benefit from it. So if your state of belief is just your betting* *odds for future events, logically you can have no state of belief about events after you are dead. So why have life assurance for the benefit of your family, for instance?*

I think it was Henry VIII that first said, I am England.

Howard's Reply

I've never heard that about Henry VIII. Does "I am England" mean something different from "England is me"? (cf. Louis XIV "L'état? C'est moi.")

I understand (I think) what you are saying, but I don't buy it.

03-11-02 *Visionlessness* (to R. W. Spekkens)

Spekkensism 13: *Admittedly, I am feeling these days that the hidden variable program envisioned by Einstein was never really pursued seriously and thus may have been abandoned too quickly.*

Your note struck a fear in me that I'm having a hard time shaking. In coming to Bell Labs, it looks like you might be wasting a year of your time. But worse than that, I fear you're going to waste a year of my short life.

I have little to no tolerance for this. The way I see it, the quantum conundrum—after 75 years of waste—is only going to be broken by a leap of faith and imagination. It needs something creative. And, most importantly, it needs a vision of simplicity. The thought you express above is neither of those.

Rob's Reply

Perhaps you are reading more into my comments than was intended. Your reaction reminded of the following Emo Philips joke:

> I was walking across a bridge one day, and I saw a man standing on the edge, about to jump off. So I ran over and said "Stop! Don't do it!" "Why shouldn't I?" he said. I said, "Well, there's so much to live for!" He said, "Like what?" I said, "Well … are you religious or atheist?" He said, "Religious." I said, "Me too! Are you christian or buddhist?" He said, "Christian." I said, "Me too! Are you catholic or protestant?" He said, "Protestant." I said, "Me too! Are you episcopalian or baptist?" He said, "Baptist!" I said,"Wow! Me too! Are you baptist church of god or baptist church of the lord?" He said, "Baptist church of god!" I said, "Me too! Are you original baptist church of god, or are you reformed baptist church of god?" He said,"Reformed Baptist church of god!" I said, "Me too! Are you reformed baptist church of god, reformation of 1879, or reformed baptist church of god, reformation of 1915?" He said, "Reformed baptist church of god, reformation of 1915!" I said, "Die, heretic scum", and pushed him off.
>
> — Emo Philips

In the spectrum of views on quantum foundations, ours are not so dissimilar.

04-11-02 *Yoo-Hoo* (to J. W. Nicholson)

I've been spreading the word with van Enk and my management that I'm still in Canada ... and will be through Wednesday ... but the truth of the matter is I'm exhausted (at home) and need to get a load of housework done or I'll go insane.

Also I had an epiphany on the plane yesterday—something I thought to be exceedingly deep—and I struggled much of last night to get it written up.

Shall we have lunch Friday?

04-11-02 *Got It!* (to R. Schack, C. M. Caves & N. D. Mermin)

> **WARNING! WARNING! WARNING! WARNING! WARNING!**
>
> Subsections 2 and 3 of the present note are UTTER RUBBISH, and I entirely withdraw the ideas put forth there. (I still agree with subsections 1 and 4, however.) See the notes "Utter Rubbish and Internal Consistency, Parts I and II" of 27 and 28 June 2003 for an explanation. I nevertheless leave the ill-fated sections in this collection because, despite their embarrassment for me, I deem them crucial for understanding the surer path we all explored later.
>
> **WARNING! WARNING! WARNING! WARNING! WARNING!**

It finally sank in completely and whammed me over the head! It's beautiful; it's compelling; it was implicit the whole time. It's trivial even, and I am so excited! There is absolutely no mystery in the Penrose example, and there is absolutely no mystery in the Einstein-Podolsky-Rosen example once one has accepted that POVM ascriptions have the same non-ontic character as the state vector itself. Since my first note to you on my "Identity Crisis" in August of last year—("Quantum States: WHAT?", page 35)—the solution has been staring us in the face. In fact, the examples above now appear as hardly worthy of ever having been called conundrums or paradoxes in the first place.

I guess this is what Rüdiger was trying to get across to me (and to David?) on Friday before he left. Let me put the story in my own words.

I'll break the note below into four sections for clarity:

1. A general exegesis on how to think about POVMs
2. The Penrose example
3. The EPR example
4. Forward-looking thoughts on POVMs and a pluralistic universe

1) The POVM as a function from raw data to meaning

We generally write a POVM as an indexed set of operators, E_d. Here is how I would denote the referents of those symbols. The index d should be taken to stand for the raw data that can enter our attention when a quantum measurement is performed. The whole object E_d should be construed as the "meaning" we propose to ascribe to that piece of data when/if it comes to our attention. It is important here to recognize the logical distinction between these two roles. The symbol d stands for something beyond our control, something that enters into us from the world outside our head. The ascription of a particular value d is not up to us, by definition. The *function* E_d, however, is of a completely different flavor. It is set by our history, by our education, by whatever incidental factors that have led us to believe whatever it is that we believe when we walk into the laboratory to elicit some data. That is to say, E_d has much the character of a subjective probability assignment. It is a judgment.

I have tried to say this in various ways before. Maybe the first place in "Quantum States: WHAT?" is in the note "Note on Terminology," pages 49–50, or in more detail in "Replies to a Conglomeration," page 92. Maybe there are still better shots at it, but I didn't look further. (I guess I also give another variation on the matter on page 42 of `quant-ph/0205039`). You can have a look at those if you think it'll help, but I think the paragraph above says it as well as anything. And I know that at least Rüdiger is on board with all this. Carl probably is too, but I'm not as sure.

2) The Penrose example

Here's one of the passages I recorded [from Penrose's book] in the Samizdat, pages 402–404,

> One of the most powerful reasons for rejecting such a subjective viewpoint concerning the reality of $|\psi\rangle$ comes from the fact that whatever $|\psi\rangle$ might be, there is always—in principle, at least—a *primitive measurement* whose **YES** space consists of the Hilbert-space ray determined by $|\psi\rangle$. The point is that the physical state $|\psi\rangle$ (determined by the ray of complex multiples of $|\psi\rangle$) is *uniquely* determined by the fact that the outcome **YES**, for this state, is *certain.* No other physical state has this property. For any other state, there would merely be some probability, short of certainty, that the outcome will be **YES**, and an outcome of **NO** might occur. Thus, although there is no measurement which will tell us what $|\psi\rangle$ actually *is*, the physical state $|\psi\rangle$ is uniquely determined by what it asserts must be the result of a measurement that *might* be performed on it.

Let's think about this from our perspective. From our point of view, both the state $|\psi\rangle$ and the measurement Penrose speaks of are subjective judgments. They both count as priors. In principle, they are distinct subjective judgments, but in this case they happen to coincide in a meaningful sense. Here's the meaningful sense. When accepting quantum mechanics as a theory for reasoning, we are, among other things, accepting the consequences of Gleason's theorem. And with that comes the coordinated states and measurements Penrose speaks of. WHEN the state, THEN the given measurement outcome with probability one.

But what does that mean? It means little more than that, no matter what the objective character of the raw data we find, we *ascribe* to it a meaning appropriately associated with the given state. That is, the **YES** boils down to essentially a convention. The meaning we ascribe to the raw data has no choice but to be labeled **YES**.

And that, I think, is the whole story. I'll add some more metaphysics to this in a minute, but first let me move on to the EPR situation before going further.

3) The EPR example

To illustrate this one, consider a bipartite set of qubits in a maximally entangled state and suppose one measures the same von Neumann measurement on each. In this case, we can't predict the outcome of either measurement, but we can predict that the two will match identically. Is there anything mysterious about this? Nicolas Gisin tells me there is. For he says, "I'll grant you that at least one of the results is a sort of free creation or birth—i.e., it does not arise because of a local hidden-variable theory—but then how does the other creation in the pair know how to go the same way?" "That simply can't happen unless there is some kind of superluminal communication going on or, even more radically, spacetime itself is meaningless."

Well, what do we have here but little more than an extension of the situation above? Now, there are simply three priors rather than two. And those priors command us to interpret the two pieces of raw data (coming from the two separated parts of the experiment) as having the same meaning. That is to say, in analogy to our solution to the Penrose conundrum, it is simply a convention that the clicks are the same in the two wings of the experiment.

And that too is the end of the story. It is just a triviality that the measurements come out perfectly correlated. They came out that way because WE labeled them that way with the choice of our priors.

4) POVMs and radical pluralism

Now let me go into a bit of the metaphysics of this. Here's a point of view that I'm finding myself more and more attracted to lately.

I think it is safe to say that the following idea is pretty commonplace in quantum mechanical practice. Suppose I measure a single POVM twice—maybe on the same system or two different systems, I don't care—and just happen to get the same outcome in both cases. Namely, a single operator E_d. The common idea, and one I've held onto for years, is that there is an objective sense in which those two events are identical copies of each other. They are like identical atoms ... or something like the spacetime equivalent of atoms. But now I think we have no warrant to think that. Rather, I would say the two outcomes are identical only because we have (subjectively) chosen to ignore almost all of their structure.

That is to say, I now count myself not so far from the opinion of Ulfbeck and Bohr, when they write:

> The click ... is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety .... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

For though I have made a logical distinction between the role of the d's and the E_d's above, one should not forget the very theory-ladenness of the set of possible d's. What I think is going on here is that it takes (a lot of) theory to get us to even recognize the raw data, much less ascribe it some meaning. In Marcus Appleby's terms, all that stuff resides in the "primitive theory" (or perhaps some extension of it), which is a level well below quantum mechanics. What quantum mechanics is about is a little froth on the top of a much deeper sea. Once that deeper sea is set, then it makes sense to make a distinction between the inside and the outside of the agent—i.e., the subjective and the objective—as we did above. For even in this froth on the top of a deeper sea, we still find things we cannot control once our basic beliefs—i.e., our theory—are set.

Without the potential d's we could not even speak of the possibility of experiment. Yet like the cardinality of the set of colors in the rainbow—Newton said seven, Aristotle said three or four—a subjective judgment had to be made (within the wide community) before we could get to that level. If this is so, then it should not strike us as so strange that the raw data d in our quantum mechanical experience will ultimately be ascribed with a meaning E_d that is subjectively given. (I expressed some of this a little better in a note I wrote to David last month; I'll place it below as a supplement.) More particularly, with respect to the EPR example above, it should not strike us as odd that the phenomenon comes about solely because of an interpretive convention we set: All quantum measurement outcomes are unique and incomparable at the ontic level. At least that's the idea I'm toying with.

I think that's enough for tonight. I now intend to sleep for at least 14 hours! (It was a long three weeks.)

David's Reply

Thanks cc'ing me the latest epiphany. I've only had a chance to glance at it and am still torn between whether my response is "that was how I understood your position to be all week" or "what utter rubbish" so I will take a little time before responding. (I have to go to Germany for a week in a week so it may be a couple of weeks.)

Rüdiger's Reply

I am happy with your section 1) on the POVM as a function from raw data to meaning. But I don't get the point you are making in section 2). I would say that the sentence "a σ_x measurement of a particle in the state $|+\rangle$ gives the outcome YES with certainty" is saying exactly the following: If our states of belief about the POVM and the state are expressed by σ_x and $|+\rangle$, then consistency requires us to assign probability 1 to the outcome YES. It's a statement about consistent application of the formalism. To say about a particular experiment that it performs a σ_x measurement of a particle in the state $|+\rangle$ is a subjective judgment. So far we are in agreement.

But then you write

> But what does that mean? It means little more than that, no matter what the objective character of the raw data we find, we *ascribe* to it a meaning appropriately associated with the given state. That is, the **YES** boils down to essentially a convention. The meaning we ascribe to the raw data has no choice but to be labeled **YES**.

I probably just misunderstand this paragraph. It seems to me that once we have ascribed the state (the pair σ_x, $|+\rangle$), we are making a very strong statement. We have identified two possible outcomes, YES and NO, and we make a commitment to accepting bets on YES with arbitrarily unfavorable odds. It is logically possible that the outcome will be NO. If the outcome is NO, there will be a crisis. Ruin. Your paragraph suggests that in this case we just relabel the raw data. If that's what you mean, then I agree with David's "what utter rubbish". But I probably misunderstand you here. (In a year's time, you'll write "I never said it better than in my note headed 'Got It!' from 3 November 2002" :-)

I agree that for the purpose of this discussion, the EPR scenario does not add much. But you get me worried again in section 3):

> That is to say, in analogy to our solution to the Penrose conundrum, it is simply a convention that the clicks are the same in the two wings of the experiment.

Either I don't get it, or it's utter rubbish.

Section 4), I like. It highlights the chasm that exists between our approach and the many-worlders and decoherence people. We all agree that a click is not an elementary phenomenon. They want to reduce it to something more fundamental. We say it's irreducible (which does not mean that we cannot analyze a measurement apparatus or decoherence of a quantum register in as much depth and detail as anybody else). Your metaphysical bit is nice in that it makes clear that "irreducible" is not the same thing as "elementary". As you say it,

> All quantum measurement outcomes are unique and incomparable at the ontic level.

I hope you got your 14 hours of sleep. My best night so far has been 5 hours long.

04-11-02 *Carts and Horses, 2* (to A. Kent)

Kentism 5: *There's a long tradition of physicists becoming over-enthused with current technology as a source of metaphors and even fundamental explanations for physics. ... I wonder if there's maybe a danger of your program falling into the same class.*

Ulrich Mohrhoff once wrote something similar in sentiment. It's a good thing you're one of my friends, and I would never treat you like I treated him. [See 04-07-01 note "Carts and Horses" to U. Mohrhoff.]

04-11-02 *One Day* (to A. Kent)

Kentism 6: *I take the point that you were interested in information-theoretic aspects of quantum theory before quantum information became popular, and this earns you several cosmic brownie points for originality (which are not dispensed lightly). Still, for me, the fundamental worry stands: so much of physics doesn't look reducible to information theory that I wonder if any of it is.*

You're right about GR not looking like anything to do with information. Nor would any physics whose main task is to set a Lagrangian, like QCD say. However, quantum theory (as a principle theory in Bub's sense) stands out like a sore thumb to me. I see so many problems melt away with an epistemic view of the quantum state, with no new problems coming back to haunt in its stead. The task, as I see it, is to see how many further terms and rules in the theory (quantum operations, the Born rule, the tensor-product rule, the Hamiltonian, etc.) can also be taken to be epistemic in a fruitful way. The ones that can't, finally with a little breathing space around them, will be able to scream out their ontic significance loudly and clearly. We'll hear it, and, I'll bank money, know what to do with it for that next step in physics beyond flat quantum mechanics.

14-11-02 *Probabilismo!* (to D. M. Appleby)

I'm sorry, I forgot to send you those references, didn't I? Let me dig them up.

- B. de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989).
- R. Jeffrey, "Reading *Probabilismo*," Erkenntnis **31**, 225–237 (1989).

Also, I think that whole issue of Erkenntnis is worth perusing. I think there is an article on de Finetti's approach to the "problem" of induction in there that Dick Jeffrey recommends ... unfortunately, I can't remember the author at the moment. It might have been von Plato or Zabell.

Applebyism 1: *I hope my niggling doubts weren't too exasperating.*

Not at all; I very much enjoy your company. And I appreciate your worries, even if I am only now learning how to respond to them adequately … and, indeed, only now recognizing some of the weaknesses in what I spout. (Though the latter is an ongoing thing with me.)

I hope one day you'll post a version of your "Elements of a Prolegomenon" to `quant-ph`. Rüdiger and I, at least, found parts of it very, very helpful. And, self-servingly, I can't imagine a more flattering appraisal of the foundations program I'm trying to define than the one in your introductory section. Inspiring words can help recruit a workforce, and you know I'm all for that!

Let me end this note by pasting in part of another note that I wrote soon after the Montréal meeting. (I'll paste in the part I still trust; parts of it, I think need to be redone now.) [See 04-11-02 note "Got It!" to R. Schack, C. M. Caves & N. D. Mermin.] Anyway, it leans heavily on explanatory modes that I developed while talking to you.

17-11-02 *Växjö* (to J. Finkelstein)

Good to hear from you. You haven't seen any details on the meeting because none have been worked out yet. Thus, I was a little surprised to see Andrei's announcement of the meeting in Y. S. Kim's conference listing; he probably just wanted to get something announced fast (for administrative purposes). The last time we had talked, we were thinking hard (and I thought it had already been agreed to) about giving the meeting the slant: Quantum Information meets Quantum Logic. The idea being to get predominantly quantum information people who had some interest in learning about the quantum logic community's results and quantum logic people who were interested in learning about the results of quantum information. And so on. That, by the way, is Howard Barnum's predominant research theme at the moment.

We're definitely going to have to get geared up over the holidays to get the planning settled.

In any case, I think it'd be great if you'd come. It'd be nice to meet you finally. The last meeting was pretty successful, and I think once we put in some work, this one will probably come out the same. I think there's a good chance we'll have Hardy, John Smolin, and Jeff Bub there. And from the quantum logic and convex structures side, we're going to try to attract Dave Foulis, Alex Wilce, Dick Greechie, Paul Busch, and Reinhard Werner. Clearly some in that list have already taken part in a cross-culturation. Also, I guess there'll be the people that Andrei has already listed in his announcement. Some of those, too, like Holevo, have done the same thing.

Would you need any funding, or would you be able to make it on your own travel grants. We'll have some small amount of money, but I want to use it as advantageously as possible so that we'll be able to get the maximum number of interesting people there.

17-11-02 *Fertilization?* (to J. Finkelstein)

Well I just discovered that "culturation" doesn't seem to be a word. Maybe you understood what I was trying to get at anyway.

19-11-02 *Ridiculous Interpretation* (to S. L. Braunstein)

Braunsteinism 1: *Hi! Do you recall an odd interpretation about how the quantum information gets across in quantum teleportation? It suggested the quantum info goes backwards in time from Alice's Bell measurement to the point of creation of the entanglement and then forward in time to Bob. Do you recall where this odd idea came from? Or would you have any idea whether it actually appears in print somewhere?*

Yes, you can find that in the original quantum teleportation paper itself. It was an idea of Ben Schumacher's.

Personally, I hate the idea and think it is misleading. For it gives the image that a quantum state is an objective property of something, and that that objective property just travels along a wire (and through time) in the process of quantum teleportation. Whereas I would say, the only thing that travels backwards then forwards in time is our *chain of inference*. Then, once it is realized to be inference, you don't need to talk about traveling at all: Quantum teleportation is just a case of taking some prior information, gathering some data, and updating to a posterior. See my description of teleportation in Section 3 of `quant-ph/0205039`. (And note footnotes 14 and 15.)

03-12-02 *Poetry, Subjectivity, Mirroring and Algorithms* (to D. M. Appleby)

This is just to let you know that I got your email a while ago, and even read it once, but I've had almost no time to get philosophical for a while. (Rather serious business concerns.) What I'm trying to do at the moment is just clean out my email box before I go crazy.

So, please accept my apologies, but I'm not going to be able to reply in detail for still a while further.

Let me just comment on one key piece, for which much seems to hinge for you:

Applebyism 2: *It is worth noting, incidentally, that you do ascribe objective status to the dimension d, and to the bound on the volume of that shape in the probability simplex. This means you are still playing the same game as Caves: trying to find something in our heads which genuinely is the reflection of something real out there. It is just that you are much harder to please than Caves.*

I don't think you have me right on that count. Please reread my paper `quant-ph/0204146`—especially the part written to Wootters—and tell me whether you still think what you said above syncs with me.

06-12-02 *SHPMP Issue on Quantum Information* (to the SHPMP participants)

We are now in the very final stages of collecting manuscripts from contributors to the special quantum information issue of *Studies in the History and Philosophy of Modern Physics*. The issue will contain the following papers:

1. Howard Barnum: 'Quantum information processing, operational quantum logic, convexity, and the foundations of physics'

2. Armond Duwell: 'Quantum information does not exist'

3. Lucien Hardy: 'Probability theories in general and quantum theory in particular'

4. David Mermin: 'Teaching computer scientists quantum mechanics, or how I discovered the Copenhagen interpretation'

5. Itamar Pitowsky: 'Betting on the outcomes of measurements: a Bayesian theory of quantum probability'

6. Andrew Steane: 'A quantum computer only needs one universe'
7. Chris Timpson: 'On the supposed conceptual inadequacy of the Shannon information'
8. David Wallace: 'Quantum probability and decision theory revisited'

The special issue is scheduled for publication as the September, 2003 issue of SHPMP. To avoid being bounced to a later issue, we absolutely *must* have the final version of your manuscript before the end of December. So please send (or re-send) us a LaTeX file of your paper, prepared in accordance with the journal's guidelines below, by December 20.

We are excited about this project and we look forward to hearing from you shortly.

Jeffrey Bub and Chris Fuchs

06-12-02 *Call Me, Let Me Call You* (to C. H. Bennett)

When we were in Montréal together, I remember one sleepy 4:00 AM conversation when you said that Jeff Bub and I could use your paper `physics/0210005`, "Notes on Landauer's Principle, Reversible Computation and Maxwell's Demon," for our special issue of *Studies in History and Philosophy of Modern Physics*.

Is the paper still up for grabs? If it is, we would really, really, really love to have it. It would be perfect for the issue. I'll put the issue announcement below, so that you can see who you're potential neighbors would be. They're all really good papers.

All you'd have to do is say *yes* ... and I'd even format the paper properly for you. I'll just take it directly off the archive. You couldn't find a better bargain in Filene's Basement.

Just let us know as soon as possible (i.e., as soon as you read this note). Also, you could either call me, or I could call you tomorrow/today. Where will you be?

06-12-02 *From the Real Chris* (to C. H. Bennett)

Bennettism 19: *The subject line of your email almost motivated me to throw it into the junk bin. I expected that someone had hijacked your From: address and was about to continue with, "Nice lady wants to meet you." I would be glad to have you publish* `physics/02010005` *but I am currently working on version 2, which will be much better, and will address more critics of Landauer's principle. I prepared version 1 in a hurry so as to get an archive number in time for Rex and Leff to cite in their new edition of the Maxwell's Demon book.*

The paper is already great as it stands (by our standards). But, if you could finish the new version before December 20—at the absolute, absolute, absolute latest (see explanation below)—we could still accept that.

Don't forget, by the way, that Earman and Norton, Shenker, and Bub have all published their papers on the same subject in the same journal. So, this really is the appropriate place (and the appropriate time) for your paper.

You want me to give you a call? Where are you?

You encourage me!

06-12-02 *Enjoyed* (to D. M. Greenberger)

I enjoyed listening to you talk about an evolutionary approach to quantum mechanics the other day. When we get together next time please tell me more! You've got my ear on this subject.

Below I'll place some notes I've written on things to do with Darwinism and quantum mechanics ... and their connections. The notes come from my samizdat, *Quantum States: What the Hell Are They?* [See 18-02-02 note "Psychology 101" to J. Preskill, 25-02-02 note "A Wonderful Life' to W. K. Wootters, and 24-06-02 note "The World is Under Construction" to H. M. Wiseman.]

Danny's Reply, "Tiny Answer to Large Questions"

> Your emails, like your papers, are quite prolix, lots to read (not meant as a criticism, only as an excuse for having not had time to digest it yet), and so I can only tell you where I am at on a few issues. I will have to reread most of the comments in the email and ruminate upon them.
>
> But I strongly believe in Darwinism as the mechanism that drives science. In fact I define science by saying that man is always reacting to the environment presented to him, in order to better adapt to it, and science is his conscious effort to maximize this adaptation. So I strongly believe that we frame the theories that we do because we perceive the world the way we do. Our senses and conceptualizations are partly built in and are partly a response to what is out there, and we build theories and instruments to help us better interact with it. But if our senses and brain structure were different, we would have evolved a different response.
>
> For example, if like a snake we could sense infrared, our whole world view would be different. The snake sticks out his forked tongue and says, "ah, a mouse passed this way a while ago, and it is hotter to the left than to the right, so I can track him to his lair." The mouse and his infra-red image aren't very separable.
>
> Now we see objects in visible light, of short wavelength, and so we see sharp images. So we develop rules that give things sharp boundaries, and we say two things can't occupy the same place at the same time. This in turn leads us to think that the integers are important, and that enumerating things is important. Our whole counting system and mathematics is based on it. If we perceived things like a snake, nothing would be sharp, and I suspect the integers would be way down on our list of what's important in mathematics. We would think a continuum approach would be much more intuitive.
>
> So you see, I don't even think mathematics is god-given. I think it is part of our response to our environment, based on our senses. And when people tell me, as almost all mathematicians do, that mathematics is objective, and corresponds to a Platonic something "out there", my answer is that "The reason you believe that two plus two equals four, is because those of your ancestors who also believed that, ate the ones who believed that two plus two equals five." There was survival value in it for us as we are. But that might not have been the case if we were different. It's not necessary, it's Darwinism.
>
> And so science evolves in a manner reminiscent of the man who looks for things under the lamppost, on a dark night, because the light there is better. We do the experiments we can do, with the equipment we have, because otherwise we couldn't do anything. But we end up with a very skewed view of the world. One of the things Darwinism gave us was an insatiable curiosity, and so it is good to dream of final theories, but the

dream is rationally a silly one. Every time we open a new window on nature we see all sorts of things we never dreamed of. We are under a lamppost somewhere in the middle of nowhere, and most of creation is totally unknown to us. Topics like consciousness and ESP, etc, we ban from science, because we can't begin to get a handle on them. But that doesn't make them unimportant. They are actually the essence of things for us. Our lamppost just doesn't throw any light in those directions.

About a real theory of everything, I always tell people that I wouldn't want to live in a universe where I could understand everything. Or even the important things. And I don't think there's any danger of that.

So you see, I think the search for knowledge is a wonderful dream, but it's a romantic, even Quixotic one. I think we'll learn a lot about controlling the small part of nature that we see. But truly understanding anything deep, when we aren't even aware of most of what's out there? Not very likely.

I'm afraid I can't even take science seriously at some level, although I thrill to its beauty, and am willing to dedicate my life to the search. But it's hubris to expect too much, because any reality we can perceive now, can only pertain to where we are now on the evolutionary ladder. I think that when we die, and go to heaven, and ask God how things really work, he will look at us and smile, and say, "first you need a brain transplant."

As for my views on quantum theory, I'll tell you sometime when we're drunk. Anyway, thanks for coming, I enjoyed it very much. And I very much would like to come down and see you soon.

06-12-02 *Quantum Information Does Not Exist* (to A. Duwell)

Jeff Bub passed on a copy of Appendix B to your PhD thesis to me, and I've got to tell you, I just love it. And I love the title: I don't know if you know it, but it is a wonderful play on the words Bruno de Finetti wrote in boldface in the preface to his book on probability theory:

> My thesis, paradoxically, and a little provocatively, but nonetheless genuinely, is simply this:
>
> PROBABILITY DOES NOT EXIST.
>
> The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time, . . . , or Fairies and Witches, was an essential step along the road to scientific thinking. Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading conception, an illusory attempt to exteriorize or materialize our true probabilistic beliefs.

I think you make a sound point in your paper and an important point, and I am behind you 100%. Indeed I have tried to make the same point many times over to my friends (like Jozsa), but I've never done it so clearly or thoroughly as you have done it in this document. (As points of reference, you can have a look at the note titled "Colleague", on page 132 of my samizdat *Quantum States: What the Hell Are They?* posted on my webpage (link below), or look at the "Swedish Bikini Team" and "More Swedish Bikini Team" stories on page 34 of my `quant-ph/0105039`.) The point is, Landauer has caused a lot of trouble with his slogan "information is physical." The real issue is that "information carriers are physical," and by studying the information carrying properties and capabilities of those carriers we may get a new, more insightful way of expressing their very essence.

Anyway because of this respect for your work, Jeff Bub and I would like to invite you to include a copy of your paper in a special issue of *Studies in History and Philosophy of Modern Physics* devoted to issues to do with quantum information that we are co-editing. This is extremely short notice to give you because, essentially, we will have to have a final version of the paper by the end of next week. But still, we hope you will say yes. Let me exhibit a list of the confirmed papers (imaginatively including yours), so that you can get a feel for the gist of the issue.

1. Howard Barnum: 'Quantum information processing, operational quantum logic, convexity, and the foundations of physics'
2. Armond Duwell: 'Quantum information does not exist'
3. Lucien Hardy: 'Probability theories in general and quantum theory in particular'
4. David Mermin: 'Teaching computer scientists quantum mechanics, or how I discovered the Copenhagen interpretation'
5. Itamar Pitowsky: 'Betting on the outcomes of measurements: a Bayesian theory of quantum probability'
6. Andrew Steane: 'A quantum computer only needs one universe'
7. Chris Timpson: 'On the supposed conceptual inadequacy of the Shannon information'
8. David Wallace: 'Quantum probability and decision theory revisited'

As I said above, we realize we are asking this on very short notice. But I have read your paper twice now, and I would say that we can accept it almost exactly as it presently is … and hope that that will be extra incentive for you to say yes.

Below, I'll just list a few (very minor) points that caught my eye, and suggest that you fix those things up if you do submit it for the special issue. The scheme will be the following: In a separate email, I'll send you the original manuscript that Jeff had sent me (in the form of a pdf file) for the purpose of knowing which pages I am talking about. (I figure you might have changed things, or used a different format, since the time of the draft that I read.)

Hoping to hear from you soon (almost immediately actually),

Chris Fuchs

PS. You might also enjoy the notes "Short Thoughtful Reply," "Qubit and Teleportation Are Words," "Chris's World," and "King Broccoli," on pages 175, 184, 185, and 187, respectively, of my *Quantum States: What the Hell Are They?* These notes also connect somewhat—though not directly—to the points you make in your paper.

Notes on "Appendix B: Quantum Information Does Not Exist"

1) p. 3, just a comment, not a request for change. You write, "… it seems that Jozsa thinks that quantum information is somehow real." I can confirm that. We've argued this over far more times than I care to remember!

2) p. 5, footnote 2. You give Brukner and Zeilinger more credit than they deserve on this. That property goes back to Faddeev in the 1950s. I think you can find a reference in the Timpson paper listed above (it can be found on quant-ph). If you can't find it there, I'll dig harder.

7) p. 15, bottom sentence. I'm not sure I get this sentence. Even epistemic entities or, if you like, classical information, can have qualities. $H(X:Y)$, for instance, is bounded above by $H(X)$—that is a quality mutual information possesses. But you must mean something more specific to the context here; so maybe just strive to say it a little better.

9) p. 20, bottom paragraph. You call $H(X:Y)$ the "accessible information," but that is not the common usage. Accessible information, as the term was coined by Schumacher in his PhD thesis, is defined as $\max H(X:Y)$ where the maximization is taken over all POVMs that can be performed at the destination.

10) p. 20, bottom paragraph. Also, the Holevo bound actually tells you very little in detail about the accessible information, in distinction to what you seem to indicate. For instance, take a pure state ensemble whose density matrix is I/d (the identity operator divided by the dimension). If the ensemble happens to consist of orthogonal states, then the accessible information will equal the Holevo bound and be given by $\log d$. On the other hand if the ensemble is the "Scrooge ensemble" that Jozsa, Robb, and Wootters talk about [Phys Rev A, **49** (1994), p. 668], then the accessible information will never exceed 0.61 bits, regardless of d.

11) p. 21, first sentence, second paragraph. I didn't like your phrase, "one member of the singlet state." It seems that all too often I find people in the field of quantum information—for some reason that is mysterious to me—using the words "system" and "state" synonymously. You might think that slip doesn't cause much trouble, but I think it causes a lot of damage. (Especially if you are someone like me who tries to think of quantum mechanics as consisting of both ontic and epistemic terms, and seeing the interpretive task as classifying which is which. For me, the system is ontic in nature, whereas the state is epistemic.) Anyway, I might have said something like, "Alice and Bob each possess a system drawn from a pair of systems prepared in the singlet state."

12) p. 21, last sentence, second paragraph. Just a remark. What is remarkable is that local operations alone can transform the initial state to states that live with the full span of the Hilbert space. But I think you make this point somewhere later in the paper.

13) p. 22, second paragraph. I didn't like at all the part where you write, "[S]he can establish correlations nonlocally. This is not a perplexing property of information, but rather a well-known fact about quantum states." But I won't bother you about that.

14) p. 23, first sentence in Section 8. Again the system/state distinction problem. Actually read the note "Short Thoughtful Reply" that I mentioned above, and you'll see more clearly why I'm bothered by this.

24) p. 33. You write, "It is possible that given some ensemble which describes the information source, the number of qubits required for q-reliable communication can be less than the number of cbits required for reliable communication." It is not only possible, it is always true. In the language Caves and I used in the 1996

paper of ours that you cite, "The preparation information is always greater than or equal to the von Neumann entropy." I think we prove it there.

25) p. 34, the part below the displayed equation. On my first reading of this, I thought you had just got it wrong. I thought you were suggesting that the encoder in a Schumacher compression scheme could just throw away the qubits that were given to him and generate some new qubits prepared in an eigenstate of ρ (doing so with the appropriate probabilities) and send those on. But on second pass I realized you were not saying that. The thing that saved you was the phrase "formally equivalent." Still, I think it might be useful for your readers to make the distinction between this formal equivalence, and what is really done in the physical process of Schumacher compression.

28) I would appreciate it very much if you would cite my samizdats (maybe the specific parts I listed above) in connection to the main thesis of your paper ... rather than just citing Caves and Fuchs, 1996. A good place for that might be your sentence on page 3, "In fact, I will defend the position that quantum information theory is properly about properties of classical information in quantum systems and that no new concept of information is needed." The more readers I can draw into those samizdats, the more I can hope to have an impact on the community.

07-12-02 *The Holiday Season* (to T. A. Brun)

Brunism 8: *If you do go to the Republic of Ireland for this QI-fest, were you planning on following up on the sort of stuff you talked about in your "Quantum Mechanics as Quantum Information" paper? I don't mean to sound like a broken record (and how long before no one remembers what that simile refers to?), but I really did enjoy that paper; it's one of the most interesting I've read in a long time, and I'm keen to see where you go with it. For instance, can you relate your information-based approach to the physical process of gathering information in an intuitive way? You made a start at that, I think, with your Bayes-rule-plus-disturbance formulation, but it seemed like more might be possible along those lines. That sort of thing interests me very much.*

Thanks for the encouraging note; I'm flattered. Let me apologize for taking so long to reply. I haven't had a chance to write nearly so much email in the last few months as I'd like to (and need to).

Concerning your first question, in fact that's all I plan to do in Ireland! There's just so much to be done before this Bayesian picture can have any real substance.

The main lines I'd like to develop (or see developed) fall into two categories. The first has to do with the shape of the restricted region in Figure 1. Depending upon which measurement device is taken to be the "standard quantum measurement device," that region will have various different shapes. In particular, the volume of the region can be arbitrarily small. It cannot, however, be arbitrarily large. There is a supremal volume, and I'd like to know it. For a qubit, the supremal volume is just the shape of a sphere. In higher dimensions, unfortunately, it gets more complicated. But I think finding a bound on the biggest volume could already be interesting. In particular, I'd like to know how the ratio of sup region volume to full simplex volume scales with dimension. Does it go to zero? I suspect it does, but that's based on philosophy.

Beyond that, though, just the more we can say about the region, the better. For instance, for a supremal-volume region, how many edges of the simplex will the region touch? How many faces? Etc. What is for sure is that if we can get a characterization of these regions without having first started with POVMs, then we will have a new characterization of the state spaces of quantum mechanics.

The second line I'd like to see developed is the business I started in Section 7 of the paper. Namely, giving some substance to the slogan, "A quantum operation is just a density operator in disguise." Or more accurately, just as a quantum state can be viewed as nothing more than a probability distribution $P(x)$ restricted to a certain region of the simplex, a quantum operation should be viewable as nothing more than a conditional probability distribution $P(y|x)$ restricted to a certain region of the appropriate enveloping space. Then, all the questions above arise again, but within this new regime.

I've shored up the ideas (and the arguments for them) in that Section pretty significantly since first writing it ... and also since writing the stuff on the subject in my samizdat *Quantum States: What the Hell Are They?* posted on my webpage. I'd like to get all that into a paper; it's just finding the time to do it. If God will grace me, maybe I'll get it done in January after the holiday season passes. In any case, some statement of it will definitely come out in the near future in the introduction and conclusions to the paper Rüdiger, Petra Scudo and I are writing on a de Finetti theorem for quantum operations. Since Rüdiger is in charge of completing the draft that'll probably get done in short order.

Anyway, as I said above, I'm flattered that you find some of this stuff interesting. And I'll be even more flattered if you find some of the answers to these questions! If you get any good ideas, do let me know. And if you'd like to collaborate on anything, let me know that too: I'm hoping that after mid-January, I'll be able to regain my identity as a scientist again.

07-12-02 *Prolix Boy* (to D. M. Greenberger)

Thanks for the great note! I especially liked the snake example. I didn't realize before that we are so in synch with each other.

For fun, let me send you a PART of one my stories. This one comes from page 237 of my samizdat, `quant-ph/0105039`. It was originally written for Mermin. By "Firing Line," I really meant "Reference Frame"—that always annoys him.

Why don't you come to Bell Labs some time in January. You ought to give us a talk.

09-12-02 *No Jack Kennedy* (to C. M. Caves)

"Senator, I served with Jack Kennedy, I knew Jack Kennedy, Jack Kennedy was a friend of mine. Senator, you are no Jack Kennedy."

I did tell you that Dick Slusher is my supervisor now, didn't I? Well, he started reading my fat paper "Quantum Mechanics as Quantum Information," today, and he came into my office for a while this afternoon to make a few comments on it. In the course of that he also told me how he was concurrently reading the paper you wrote for a 1991/92 Spain meeting. Apparently he has some interest in quantum chaos now. I said, "You shouldn't start with that; I think some of his more mature works were much better." He insisted that he needed to start at the beginning, saying, "Carl writes so clearly and methodically." I said—referring back to the part of the conversation about my own paper—"You know that's what I strive for too; Carl's been my model since the beginning." Dick was just about to walk out, but before he went, he looked me straight in the eye and said, "Yeah, and you've got some way to go."

I couldn't help but think of that Lloyd Bentsen debate with Dan Quayle.

13-12-02 *More Addenda* (to A. Peres)

Concerning Cabello, I find him just wonderful. He gave a talk at our meeting in Montreal on various KS things, but there was a part of it on MKC in particular. I learned a lot in that section. The main thing being, that just because *a* KS coloring exists, it does not mean that an *arbitrary* coloring exists. Of course, that logical point should have been obvious, but it didn't strike me so much until Adán's presentation.

I had a funny conversation with David Meyer in Nashville this summer. He told me, "I had known about the Godsil and Zaks result for years, but didn't realize it was useful for anything until you asked me about the existence of KS theorems for rational vector spaces at an AMS meeting." It dawned on me then that I had indeed first asked that question, to Itamar Pitowsky, David Meyer, and probably to a few others, but I didn't recall ever having been thanked in the literature. (All this raced through my head in a flash as I was talking to him.) I said somewhat smilingly, "Well, did you thank me for the question in your paper?" He must have caught on, because his reply was, "I don't know, did I?" Since leaving the lobby and going back to my room that night, the smile turned into a little bit of a frown. See pages 448 and 449 in my samizdat.

19-12-02 *Which One Really?* (to G. L. Comer)

Great new little poem. Here, the lines that took me were: "The freedom to choose! The source of chance." Often someone will say to me, as Howard Barnum did,

Barnumism 3: *Here's a caricature, so feel free to object: Bell's worry about the foundations of QM has been: that we have "measurement" as an "unanalyzed primitive" of the theory. Everett shows us how to get around that. You don't like Everett's resolution because you* want *to have an unanalyzed primitive around so it can be the locus of free will.*

And I say it is not that. The universe has within its categories two species, one is chance, and one is free will. Free will does not rely on chance as its source. Instead, it's only through the intercourse of the two that we get a real birth.

Gotta run to a big division meeting. Such things are always scary to me.

20-12-02 *Oh Haight Ashbury* (to N. D. Mermin)

Merminition 96: *Footnote 13 repeats without acknowledgment a point that you, Carl, and Rüdiger love to make. More disturbingly, it promulgates a joke that arose (for me) in the course of an email exchange with you that I can no longer find. For the life of me I can't remember whether I made the joke or you did. Googling on +"go ask Alice" +"entangled state" produces nothing. Feel free to delete footnote 13 if you think I'm stealing your line.*

Funny you can't remember the origin of the "Go Ask Alice" line. We had a conversation about it the moment I arrived at your MerminFest. Since it didn't sink in then, I'll tell you the story again. At 6:56 PM that morning, I sent off a note to my old friend Greg Comer titled "Go Ask Alice"—I'll place the first part of it below. After the part I quote here, however, the letter becomes quite personal.

So, you can imagine the shock when at 12:15 PM I receive a note to you titled "Go Ask Alice"!!! I have always had a fear of getting a note written and then, by accident, sending it to the wrong person! I thought, "Oh my God, I finally did it." However, reading your note, I quickly realized that there was no connection at all . . . (other than a Jungian synchronicity).

Merminition 97: *I don't see what your teleportation example (pages 11, 12) adds to ordinary EPR. Aren't all the issues exactly the same if Alice "in her laboratory prepares" the single qubit in (1) that she possesses by an appropriate measurement (to be sure, she can't control which outcome she'll get, but that doesn't seem to be central to your point, or is it?) after which she and only she knows what the outcome of the corresponding yes-no measurement on Bob's qubit will be.*

But this, of course, has been debated in the EPR context for generations, and I don't see the force of your argument that the would-be informationist should not be weak in the knees. Everybody agrees that Alice is the only one who can do the trick. There's no problem if she only does it once. Bob says the YES was random and only Alice knows that it had to be YES. Bob can think she has delusions of grandeur. But if she does it right 10,000 times, then on run 10,001 Bob would be a fool if a certain confidence that Alice can call it every time should start to enter his mind too.

So to find out what the qubit will do you do indeed have to go ask Alice. (Isn't that an old Jefferson Airplane (pre-starship) song?) You ask her; you don't ask the qubit (as you guys like to say). But by run 10,001 everybody who has been paying attention (except confirmed Humeans) will be fairly sure that "the system [is] prepared to reveal" whatever answer Alice has sent over in a lock-box.

I'm not saying I agree with Penrose on this. Just that I don't see that you've helped very much in relieving the queasiness one is left with when one denies the psi-ness.

On 5/29/02 I finally replied at length in a note titled "I Think She'll Know." The opening lines of the note were, "Remember what the dormouse said; feed your head."

But back to your question: I think beyond a doubt, for the present context, you were the inventor of the phrase "Go Ask Alice." It's a nice way to put it. (But only people of our generation are gonna get it.)

But the importance of the point in our little group, as far as I can remember, was first brought out clearly by Carl. It's always been Carl's favorite "argument" for the subjectivity of the wave function. This piqued my own curiosity to see where we first put it in print. I could find a trace of the idea in Footnote 44 of our paper `quant-ph/9601025`. Also I could see it shining through just after equation 4.105 on page 120 of my `quant-ph/9601020`.

If you think `quant-ph/9601025` gets sufficiently close to the mark, it might be nice if you'd cite it.

24-12-02 *Give Us a Pluriverse* (to G. L. Comer)

Comerism 3: *I've been fiddling around with the idea, though, that chance is a result of the intercourse between free-willed entities.*

Yes, I think I like that better. I had played with still a different turn for a while. One that I might sloganize like this: Chance is what you call "it" when viewed from the outside; free will is what you call "it" when viewed from the inside. What I wrote you in the last note, was a small attempt to get away from the monism of that slogan. But what you said above, I think, might now appeal to me even more.

Merry Christmas.

28-12-02 *Two Things* (to J. W. Nicholson)

Just reading a NYTimes article on cloning in which I read:

> Senator Bill Frist, the Tennessee Republican who was just chosen Senate majority leader and who favors a ban on all forms of cloning, called Friday's announcement "disturbing" and added: "While its validity is unclear, it should serve as a chilling reminder that individuals are still trying to clone human beings. These actions offend our sensibilities and undermine fundamental respect for the decency of human life."

Now, tell me how could someone say that if he didn't believe that the genetic makeup IS the person ... rather than predominantly the watermark? (Recall our conversation on the way to lunch one Friday.)

29-12-02 *Erratum?* (to A. Peres)

I just noticed that Conway (of the Conway-Kochen noncolorability result) makes no appearance in the Author Index of your book. Maybe that is an erratum, or maybe I don't properly understand your criteria for admission into that index. Either way, cheers!

Asher's Reply

> I did not include Conway in the author index because there was no reference to cite. Recently he and Kochen published a paper
>
> - J. H. Conway and S. Kochen, "The geometry of the quantum paradoxes", in R. A. Bertlmann and A. Zeilinger (eds.), *Quantum [un]speakables: From Bell to quantum information (Vienna, 2000)*, Springer-Verlag, Berlin, 2002, pp. 257–269,
>
> but these are mostly anecdotes. The only proof I know of their construction is the one in my book.

02-01-03 *Foundations Site?* (to P. Busch)

Thanks for the inclusion of a link on your webpage. The more people I can draw into my "quantum dreams" page, the more people I can hope to take the bait of thinking about quantum mechanics as the hint of something deeper and more wonderful. And the more people I can hope to start applying their brain pulp to the project! So, thanks again.

Congratulations on your promotion. When I look at your research record I guess I would have thought you to have become a full professor long, long ago! Certainly this is overdue.

I will be on a sabbatical from Bell Labs in Dublin in the coming year, so maybe I'll get a chance to drop by Hull for a visit.

Speaking of seeing you, Howard Barnum and I are organizing a session for one of Andrei Khrennikov's meetings in Sweden this summer. (I think it is in the early part of June.) Our plan is to give it the theme "quantum information meets quantum logic" and we're trying to figure out how to stretch the money we've been given to get as many interesting people there as possible. Some of the people we're thinking about trying to attract are you, Jeff Bub, Lucien Hardy, Dave Foulis, Alex Wilce, Dick Greechie, and maybe a few others. a) Would you be interested, and b) if so, would you need travel funding to get there?

02-01-03 *Glory Days* (to A. Peres)

Asherism 29: *Yesterday the Dept Chairman informed me that it was impossible to extend by one more year my professorial position, because of the dismal financial situation of Technion (well, the whole country is bankrupt). ...*

I am not unhappy to "retire" this fall. I won't have to teach or bother with administrative duties, and I'll have more time for research.

Congratulations on your new career move! You should think of yourself as being a postdoc again, and what glorious days you will have!

Here: I'll give you your first postdoctoral research project! I learned predominantly from you that the proper analog to the quantum state in classical physics is the Liouville distribution. However, I learned from Wigner that the only time evolutions for quantum states that are overlap preserving are (up to phase equivalence) the unitary evolutions:

- V. Bargmann, "Note on Wigner's Theorem on Symmetry Operations," J. Math. Phys. **5**, 862–868 (1964).
- C. S. Sharma and D. F. Almeida, "A Direct Proof of Wigner's Theorem on Maps Which Preserve Transition Probabilities between Pure States of Quantum Systems," Ann. Phys. **197**, 300-309 (1990).
- U. Uhlhorn, "Representation of Symmetry Transformations in Quantum Mechanics," Arkiv För Fysik **23**, 307–340 (1963).

My question is, what are the complete set of time evolutions on a classical phase space that are overlap preserving for Liouville distributions. As far as I can tell, this question has never been tackled in the literature.

02-01-03 *The Quantum Shirt* (to A. Cabello)

Yesterday morning, New Year's Day, as I went to get my wife's newspaper, I discovered that the previous day's mail had been accidentally delivered to my front door, rather than to the usual mailbox in the back. Anyway, in it, there was a package from Spain. Thanks so much for the shirt! I showed it off to my whole family and the friends that came to our afternoon celebration.

You are a great friend, and I am flattered.

03-01-03 *Growing Old* (to L. Hardy)

The last few days I've been editing my big samizdat—mostly just working on completing the name index—and what a task it has been! (The index is now almost 10 pages long all by itself.) But maybe one interesting thing (to me anyway) has come from the project: I'm starting to realize just how little the broad outline of my program has changed over the years. I don't know if that's good or bad, but it seems true. Apparently things started to gel in me somewhere around 1995/1996, and I've been trying to make the thought more precise ever since.

For instance, in my `quant-ph/0205039` this year, I write in a footnote:

> It is at this point that the present account of quantum mechanics differs most crucially from Refs. [Hardy01a] and [Hardy01b]. Hardy sees quantum mechanics as a generalization and extension of classical probability theory, whereas quantum mechanics is

> depicted here as a restriction to probability theory. It is a restriction that takes into account how we ought to think and gamble in light of a certain physical fact—a fact we are working like crazy to identify.

and I had thought that this characterization of what I'm shooting for arose only after reading your papers. But just a few minutes ago, I found this old note to Sam Braunstein:

> **19 July 1996, to Sam Braunstein, "The Prior"**
>
> While in Torino, you really got me interested in the old Cox Box question again. I noticed in this version of the book that Jaynes makes some points about how there are still quite a few questions about how to set priors when you don't even know how many outcomes there are to a given experiment, i.e., you don't even know the cardinality of your sample space. That, it seems to me, has something of the flavor of quantum mechanics ... where you have an extra freedom not even imagined in classical probability. The states of knowledge are now quantum states instead of probability distributions; and one reason for this is that the sample space is not fixed—any POVM corresponds to a valid question of the system. The number of outcomes of the experiment can be as small as two or, instead, as large as you want.
>
> However I don't think there's anything interesting to be gained from *simply* trying to redo the Coxian "plausibility" argument but with complex numbers. It seems to me that it'll more necessarily be something along the lines of: "When you ask me, "Where do all the quantum mechanical outcomes come from?" I must reply, "There is no where there." (with apologies to [Gertrude Stein] again!) That is to say, my favorite "happy" thought is that when we know how to properly take into account the piece of prior information that "there is no where there" concerning the origin of quantum mechanical measurement outcomes, then we will be left with "plausibility spaces" that are so restricted as to be isomorphic to Hilbert spaces. But that's just thinking my fantasies out loud.

Maybe it's just evidence of hardening of the arteries.

By the way, when are you going to send us the final version of your SHPMP paper, with references added? I hope you'll read this sentence and say, "Now."

03-01-03 *QM with Finite Fields* (to J. M. Renes and C. M. Caves)

I just came across a paper title that might interest some combination of us three:

- J. P. Eckmann and Ph. Ch. Zabey, "Impossibility of quantum mechanics in a Hilbert space over a finite field," Helvetica Physica Acta **42**, 420–424 (1969).

I have no clue what the actual content of the paper is, but it might be worth a look one day. In particular, does it connect any with the fact that the POVM-version of Gleason probably breaks down when the vector spaces are over a finite field? Or does it?

05-01-03 *ForAsher.tex* (to A. Peres)

Asherism 30: *Thank you for telling me that I'll be like a postdoc. It's so true. Now this will be my way of telling it to friends. Only one thing will be missing: a good adviser for my research (my first adviser in 1961 was Wheeler, although I was formally the postdoc of Misner). Will you be willing to play that role?*

Those are awfully big shoes, and I wouldn't dare to fill them. Nor would I even dare to the presumption of being your advisor! You have been my teacher since before we met, and my teacher you remain. (I think I first read one of your papers in 1989.)

Asherism 31 : *You already asked:*

> What are the complete set of time evolutions on a classical phase space that are overlap preserving for Liouville distributions?

All I can think of is Koopman's theorem (my book, pp. 317–318). The phase space evolution is also a unitary evolution. The overlap of two Liouville functions is constant in time in any Hamiltonian evolution.

Yes, that's right. And what I'm asking is whether there is a converse to this. If the converse were true, then it would mean that the sum content of Hamiltonian evolution is overlap preservation for Liouville distributions. I think that would be pretty if it's true. Also, if it is true, I don't think it is a completely trivial consequence of the usual Wigner theorem. I say this because I am asking for the preservation of Liouville overlaps only, not necessarily the preservation of overlaps between arbitrary functions on the phase space.

Asherism 32 : *Have you seen* `0301001`*, the first* `quant-ph` *of the year? It's by Grangier, who participated in the first Aspect experiment, and now writes some "foundational" papers. I like the expression "quantum holism" instead of "nonlocality." But there are many errors in that paper.*

No I haven't seen that one. I'll try to have a look at it eventually. Philippe and I have had several discussions on quantum foundations, to no great avail.

Asherism 33 : *I searched* `quant-ph` *for the string "unknown" and I found your* `0104088`*. Where was this published? Now I also found Mermin's "Whose knowledge?"* `0107151`*. Probably these two references are enough for my purpose.*

Yes, I think that will do. Here's my full reference on it:

- C. M. Caves, C. A. Fuchs and R. Schack, "Unknown Quantum States: The Quantum de Finetti Representation," *Journal of Mathematical Physics* **43**(9), 4537–4559 (2002). [Reprinted in *Virtual Journal of Quantum Information* **2**(9).] `quant-ph/0104088`.

The package of things I put together for you is concerned with some discussions I had with Charlie Bennett, John Smolin, and Philippe Grangier about the perils of thinking of quantum states as "properties" of the systems they refer to. I think that gets singularly in the way of Charlie's public expositions of what quantum teleportation is about. Putting together a package seemed a little relevant since you said you'd be giving a lecture on the subject: I don't think you will learn anything from the content, but you may enjoy some of the turns of phrase.

I'll place the code of my file "ForAsher.tex" below. [See 25-04-02 note "Short Thoughtful Reply" to C. H. Bennett, 14-05-02 note "Qubit and Teleportation Are Words" to C. H. Bennett and others, 14-05-02 note "Chris's World" to J. A. Smolin and others, 16-05-02 note "King Broccoli" to J. A. Smolin and others, and 01-06-02 note "High Dispute" to P. Grangier.]

06-01-03 *Pedagogy* (to N. D. Mermin)

Now I'd like to use your patient ear as an excuse to think out loud. This is what I was referring to when I wrote you yesterday, "Much more soon."

Today, I've got to spend part of the day preparing a talk to give at Bell Labs tomorrow. Here's the title and abstract I sent in for it:

> Title: Representing Quantum Mechanics on the Probability Simplex
>
> Abstract: Classical information theory is about input probability distributions, output probability distributions, and the transition functions that connect them. Quantum mechanics and so far quantum information theory, on the other hand, have been traditionally formulated in terms of linear operators on a complex vector space and the linear superoperators that connect them. To automate a comparison between the two theories, a means for expressing the newer theory in a way that leans toward the older, more established one ought to be sought. It can be done. This talk is about a small part of that project and a couple of mathematical questions it poses.

In substance, the talk will focus predominantly on the stuff I presented to you (privately) in Montréal, but will give a little more emphasis to the stuff about symmetric POVMs that Gabe looked at this summer.

However, I'd like to give the beginning of the talk a little different slant than I had previously. Here it is. (Here's the part where I'm thinking out loud.)

Everybody has their favorite speculation about what powers quantum information and computing. Some say it is the superposition principle, some say it is the parallel computation of many worlds, some say it is the mysteries of quantum entanglement, some say it is the exponential growth of computational space due to the tensor product. For my own part though, my favorite speculation is that it is Newton's Third Law: For every action, there is an equal and opposite reaction. Indeed I sometimes wonder if the very essence of quantum mechanics isn't just this principle, only carried through far more consistently than Newton could have envisioned. That is to say, absolutely NOTHING is exempt from it.

What do I mean by this? What might have been exempt from the principle in the first place? To give an answer, let me note an equivalent formulation of old Newton. For every REACTION, there is an equal and opposite ACTION. Strange sounding, but there's nothing wrong with it, and more importantly, this formulation allows for the possibility of an immediate connection to information theory. In particular, we should not forget how information gathering is represented in the Shannon theory. An agent has gathered information—by the very definition of the process—when something in his environment has caused him to REACT by way of revising a prior expectation $p(h)$ (for some phenomenon) to a posterior expectation $p(h|d)$ (for the same phenomenon).

When information is gathered, it is because we are reacting to the stimulation of something external to us. The great lesson of quantum mechanics may just be that information gathering is physical. Even something so seemingly unimportant to the rest of the universe as the reactions that cause the revisions of our expectations are not exempt from Newton's Third Law. When we react to the world's stimulations upon us, it too must react to our stimulations upon it.

The question is, how might we envision a world with this property—i.e., with such a serious accounting of Newton's law—but in a way that does not make a priori use of the information gathering agent himself? If the question can be answered at all, the task of finding an answer will be some tall order. For never before in science have we encountered a situation where the theorizing scientist is so inextricably bound up with what he is trying to theorize about in the first place.

It's almost a paradoxical situation. On the one hand we'd like to step outside the world and get a clear view of what it looks like without the scientist necessarily in the picture. But on the other hand, to even pose the question we have to imagine an information gathering agent set in the middle of it all. You see, neither Shannon nor any of modern information theory has given us a way to talk about the concept of information gain without first introducing the agent-centered concept of an expectation $p(h)$.

So, how to make progress? What we do know is that we actually are in the middle of the world thinking about it. Maybe our strategy ought to be to use that very vantage point to get as close as we can to the goal. That is, though we may not know what the world looks like without the information gathering agent in it, we certainly do know something about what it looks like with him in: We know, for instance, that he ought to use the formal structure of quantum mechanics when thinking about physical systems. Beyond that, we know of an imaginary world where Newton's Third Law was never taken so seriously: It is the standard world of classical physics and Bayesian probability.

Thus, maybe the thing to do first is to look inward, before looking outward. About ourselves, at the very least, we can ask how has the formal structure of our *behavior* changed since moving from what we thought to be a classical Bayesian world to what we now believe to be a quantum world? In that DIFFERENTIAL—the speculation is—we may just find the cleanest statement yet of what the quantum world is all about. For it is in that differential, that the world without us surely rears its head.

To do this, we must first express quantum mechanics in a way that it can be directly compared to classical Bayesian theory, where the information-gathering agent was detached from the world. That is what this lecture is about ...

As I say, just thinking out loud. Thanks for the imaginary ear.

07-01-03 *Newton's Third Law* (to R. E. Slusher)

By the way, here's the set of notes I wrote for myself before giving the talk Monday. Since the business about quantum mechanics being an expression of something like Newton's third law, but much deeper, seems to strike a resonance with something you were groping for one day in one of our discussions—i.e., when you were saying things like "you can't leave anything out in the quantum world"—I thought might enjoy reading this. [See 06-01-03 note "Pedagogy" to N. D. Mermin.]

08-01-03 *Your Note to Grangier* (to N. D. Mermin)

I haven't read the Grangier paper outside of the sentence he quoted from me, but I did read your letter. I think you do me pretty good justice, and I rather liked your explanation.

Merminition 98: *At the beginning of section II of* `quant-ph/0301001`*, I believe you miss Chris Fuchs's point. He is not talking about Bell's theorem; he is talking about something rather like pre-Bell EPR. After you have made a measurement on subsystem A, the information you acquire permits you to assign a quantum state (as defined by your second paragraph [in boldface type]) to subsystem B. Before the measurement on subsystem A, subsystem B had no quantum state.*

Fuchs uses this to argue that the quantum state of a system (or subsystem) — when it has one — cannot be an objective property of that system since statehood can (under EPR conditions) be conferred on a system from afar.

You want to have it both ways — denying action at a distance, yet maintaining the objectivity of the quantum state. I believe you can do it, but only if you acknowledge that the objective state of subsystem B after subsystem A has been measured (so B does indeed have a state) is not a local property of subsystem B.

I would say that the state of subsystem B is a compact way of summarizing (1) the preparation of the A-B system that resulted in the original EPR state, (2) the fact that nothing further was done to B, (3) the fact that a measurement was performed on A, and (4) the outcome of that measurement. Because (1)-(4) are all objective facts (Fuchs would disagree about this) the state of B can be said to be objective. But it would be dangerously misleading to call it an objective property of B, because this suggests something residing in B, whereas (1)-(4) are statements about both A and B and their earlier history.

True enough. But then here's my challenge to you. It's something I should have challenged to you too long ago. Once you have that a state can be said to be objective, and once you have that a measurement specification can said to be objective, then through the Born rule you have that the probabilities generated by the measurement can be said to be objective too. If so, then you must be able to give me a definition of what it means to be a particular probability value q in a way that 1) is not circular, and 2) makes no necessary use of a gambler. (For after all, invoking a gambler is just another way of invoking the agent/observer/experimentalist once again.)

10-01-03 *Filth Under the Rug* (to N. D. Mermin)

Merminition 99: *What's wrong with the old frequentist definition? If N different sets of qubits are subject to those same objective conditions the fraction of final measurements giving the outcome x gets very close $p(x)$ when N is large.*

Granted you need more probabilistic statements to say what you mean by "gets very close" (is that what you meant by circular?) but surely that's an issue for any non-Bayesian view of statistics and not peculiar to the interpretation of QM.

Now it is MY turn to be shocked by the triviality of YOUR reply. I almost feel like it is 1996 again, and we haven't made a bit of progress in our discussions. That bugs the hell out of me. What have I been wasting my time on all these years? You might as well still be writing papers that say, "If all quantum puzzles can indeed be reduced to the single puzzle of interpreting objective probabilities, I would count that as progress."

If what you're still shooting for is to sweep the issues of quantum mechanics under THAT rug—and there's every indication you are—you're just going to find more filth and dirt there. What a shame really. I thought you had been slowly absorbing the Bayesian point all this time. I guess I had just not expected you to fail my challenge in this facile way ... and I'm taken aback.

I'm fairly confident I understand (from your notes to Philippe and Rüdiger and also your newest paper) your latest view of what the quantum state is about. And it might as well be the same view as expressed in Asher's paper "What is a State Vector?" [AJP, 1984, p. 644]. What he calls a "procedure" or an "instruction set" you call a "history," but that's essentially where the difference ends. And just as that paper led Asher to no deeper or more convincing insight for 15 years, so too it will be with you if you continue down this ... I almost said "path," but maybe I should say "dead end."

Let me pick up on your last sentence above:

Merminition 100 : *Granted you need more probabilistic statements to say what you mean by "gets very close" (is that what you meant by circular?) but surely that's an issue for any non-Bayesian view of statistics and not peculiar to the interpretation of QM.*

You get this completely backwards. The great insight Ed Jaynes had, and that Carl and Rüdiger and I have slowly been reckoning with, is that because quantum mechanics is so intimately tied up with probability, one cannot hope to disentangle the troubles of quantum mechanics without FIRST clearing up what the formal structure of probability theory is actually about. And on that first count, we think the Bayesians are the winners.

Once that is accepted—clearly you haven't accepted it yet, but that is no matter for the argument I want to make—then the task is to ask, what are the IMPLICATIONS of that acceptance for our understanding of quantum mechanics? What my debates with you, and to a lesser extent Carl, and to a still lesser extent Rüdiger, have been about since the beginning of your BFM murmurs is just this: What are the implications?

My starting point has been the unbending acceptance that probabilities are of the (de Finettian) subjective caste. What are the IMPLICATIONS of this? Well, the first thing one gets is that the quantum state is of the same subjective caste. But then—and I don't know why it was so hard to stumble across this, except possibly for sheer prejudice—the next thing one gets is that at least some quantum operations are also of the same subjective caste. For beauty's sake, I then go further than Carl and Rüdiger are presently willing to go, and say, "If so be it for SOME quantum operations, then so be it for ALL quantum operations." But the main point is that the first three steps of this paragraph are pure implication.

You called it poetry at the Montréal meeting—yes, it did hurt a little—but it is logic just as clean as you can get it. And it's of the most elementary sort. (That's what made your remark hurt.) The only thing that makes it appear to be poetry to you is some deep resistance and, I suspect, fear of where it leads.

So, I have implications that run FROM interpretation of probability TO quantum mechanics. So what? If it just stays at you saying objective every time I say subjective, then this is a worthless exercise and a waste of time. There had better be more to it. And I claim there is.

For, I would say the implications above lead me down a mathematical path. Whereas your hope to retain the word objective for these structures leads you nowhere.

1. My point of view COMPELS me to ask whether there is a way to think of a quantum state as a (single) probability distribution, plain and simple. With a little toil, I find there is.

2. My point of view COMPELS me to seek out the analogies between Bayes' rule and quantum collapse. With a little toil, I find an analogy that's never been found before.

3. My point of view COMPELS me to ask why the notion of quantum measurement is anything other than the refinement of one's belief, i.e., exactly what classical (Bayesian) measurement is about. With a little toil, I find that it is precisely that after all ... just that in the quantum case there is an extra little kick given to my final state of belief.

4. Here, I think this is the most important one: My point of view COMPELS me to ask, if a quantum operation is as subjective as a quantum state, then why are the two mathematical structures not formally identical? And we are led back to Jamiołkowski's and Choi's old insight: A quantum operation IS a density operator. With a little reflection, one sees that that had to be ... in the same way that *prior* probabilities $p(h)$ and *conditional* probabilities $p(h|d)$ are both probabilities nevertheless.

I would dare say that your point of view—where probability theory is, at very best, secondary to, or at very worst, absolutely detached from the deeper issues of quantum mechanics—would leave all of these things as little more than coincidences. "There is a way to map quantum operations and unitary operators to density operators? Who cares? It's just as mysterious as the structure of quantum mechanics to begin with."

But as long as there are coincidences in the structure of the theory, that structure will always be a mystery. What I think Bayesian probability theory does for us is COMPEL us to view as natural the connections we see within the axioms of quantum theory, rather than as miracles plain and simple.

So you see, you have depressed me. If I can't make any headway with my best and most sympathetic friends—you're one of them—I don't see how I'm going to make any headway in the wider world. Even YOU had not realized that all this talk about Bayesian stuff was meant to LEAD us, and not just be an afterthought tacked on for NOTHING BUT philosophical reasons.

Have you read Carl's document "Resource material for promoting the Bayesian view of everything" posted at his website `http://info.phys.unm.edu/∼caves/`? It would do you some good. It's about time you took a course in Bayesian Ideas 101. "What's wrong with the old frequentist definition?"—that about knocked me over!!

If after reading this note, you don't think it is too offensive, I may forward it to Carl and Rüdiger. I'll bet they too will be shocked—though much more polite in reaction than me—by this dangerous frequentist tendency you're starting to reveal.

I thought I was going to write a little report on your Copenhagen Computation today, but I knew I couldn't touch it until I got this off my chest. Sorry about that. I won't be able to write you a report until Sunday now. (I'll be in NYC tomorrow.)

In friendship, disappointment, and enduring hope, ...

13-01-03 *Ouch* (to N. D. Mermin)

I told Kiki the other night, "Well, I probably lost a friend today." In an ominous voice she replied, "What'd you do?" I said, "I wrote David Mermin a scathing note about some stuff in quantum mechanics." In a scolding tone, she said, "Why do you always do that?" I said, "I just couldn't take it. This guy hasn't hardly absorbed a thing in our six years of discussion!" She said, "Why do you get so upset? You know your work's never going to be done: Churches never go out of business, do they?"

Ouch.

13-01-03 *Der Kopenhagener Geist* (to N. D. Mermin)

I actually don't have much to say in the capacity of a referee.

First, a couple of typos: [...]

Finally, a little technical point: [...]

Now, let me tell you the thoughts you provoked as I was reading the paper.

Merminition 101: *The state of n Qbits has no meaning going beyond the abstract state vector itself, together with the rules for how it can be constructed and the computational uses to which it can be put. We return to this below, merely noting for now that although we shall speak often, as everybody does, of "the state of n Qbits" the terminology is potentially misleading. It must not be taken to imply that the state characterizes a property possessed by and directly inferable from* *those Qbits, as it does for Cbits. A better, but clumsier usage, would be always to say "the state associated with n Qbits".*

For my own part, I would say, "the state ascribed to n Qbits." "Ascribed to" is not a hell of a lot clumsier than "of," and it has the advantage that it makes clear and serves as a constant reminder that the origin of the "state" is not in the system itself, but in a system external to it—namely, the agent.

Merminition 102: *The fact that the generic multi-Qbit state is incompatible with associating states with the individual Qbits is already an indication that Qbit states have a much more abstract character than the states of Cbits, which are always products of one-Cbit states.*

Nice sentence. I like it. That's the way everyone ought to view the issue.

Merminition 103: *The state associated with the Qbits is merely an extremely convenient way of recording the potential consequences of the past actions of the computer on those Qbits. The consequences of those past actions can become accessible in only one way, and this way is the only way to extract information from n Qbits: one can measure them.*

There's something about this that I don't quite like, but I'm having trouble putting my finger on it. Probably has something to do with your using the word "consequences" in a way that I wouldn't endorse. See glossary on page 49 of my *Quantum States: W.H.A.T.?*

Merminition 104: *The Born rule contains, as a special case, a quantum imitation of the unproblematic (and therefore usually unremarked upon) process of extracting information from Cbits. ... The statistical, state-altering character of the outcome of a measurement of n Qbits in a general state becomes the deterministic, state-preserving, unproblematic classical extraction of information when the state is one of the* 2^n *classical-basis states. (page 7)*

and

Merminition 105: *The view of quantum mechanics I gave my computer scientists relies on a primitive notion of measurement, without which the computation has neither a beginning nor an end. A measurement gate is a black box whose interaction with the n Qbits results in an unambiguous output on a display, whose reading is as unproblematic as reading the display of an ordinary classical computer. Measurement is where the quantum-computational process starts, by permitting the association of an initial state with the Qbits, and finishes, by producing an unambiguous digital output. Quantum computer science delves no more deeply into how information is actually extracted from a measurement than does classical computer science, where the preparation of the initial state of the Cbits and the reading of their final state are steps too trivial to warrant explicit theoretical attention, though they are certainly of concern to the engineers who design the computer. (page 9)*

I like these lines. But ask a philosopher if this is unproblematic. The reason the physicist finds it unproblematic is the same reason he finds the independence or nonindependence of the continuum hypothesis unproblematic in transfinite set theory: He never thinks about it. But philosophical lives have come and gone on the question. (Probably, the question first came to life for me in reading William James's little book, *Pragmatism*.)

Anyway, I view your lines above as the greatest contribution of the present paper. That is, because they sort of soften up the western front for a (one-day-in-the-future) full-fledged assault. To the extent that you can find yourself willing to do that, I'm happy.

The way I would emphasize the issue, though, is to say that quantum measurement is either as *problematic* or as *unproblematic* as classical measurement, take your pick. The main point is that it is not MORE problematic.

Here's the way I put it in my `quant-ph/0205039`:

> As far as Bayesian probability theory is concerned, a "classical measurement" is simply any *I-know-not-what* that induces an application of Bayes' rule. It is not the task of probability theory (nor is it solvable within probability theory) to explain how the transition Bayes' rule signifies comes about within the mind of the agent.

And here's the way Rocco Duvenhage put it in another paper in `quant-ph`:

> In classical mechanics a measurement is nothing strange. It is merely an event where the observer obtains information about some physical system. A measurement therefore changes the observer's information regarding the system. One can then ask: What does the change in the observer's information mean? What causes it? And so on. These questions correspond to the questions above, but now they seem tautological rather than mysterious, since our intuitive idea of information tells us that the change in the observer's information simply means that he has received new information, and that the change is caused by the reception of the new information. We will see that the quantum case is no different.

The reason I say you are softening up the western front is because, though you seem to admit this for a single, particular quantum measurement—the computational basis—you haven't yet had the heart to admit it for ALL quantum measurements.

The only things, it seems to me, that set the quantum case of measurement apart from the classical case is A) what we do with the information we gather, and B) what we concede the information is about. In the classical case, we enact "Bayes' rule full stop" with the information we've just gathered. In the quantum case, we generally do something more (unless we are confident that the system we are talking about was not touched physically by our measuring device). Concerning B), we had gotten in the habit classically of thinking that the information we've just gathered is about "what is" or "what was." Quantum mechanics instead teaches us to look to the future. The information is about "what will be." (I use the measurement device LOCKED AWAY in the bureau of standards to make it dramatic.)

So, the action, the excitement, of quantum mechanics is not in the measurement, but in what it is that we're presupposing about the world that causes us to process our data differently than we would have classically.

Merminition 106: *A state can be associated with the Qbits only if their prior history is of a certain special form. The state can then be constructed out of the particular features of that history: the outcome of the initial measurement and the particular sequence of unitary gates subsequently applied to the Qbits prior to the moment at which one associates the state with the Qbits. But there is no way to determine that state if one is simply presented with the Qbits; only those who know this history know what state to assign to the Qbits. The state does not reside on the Qbits; it is a concise encapsulation of those features of their history, back to the initial measurement, that are relevant to the outcome statistics of a subsequent measurement.*

and

Mermინition 107: *But to describe this as a collapse of the state of the Qbits and to regard it as a second kind of time dependence that Qbits can have in addition to their unitary evolution, is to ignore the whole point of the state vector and its unitary evolution, and, of course, to confuse the Qbits themselves with the state vector that compactly summarizes the statistical implications of their past history for future measurement outcomes.*

Beside these two passages, I wrote in the margin, "Asher's 'What Is a State Vector?'." It's interesting that you (in your note Friday evening) called this accusation to be that of a "regress." This is because I think an infinite regress is all this view is gonna get you. That is to say, ultimately you're going to have to admit that the quantum state "compactly summarizes the statistical implications of [the] past history" of the entire universe.

Look at the discussion around Eq. (14) in my `quant-ph/0205039`. What you would call a measurement is always determined by a further quantum state for the apparatus. And off to infinity (or the boundaries of the universe) it goes.

Merminition 108: *Indeed, the generalized Born rule demonstrates that their state cannot be regarded as a property carried by the Qbits, since it provides an indirect method for associating a state with n Qbits that share an entangled state with an additional Qbit, and therefore cannot initially be associated with any state of their own. By measuring only the additional Qbit, one disentangles the $(n+1)$-Qbit state and is able to assign a state to the n Qbits, even though nothing interacts with them during or after the one-Qbit measurement. This is only possible because the association of all $n+1$ Qbits with an entangled state prior to the measurement requires the additional Qbit to interact with the n of interest at some time before it is measured. If one knows enough about that past interaction to determine the original $(n+1)$-Qbit entangled state assignment, then it is not surprising that the outcome of the measurement on the single Qbit can provide enough additional information to permit the assignment of a state to the n Qbits. This newly assigned state cannot be a property inherent to the n Qbits, because nothing interacts with them during the process that takes them from stateless and externally entangled to having a state of their own.*

Tell this to Philippe Grangier a thousand times and tell me whether you've made any more progress than I have. I think it is just silly to say "a state is a property" or "a state is a reality," and then say "some systems have no properties" (when they are entangled). The realization he should have is rather, systems have whatever properties they have, it is just that "the" quantum state is not one of them.

Finally,

Merminition 109: *[I]n our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience. — Bohr (1934)*

I don't like this quote, precisely because it is going to cause people to accuse you of what they always (unjustly) accuse me of: BANNING a set of questions that every physicist in his right mind ought to be asking. What is the real essence of the phenomena?

I have a pretty good reply to that now. I'll attach it (`ForSlusher.pdf`) for your midnight reading.

14-01-03 *Impact Analysis* (to L. Hardy)

As I was driving in to the office yesterday, I heard that January 13 is officially designated "Make Your Dreams Come True Day." That seemed apropos, as I was using the long drive to tumble some things over in my head about Perimeter.

I do have a dream, and it is simple and obvious and just about written on my forehead: It is to get at the heart of quantum mechanics (or help the friends around me to do it), and then turn that understanding toward the next big phase of physics. I do not think I am fooling myself by believing so strongly that we are within arm's reach of this goal. It is just a question of concentrated, right-headed thinking with a smidgen of creativity ... and probably the main issue is simply amassing enough intellectual resources in one place for a final resonance.

15-01-03 *Vanity, Sayeth the Preacher* (to J. W. Nicholson)

Nicholsonism 8: *Is everyone in quantum foundations as verbose as you?*

No one else in science is as verbose as me.

15-01-03 *The Ability to Write* (to N. D. Mermin)

I got a critique of one of my email expositions from a friend this morning, and he said:

Nicholsonism 9: *... the tone is probably appropriate. [But] it's about twice as long as (I thought) it needs to be. Is everyone in quantum foundations as verbose as you?*

He's got a grain of truth there. But I find it so hard to rope myself in sometimes. I guess the best writer says what needs to be said and little more. I've got to work on the demons inside me.

15-01-03 *Memory Lane* (to O. Cohen)

Pasted below is a letter I started to write to you in early December but never finished because of the holidays and some personal things that got in the way. ...

It seems I have email contact with you only about every three years. I hope things are going well. Where are you now, and what are you doing professionally? The last I remember, I recommended you seek a postdoc position with Carl Caves in New Mexico; I guess that never materialized.

Anyway, I'm writing you because I've been reading your paper "Classical Teleportation of Quantum States" this week. It's a nice paper, and I very much like the simplicity of your scheme and the point you make with it. I am in complete agreement.

In fact it took me a little down memory lane. You see, Asher Peres and I had used teleportation as an example in our March 2000 *Physics Today* article, "Quantum Theory Needs No 'Interpretation'," precisely to illustrate the sensibility of the conception of a quantum state as a "state of knowledge, rather than a state of nature". When the paragraph peaked in clarity (i.e., before the editor's knife), it went like this:

> The peculiar nature of a quantum state as representing information is strikingly illustrated by the quantum teleportation process. In order to teleport a quantum state from one photon to another, the sender (Alice) and the receiver (Bob) need a pair of photons in a standard entangled state. The experiment starts when Alice receives another photon whose polarization state is unknown to her, though known to some preparer in the background. She performs a measurement on her two photons, and then sends Bob a classical message of only two bits, instructing him how to reproduce the unknown state on his photon. This economy of transmission appears remarkable

> because to completely specify the state of a photon, namely one point in the Poincaré sphere, we need an infinity of bits. However, the disparity is merely apparent. The two bits of classical information serve only to transfer the preparer's information, i.e., his *state*, to be from describing the original photon to describing the one in Bob's possession. This can happen precisely because of the previously established correlation between Alice and Bob.

At the time I was basing my opinion predominantly on the result of Cerf, Gisin, and Massar (`quant-ph/9906105`, "Classical Teleportation of a Quantum Bit") along with the long heartfelt conviction that the idea of a quantum state as a state of knowledge gave the most sensible and constructive point of view about quantum mechanics. Sometime after that though, Steven van Enk and I—to the best of my recollection—worked out a scheme pretty similar to your own at the chalkboard at Bell Labs. We never wrote it down however.

But that doesn't take away from your discovery. It's a very clean example, isn't it? The conclusion you draw, I think, is particularly important: the phenomenon of quantum teleportation only looks surprising and remarkable if one takes an ontic view of the quantum state. In fact, in the past, I have accused some of my friends (some of whom were authors on the original teleportation paper) of sticking with an ontic interpretation of the quantum state precisely because it is the only way to keep the phenomenon surprising and newsworthy. You might enjoy reading some of my correspondence with them on the subject: Have a look at pages 175–176 and 184–189 of my samizdat *Quantum States: What the Hell Are They?*. The said accusation comes on page 189. That correspondence occurred, by the way, as they were debating about how a dictionary definition of "quantum teleportation" should be written. (To get a copy of this samizdat, you can download the pdf file for it at my webpage; there's a link to the webpage below in my 'signature'.)

If you are interested in seeing the struggle Asher and I had in constructing the paragraph above (and the reasoning and cues behind it at the time), have a look at the discussions in my other samizdat, *Notes on a Paulian Idea*, `quant-ph/0105039` (or you can download a better indexed version of it from my webpage). The pages to look at are 312, 316, 319-320, 322, 326, 327-330. I have pages 326, 328, and 329 marked as the most interesting in my notebook (can't quite remember exactly in what way though). Maybe the main lesson in those discussions is how difficult it is to give up an objectivist language when using quantum states, even for a recalcitrant positivist like Asher, and even in an example intended to be illustrative of why quantum states should be viewed as states of knowledge, rather than states of nature. (The philosophy being: if quantum mechanics looks too very mysterious, then you're probably being wrong-headed about it. Case in point: if teleportation looks mysterious, then you're probably being wrong-headed about it too.)

The sense I get from your paper is that you are much more neutral about the lesson than I am. You say simply: "[O]ur classical version of teleportation is just as impressive as the original protocol, if we think of quantum states as representing states of knowledge. . . . If, on the other hand, we think of a quantum state as having ontological content, . . . , then our classical version of teleportation is not equivalent to the quantum case," and leave it at that. However, there is a spate of evidence starting to come out that a significant fraction of some of the most 'remarkable' phenomena in quantum information theory can be mocked up with classical toy models just as your own. The only requirement for seeing it is that one must focus on the epistemic states (i.e., the states of knowledge) in such models rather than the ontic states (like the actual H or T in your own model). For instance, Rob Spekkens has a toy model which he has presented in several conferences and which he is writing up presently as a paper, "In Defense of the Epistemic View of Quantum States: A Toy Theory," in which he can reproduce the following quantum mechanical and quantum information-theoretic type phenomena in a pretty NONremarkable way:

the noncommutativity of measurements, interference, a no-cloning theorem, a no information-gain-without-disturbance principle, the multiplicity of pure state decompositions of a mixed state, the distinction between two-way and intrinsic three-way entanglement, the monogamy of entanglement, superdense coding, mutually unbiased bases, locally immeasurable product bases (i.e., what we originally called 'nonlocality without entanglement'), unextendible product bases, the possibility of secure key distribution, the impossibility of bit commitment, and many others. (In particular, he gets teleportation too, just like you do.) As Rob puts it in his abstract:

> Because the theory is, by construction, local and non-contextual, it does not reproduce quantum theory. Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. ... The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

So, given that your paper is an independent and particularly notable link in that, and as opposed to his paper, your result is not buried within over 70 pages (and counting) of text, I very much endorse it. I think the lesson is this: A good lot of quantum information theory is simply regular probability theory and information theory applied in ways that had not been deemed interesting before. What is interesting and unique to the quantum itself, thus, must be something else.

In my paper `quant-ph/0205039`, "Quantum Mechanics as Quantum Information (and only a little more)," I tried to give the community to a call to arms by saying this:

> This, I see as the line of attack we should pursue with relentless consistency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

What your work and Spekkens' work does, from my perspective, is give the best illumination yet of what I was hoping for when I was speaking of "combinations of terms" in that passage. Teleportation—being a certain combination of uses of the axioms of quantum mechanics—is nevertheless a purely probabilistic or information-theoretic effect. As such, it tells us very little about the ontology behind quantum mechanics.

My own view—and the thrust of my research program presently—is that these examples help us to realize that what is unique in quantum mechanics is not the probabilities (i.e., the quantum states) but what the probabilities are applied to. There, I think, lies the essence of quantum mechanics: It is localized in the Kochen-Specker theorem. "Unperformed measurements have no outcomes," as Asher Peres likes to say. That is to say, where quantum mechanics gets its uniqueness is from breaking with the old idea that a probability (as a subjective state of knowledge) must be knowledge about a pre-existent reality. Instead, probabilities can just as fruitfully be applied to capturing one's knowledge of "what will come about due to one's actions." The predominant issue becomes how to formalize the difference between probability theory as applied to pre-existent facts and probability theory as applied to "creatables" (for want of a better word).

There are some lines for tackling this idea buried within Sections 6.0 and 6.1 of my `quant-ph/0205039` ...

16-01-03 *A Footnote* (to H. J. Folse)

I've been reading William James's book *Essays in Radical Empiricism* and I came across a footnote in "How Two Minds Can Know One Thing" to do with something Harald Høffding said. It just piqued my interest because I know from your papers (and some other things) that there is an interest in the extent to which William James may have influenced Bohr via the conduit of Høffding. What I have never seen any talk of is the extent to which Høffding may have influenced James. Do you know of any works in that regard?

On a related point, now that I've started to take such an interest in James's theory of truth and what I perceive as its similarity to the stance Asher Peres and I have taken on quantum mechanics, I've started to wonder why there has been so little scholarly work on this before. In Henry Stapp's 1972 paper "The Copenhagen Interpretation" there is a decently extensive discussion on pragmatism, but I don't think I've seen anything really beyond that. Could it be that there's a big gap in my canvassing of the literature? Do you have any leads? Or is it just, unfortunately, a subject that has been overlooked heretofore?

Henry's Reply

> There's no doubt that Høffding was very excited by James' radical empiricism, and I think that it is reasonable to say something like they were "on the same wavelength" but that they had arrived at their views more or less independently. Both men were psychologist-philosophers in a time when psychology was just beginning its break away from the womb of philosophy. Both were empiricists, and both took the "enemy" to be the then dominant force of idealism. H's interest in the question of free will was no doubt stimulated by his interest in Kierkegaard, but surely resounded strongly with James central interest in that same issue. H's *History of Modern Philosophy* which was published in 1895 makes no mention of James. So I suspect his admiration for James only began sometime between 1895 and 1904. He was one year younger than James, when they met in 1904 James would have been 62 and H 61, so I think that their outlooks were pretty well already formulated by that point. In the spring of 1905, right after his visit to James in the fall of 04, H gave a series of lectures on "the psychology of free will" which definitely featured James (he also lectured on Renouvier, whom James admired, and Boutroux, both of whom Jammer has pegged as expressing the same theme that experience can never be completely captured by any conceptual scheme). It was also in 1904-05 that James wrote and put in an envelope marked "essays in radical

empiricism" most of the articles which Perry later posthumously published as *Essays in Radical Empiricism*. Also, right after his return from America in 1905 H published an English translation of his 1902 Danish book called *Philosophical Problems* which included a preface by James himself. We're not certain, but there is a good bit of circumstantial evidence that Bohr attended at least some of H's 1905 lectures. Even if he didn't, it seems to me that it is inconceivable that he never discussed James with Høffding. So clearly J can be said to have "influenced" H, but perhaps it would be best put by saying J's influence was one of underscoring themes that were already present in H's thinking before he visited James.

As to whether or not H "influenced" J, I am not sufficiently familiar with the James materials to venture more than a speculation. However, I do know who probably could answer that. He is John J. McDermott, who's at Texas A&M and has edited a lot of James materials. When he visited Loyola 15 years or so ago I talked with him about James and Høffding, but I was of course primarily interested in both of their influences on Bohr, not on each other. James had only 6 years left after he met H, and I doubt if he had read any of H's works earlier. So there would not have been a lot of time for any great amount of influence, though James's work continued to evolve right up to the end, so one can't rule it out. Of course most of H's writings were in Danish, which James would not have known, but he did know German, and I believe H had published his Kierkegaard book in German. It is just possible that J could have read that, but I think what brought the two men together was more likely to have been the common interest in psychology. In 1906 H published an English translation of his book on *Philosophy of Religion*; I would imagine he very probably sent James a copy, and that surely would have interested James, but I think that H was a good deal less tender minded when it came to religion than was James. So I would think that if there were any influence from H to J, it would have most likely had to come through their conversations in the fall of 04 and the subsequent correspondence that must have been concerned with the publication of James's Preface to H's English translation of his book. As I recall, McDermott had definitely read this correspondence (I imagine it's at Harvard?) but as I said, I didn't ask about any influence of H on J. I also recall one of my old mentors, Andy Reck at Tulane, who wrote a book on James for the French, knowing something about Høffding and James, so I'll ask the next time I see him, but I would think McDermott would be a better bet.

> On a related point, now that I've started to take such an interest in James's theory of truth and what I perceive as its similarity to the stance Asher Peres and I have taken on quantum mechanics, I've started to wonder why there has been so little scholarly work on this before. In Henry Stapp's 1972 paper "The Copenhagen Interpretation" there is a decently extensive discussion on pragmatism, but I don't think I've seen anything really beyond that. Could it be that there's a big gap in my canvassing of the literature? Do you have any leads? Or is it just, unfortunately, a subject that has been overlooked heretofore?

I think that you're right if by pragmatism you mean particularly William James, and/or the so called "classical" pragmatists Peirce and Dewey. But in a larger sense pragmatism has seeped into a great amount of American philosophy and can be said to be a major element in the thought of people like van Fraassen (BTW, I had a very brief conversation with him about you last November) and Arthur Fine. Certainly I would say my own interpretation of Bohr reflects a lot of pragmatist themes, and you could reasonably say I characterize Bohr as a "pragmatic realist." (There is a different anti-realist branch of pragmatism which is represented by vF and Fine.) If I had to name a pragmatist to affiliate this view with, I'd say it is close to C. I. Lewis. In my years of discussion of Bohr with my colleague Jan Faye, I've always pushed Bohr closer to pragmatism, while Jan pushes him closer to positivism. Indeed I would say that a lot of what people see as "positivist" about Bohr, are really the themes that positivism shares in common with pragmatism, namely a hard nosed empiricism.

Have you looked at Max Jammer's two books? I'm quite certain he discusses Bohr and James, but I don't own copies of his books. Gerald Holton also does the James-Bohr issue in his well known article in *Thematic Origins of Scientific Thought* but there's nothing you'll learn there that isn't in my book, and I think he caves in too easily to Rosenfeld's "authority" on the James/Bohr influence. Jan doesn't discuss pragmatism per se but he does discuss James a fair bit in his book on Bohr and Høffding. You ought to just take a look at his book, *Niels Bohr: His Heritage and Legacy*, for its exposition of Høffding in Chap IV, which is without doubt the most detailed exposition of H available in English.

Now that you bring it up, I think one thing is that a lot of those who've written about the history of QM are Europeans and therefore perhaps less likely to emphasize pragmatism or see pragmatism at work in the quantum revolution than would Americans. Most of the younger generation of philosopher physicists writing these days are not so interested in historical kinds of questions, but just take off from the formalism (you're exceptional in that you seem to be going in the reverse direction). Another thing is that the only classical pragmatist to live long enough to actually write on QM was Dewey, and he got uncertainty all messed up as a disturbance caused by elephants trying to measure grains of sand. So that would hardly be likely to enthuse people to look for insights into QM from Dewey. But apart from stupid things he might have said about QM, there are in Dewey's vast corpus a lot of interesting insights about the scientific description of nature which could possibly be useful to someone looking to make sense of quantum mysteries. The same thing might be said about Peirce who saw "truth" as what an ideal rational body of inquirers would reach given an infinite amount of time. Rather than explain the process in terms of the end goal, he explained the goal in terms of the end of the process, and that's really a very profound notion. But if you're looking for contemporary pragmatists, there's no doubt that the leading "pragmatist" expositor on QM these days is none other than van Fraassen in your own backyard. He's of course a pragmatic anti-realist, but one who oddly retains a realist correspondence notion of truth that pragmatists would typically reject. It's just that he thinks this realist notion of truth has nothing to do with "acceptance" of scientific theory which is done solely on the basis of its empirical adequacy and supplementing pragmatic virtues.

I'll let you know if I think of anything else.

18-01-03 *The Quantum Principle, whatever it might be* (to L. E. Ballentine)

Somehow I've been spared from the Volovich, Khrennikov, etc., distribution list on "What is Quantum Mechanics?" ... and I'd like it to stay that way! But this morning David Mermin forwarded me one of your notes (pasted below), which I did enjoy. The point you make is an important one, and I think the whole nub of the matter.

I try to express something similar in my paper "Quantum Mechanics as Quantum Information (and only a little more)", `quant-ph/0205039`. I'd like to think that paper makes a little progress toward your goal, but, if so, there is still a long, long way to go. In any case, I think you might enjoy the paper because of some things I remember reading in your papers and your book several years ago and also because of some discussions I've had with your student Joe Emerson. In particular, playing up the analogy between the quantum state and the Liouville distribution for all it is worth is, I think, the most important move for getting the whole project off the ground.

If you have any comments on the project set forth in that paper (and expanded at my website, link below), and in particular on how we might go further, I'd certainly love to hear them.

Ballentine's Entry in the Discussion

I think that this question is best interpreted as, "What are the principles of Quantum Mechanics?". (Indeed, Igor Volovich referred us to his Seven Principles ....)

The first two principles are usually given as:

(a) Observables are represented by self-adjoint operators on a Hilbert space;

(b) Pure (mixed) states are represented by vectors (statistical operators) in Hilbert space.

What, if any, is the physical content of these postulates? They contrast most unfavorably with Einstein's two postulates, from which all of Special Relativity can be derived:

(1) Equivalence of all uniformly moving frames of reference;

(2) Invariance of the speed of light, c.

These are physical postulates. At least (1) has strong intuitive appeal. Both can be directly tested by experiment. And all of SR follows from these two postulates.

Return now to postulates (a) and (b) of QM. They lack any intuitive appeal. They cannot be directly tested. They are insufficient to solve even one real problem in QM.

By itself, (a) seems to impose no physical restriction, since the spectrum of an operator may contain any possible range of discrete and continuous values. The Indeterminacy Principle is permitted, but not implied, since the observables might, at this stage, be commutative or not. Perhaps (b) together with (a) imposes some physical restrictions, but that is far from obvious. And Gleason's theorem seems to make (b) inevitable after (a).

The specific content of the theory (needed in order to solve any physical problems) enters when we specify which particular operators correspond to which particular observables. Although this is sometimes done by means of extra postulates, in fact the most important operators can be derived by requiring invariance under the space-time symmetries (displacements, rotation, and Galilean transformations). [See my book, *Quantum Mechanics – a Modern Development*, World Scientific, 1998.] In particular, the commutation relation between position and momentum, and hence the Indeterminacy Principle, now follows.

But the same space-time symmetries hold in Classical Mechanics, where there is no Indeterminacy Principle. So postulates (a) and (b) must have imposed some restrictions on the physical contents of the theory, in spite of the appearance that they only defined a general mathematical form without specific physical content.

So my questions are:

- What are the *physical* principles of Quantum Mechanics?
- Can they be expressed in a more transparent form?
- To what extent do the usual postulates (a) and (b) restrict the physical content (not merely the form) of Quantum Mechanics?

20-01-03 *Munificence* (to S. Aaronson)

Thanks for teaching me the word munificence. I'll try to incorporate it into my vocabulary!

Aaronsonism 3: *How far would you take your famed battle cry, "Give an information-theoretic justification if possible"? In particular, venturing beyond QM, would you want/expect an information theoretic justification for why space has 3 visible dimensions, or why the cosmological constant is positive? Both things have been worrying me a lot lately.*

Unfortunately, I don't know how to answer that yet. My feeling is that eventually the battle cry ought to stop. But where, I don't have a strong feeling of yet. I try to lay out the philosophical underpinning of this idea in my `quant-ph/0204146`, where I talk about the "core" of a theory—i.e., a part that at some level likely does not have an agent-centered information-theoretic reason.

I also try to say it somewhat differently in a recent essay I wrote; I'll paste it below. [See 06-01-03 note "Pedagogy" to N. D. Mermin.]

Aaronsonism 4: *I've gotten increasingly annoyed with people who whine, "We need more algorithms! Shor's and Grover's are not enough!" I'm starting to suspect that large-scale quantum computers would be incredibly useful, but for entirely different reasons than the ones that excite us computer scientists. Is that reasonable? I.e. am I mistaken to think that in whole areas of chemistry and high-energy physics, the biggest bottleneck right now is the lack of a quantum computer (to do things like perturbation sums)?*

Yeah, I think that's probably true. But now a question to you. To what extent has it been proven *rigorously* that quantum computers are efficient at performing simulations to do with interesting questions about *other* quantum systems?

20-01-03 *And Believing Simulations* (to D. Poulin)

You're going to be ashamed of me—or maybe you always expected it!—but I've only now read (in detail) the note you sent me December 20!

I liked your phrase "objectivity distillation."

Poulinism 7: *I came across a very simple problem lately which forced me to choose an interpretation for the wave function. It is a little bit hard to explain in an email but I'll gave it a shot.*

I did gather that your note is a specific—and, as opposed to what my lazy butt was willing to do, actually worked out—instance of the sort of thing I talked about a lot in my file *Quantum States: What the Hell Are They?* I.e., of a program I spelled out to Brun-Finkelstein-Mermin in a 7 August 2001 note in this way:

I have the feeling that if quantum mechanics is really about knowledge and only knowledge—or better, belief convergence and *only* belief convergence—then FOR ANY GIVEN METHOD OF GATHERING INFORMATION, there *should* be a way to ferret out of quantum mechanics the necessary and sufficient conditions on two observers' initial state assignments, so that the gathered information leaves them in a better agreement than they started out with.

But I missed this: Which interpretation were you forced to choose!?!? (I'm keeping my fingers crossed we'll see each other in heaven!) Seriously, does this example cause you to lean more toward the epistemic interpretation of the wavefunction or further away? And why? I didn't understand your reasoning on that.

David's Reply

Yes, my note was definitely in this spirit. When I tried to find a way of simulating this system, it was screaming out for an epistemic interpretation for the wave function. But I don't have a complete understanding of it. In its current formulation (which I would call hybrid epistemic-ontic), the monte-carlo simulation will inevitably be observer dependent. What I would like to do is to formulate it in a purely epistemic way, just like you guys did with the tomography problem. Thus, my goal would be to run a monte-carlo which simulates the measurements performed by the observers but whose branching probability is not observer-dependent. This would allow them to dilute objectivity in the sense that I have tried to explained in my previous message. In fact, whether the observers come to an agreement or not (a sort of agreement measure) will also be observer dependent in this simple model which is quite strange!!!

I could tell you many strange things about this simple model but there are probably things you have already thought about ... and they are quite easy to discover when you think of how you would go and numerically simulate this system.

By the way, one of the Montréal talks has really influenced me ... my own! I am taking this physics simulation business quite seriously. In an attempt to show that there is no fundamental (i.e. sub-polynomial) advantage at using a Q-computer to simulate a physical system, I am looking into information processing at the Planck scale. That's right!

20-01-03 *More Pragmatism* (to H. J. Folse)

Thanks for the detailed note. Let me go through it sequentially and make just a few comments.

Folsesm 13: *Also, right after his return from America in 1905 H published an English translation of his 1902 Danish book called* Philosophical Problems *which included a preface by James himself.*

I'll try to dig up a copy of that. That sounds interesting to me.

Folsesm 14: *He is John J. McDermott, who's at Texas A&M and has edited a lot of James materials.*

In fact, I have a massive collection that he put together—*The Writings of William James: A Comprehensive Edition*. It was one of the first things I bought when I took an interest in James. However, I soon discovered that I preferred the original (little) books; they're much easier to carry around in a coat pocket. I'll have a look in the index to that book when I get home. Also, though, I had planned on visiting Marlan Scully and Ed Fry at Texas A&M sometime this year; so maybe I'll try to work up a visit with him too.

Folsesm 15: *Well, I don't remember being much impressed by the exposition of "pragmatism" in Stapp's paper, though it was ages ago that I read it.*

Yep, don't get me wrong: I wasn't particularly impressed with it either. I was only trying to imply that that is the only thing I know of along those lines.

Folsesm 16: *I think that you're right if by pragmatism you mean particularly William James, and/or the so called "classical" pragmatists Peirce and Dewey.*

Yes, that is exactly what I meant.

Folsesm 17: *But in a larger sense pragmatism has seeped into a great amount of American philosophy and can be said to be a major element in the thought of people like van Fraassen (BTW, I had a very brief conversation with him about you last November).*

Aha, that might explain why I got an email from him November 21 that started out like this:

> I know I have not been very communicative recently, but I thought in any case I'd let you know about this bit of Bayesianism ...

I have wondered a little bit about whether I've rubbed him raw, since I didn't hear too much from him after his fairly enthusiastic introduction.

Folsesm 18: *Have you looked at Max Jammer's two books? I'm quite certain he discusses Bohr and James, but I don't own copies of his books.*

Yes, I have read them both (years ago). And more recently I have noted the relevant material in them on this subject. But it's not deep enough for my tastes.

Folsesm 19: *Jan doesn't discuss pragmatism per se but he does discuss James a fair bit in his book on Bohr and Høffding. You ought to just take a look at his book.*

I read it too, about 10 years ago. I remember there were parts in it that I absolutely loved. But then I remember there were parts (like his discussion of Bell inequalities) that I absolutely hated—I thought he went way off the mark. I should reread the book from start to finish, to see how it affects me now. I have certainly changed a lot in my opinions about quantum mechanics since those days. In fact, maybe I'll try to see if I can buy the book.

Folsesm 20: *Another thing is that the only classical pragmatist to live long enough to actually write on QM was Dewey, and he got uncertainty all messed up as a disturbance caused by elephants trying to measure grains of sand. So that would hardly be likely to enthuse people to look for insights into QM from Dewey.*

Yep, that's sad. The insights I'm looking for have to do with a) James's theory of truth, b) James's pluralism, c) and the lovely idea that the universe is still under construction.

Let me attach a couple of essays I've written in connection to this cluster of issues.

1) To contrast myself to the Deweyian thing you mention above, I'll attach a note titled "Pedagogy." [See 06-01-03 note "Pedagogy" to N. D. Mermin.]

2) To expand on the pluralism thing, I'll attach part of a note titled "Probabilismo!" It builds on some previous stuff that I had sent you about Ulfbeck and Bohr the younger. [See 14-11-02 note "Probabilismo!" to D. M. Appleby.]

3) Finally, concerning c) above, now that you can download my files, why don't you have a look at two notes I wrote Howard Wiseman in the collection "Quantum States: What the Hell Are They?" The titles are "The World is Under Construction" and "Probabilism All the Way Up" and start on pages 210 and 217, respectively. This is where pragmatism strikes me the most.

21-01-03 *Høffding and James* (to H. J. Folse)

I did manage to find the following by looking through Ralph Barton Perry's *The Thought and Character of William James*. (I had previously read the condensed one volume version, but just last week in Manhattan I picked up the full two volume version for $30. Høffding was not mentioned in the condensed version. Now I'm chompin' at the bit to read the full thing—it has loads more stuff about Boutroux, etc.)

The first mention of Høffding recorded there is in a letter from James to John Dewey dated 17 October 1903:

> It rejoices me greatly that your School (I mean your philosophic school) at the University of Chicago is, after this long gestation, bringing its fruits to birth in a way that will demonstrate its great unity and vitality, and be a revelation to many people, of American scholarship. I wish now that you would make a collection of your scattered articles, especially on "ethical" subjects. It is only books that tell. They seem to have a penetrating power which the same content in the shape of scattered articles wholly lacks. But the articles prepare buyers for the books. My own book, rather absurdly cackled about before it is hatched, is hardly begun, and with my slow rate of work will take long to finish. A little thing by Harald Høffding, called *Philosophische Probleme*, which I have just read ... is quite a *multum in parvo* and puts many things exactly as I should put them. I am sure of a great affinity between your own "monism," since you call it, and my "pluralism." Ever gratefully and faithfully yours,

The second comes from a 1910 article in *Nation* titled "A Great French Philosopher at Harvard." James writes:

> The great originality of M. Boutroux throughout all these years has been his firm grasp of the principle of interpreting the whole of nature in the light of that part of it with which we are most fully acquainted, namely, our own personal experience. ... Those readers who know something of present-day philosophy will recognize in my account the same call to return to fullness of concrete experience, with which the names of Peirce, Dewey, Schiller, Høffding, Bergson, and of many minor lights are associated. It is the real empiricism, the real evolutionism, the real pluralism; and Boutroux (after Renouvier) was its earliest, as he is now its latest, prophet.

21-01-03 *Choice Quotes* (to G. J. Milburn)

I just canvassed all the book reviews in QIC, in preparation for the one I've got to write on Holevo's new book. I just loved a couple of choice quotes from your article in QIC **1**, p. 89.

> [N]o one has seen a 'probability' in the same way we see a coin land heads up or a pointer deflection on an instrument.

That one was fun because I had just had a conversation with Steven van Enk a week ago, in which he asked, "How would you explain that we never experience a superposition?" I replied, "Have you ever experienced a probability distribution?" A few days later, he came in and told me that that single line had made a big effect on him. He also suggested that I should write a paper of single-liners (maybe in question and answer format): He thinks that my usual papers are far too long, and that I'd get the message out more effectively if I'd trim them so that people can see what the point is! If I ever do follow through, I'll make sure to cite your book review.

> There is no consensus view on why quantum mechanics offers a bonus, if any, in computational efficiency. The allusions to the many-worlds-interpretation of quantum mechanics in the chapter introducing quantum computation, provide an indication of how desperate the situation is. The many-worlds-interpretation provides most certainly not a consensus view of quantum mechanics and, while seductive, it seems to me little more than the last desperate refuge of the classically minded. As far as I am aware photons do not interact with each other in this universe, let alone with photons in other putative universes.

That is great!! It's somewhat like another line that I love, that you might enjoy too. It comes from David Mermin's pedagogical paper, `quant-ph/0207118`. He writes,

> There are nevertheless some who believe that all the amplitudes … have acquired the status of objective physical quantities, inaccessible though those quantities may be. Such people then wonder how that vast number of high-precision calculations (10^{30} different amplitudes if you have 100 Qbits) could all have been physically implemented. Those who ask such questions like to provide sensational but fundamentally silly answers involving vast numbers of parallel universes, invoking a point of view known as the many worlds interpretation of quantum mechanics. My own opinion is that, imaginative as this vision may appear, it is symptomatic of a lack of a much more subtle kind of imagination, which can grasp the exquisite distinction between quantum states and objective physical properties that quantum physics has forced upon us.

Anyway, as usual, I just wanted to get this stuff into my computer and I used you as the sounding board. Thanks for the opportunity. I'll repay you with a little essay I wrote the other day (on the subject matter of 2 above); I'll paste it below. [See 06-01-03 note "Pedagogy" to N. D. Mermin.]

21-01-03 *The Thawing Heart of David Poulin* (to D. Poulin)

Your notes are warming my heart! To see you embrace this distillation idea (and develop it from your own perspective) so keenly, and to see you actually do something with it, is just great.

Poulinism 8: *In fact, whether the observers come to an agreement or not (a sort of agreement measure) will also be observer dependent in this simple model which is quite strange!!!*

No, I think that's quite marvelous and something that one shouldn't loose sight of. I'm guessing it's a general feature of all models that do this problem correctly. See my attempt to formalize the issue on pages 23 and 24 of my *Quantum States: What the Hell Are They?* on my webpage. You'll see I accounted for the potentiality of that feature too.

Poulinism 9: *By the way, one of the Montreal talks has really influenced me ... my own!*

I'm glad you got something out of one talk!

21-01-03 *Peeling Away the Agent* (to S. Aaronson)

Aaronsonism 5: *I loved your diatribe to Preskill. But I realized we have different ideas of what it means for a fact to have an "information-theoretic justification." For you, it means the fact is observer-dependent, and therefore we should "peel it away" if we want to uncover a theory's true physical content (as in your general relativity example).*

For me, the fact could be as observer-independent as anything we know – e.g., the dimensionality of space. Giving an "information-theoretic justification" means you show 3 things:

(1) The fact in question has some implication for the computational complexity, communication cost, etc. of some problem. (That is, anything that would interest a computer scientist.)

(2) If the fact were different, the implication would change.

(3) We (or at least I) have strong intuitions regarding computation: for example, NP-complete problems should not be solvable efficiently. Changing the fact would violate these intuitions – thus yielding an "information-theoretic justification" for why the fact is the way it is.

The ground for a disagreement with that, actually, is maybe the biggest thing I took away from Timpson's thesis. And I guess that's why I liked it. In particular, I had not appreciated so much before the very human-centeredness of the Turing-machine concept. That is, a mathematical model for what is humanly computable.

Also, just focus on the phrase "communication cost." When it comes time to quantify that, a probability distribution is going to be introduced somewhere. At that point, I say, you may not realize it but an agent has been invoked. For, probability ultimately only makes sense as a gambling commitment an agent would be willing to make—i.e., probability only makes sense in the Bayesian sense.

Below, I'll paste a (part of a) note I wrote to Ari Duwell and I'll attach a copy of the paper he's putting in the special issue of *Studies in History and Philosophy of Modern Physics* that Jeff Bub and I are editing. [See 06-12-02 note "Quantum Information Does Not Exist" to A. Duwell.] Between the two of them (including the references I put in the note), I hope it better explains this viewpoint I'm building up.

Aaronsonism 6: *Cases where I think the program has already been carried out include "why is quantum mechanics linear?" and "why is there a minimum length scale (i.e. the Planck scale)?" Also, your de Finetti paper goes some way toward carrying it out for "why are amplitudes complex rather than real or quaternionic?"*

And I'd like to think that Sections 4.2 and 6 of my paper `quant-ph/0205039` get at "information-theoretic reasons" for why the structure of quantum measurements takes on that of the POVMs and, also, why the state-change rule (for what happens upon the completion of a measurement) is what it is.

Finally, let me comment on something that I missed in your last note.

Aaronsonism 7: *PS. I remember you talked about William James in the samizdat . . . Have you read the* Principles of Psychology*? I'm working through it now. I've decided to recommend it to people as "the most up-to-date, state-of-the-art book about consciousness" (without telling them the publication date).*

Is that really true? Do you really believe that? In any case, the comment warms my heart, as I have turned into a huge fan of William James since my first samizdat. I only sort of knew him cursorily when I wrote that stuff. But now he is splattered all over the place in the new samizdat, "Quantum States: What the Hell Are They?"

You might enjoy the stories of my real discovery of him recorded therein. Take a look at "The Reality of Wives" on page 15 (sent to Landahl and Preskill) and the bottom of page 218 in the note "Probabilism All the Way Up." The document is posted at my website.

I haven't read *Principles of Psychology* yet, but I have read *Pragmatism*, *The Meaning of Truth*, *Some Problems in Philosophy*, *Essays in Radical Empiricism*, and a few other scattered articles like "The Sentiment of Rationality." Also—though not written by James—I've read *The Cambridge Companion to William James* and a couple of other second source books. Finally, Ralph Barton Perry's book, *The Thought and Character of William James (Briefer Version)*, is just wonderful. And it's chock full of great letters. Just last week in Manhattan I finally found a copy of the full version, in two volumes, and I'm chompin' at the bit to get at it.

22-01-03 *Another Footnote* (to H. J. Folse)

This one I dug up from somewhere on the web.

> The classic discussion of the "stream of thought" is, of course, to be found in James, W. (1890). *The Principles of Psychology* (2 vols.). New York: Henry Holt; for a discussion of James's view on the "stream of thought," see the essay on James in this volume. While there are only two references to Høffding in James's *Principles*, one having to do with Høffding's theory of recognition memory, a second with Høffding's position on the role of bodily sensation vs. "spiritual affection" in emotion, and both are mildly critical, James nonetheless goes out of his way to indicate a general approval of Høffding's work, in one instance referring to "his excellent treatise on Psychology" (James, op. cit., Vol. 2, p. 455) and in the other professing his "respect for him as a psychologist" (ibid., Vol. 1, p. 674).

29-01-03 *Elegance* (to A. Peres)

Elegance is always in the eye of the beholder.

I just ran across `quant-ph/0212062`, where the last sentence of the abstract reads, "Thus we recover Fuchs and Peres' formula in an elegant manner."

Still, it would be interesting if they have developed a powerful formalism.

05-02-03 *Accuracy (Urgent)* (to J. M. Renes)

I'm hurriedly writing the last section of a long overdue paper on a measure of quantumness for ensembles of states, and I mention symmetric informationally complete POVMs there. Could you take a look at my footnote on page 23 and check it for accuracy? (I mention the business about the long overdue paper to hopefully give you a sense of my urgency!) In particular, I have heard of some other people involved in the SICPOVM project—for instance, the guy who constructed the $d = 4$ case—and I know I should mention them too if I'm going to mention anyone. But I don't know who they are. I'll attach the paper as a PDF file.

06-02-03 *Midnight Oil* (to M. Sasaki)

Do you want a house in America? Ours just showed up in the real estate listings today: [...]

I am going to miss the place. Do you see the rightmost section of the house, with two windows toward the front, and three windows on the side? There are also two windows on the backside that you cannot see. That is the section of the house that I used as my study. I surrounded myself with sunlight and the philosophy of William James in there. Too bad I never got a chance to invite you over for a stay.

But the science will be good for me in Ireland. And that is always what keeps my heart beating. John Lewis (of Davies and Lewis) will be my boss. Also I will have a significant overlap with Chris King and Mary Beth Ruskai, who will themselves be visiting there for something like 2 and 4 months respectively. My plan is to greatly develop the ideas of Section 7 in my paper `quant-ph/0205039`. This will require that I much better understand the structure of the set of completely positive maps. I think I have a set of questions in that regard that no one has ever explored before. Basically, I want to develop the slogan, "A quantum operation is nothing but a quantum state in disguise." Lately I am taken with the idea that the Jamiołkowski representation theorem—equation 116 in my paper—is one of the deepest statements in all of physics ... second only to Einstein's principle of equivalence! (I am only joking a little!)

Anyway, I want to very much apologize to you again for causing such a great delay in getting our paper into the public eye. Deep inside I am a very selfish and undisciplined person, and it can hurt my friends and associates. It is a shame that you got caught up in that.

06-02-03 *Looking at the List Again* (to N. D. Mermin)

Merminition 110 :

> [CAF wrote:] I'm particularly keen to see the reaction Spekkens gets. (Did he give you a private version of his talk in Montréal as I had asked him to?)

He may have talked to me in Montréal. In my usual irresponsible muddle-headed way, I can't remember who he is or who I talked with.

He has a talk on about 27 reasons why you ought to think that the quantum state is epistemic in nature:

1. You can't clone a quantum state. Guess what? You can't clone a Liouville distribution either.

2. You can change a quantum state from afar by gathering information nearby. Guess what? You can do the same with classical joint probability distributions.

3. … and so the list goes.

Moral: You can believe the quantum state to be ontic in character if you want. But then you've got to create an ad hoc reason for justifying each and every one of the effects above. Wouldn't it be so much simpler and more natural to accept the epistemic hypothesis and follow out its consequences?

06-02-03 *The Minimum Maximally Sensitive Set of States* (to J. W. Nicholson)

… or something like that will be our title. Now that we've got a title, we've got to write a paper.

06-02-03 *No It Doesn't [AJP 53(3), 70 (2000)]* (to D. Poulin)

Now the last note of yours was even sweeter music to my ears! Sorry I haven't replied, but I've been trying to get a paper ready to get on quant-ph next week, and I was quickly losing all face with my Japanese coauthor. But maybe I've also gained face with you in the process: In contrast to my paper `quant-ph/0204146`, this paper has 135 equations. (Have a look at the abstract on quant-ph and you'll see what I mean.)

And speaking of `quant-ph/0204146`,

Poulinism 10: *Last night, I went through the first 20 pages or so and found a recurrent theme: if reality was deterministic, there wouldn't be any distinction between dreams and awareness. I am not sure I fully understand the deep meaning of this but I sure like it. Basically, you are saying that nondeterminism is a proof that there is something out there, right?*

I don't know if I'd go as far as saying "proof", but, yes, that is basically the idea. I think the place where I give the fullest account of what's on my mind about that—though definitely my opinion is still forming on the subject—and say it all the most clearly is in `quant-ph/0204146`, actually. The sections to read are the last two, devoted to Preskill and Wootters. If you read them, please let me know if you get anything out of them. I got a great compliment from Michael Berry, who told me he really liked the paper. But Rüdiger told me he couldn't understand a thing of it; and Carl Caves said something like "it's sickening trash." It's important that you first rent and watch the movie *It's a Wonderful Life* if you haven't ever seen it before. (Too bad it's not Christmastime.)

Anyway, you helped ease my depression with your note. If confident, young David Poulin will invest the time to read 20 pages in my samizdats, maybe just maybe I'm not writing for nothing. And maybe just maybe our community will pool its strength enough to conquer this quantum beast. I'm serious: It made me happy that you took me seriously, and I know I'm gonna sleep better for it tonight.

David's Preply, "Quantum Mechanics Needs an Interpretation"

> How are you? Have you had a chance to look at the problem I send you? And if you did, were you able to understand anything? I believe that this problem and the compatibility problem are both quite interesting because they are well defined mathematical problems for which any solution forces you to give the wave function a meaning. For me, it went the other way around with compatibility. When I wrote the paper with

Robin, it seemed clear that the solution was in the lines set by BFM. It is only later that I realized that underlying this solution was an interpretation of the wave function: a mixed state "can be" a derived product due to lack of knowledge while pure states are really out there. I have put "can be" in quotes since they could also be fundamental when entanglement is present: the adepts of the Church of the Enlarged Hilbert Space would see no difference between these two cases. As you probably suspect, I am no longer so much sympathetical with this point of view. Nevertheless, I find it exciting that there are mathematical problems which require an interpretation to be solved.

I think that the day I can find a satisfactory answer to both these problems will be the day I have made up my mind on the correct interpretation. Of course, this raises the problem of what is a satisfactory answer. My only hope is that it will be like pornography: I'll know it when I'll see it.

The last few emails you have send me contain something like "of course, this is in my notes…". I have glimpsed through your notes before but never had the time (or motivation) to go through the entire thing. I am attempting it now. It will be hard since I am so busy with my two classes and learning spin foams … but it is quite entertaining. Last night, I went through the first 20 pages or so and found a recurrent theme: if reality was deterministic, there wouldn't be any distinction between dreams and awareness. I am not sure I fully understand the deep meaning of this but I sure like it. Basically, you are saying that nondeterminism is a proof that there is something out there, right?

08-02-03 *Is Is* (to M. Sasaki)

I was just reading the *New York Times* (my usual newspaper on the web), and I thought of you when I read the following paragraph:

> "The question is, is this about American power, or is it about democracy?" Mr. Asmus asks. "If it's about democracy, we'll have a broader base of support at home and more friends abroad. The great presidents of the last century — F.D.R., Wilson, Truman — all tried to articulate America's purpose in a way that other parts of the world could buy into. Bush hasn't done that yet." Before long, we'll find out if he cares to.

Actually, you had me worried about the "is, is" construction last week. Your change caused me to reflect that, actually, I was not sure if the thing was proper after all. I'm still not sure. But I do see that others use it too.

We now have three offers on our house! We have decided that we will accept the highest as of noon Monday. I will probably be able to put our paper on `quant-ph` Tuesday.

12-02-03 *Your Recent* `quant-ph` *Posting* (to C. M. Caves)

Let me emphasize the possible connections between this quantumness stuff and some of your more recent work. Maybe the thing that intrigues me the most at present is the conjectured connection between A) symmetric informationally complete POVMs and B) the minimum-cardinality maximally-sensitive quantum alphabet.

What is the deeper meaning of that? The main thing (maybe the only thing) that intrigues me about the SIC-POVMs is that they have got to give A) the prettiest region on the simplex (in the spirit of my National Bureau of Standards stuff that I keep drawing in my lectures) and

B) the maximal-volume region on the simplex. Both these are ingredients that one might think makes quantum mechanics look as close to simple Bayesianism as it can be. Yet, that happens for a "maximally quantum alphabet." Why?

I don't have a clue what the connection is, but my religion tells me there is one.

13-02-03 *Your Schedule* (to K. R. Duffy)

Quantum theory—I predict it will ultimately turn out—is nothing *but* control theory ... when the components have a wonderful sensitivity to the touch (more slippery than the classical kind). So, you've been doing the right thing.

13-02-03 *Doubting Our Coherence* (to C. M. Caves)

See page 5 of `quant-ph/0110107` by Halvorson and Clifton.

19-02-03 *Irish Dates* (to C. King)

My main research plan for my time in Dublin is to get a better handle on the structure of CPMs. What I'd like to get are some characterization theorems in line with my foundational ideas. In particular, I'm thinking of the main issues in Section 7 of my `quant-ph/0205039` and also the samizdat *Quantum States: What the Hell Are They?* on my webpage. That is, I want to put some mathematical meat on my slogan, "A quantum operation is truly and only a density operator in disguise."

20-02-03 *OK* (to C. M. Caves & J. M. Renes)

OK, I desist. Each day and each evening, I tell myself, "Tomorrow I'll really get to that paper," and each morning following something transpires against me. I'll give you what I have: It's only a minor variation of what you originally sent me all those months ago. Do with it what you please: I know that I should be shot for my behavior.

Let me just tell you what I wish were strengthened, in case you decide to be better citizens than me: [...]

One point of philosophy. I don't buy for a minute statements like, "Properties of physical systems, though useful if they help in this task, are ultimately irrelevant." That kind of positivism has never been my guide (though it does fit Asher Peres quite well). My only point has been that one should not mistake the quantum state for a property. The foundational task, as I view it, is precisely to uncover what can be called a property of a quantum system and what cannot. To that task, I see this paper as making a sound contribution.

20-02-03 *Intro Draft* (to J. Bub)

The more times I read over your draft for the intro, the more I like it. I'll send you a very mildly revised version this afternoon.

Possibly the only delicate point will be how I might de-emphasize the role of entanglement enough to suit me, while still pleasing you. A lot of quantum information does not depend upon entanglement at all (most quantum key distribution schemes, for instance, but also quantum non-locality without entanglement, incompleteable product bases, etc.) Or look at the present debate on the power of unentangled states for quantum computation (cf Gilles' talk at the commune). Finally, you ought to know by now that my own opinion is that entanglement is likely to be a red herring in the deeper vision of things: I see it as subordinate to the structure of measurements. Derived and secondary.

20-02-03 *New Draft, Only a Drop More of Poetry* (to J. Bub)

Attached are my modifications to your draft.

I debated for a long time toning down the theme on entanglement, but in the end gave up. I know that I compromise my beliefs somewhat with some of the phrasings, but, very probably, only *I* know that ... and, in the end, I may be able to make an easier connection to the entanglement stampede this way. (My role has always been that of a mole.)

Just some notes for your reference, so that you'll know what I did and what I thought: [...]

2) I inserted "they thought" between "which" and "spelled" in "could exist in certain states which spelled trouble for the Copenhagen interpretation" (second sentence, first paragraph). I did this to leave a window open for the possibility that EPR were on the wrong track with their argument. From my view, the only thing they demonstrated ultimately is that the quantum state is epistemic in character. If Bohr had known that word—epistemic—I think he would have agreed with them up to that point. He would have only disagreed that a more complete epistemic characterization could be given.

3) Second paragraph, fourth sentence: "That is, depending on what measurement Alice chooses to perform ... and the outcome of the measurement, Bob's system will be left in one of the states of some mixture ..." I'll just note this as a sentence that I would not normally write anymore, even though I left it intact. The trouble with it is that it conveys the image that the "state" is something possessed and inherent in the system—i.e., that it is a "property" of the system. Instead, I view the state in question as a property of Alice's head—i.e., her information about the system—and would normally use language appropriate to convey that idea. Since, however, we are contrasting things on Schr̈odinger's understanding, I can be OK with usage in this instance. [...]

10) Last paragraph. Added one small drop of poetry.

22-02-03 *Lorentz Chair* (to J. Preskill)

By the way, what was it like to sit at Lorentz's desk? If you give me a good anecdote, I'll record it for posterity.

24-02-03 *Born and Toeplitz* (to A. Peres)

Asherism 34: *Do you know of an autobiography of Max Born? Danny told me that he saw it (in Russian translation?) and that Born wrote that when he was a student, he was a baby-sitter for the daughter of Prof. Toeplitz in Gottingen. The reason I ask is that the daughter later came to Israel, and recently passed away. She was a cellist in Lydia's orchestra. Her father was an amateur photographer, and she had pictures of all the great men, Hilbert, etc. Lydia borrowed them to show to the Dean of Maths, who had a shock, and asked permission to display some of the pictures in the hallways of his building.*

I think there are two books that could be reasonably counted as autobiographies of Born: 1) *My Life and My Views*, and 2) *Physics in My Generation*. (I just looked them up in my list of burnt books.) I don't remember if there was any overlap in the essays there—I seem to recall that there may have been—but I read them both 10 or more years ago. I do remember enjoying them. (I actually read the Born-Einstein letters twice, but that's a different subject.)

Interesting to hear about Toeplitz. I hadn't noticed his name until last year. I used facts about Toeplitz matrices to calculate Eq. (40) in my `quant-ph/0205039`.

24-02-03 *Fall Calendar and Symmetry* (to W. E. Lawrence)

Lawrencesm 1: *I personally have gotten interested in two directions — unbiased basis sets and related operator sets (and the question of why nobody has found a proof that these exist other than for power-of-prime dimension). Is this just number theory, or is it Physics that I don't understand yet?*

I'll bet that's a number theory reason, rather than a physics reason ... but you never know. Still, though, I'm glad you're interested in the question. I've been interested in quite a similar one: the one of the existence of "symmetric informationally complete POVMs." You can find these things defined on page 23 of my new paper `quant-ph/0302092`. They are much like a complete set of mutually unbiased bases, but even more symmetric. In contrast to the MUB question, though, we do know (numerically) that they exist in, at least, all dimensions from $d = 2$ to $d = 14$. Mostly I'm interested in them on three counts:

1. I think they will give the most beautiful definition for the "standard quantum measurement device" in Section 4.2 of my `quant-ph/0205039`. The prettier I can make the region in Figures 1 and 2—the latter on page 38—the more insight I think we'll be able to glean from such a representation of quantum mechanics.

2. Also, in connection to that, I suspect that the symmetric POVMs define the largest such region possible. Thus, thinking of quantum mechanics as a modification of usual Bayesian reasoning (which makes use of the full probability simplex, not just a restricted piece of it), such a measurement defines a venue in which our reasoning can be as close to Bayesian as possible. That is to say, if you're looking for the minimal change quantum mechanics makes to our methods of probabilistic reasoning, it will be there.

3. Finally, referring to page 23 of `quant-ph/0302092` again, there's something quite interesting about these POVMs that I've just come to realize. There appears to be a sense in which the elements in that set are just as quantum as they can be with respect to quantum eavesdropping. (Since posting that paper, by the way, I have proven that the accessible fidelity for such an ensemble really is $2/(d+1)$, after all.)

So, now I wonder what is the deeper connection between all these statements.

25-02-03 *Book Review* (to A. S. Holevo)

I don't know whether you are able to get the journal QIC (*Quantum Information and Computation*) at your institute, so I will send the file attached directly to you. It is a small (very small) review of your new book that I wrote for the journal. I hope you enjoy it, and that it does not offend you too much (via my greed of always wanting a deeper and deeper explanation of quantum mechanics).

I have distributed copies of your book to Caves and Bennett so far. Friday, when I see Peter Shor, I will give him a copy too. Unfortunately, I forgot to give one to Schumacher the last time I saw him in November (even though I had packed one up for him). I will, though, give him a copy when I see him in May at the Bennett 60th-Birthday Symposium.

25-02-03 *9909073* (to P. Busch)

Carl Caves's student Joe Renes has finally put together our work on Gleason-like theorems for POVMs into a manuscript, with special emphasis on 2-D Hilbert spaces (where one can restrict the class of POVMs and still get the result). We need to cite you. Has `quant-ph/9909073` still not been published?!?!?! I just looked at your webpage and see no update of it. We're hoping to post the manuscript this week.

26-02-03 *Distribution List* (to A. S. Holevo)

By the way, I count that I have three copies of your book left, beside my own. (QIC had also sent me a copy since I was a reviewer.) Who would you like me most to distribute them to? Below is a list of people that I will either see on or before the Bennett-fest.

M. Gell-Mann
E. Fredkin
T. Toffolli
S. Wiesner
G. Brassard
N. D. Mermin
W. Wootters
A. Peres
R. Jozsa
P. Shor
B. Schumacher
J. Smolin
S. Popescu
R. Horodecki
A. Zeilinger
M. B. Ruskai
C. King
J. Lewis
O. Hirota

05-03-03 *Quantum Anti-War Petition: Need Exponential Growth* (to 58 people)

Dear friends and quantum colleagues, (i.e., this really is Chris), If you are as disturbed about the prospect of war in Iraq as I am, the world-wide destabilization it may cause, and the trashing of the United Nations it indicates, I hope you will consider signing the petition at:

`http://www.moveon.org/emergency/`

As far as I can tell, `MoveOn.org` is a serious, significant organization and one to be taken seriously. The petition seems to be at least 300,000 signatures strong presently, but I hope it will grow exponentially before its delivery to the United Nations tomorrow (March 6). You can read about the organization's legitimacy by having a look at:

`http://www.moveon.org/moveonpress.html`

I apologize if this note is an intrusion on your politics, but I tried to be selective in my distribution list, based on conversations I've had with each of you in the past. (I hope I have remembered correctly.) Most importantly, I hope you will pass on this note, or a similar one, to your own friends before tomorrow's deadline.

07-03-03 *Quantum Anti-War Petition: Need Exponential Growth* (to A. Plotnitsky)

You're probably right about the fatalism—that things have progressed too far to be turned around—but there is still time to sign the petition today. The number of signatures has now reached over 550,000. It's one of the very largest petitions in history. They're now aiming for 750,000 signatures. So, I hope you will spread the word.

24-03-03 *Har Har* (to J. M. Renes)

Renesism 8: *Read any good papers on Gleason's theorem lately?*

Point well taken, and I assure you I was already feeling guilty before I got your note. It's just that this darned move and house sale has been pretty much all-consuming. I will, however, be in Ireland away from the family (on my house-hunting trip) for the full week this week. I am hoping to get quite a bit of work done with this opportunity. Your paper is at the top of the list.

27-03-03 *Crypto Scheme* (to B. Yurke)

Here's a simple crypto scheme for these states.[43] It's a little BB84 and a little B92. Alice sends one of the d^2 states drawn at random. After Bob receives the signal, but before he performs a measurement, Alice reveals that the state she sent belongs to a set X of d linearly independent states (X being a subset of the original set of d^2 states). Bob then performs an unambiguous state-discrimination measurement appropriate for the set X. In this way, through a pre-agreed numbering of the states within each set X, Alice and Bob should share a key on the successful outcomes.

The questions are a) how secure is this system, and b) how efficient is it? Clearly it is more efficient in the limit of large d. But that should also be the limit in which it is the most secure … unless I'm missing something. In any case, to do this right is going to take some analysis.

[43] As stated here the crypto scheme is incorrect, but easily modified into something workable.

21-04-03 *A Question of Condensation and Time* (to J. I. Rosado)

Thank you for your interest in my paper. Have we met before? Perhaps in Oviedo? Anyway, I apologize for taking so long to reply to you, but for over a month now I have been scrambling with the details of relocating my family to Dublin. Finally that task is coming to an end, and I hope to be able to turn my attention back to science soon.

Rosado-ism 1: *In section 3 you give an argument, EPR+teleportation, to support the idea that a quantum state is a state of beliefs, so that they have nothing to do with real properties of the real physical state. At the same time, I think, your argument also implies that the outcome of a measurement doesn't reveal a preexisting property of the physical system, inclusive for pure states, inclusive of the case of a measurement of an observable that has that pure state as eigenstate. Do you think that this conclusion is true? If true why don't you have stressed it? I think it is very important to be explicit in this conclusion because in the vast majority of the quantum literature we can find phrases like: "quantum measurement, with very few exceptions, cannot be claimed to reveal a property of the individual quantum system existing before the measurement is performed." so that the vast majority of the scientific community thinks that if we can predict with certainty the outcome of a measurement then this is so because we know a real property of the individual quantum system.*

Yes, that is true. I have tried to stress it very significantly to many of my friends, but unfortunately I may not have yet carried the point through as clearly as possible in my papers. It is a question of time, finding the right phrases, and biting the bullet for a big job. What I mean by the latter is condensing the notes in my document "Quantum States: What the Hell Are They?" into a proper paper. If you are not familiar with that document, I would encourage you to try to follow some of the arguments there, starting around page 35. The content of the samizdat follows the format of email messages, but often they are self-contained. (You can download the samizdat from my webpage; there is a link for it below.) I have it as a goal to try to write a real paper on the subject before the end of the year, but we shall have to see if that really materializes.

Rosado-ism 2: *My conclusion, after I have read your paper, is that if we know with certainty that a measurement will obtain a particular outcome we cannot conclude that this implies a preexisting property of the individual quantum system, only that if we perform that measurement in that physical system we will obtain that outcome with certainty.*

Yes, and I think this realization has the potential to lead us down a profound path. (But that is only a dream at the moment; it is not science yet.) Anyway, it has in part led to my newfound attraction to the philosophy of William James, which you can also read about in the same document. Maybe the best notes are the ones to Wiseman, starting on page 210.

There is much to do to put some technical flesh on these little observations and to follow their implications unflinchingly wherever they lead. I am flattered that you have taken an interest in these ideas, and I hope you'll help carry the torch in your own work.

23-04-03 *The Rumor Mill* (to S. J. van Enk)

van Enkism 3: *But I read several chapters in Dennett's book* Freedom Evolves*.*

I have read parts of his (early) book *Elbow Room* (also on free will), in which I suspect he makes the same argument. He certainly tried to draw the same conclusion. But I should read this other book; I've never heard of it before. Dennett is always good to read. And he's an impressive speaker too.

25-04-03 *Infinity* (to S. J. van Enk)

"Jesus love me, this I know, because the Bible tells me so."

"Separable Hilbert space only so, because von Neumann told me so."

There's probably a deeper answer, but I don't know it. It probably just means that the mathematicians had too many counter-examples to what they wanted to be true in nonseparable Hilbert spaces. Physicists, never worrying about such matters, then just fell in line.

And yes, the answer to 2) is the same as the answer to 1).

I know that's of little help, but I'm in an airport again.

Steven's Preply

> I have 2 questions about infinite Hilbert spaces: I hope you find some time to answer my no doubt simple questions:
>
> 1) I remember that you once said that one always assumes Hilbert spaces to be separable, which if I remember correctly for infinite Hilbert spaces means their dimension is countably infinite. Is that so? Why does one assume that?
>
> 2) For our laser paper we made use of a trick from a paper by Blow and Loudon et al.: starting from a continuum of modes, characterized by a continuous frequency ω, one goes to a discrete set of modes by first defining a complete discrete set of functions of time (or frequency), and subsequently constructing a discrete set of modes. That always looked convincing to me, but why is that allowed? Why doesn't it matter that we go from an uncountable to a countable set of modes? [Maybe this has the same answer as 1)!]

26-04-03 *Vancouver Books* (to P. C. E. Stamp)

Are you serious, "Vancouver has no good used bookstores downtown"?!?! I had a field day! I'm still downtown, in fact, having a pint. Let me tell you what I found:

- Arcanum Books, 317A Cambie St.

  Tom Burke, *Dewey's New Logic: A Reply to Russell*, $12.00.

- Albion Books, 523 Richards St.

  Paul Feyerabend, *Killing Time: The Autobiography of Paul Feyerabend*, $10.00.

- MacLeod's Books, 455 West Pender St.

  W. V. Quine, *Ontological Relativity and Other Essays*, $8.00.

  Michel Foucault, *The Archeology of Knowledge*, $15.00.

  William James, *The Letters of William James*, 1926 edition, $25.00.

  and the real catch of the day

  William James, *The Correspondence of William James, Volume 7: 1890–1894*, $45.00.

That amounts the best book-buying day I've had in a while. (You can see I like to record things.)

Thanks again for inviting me to this meeting. I think much did come out of it for my quantum foundations program, and I am grateful.

27-04-03 *Bull by the Horns* (to R. W. Spekkens)

I just wanted to tell you again how excited your letter has gotten me. This sounds like a real progress in our understanding! Have a look at the note I wrote Sam Braunstein in 1996 [*Samizdat*, page 434] ... or better the 24 April 1998 letter to Rüdiger Schack on page 390. [I think I also put some words like that in `quant-ph/9601025` ... Aha, yes I did; see page 25 of it.] I may have sloganized the idea, but you took the bull by the horns! I must say, *I am jealous*!! But I am also proud of you in this regard: You should have rightly been my postdoc this year, not Perimeter's!

Let me go through a few points of your letter for clarification and a couple of suggestions.

Spekkensism 14: *I've been busily trying to finish up a bunch of projects, and get settled in here. In my spare time, I've been working on a toy theory (Lucien has also been looking at something very similar) that I think strengthens the argument for the epistemic view. Basically, it's classical probability theory with an additional constraint: that the amount one knows about a system must always equal the amount one doesn't know about the system when one is in a state of maximal knowledge. Basically, one demands a balance between certainty and uncertainty.*

Mostly I'm wondering how to interpret this precisely. Presumably, you're starting off with a fixed probability simplex over some number, K, of outcomes. Are you explicitly taking K to be a perfect square as it would be in quantum mechanics? Alternatively, if you're not: I wonder what special properties a perfect square has in this regard?

More importantly though, it sounds like the arena for your considerations is that the "states" ought to correspond to a convex region within the simplex. The states of maximal knowledge are then the region's extreme points. Is that right, or do you mean something else? For instance, I could imagine that you might be thinking of the boundary rather than the extreme points ... though that is less likely.

Now, what is the nature of the constraint that defines your states of maximal knowledge? Are you saying that for an event space of size K, the Shannon entropy of a state of maximal knowledge ought to be $\log \frac{K}{2}$ bits? Or do you have a different way to quantify the idea.

In either case, let me tell you about the move I would make at this point. The tack is look back at quantum mechanics, see what it says about states of maximal information in this language, and then try to make sense of it as a very simple statement, like in your toy model. What I'm thinking of in particular, is to think of the allowed region on the simplex as generated by a "standard quantum measurement" of the SIC-POVM variety, i.e., the symmetric informationally complete POVMs. You recall these from Montréal, right? In case not, I'll attach a note from another collaboration of mine that gives some relevant definitions.

What is most interesting about that POVM in this context, is that it causes all the pure states to live on a sphere surrounding the center of the simplex (the maximally mixed state). By this, I mean in terms of usual Euclidean distance. You'll see this in Eq. (13) of the document I'm sending you.

In particular, it is not the case that all pure states have an equal Shannon entropy in this representation (if that indeed is the assumption of your toy model). Rather they have constant, but not maximal, distance from the state of maximal ignorance. What is the meaning of that? I don't know in any detail, but clearly it is one possible statement that maximal information cannot be complete.

Anyway, question: Making the assumption of "constant distance from center" rather than your assumption of "balance between certainty and uncertainty", how much closer still (if any) does one get to the full theory of quantum mechanics?

I know that it will not go the whole way to quantum mechanics, either. This is simply because true pure quantum states do not make completely fill the surface of the sphere that they live on. So, we're still missing something.

But mostly I'm intrigued in what you've already found with the following:

Spekkensism 15: *such as the impossibility of achieving disturbance-free information gain about non-orthogonal states with certainty, [...] The toy theory includes all these phenomena.*

My immediate reaction when reading your note was that this could not be right, but upon a little thought, I might be willing to buy it. The point is, there can be no absolutely clean probes in such a theory. Eve has to always have a kind of ignorance about her probe when it is quantum too. Following the algebra of the assumption (i.e., that system+probe state must also be a state of maximal information) must always leave both states more mixed (with respect to the standard quantum measurement) than they started off being. Is that the sort of thing you're finding?

Spekkensism 16: *I'm far from done, but I'm sending you a tiny section that addresses Penrose's argument against the epistemic view. I suspect that you will disagree with my response, but, if so, I'd like to hear your objections.*

Yeah, you're right: I enjoyed half the argument. I was trying to capture something similar at the meeting in Montréal, in that I kept trying to dream of a way in which a von Neumann measurement could always be viewed as a "coarse-grained measurement" (the words I was using then) rather than the fine-grained variety everyone always assumes it to be. But I could never quite pull the analogy together in a satisfactory way. What you are talking about here is something of the same flavor ... right before you fall into the quagmire of "ontic significance."

Anyway, you're doing great work. Can't wait to see you in Maryland.

Rob's Preply

> I've been busily trying to finish up a bunch of projects, and get settled in here. In my spare time, I've been working on a toy theory (Lucien has also been looking at something very similar) that I think strengthens the argument for the epistemic view. Basically, it's classical probability theory with an additional constraint: that the amount one knows about a system must always equal the amount one doesn't know about the system when one is in a state of maximal knowledge. Basically, one demands a balance between certainty and uncertainty. As you know, classical probability theory includes a whole bunch of phenomena typically deemed quantum, such as no-cloning, steering, no exponential divergence of states under chaotic evolution, and all the others from my checklist in Montréal, but excludes many other phenomena, such as the impossibility of achieving disturbance-free information gain about non-orthogonal states with certainty, the existence of sets of states that cannot be broadcast, the impossibility of multi-party steering (a.k.a. the monogamy of entanglement), the fact that there are many extremal convex decompositions of a state, the non-commutativity of measurements, the existence of interference, the impossibility of teleporting non-orthogonal states with

certainty in the absence of perfect correlation, the existence of locally immeasurable product bases, the existence of unextendible product bases, and, of course, all the quantum information processing tasks that can be built on these. The toy theory includes all these phenomena. The phenomena that don't arise in the toy theory are things like contextuality, Bell correlations, the fact that there are POVMs that are not convex combinations of PVMs, the gap between the probability of discriminating non-orthogonal states using an optimal measurement and using an optimal unambiguous measurement, and the fact that 2 levels of a 3 level system behave like a 2 level system. (These lists aren't meant to be exhaustive – they only include some of the things I've had a chance to look at) So, it seems to me that the toy theory makes about half of the mysteries of quantum mechanics intuitive. I'm hoping that there is an additional constraint that can be imposed that gets the rest of it. I have some ideas on that, but it's all very speculative.

I'll be talking about it in Washington. Will you be there? I'm also trying to write it all up. I'm far from done, but I'm sending you a tiny section that addresses Penrose's argument against the epistemic view. I suspect that you will disagree with my response, but, if so, I'd like to hear your objections.

27-04-03 *Delighted to Meet You* (to G. Valente)

Thank you for your kind letter. It is always very flattering to know that someone is reading your papers, but it's absolutely dazzling to know that someone is enjoying and, perhaps, getting something useful from them!

Concerning your query about whether I can enter into an extended discussion: Unfortunately, you've hit me at a moment of almost complete correspondence blackout. With all the stresses added by my recent move to Ireland, several of my collaborators beating down on me to finish our incomplete papers, and a very busy travel schedule at the moment, I have had to essentially shut down my email production. (Believe me, that is a very painful thing!) Of course, please feel free to write me anything you wish, but it may be quite some time before I am able to reply in detail.

However, I have two suggestions for a substitute:

1) Chris Timpson, a recent graduate of Harvey Brown, understands the quantum foundations program I'm pushing quite well. If you don't know him already, he'd be excellent to talk to.

2) I very much encourage you to attend a meeting on quantum foundations in Sweden that I am co-organizing this Spring. The dates are June 1–6, I believe, and you can find information about it here:

`http://web.archive.org/web/20030425003339/http://www.msi.vxu.se/aktuellt/konferens/`.

Beyond the attendees listed on that page, Barnum and I are also organizing a separate session on "Quantum Logic meets Quantum Information" which will be attended by:

Scott Aaronson	(U. California at Berkeley, USA)
Paul Busch	(U. Nottingham, UK)
Bob Coecke	(Oxford U., UK)

Richard Greechie	(Louisiana Tech U., USA)
Hans Halvorson	(Princeton U., USA)
Lucien Hardy	(Perimeter Inst., Canada)
Rüdiger Schack	(Royal Hollaway, UK)
John Smolin	(IBM Research, USA)
Alexander Wilce	(Susquehanna U., USA)
Guido Bacciagaluppi	(U. California at Berkeley, USA)
Rob Spekkens	(Perimeter Inst., Canada)

Thus you will have plenty of interesting people to talk to, some of which have tendencies in our same direction, but also I will have plenty of time to talk to you when Barnum, Hardy, Smolin and I inevitably go to the pubs in the evening. (It is a usual thing; and, within that, usually a very philosophical time.)

Giovanni's Preply, "Touchable and Untouchable in QM"

I'm an Italian student from Padua University. My field of work is philosophy of physics and nowadays I'm in Oxford to write my dissertation. In particular I'm dealing with the question (maybe a bad question, in a Wittgensteinian sense) of realism in Quantum Mechanics. I share a skeptical point of view about the possibility to directly touch or to understand an ontological level and I also consider the probabilistic structure of QM as impossible to disregard. Thus, after our first discussion, Dr. Harvey Brown, my supervisor here, asked me if I knew Chris Fuchs' works and I professed my ignorance of it (I'm sorry, sir…): he really looked amused by my convergence at something unknown (for me). Following his advices, I started reading your papers ("Quantum Mechanics as Quantum Information (and only a little more)") and "eavesdropping" your private correspondence ("Quantum States: What the Hell Are They?"). I found your approach very deep, consistent and, most importantly, completely *immanent* to QM. I think we can't forget we are *disentangling* a physical theory; our aim is a reflection over the foundations of quantum mechanics. If one adds further elements (and nothing prevents to do this!), one must be aware of the fact that one is no more doing QM or philosophy *of* QM (but something else!). Physics can be regarded as a linguistic game (Wittgenstein): a tangle of mathematical, experimental, syntactical and hypothetical ropes, where the empirically successful is the premise and the goal at the same time. Our task should be to see deeply *inside* the theory and clarify *its* nature. Philosophy is "only": to play this game with the consciousness that it is a game. Science could be just a working fiction, why not? I deem your works are very eye-opening in this sense, especially for QM. I'd like to explore this track and I hope you'll agree to answer me about some specific doubts I have.

In any case I've been appreciating your perspective and your way of reasoning. Furthermore, I find quite amazing (and sometimes annoying) to discover such a similarity between my still raw view and your articulate and solid plan. I must reveal that sometimes I feel frightened. For example. I woke up thinking about our alchemistic touch to the nature yesterday morning. As soon as I started studying, I read the same idea (alchemy) in your first statement about Pauli. I closed my books and I run away to phone my mum in Vicenza …

Thank you very much for your willingness

P.S. I love Arthur Schopenhauer, too. He is a very deep philosopher. I think his thought was splendidly understood and embodied by Ettore Majorana. In fact he mystically (more than mysteriously) disappeared ...

30-04-03 *Screed* (to A. Wilce)

screed:
noun,
1. long usually tiresome letter or harangue.
2. layer of cement etc. applied to level a surface.

Glad to hear you'll be in Sweden! And can't wait to hear about test spaces! (But finite dimensional ones, please ... if that concept applies.)

01-05-03 *Screed Again* (to H. Barnum)

I'm finally reading Hacking's book *Representing and Intervening*, per your, and Chris Timpson's, and Rob Spekkens's, and Steven Savitt's suggestions. It reads like eating candy; very nice. Just in the interstices of today's flight when I wasn't working on the present paper with Renes and Co., I managed to read 67 pages. That's a phenomenal amount for me! I generally read much slower than that.

05-05-03 *Pitowsky* (to T. Rudolph)

Rudolphism 8: *Did you ever ask Itamar about your "shape of the q-states within the simplex" problem?*

I just remembered that when I first met him at ESI all those years ago (back when we both were still young) he told me he was working on some aspects of a problem to do with parameterising the shape of the convex set of q-states (though not on the same simplex you are I don't think).

He also knows a lot of the math literature, so could probably guide you to anything that has been done ...

No, I haven't. That's a good idea, and I'll follow through with it. I know which convex set you're talking about with earlier work, and, you're right, it's not the same one I'm thinking about. But he'd definitely be a good source of techniques that may be relevant for the present problem.

I decided not to come to Bell Labs today. I'm just too exhausted; maybe a day's recuperation is what I need most. I'll probably be in tomorrow. I very much enjoyed this weekend's discussions, by the way.

05-05-03 *On Bohr and Realism-X* (to R. W. Spekkens)

Attached is my compendium-under-construction, "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature." To find the best reading on Bohr's particular flavor of realism, look at the Barad and Folse areas of the compendium.

I have read Bohr thoroughly, and I have read Folse thoroughly, and I think no one does a better job of rationally explicating Bohr than Folse. Tell me if you disagree.

10-05-03 *Departing for Ireland* (to C. H. Bennett)

A small note to tell you that the information I ate for lunch was just great! You ought to try the place; it's a nice little cafe outside the Aer Lingus lounge in JFK. The lunchtime special was "all you can eat, $5.99." They brought out the NY Times for desert, but I was absolutely stuffed by then.

Happy birthday again!

17-05-03 *Good News, Bad News* (to J. W. Nicholson)

Thanks for the good news; too bad about the bad news. The good news got me percolating ... since I watched an episode of *Scrubs* the other night, and they explained what *always* happens on a third date! If you can't trust TV, who can you trust?

About the bad news, for some reason it took my mind to a story I once heard from John Wheeler about a physicist who was to be executed—I can't remember—either sometime in WWII or during the Stalinist purge. Anyway, the fellow was put in a death-row cell one evening, with the execution scheduled for the next morning. When morning time came, for whatever reason he was set free. Years later, Wheeler asked him how he survived the night. How could he manage with that horrible dread all night long? The man told him, "I found myself thinking over and over about Hamilton's beautiful equations. It set my mind at ease."

I suppose everyone has to have a Hail Mary in some form or another.

22-05-03 *Your Paper, Hope You Can Come* (to P. G. L. Mana)

Chris King pointed out your paper `quant-ph/0305117` to me, and I downloaded it today. It looks quite nice. I have a significant interest in characterizing quantum mechanical state spaces as portions of a probability simplex (or simplexes), and I'd like to see the prettiest way to do that. It's my hope that your paper will shed some light on the problem.

As for my own efforts in that direction, please let me recommend Sections 4.2 and 6 in my paper, "Quantum Mechanics as Quantum Information (and only a little more)", `quant-ph/0205039`. There I don't use a table, but a vector representation for the outcomes of a single, sufficiently rich measurement. So, the representation is different, but some of the ideas are the same. Ultimately what I would like to do is divorce the representation away from quantum mechanics, and then start over (in the manner of Hardy and you) and recover the quantum formalism as the result of further physically-significant requirements ... but that is a project still in the making.

Anyway, I write to you mostly because I would love to get the chance to talk to you in person and, reciprocally, give you an opportunity to work with several people interested in problems of this ilk. I am one of the organizers of a meeting in Växjö, Sweden, to be held June 1 through 6, on quantum foundations. You can find out information about the meeting at this website:

`http://web.archive.org/web/20050427164645/http://www.msi.vxu.se/aktuellt/konferens/`.

As I say, there will be several people there that I know will be interested in your work. I'm thinking in particular of Howard Barnum, Lucien Hardy, Joe Renes, Rüdiger Schack, and Rob Spekkens. It would be wonderful if you could come. Seeing that you are stationed so close at the KTH, perhaps our chances are better than zero to meet you!

24-05-03 *Mechanica Quantica Lex Cogitationis Est* (to H. Barnum)

Barnumism 4: *Do you know the original source of the words "quantum mechanics is a law of thought?" Is it you, JAW, or someone else?*

It was me. I guess I started using the phrase early in 1996. You can find the story on pages 247 and 370–371 of my samizdat *Notes on a Paulian Idea*.

24-05-03 *Maybe Do Give a Talk ...* (to J. M. Renes)

I just got the note below from Andrei. It got me thinking about your talk again. Maybe it would be a good idea for you to give a talk after all. I told Andrei to go ahead and set aside the time. I think it would be good to talk about the trine and the Platonic solids in relation to Gleason. It could be good to refresh everyone's memory on this. Also you could speculate on what might be the higher dimensional analogues of this. Maybe your talk would prod others to think about these issues and — even if we're not going to work on it — induce them to do something interesting in that direction.

Have you ever thought about how a POVM-version of Gleason where the frame functions are only defined on the extreme points of the POVMs. That strikes me as an interesting arena of investigation. Since such generalized frame functions include the ones from the standard Gleason theorem, the project has to work. But I wonder to what extent the proof might get simplified in this way? Also, the skeptics would not be able to claim that we're overtly assuming the linear structure by going into the interior of the convex set. You might also propose something like this in a talk.

26-05-03 *Practice?* (to P. G. L. Mana)

Thanks for the invitation to Stockholm. However, at the present time my travel schedule is already over-crowded, and I will be flying, both literally and figuratively, in and out of the meeting. Maybe I could visit sometime in the coming year, when I am making my way to Växjö again. I have also wanted to meet Göran Lindblad for many years.

It's great to hear that you'll be at our meeting next week. You've probably already guessed that we won't be able to provide any further support for participants ... but I wonder if you would be interested in giving a talk on your paper nevertheless? One of the speakers from Barnum's and my part of the meeting (Hans Halvorson) had to drop out because his wife had her baby early; so we've got a little extra space for a speaker. If you're interested, let me know immediately. I cannot promise that the time will not already be gobbled up, but the sooner you let me know, the better chance you'll have if you're interested.

If you're a first year graduate student, it could be good practice. Just remember to keep it simple, focusing more on the ideas and motivation, and far, far less on any derivations. Less is always more in a talk. Also it would be quite interesting to see the contrast between your ideas and Hardy's.

27-05-03 *Popper Seminar, with a Side of Oxford* (to S. Hartmann & J. Butterfield)

Thank you both for the invitations. I would be delighted to come give talks at LSE and Oxford. Presently my schedule is completely open in February. So why don't you two decide when you want me, and for how long you want me, and I will put the dates on my calendar.

The papa Bayesian of our group—Rüdiger Schack—by the way, was a student of Schenzle too.

27-05-03 *Got It: Thanks!* (to O. Hirota)

Thanks so much for sending a copy of the book *The Foundation of Quantum Information Science*!! Are you the author of the whole book? Or is it a compilation of many contributions? In any case, it looks very impressive.

I found my picture on page 214, as you had suggested. Now, please translate what it says! Do the four characters below my picture signify the word "Fuchs"? (Or fox, since fuchs means fox in German?)

It was an interesting coincidence that I would get your book today. Just this morning, the secretary in Sweden told me that she mailed off several copies of my samizdat *Notes on a Paulian Idea*. You were in the list of recipients. So it should be arriving soon.

Things are going well for quantumness here in Ireland. Chris King and Koenraad Audenaert were able to prove that the accessible fidelity is a multiplicative function over tensor product ensembles. They are presently writing a paper on the subject. This had been one of the open questions Sasaki and I listed at the end of our paper. What is especially pleasing about this new result is that it shows a very deep connection between quantumness and capacity questions for entanglement breaking channels.

29-05-03 *See You Soon* (to A. Wilce)

10.35–11.15. Alexander Wilce (Susquehanna Univ., USA)
Compactness and Symmetry in Quantum Logic
plus up to ten minutes for questions.

See you soon.

09-06-03 *Quantumness* (to P. Busch)

Thanks for coming to the conference! Your presence really made a difference, especially for Rüdiger and me.

Let me give you a pointer to the background of the problem I mentioned on the last day. It is:

- C. A. Fuchs and M. Sasaki, "Squeezing Quantum Information through a Classical Channel: Measuring the 'Quantumness' of a Set of Quantum States," *Quantum Information and Computation* **3**(5), 377–404 (2003). `quant-ph/0302092`.

After some massaging, the quantity in Eq. (16) there can be turned into the quantity I showed you, i.e., the $\inf \mathrm{tr} X$. Of the open questions in the back, numbers 1, 2, and 6 have now been solved. Koenraad Audenaert, Chris King, Peter Shor, and I will be writing up all that soon. Question 6 especially required some nontrivial mathematics, and led to the expression I showed you.

The biggest problem on my mind at the moment is to show (or find a counterexample) that an ensemble of at least of d^2 states is required to achieve the quantumness of a Hilbert space. That is, if an ensemble consists of $d^2 - 1$ states, then the quantumness of that ensemble is always bounded away from the quantumness of the Hilbert space. King and I mostly, are working on that at the moment, but if you have any insight I'd love to hear it. With you being a B, and he and I being a K and an F, you'd be assured to be first author on the paper!

10-06-03 *Quantum Information* (to T. Siegfried)

We met briefly years ago at a meeting on quantum information and computing, but I doubt you remember me particularly. Anyway, I've been meaning to write you for some time to tell you about a neat little coincidence I ran across as I was proofreading my samizdat *Notes on a Paulian Idea: Foundational, Historical, Anecdotal & Forward-Looking Thoughts on the Quantum* for a slightly more official publication by Växjö University Press.

In the foreword to the book David Mermin writes, "If Chris Fuchs (rhymes with 'books') did not exist ...". On the other hand, in your book *The Bit and the Pendulum*, there is some point where you write, "Chris Fuchs (rhymes with books) applied Landauer's principle ...". In fact I actually report your line in my samizdat, but I doubt David was aware of it, as even I had forgotten of its existence until the final editing. (I guess I say "Fuchs rhymes with books" to a lot of people.)

In any case, all this brings to mind that maybe you'd like a copy of the book. Here and there you might find a story in it that you'd enjoy. If you'd like, I can send you a copy: Just give me your snail-mail address. You can also find the book in PDF format at my webpage (link below), but you wouldn't want to actually print it out. (That version is about 500 pages.) Have a browse of the document, and if you think you'd enjoy a hard copy, I'll have the VUP secretary send you one.

10-06-03 *Too Late, but Sympathetic* (to R. D. Gill)

I wanted to apologize to you for not getting my act together in time to write a few words about your paper "On Quantum Statistical Inference" with Barndorff-Nielsen and Jupp for the Royal Statistical Society. (They had invited me to, but gave a deadline of June 4; now I am away to conferences again until June 14.)

I wanted to particularly endorse your statement in Section 7, "As statisticians, we would like to argue (tongue in cheek) that quantum probability is merely a special case of classical statistics," and write a few comments thereabouts. I think one shouldn't be so tongue in cheek about this.

Anyway, as I say, I'm sorry I passed the deadline.

Too bad you didn't come to the Växjö meeting: I think you would have found it quite a bit more productive than last year's. Several of the talks, in fact, addressed your tongue-in-cheek comment above: Guido Bacciagaluppi, Paul Busch, Lucien Hardy, Luca Mana, Rüdiger Schack, Rob Spekkens, and also my own—all in quite a direct way.

10-06-03 *Orthomodular Lattices* (to S. L. Braunstein)

Maybe suggest to your friend R. I. G. Hughes's book *The Structure and Interpretation of Quantum Mechanics*. It talks quite a bit about quantum logic from several perspectives, and it also will give him loads of references if he wants to dig deeper.

10-06-03 *Notes on a Paulian Idea* (to C. P. Enz)

I don't know if you remember me: You had very kindly sent and then re-sent me copies of all your quantum foundational papers—the latter after my house burnt down in the Los Alamos fires three years ago.

In any case, I'm writing to tell you that my book *Notes on a Paulian Idea: Foundational, Historical, Anecdotal & Forward-Looking Thoughts on the Quantum* has just been published by Växjö University Press. The book is very strongly laced with the influence of Wolfgang Pauli's ideas (to which the title refers), and is legitimized a little with a foreword by N. David Mermin. If you have an interest in reading (parts of) it, I will send you a copy. All I need is your mailing address.

If you are dubious, before making a decision, you may also have a browse through the book by downloading a copy from my website (link below). Also there, you can find various of my other writings along with a Curriculum Vitae.

Another project that I am working very hard to complete this year is a large document to be titled "The Activating Observer: Resource Material for a Paulian/Wheelerish Conception of Nature". Basically it is a compilation of quite a large number of references, along with extensive quotations from most. Presently it stands at 109 printed pages, with 443 references. By the time the project is complete, I expect it to run about 250 pages: I still have a significant amount of material to put into place and have decided to enlist a good scanner along with optical character recognition software to help me in the project. That document is not ready to be downloaded from the website, but you can read its abstract there. If you would like a preview of the project, I could mail that to you directly and perhaps you could give me a few pointers for things I've missed. For instance, I just learned last week that you yourself have just published a biography of Pauli! I look forward to reading that as soon as I can purchase a copy.

11-06-03 *Getting the Word Out* (to R. W. Spekkens)

You know sir, I continue to be excited by your toy model even if we differ in our ultimate goals. I had a conversation with John Preskill about it today, showing him the main ideas, etc. He seemed intrigued. In fact, intrigued to the point that he wants to invite you to Caltech to talk about it. I think this would be a great opportunity. I told John to just try to pick a time that I could make it too. So, a) I hope you'll go, and b) I'd like to be there so that I don't miss a thing. Can we coordinate? I've always gauged the value of the program by the caliber of the people who start out skeptical about it, but then ultimately take note. Early on I singled out both Mermin and Preskill on this count, i.e., as markers of progress. Mermin yielded quickly, but Preskill has been a bit of a holdout. So I'm particularly keen to see if we, through our combined efforts, can finally tip the scales.

While I'm on it, let me tell you about another strategical point that's been on my mind lately. When I first contacted Luca Mana about liking his paper, he wrote me back the following:

Manalogue 1: *Thank you so much for your letter, and for the words of appreciation for my paper. It is just a little more than a draft, and so its form is far from being nice, and many are the missing references. I already knew "Quantum Mechanics as Quantum Information (and only a little more)"; indeed, your idea, expressed there, that time evolution could be a density operator, stimulated (via Rodriguez' "Unreal probabilities",* `physics/9808010`*) some of the research that led to my paper. ...*

(So I hope you understand how honoured, and a little embarrassed, I feel for having received your letter!)

Yet one doesn't find a citation to any of my papers there. It could be that he was just being nice and not quite meaning it. However, if there really was an influence on his work, then his lack of a citation is a bit harmful. If I am going to be able to keep getting funding for conferences and drawing attention to our collective efforts, it'd certainly help if the larger community sees that my attempts to write and communicate and inspire are part of the active process leading to the (significantly more substantial) results of you younger fellows.

I guess all I am saying is, if it influenced you at all toward your toy model, I hope you will not forget to cite `quant-ph/9601025` and `quant-ph/0205039` in your write-up. (Sample below.)

From `quant-ph/9601025`:

> Formally, one says that in classical physics, maximal information is complete, but in quantum physics, it is not. What should we demand of a physical theory in which maximal information is not complete? Maximal information is a state of knowledge; the Bayesian view says that one must assign probabilities based on the maximal information. Classical physics is an example of the special case in which all the resulting probabilities predict unique measurement results; i.e., maximal information is complete. In a theory where maximal information is not complete, the probabilities one assigns on the basis of maximal information are probabilities for answers to questions one might address to the system, but whose outcomes are not necessarily predictable (some outcomes must be unpredictable, else the maximal information becomes complete). This implies that the possible outcomes cannot correspond to actualities, existing objectively prior to asking the question; otherwise, how could one be said to have maximal information? Furthermore, the theory must provide a rule for assigning probabilities to *all* such questions; otherwise, how could the theory itself be complete? Quantum physics is consistent with these demands. A more ambitious program would investigate whether the quantum rule is the *unique* rule for assigning probabilities in situations where maximal information is not complete. You won't be surprised to learn that we don't know how to make progress on this ambitious program.

16-06-03 *Exhaustion, Depression, Stagnation, Integration* (to G. L. Comer)

The world is a big world. And our lives are ever such a small part of it. And ever such a big part of it. But regardless, there are limits. I feel I'm not prepared for this. I tell Emma all the time that she is destined for greatness; she will do important things.

18-06-03 *Spiders, Ants, and Honey Bees* (to P. W. Shor)

I'll match your Morris Kline with a Francis Bacon:

> The men of experiment are like the ant; they only collect and use; the reasoners resemble spiders, who make cobwebs out of their own substance. But the bee takes a middle course; it gathers material from the flowers of the garden and the field, but transforms and digests it by a power of its own. Not unlike this is the true business of philosophy for it neither relies solely or chiefly on the powers of the mind, nor does it take the

> matter which it gathers from natural history and mechanical experiments and lay it up in the memory whole, as it finds it; but lays it up in the understanding altered and digested.

For better or for worse, this is the way I see this sensation I'm trying to instill into our community, "quantum foundations in the light of quantum information": I see it as a honey bee.

Peter's Preply

I just googled it.

> The developments in this century bearing on the foundations of mathematics are best summarized in a story. On the banks of the Rhine, a beautiful castle has been standing for centuries. In the cellar of the castle, an intricate network of webbing had been constructed by industrious spiders who lived there. One day a strong wind sprung up and destroyed the web. Frantically the spiders worked to repair the damage. They thought it was their webbing that was holding up the castle.

Reference given: Morris Kline, *Mathematics: the Laws of Certainty*, Oxford Univ. Press, NY, 1984.
It seems to me that this also applies to the philosophy of the foundations of physics.

19-06-03 *Lurch* (to J. M. Renes)

Renesism 9: *PS in regards to some quantum operations are subjective, is the subjectivity simply the case of assigning a state to the measurement apparatus? This is how Carl put it, and it clears up a lot of the confusion in my mind, but perhaps this isn't precisely the case.*

No. That way of putting it trivializes the point. For it makes it look as if *all* the subjectivity (in the Bayesian sense) is in the state assignment to the joint system of object + measuring device — a concept almost straight from the Tibetan Book of the Larger Hilbert Space. Then one can go around blithely thinking of the interaction Hamiltonian (+ preferred basis, so that the Hamiltonian cannot be transformed away) as objective components of the theory, well protected from the nasty subjectivity of the state. I.e., one can think of these things as classical matters of fact, independent of any agent's belief.

What I want is to go beyond that: And Carl knows that. All operations are subjective, I say, including the unitary derived from the interaction Hamiltonian above. I give fifteen arguments for this, none of which is conclusive, but in sum weighty enough for me to be willing to take the leap. Quantum foundations needs more leaps than baby steps, it seems to me, if we're going to see any real progress in our lifetimes.

19-06-03 *Guidelines* (to M. Pérez-Suárez)

From the last letter you know about my final decision with regard to Benasque. Just a word of warning: If you are allowed to attend, and you give a talk, do it on a technical subject and keep the philosophical remarks to a minimum. We will—with these ideas that you and I are attracted to—change the world. But one has to choose one's battles wisely, and there is something to be said for easing the community into the mindset at a rate slow enough for the resistance to be smaller rather than larger. The community attending the Benasque conference, for the most part, only cares about technical meat: If a foundational program makes no impact on that hunger, then it will be seen as no program at all.

Pérez-Suárezism 1: *Well, I have been to the [Niels Bohr Archive] and, of course, I asked about the William James story. I saw the transcripts of that last interview and I can tell you that things went as I remembered from my readings some years ago. There is no indication of any technical problem during the interview and Bohr's answers seem to have been completely tape-recorded with no remark in the transcript of any disturbance. You can find the content of the answers in several well-known books. Although I haven't consulted my references yet, if you want me to tell you where you can find it, I have no problem to do some research in my library (it is not as big as Howard's, I guess). By the way, I think that perhaps it can be found in Folse's book on Bohr's notion of Complementarity.*

Then the transcripts must be missing the nuance. Here is what Henry Folse himself told me:

Folsesm 21: *As you know, Bohr died after the 6th interview; 20 were originally projected. Jordan's are in German, and a few others; de Broglie in French, of course. However, there are holes in the transcripts where the transcriber couldn't make the tapes out clearly enough. (the tapes in Copenhagen are old reel to reel tapes and require an old machine to play; I don't know if the American centers have them on cassettes). Actually one such occurs when Bohr was talking about his admiration for William James. Just at that moment the noon air raid siren blew, more or less obliterating about 30 seconds.*

Of course I embellished a little in our pub discussion, but I bet they are nevertheless an interesting 30 seconds.

Pérez-Suárezism 2: *I have downloaded Richard Jeffrey's book on probability. I have browsed through it and found it seems to be very readable (and quite funny, too). I also have the recently published CUP edition of Jaynes' book, and Rüdiger told me that Savage's book on probability was quite interesting, too, so I will be purchasing a copy. Could you give me some advice on what I should pay more attention to first?*

Savage and Jeffrey, I would say.

Pérez-Suárezism 3: *P. S.: I have found some typos (to be true, of no real significance) in your paper. If you want a list of them, just let me know.*

Absolutely! I'd greatly appreciate that.

20-06-03 *Very Late Reply* (to S. Weinstein)

I apologize for the very late reply: It was just that your email hit me at a very bad time when I was in a complete email-shutdown mode.

I think your first question may refer to a problem that still has not been resolved to complete satisfaction. Unfortunately I've never taken too much note of the issue, so I don't have a strong opinion myself. I do know of Ozawa's work [M. Ozawa, "Quantum Nondemolition Monitoring of Universal Quantum Computers," Phys. Rev. Lett. **80**, 631 (1998)], but I remember hearing from Richard Jozsa and ?? Fahmy last year, that Ozawa's paper had a flaw in it. Perhaps it would be best for you to write Richard yourself; maybe he'll explain what he was talking about and give some references. I wouldn't mind hearing the answer. Richard's e-address is `rjozsa@cs.bris.ac.uk`.

Concerning your second question:

Weinsteinism 1: *I think we talked briefly about the "little more" that must be added to information-theoretic postulates in order to get quantum mechanics. It strikes me that one of the key elements of quantum theory which is not captured (at least in any explicit way) by the information-theoretic approach is that Planck's constant, a new constant of nature which has the dimension of action, or angular momentum, plays a role. Why action? Why not some other physical quantity? Classically, action is associated with dynamics via the principle of extremal action. In quantum mechanics, however, action plays a kinematical role as well. What is that telling us?*

You're certainly right. Looking solely at the *structure* of finite dimensional quantum mechanics, one never sees Planck's constant at all. Where it comes from, I don't know. Nor do I know if information-theoretic considerations will ever tell us much about its origin. A dull answer, right? I wish I could tell you more.

Would you, by the way, like a copy of my samizdat *Notes on a Paulian Idea*; it's just published and I have a number of copies for giving away. Mostly, though, I'm trying to target people who might be interested (and won't throw it away immediately). If you'd like a copy send me your mailing address.

20-06-03 *Logical Probability and That* (to J. M. Renes)

I think I agreed with much of your note. That was the upshot of last week's (month's?) discussions. To answer your final question (about Cox), I guess I don't have a strong opinion ... at the moment anyway. Do any of the others in the discussion list have a strong opinion? Mainly I never found Cox nearly as compelling as Jaynes did. (Well, I did for a short while, but then I became less than enamored.) I think one could still accept Cox's derivation of the probability calculus—even though I'm not so inclined (already said that)—without accepting Jaynes' further dogma about some priors being compelled by "THE information". That's because Cox's derivation has nothing to do with priors.

Did you forget an "all" in the title?

Joe's Preply

> These arguments are already beginning to leak out of my mind, so I'd better commit them to paper or email to help reinforce them.
>
> Let me see if I have things straight. Let's call "Bayesians" those who don't believe in objective probability, logical Bayesians those who think from background information a unique prior logically follows, and subjective Bayesians those who think that background knowledge does not determine for each subject (person/observer), a prior probability. The two approaches do share the normative aspect that Bayes' rule is the correct way to update knowledge given data. Are these fair assessments? Of course, it's not fair that the subjectivists are somehow defined as illogical in opposition to the logical Bayesians, but you know what I mean.
>
> The argument against logical Bayesian probability I picked up at the conference I think goes like this. Let's specialize to the case of picking "ignorance priors" and recall how this is meant to work in the logical probability framework. The background knowledge B gives no information about one outcome over or under another. Thus we study the symmetry group of the outcomes a_j and our prior must be invariant under this group. Presto! the symmetry group of the outcomes determines the prior probability.

But this doesn't represent an implication from propositions B to probabilities $p(a_j|B)$ because to say that the outcomes have some kind of symmetry is to say that some function on them is invariant under the swaps of labels (surely the swaps are always permitted, even if the case of symmetry doesn't obtain) and this function is precisely the probability function. I said "is to say" which is quite different than "implies" the logical structure of B that was to imply the uniform prior turned out to be the uniform prior itself. Put differently: You can't say "B doesn't favor any outcome over another" without it being a statement of probability. So there's no real implication going on except to say that "when you believe in the symmetry, you believe in the symmetry".

On some re-readings of the above, I've thought that it says nothing and I haven't stated the problem properly. But consider the argument made by Jaynes in chapter 2 of probability as logic: "But now suppose that information B is indifferent between propositions a_1 and a_2; i.e. if it says something about one, it says the same things about the other, and so it contains nothing that would give the robot any reason to prefer either one over the other." Later, he says, "It shows — in one particular case which can be greatly generalized — how the information given the robot can lead to definite numerical values, so that a calculation can get started." But the point of the above argument is that the "information given the robot" must be the uniform prior: the statement of symmetry *is* the uniform prior. You can't say that "whatever B says about one it says about the other" in a non-probabilistic way, which means there's no logical implication to it. That last statement is the one I like best.

I think there's a couple of additional points to make. The background knowledge B doesn't pertain directly to the outcomes a_j themselves: if they do we're talking logical probability. B, in some sense, has nothing logically to do with a_j; the point is that the connection between the background knowledge B and the a_j *is* the probability distribution, which is subjective, as it's made by a person thinking about the relationship between the two. This is just a clarification in light of the above argument, about what the background knowledge is or what it means.

The second is that perhaps my understanding so far is only crudely scratching the surface of the real argument beneath. Perhaps it's this. One way to see that B doesn't pertain directly to a_j is that it doesn't tell you how to carve up the outcome space into the a_j and only then make a judgement of indifference. In fact, the judgement of indifference is precisely this "resolution" of the outcome space. This seems to be the point of Jeffrey's problems 2 and 3 (though curiously labeled 1 and 2 in the sequence 1,1,2,4) in chapter one of *Subjective Probability*. The carving up of the outcome space into equiprobable events is a probabilistic judgement: "these particular outcomes $a_1, a_2, a_3, \ldots$ are equiprobable".

Greater question: Just because one isn't a logical Bayesian doesn't mean that one doesn't subscribe to the Cox arguments about probability as generalized logic. Jaynes as a logical Bayesian stresses the Cox view of course, but when it gets down to the logical aspects, he wants probability to contain the syllogism: $(a \Rightarrow b \ \&\ a) \Rightarrow b$ and its inverse $(a \Rightarrow b \ \&\ \sim b) \Rightarrow \sim a$, which it does, using Bayes' rule. So are there some pitfalls to being a subjective (= illogical, remember!) Bayesian and thinking of probability as generalized logic rather than coherent/consistent betting preferences that I don't immediately see?

Joe's Reply

No I didn't really mean to put "all" in the title, but not for any particular reason. Carl and I hashed out a lot of that argument (the main problem was that I didn't state it very well). He had a good way of putting it: First we have to decide if the symmetry is a belief or a fact. The symmetry is a belief, of course, and this can be seen in the arguments given. There are a few. First, the symmetry is obviously not in the system, since there are distinct outcomes. They wouldn't be distinct if there were really a symmetry. Second, the argument I was making, the symmetry condition is only stated (and can only be stated) in terms of probability. Ergo, it's a belief. Third, and think what Jeffrey is getting at in his book, the facts of the matter don't actually bear directly on the outcome space, so there can be no logical probability. You must decide what the relation is, and of course this is the probability. He uses an example of two coins. What's the probability that there's 2 heads showing? Is it 1/4 because there's only one way in four for the coins to be arranged this way, or 1/3 because there's three possibilities? The background info simply doesn't tell you (or as he says, there's no way to answer this question without making a judgment as to what is important). This is similar to the first point in that the background info doesn't "carve up" the outcome space for you.

Anyway, just wanted to get that down for the record, as it were. I find these arguments completely devastating to logical probability, and I agree that one can still follow the Cox approach (I've not done a heap of thinking on this last point though).

23-06-03 *Bayesianism, Yes, but then Something More* (to N. D. Mermin)

Merminition 111: *Alice measures a Qbit, gets 0, and then sends it to Bob. When Bob measures it he always gets 0. How can this be if the instruction to give 0 is not carried by ("resides in") the Qbit Alice sent Bob?*

Schackcosm 68: *I believe that this formulation is part of the problem. "He always gets 0" is a loaded phrase. In my view, it can mean one of two things. It can mean (i) that Bob makes N measurements, and each time he happens to get 0. Or it can mean (ii) that for a single measurement, it is certain that he will get 0. The second meaning is what I try to deal with in my previous email, where I claim that certainty always refers to somebody's state of belief. "It is certain" is then always short for "it is my state of belief that it is certain".*

As for meaning (i), if Bob happens to get 0 every time in N measurements, he can use this result to update his belief about future measurements. Or he can update his beliefs about where the qubits came from. But I don't see why he would have to conclude that each qubit carried the instruction "0" before the measurement.

Merminition 112: *"Have to conclude" is too strong, but wouldn't it be natural for him then to wonder why these two 0 results were so nicely correlated and entertain as an explanation the notion that Alice's measuring 0 imposed the instruction "0" on the qubit? What is it in his Bayesian training that prevents this thought from entering his head? Why should he be content with correlations without correlata?*

There is nothing in his Bayesian training to prevent the thought from entering his head. Instead, it is his training in quantum mechanics that bears the burden: Bell inequality violations and Kochen-Specker. It is a question of looking at the whole package.

Quantum mechanics, as I see it (and maybe Rüdiger too), is a layer added to the top of pure Bayesian reasoning. Bayesianism by itself does not care about the precise character of the left-hand argument in a probability function $P(h|b)$. The h could be a pre-existent fact living in the cold, hard world, or it could be something yet to come— something in existential character completely dependent upon the catalyzing intervention of the agent himself.

All we ask of the community is that it recognize the category distinction between the function $P(h|b)$ and its argument h. That is the first step in clarification. Once one can accept that into one's heart, then—the hope is—great progress will follow.

In particular, I would say, look long and hard at Figures 1 and 2 in my paper `quant-ph/0205039`. That region and the extra "rotation" within it signifies, symbolically at least[44], the extra layer on top of Bayesianism that quantum mechanics is asking us to contemplate. Within the shape of that region (or how its volume scales with system size, or some other relevant feature of it) and the style of the "rotations" that we add after pure Bayesian conditionalization lays hidden that which we are all really seeking: A precise statement about the (existential) character of the h's.

I bank on the idea that we will find that they are not correlata or relata at all, but rather creatia. (Simply trying to ontologize or reify the correlations while giving up on the correlata, as you try with your slogan, won't do.) The world is in constant birth. And to the extent that we focus our actions on anything and ask ourselves, "What will come of them?"—i.e., make note of quantum phenomena—we too are involved in that birth.

But I draw the discussion outward, while you want to bring it inward. I'll trust that Rüdiger will do all the hard work of bringing it inward for the present. You should know, though, that you yourself have the whole answer already; you said it completely: "So of course there is no mystery if you don't feel the need for an explanation of the first feature." The best I can do (and the best I will do, when I follow through with my promise) is fluff that sentence into a whole paper. Why give up on quantum measurement outcomes as the revelations of pre-existent facts? It is all and only a case of once bitten twice shy: the other route has been tried and tried too many times.

But to spin the result this way,

Merminition 113 : *Why should he be content with correlations without correlata?*

is to make the project look so very negative. "Be content with"—I hate that. It seems to forget that there is a much better world in return for this piddling little loss. To give you more detail, I'll attach the best shot I've had so far to make that convincing. (I hear Rüdiger chuckling: "I've never said it better than in my note to . . . ".) The file is titled `ForSlusher.pdf`. If for some reason you can't open it—you often seem to have troubles—it comes from notes to Wiseman on pages 210 and 217 of my samizdat *Quantum States: What the Hell Are They?* on my webpage.

Still beaming from Rüdiger's notes.

[44]I said "symbolically at least" above because I don't want to commit myself to the particular representation of quantum mechanics discussed in `quant-ph/0205039` as in any sense the key to all mysteries. Instead I view it as a kind of scaffold for sharpening the issue—i.e. as a way of giving direct comparison between quantum mechanics and general Bayesianism. But like any scaffold it has to be taken away ultimately . . . and maybe there is a way to shortcut the process without ever introducing the scaffold in the first place.

27-06-03 *Utter Rubbish and Internal Consistency, Part I* **(to N. D. Mermin)**

> What's the good of Mercator's North Poles and Equators,
> Tropics, Zones, and Meridian Lines?"
> So the Bellman would cry: and the crew would reply
> "They are merely conventional signs!

It's time for me to pay the piper. On November 4 of last year, I had a mystical experience during my flight home from the Montréal meeting: I thought for the first time in my life I had seen with complete clarity why there was no mystery, either to EPR correlations or to the Penrose argument calling for the objectivity of the quantum state. The whole strange flight from Montréal to Chicago to Newark (i.e., American Airlines Advantage Platinum Number D7E5856) I typed away. To the result, David replied:

Merminition 114: *Thanks for cc'ing me the latest epiphany. I've only had a chance to glance at it and am still torn between whether my response is "that was how I understood your position to be all week" or "what utter rubbish" so I will take a little time before responding.*

David's second instinct was the best: IT WAS UTTER RUBBISH. The great discussions I had with Rüdiger the last two days a) brought me back to the subject, which I had shamelessly not thought about again since writing the original note (a sign that there must have been some inner skepticism or reluctance on my part, but I didn't have the intellectual integrity to confront it), and b) showed me the gross errors of my ways. This note represents both a heartfelt thanks to Rüdiger and an attempt to tell the Quantum Bayesian tale in a way that does it better justice.

Let me start out by describing what I was thinking previously, both to set the tone and because I think it adds a little clarity to the recent discussions Rüdiger and I have been having with David. What my own trouble boiled down to previously was a moment of weakness—a moment when even I lost faith in our Bayesian dream (and sadly did not realize it as such).

Bell inequalities and EPR correlations, what are they good for? I think one thing only: the conjunction of the two concepts provides us with the most dramatic evidence yet that quantum systems do not themselves carry "instruction sets" for specifying the outcomes of "measurements" we can make on them. As Rüdiger said it two days ago, never was there a better exposition of this point than in some of David's writings. Thus, it is a point David should appreciate and take to heart. I know that I did, and it is this that led me astray in other directions.

The question is, how to live with this result? It's one thing to say there is a formal demonstration that there can be no instruction sets inherent within a system. But it is another thing to feel good about it. In particular, why doesn't it get absolutely under our skins that there can be situations where quantum mechanics specifies that we have certainty about the outcome of a measurement? The million dollar question. You see, if there's no instruction set, where could the certainty possibly come from? Or again: If there are no instruction sets, measurement outcomes must come out of the thin air, determined by nothing else. However, if they do truly come out of thin air, how could we ever be certain which outcome WILL occur before it actually does?

The world does what it does, but yet I have certainty? Where did that come from?!? It couldn't come from the world (as demonstrated through the force of Mr. Mermin's writings); it must come from me. Or—to be absolutely clear—that was the path of thought I found myself traveling last November.

IF THE CERTAINTY CAN'T COME FROM THE WORLD, IT MUST COME FROM ME. The interpretation we ascribe our clicks must be a convention. (As Rüdiger pointed out, it all starts to sound too much like Umberto Eco, and I agree now, but I was blinded at the time.) How could that be?

The mystical insight—the false prophet!—was that the trail had just been blazed! It all had to do intimately with our newfound realization that at least some quantum operations must have the same subjective status as quantum states. (I put the "at least some" solely so that Carl will not stop reading at this point; if it were a letter to Rüdiger alone, I would leave out the qualification completely.) A quantum operation is a density operator in disguise. That was somehow to be the key.

In rough terms, I wanted to say that *ascribing* a pure state P to a system and *ascribing* a description $\{P, I-P\}$ to an associated measurement device was just the convention I needed: No matter what the outcome of the "true" physical intervention (measurement), the result would be interpreted as P. But what utter rubbish! And it's hard to see now what I could have been thinking, or even how I could have found it pleasing. It goes against everything I have ever viewed as the great insight of the Paulian idea:

> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case . . .
>
> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

The last part of the idea is absolutely crucial. The observer must not be able to influence the results of a measurement, even by hiding it in a convention. I had lost my grip on reality during that flight, and something deep inside me must have sensed it.

Let me try to make this absolutely stark by running down the Rüdigerian path. Consider two agents, a quantum system, and a quantum measuring device. Suppose both agents agree that the relevant feature of the measuring device is that it has two possible clicks, and furthermore suppose they both agree that the clicks are to be interpreted as a measurement of the POVM $\{P, I-P\}$. In our language, these are both subjective ascriptions (more on the word subjective later), but they just so happen to agree. However suppose in contrast to this single belief about the measuring device, they are in wild disagreement about what they believe of the quantum system soon to be intruded upon: One says the quantum system's quantum state is P, the other says it is $I-P$. From our standpoint, there is in principle nothing wrong with this. A quantum state is a subjective ascription (more on the word subjective later), period. It is not determined by the world external to the agent; it is his personally. So what happens to the measurement device when the quantum system is dropped into it? There are two agents, two absolutely incompatible beliefs. Does the measurement device explode?

No. A click occurs, and one agent is *made* wrong. If that agent had bet his life on his utterly extreme belief, he will now be dead. Darwinian evolution will have stepped in to see that his extreme belief is not propagated. That is no convention. The world will smite one of the agents.

Now, let us join the Merminian discussion presently in progress. Let us conceptually erase one of the agents. What can possibly change for the other? Nothing. Once again a firm belief exists and once again there is nothing to keep the world from smiting the believer.

OK, take a restroom break, get some popcorn, and I'll be back with the second half of this note after lunch.

28-06-03 *Utter Rubbish and Internal Consistency, Part II* **(to N. D. Mermin)**

It was a long lunch. Let me pick up where I left off:

> A click occurs, and one agent is *made* wrong. If that agent had bet his life on his utterly extreme belief, he will now be dead. Darwinian evolution will have stepped in to see that his extreme belief is not propagated. That is no convention. The world will smite one of the agents.
>
> Now, let us join the Merminian discussion presently in progress. Let us conceptually erase one of the agents. What can possibly change for the other? Nothing. Once again a firm belief exists and once again there is nothing to keep the world from smiting the believer.

The ultimate issue is, is there really any difficulty with the idea of an utterly certain belief about an admittedly contingent fact? And, I'll add for later discussion, is that something uniquely quantum mechanical?

I'll tackle the first question by repeating something Rüdiger said yesterday, but in a windier way: I'll paste in two sections from de Finetti's article Probabilismo. (That, by the way, was the reason for my long lunch: I ended up reading Probabilismo from beginning to end again—I think my fourth time since 1996. If I would have only absorbed the darned thing the first time! Each time I read it, I am struck that it is the best thing on probability I have ever read. Yet each time, I miss something really important. In fact I end up feeling bad about myself, for it shows me just what an amateur I am. The article's *got* to be made standard reading!)

18.

> "A gambler wants to make a bet; he asks my advice. If I gave it to him I would rely on the probability calculus, but I could not guarantee success. That is what I would call *subjective probability*. But I suppose that an observer is there, who notes the outcomes over a long period; when he reviews the record he will see that the outcomes fall out in conformity with the probability calculus. That is what I would call objective probability, and it is this phenomenon that we must explicate." [A quote from Poincaré.]
>
> That is a difficulty that leads many into error: how can one not be persuaded—one would ask—that the value of probability is not simply subjective?
>
> In all these cases, in all similar arguments, what is impressive is only one fact: that a practically certain event actually comes about, or it is foreseeable that it will come about. But should we be impressed? When I say that an event is practically certain I mean that I should be amazed if it didn't occur: am I then entitled to be amazed at having guessed, to be amazed that an extraordinary fact whose occurrence would have amazed me did not in fact occur?
>
> Poincaré says that those who are present at the game "will see that the outcomes fall out in accordance with the probability calculus". First of all they would be able

to see that *some remarkable and practically certain circumstances* occurred, relative, e.g., to the frequencies, while it is impossible that *all* the practically certain facts have occurred. It suffices to think that it was practically impossible that the particular sequence of outcomes that has taken place would have taken place. Then we must limit attention to just one or a few remarkable and practically certain circumstances. Poincaré says that they will happen. But why does he say it? Because he is certain of it, not absolutely, but practically. And didn't we already have to assume that he was practically certain of it? When I evaluate a probability as very close to 1, I express this sensation: that, almost without doubt, the event will occur. Do I add anything more when I repeat that, almost without doubt, it will occur? Do I have the right to think: first I evaluate a probability, and then I ask myself if I can actually anticipate the event with the corresponding state of mind? This is what many do, and, when they can answer affirmatively, they say that probability has an objective value.

But, when I evaluate a probability, I only express my state of mind, and what does it mean to ask whether I can or cannot have a state of mind which corresponds with the state of mind which is actually mine? If such a doubt corresponds to something which is not meaningless and is actually mine, it was already a part of my state of mind, and I will already have used it in my evaluation of the probability. But once I have evaluated it (and as long as I suppose that my state of mind will not change: if it changes, then certainly I can modify my earlier evaluation!) it is meaningless to think that my evaluation is wrong, because it is meaningless apart from me, it has no other function than to express my state of mind.

Why, when an event appears to me as practically certain (i.e., when I evaluate its probability as close to 1) have I the right to be practically certain that it will occur? Because when I say that an event is practically certain (when I evaluate its probability as close to 1) I do not say nor can I want to say more or less than this: that I am practically certain it will occur.

If it is true that "*opium facit dormire*", can we think it true that "*opium habet virtutem dormitivam*"? This is no less difficult and no less deep a philosophical problem! I leave it to the reader's acumen to see whether the comparison is apt.

19.

It seems strange that from a subjective concept there follow rules of action that fit practice. And Poincaré keeps explaining why the subjective explanation seems insufficient to him, mentioning practical applications in the field of insurance. "There are many insurance companies that apply the rules of the probability calculus, and they distribute to their shareholders dividends whose objective reality is incontestable."

Basically, this is only the preceding case, simplified by the fact that here it is very clear what the "remarkable circumstance" is that one must consider, and it has a very concrete importance: the dividend. We make a budget in such a way that it is practically certain that the gains will be such-and-such. Naturally, it is meaningless to say "practically certain" if I don't say *for whom* they are so; in this case it will be the managers, the actuaries, the shareholders. When an enterprise is sound and has little risk, it is easy to reach a universal or almost universal consensus on this opinion, and there is nothing to be surprised about, since it is exactly because of this that the enterprise is said to he sound and have little risk.

> But this is not sufficient: it is not just a feeling of the managers, actuaries, and shareholders, someone will say. You will see that the dividend will prove them right. What does this mean? I mean that this someone shares the feeling, the persuasion, the faith, which the managers, the actuaries, and the shareholders already have. At the end of the year the dividend is regularly distributed. See that, one will say, that certainty was not just my feeling, it must have had an objective ground. But why? If he—even on the basis of a totally groundless conviction—thought it unlikely that there would be no dividend, he would have to find it very natural that there is the dividend, and it would be pointless, unnecessary, and useless to look for an explanation. Least of all for a purely verbal and abstract explanation, like the one that consists of inventing "chance" and "the laws of chance".
>
> But let us look into the function of the probability calculus in the field of insurance.
>
> Whatever enterprise one wants to undertake, whatever firm one wants to manage, one always proceeds by consciously or unconsciously making a budget, in which we equalize the hope of profits and the fear of losses, the hope and the fear that the profits and the losses will be more or less great. We can love risk more or less, we can be prudent or speculative, and our preferences will be different. We could be guided by the hope of a risky gain and risk everything, or we might prefer the modest tranquility of those who feel safe from the tricks of fortune. We are perfectly free with regard to this choice; everyone can do as he wishes. The probability calculus cannot say we are right or wrong: it is true, the concept of mathematical expectation is known, and it is very important, but its task is not (as some seem to think) that of constraining our freedom of choice in this case. The notion of moral expectation has also been introduced, which, besides not solving the problem, is also an artificial and unimportant notion. In any case, we must consider all the alternatives together with their probabilities and their consequences, and then act as we see fit.
>
> In the case of an enterprise that must be secure and have little risk we must act so that, as in the case of insurance, our profits may not be fantastic, but they should be sufficient and practically safe. That's all that non-speculative firms do, without using the probability calculus, and nevertheless this certainty is not too often belied by the facts. And there is nothing strange in this, for an obvious reason: if these forecasts were always belied, we would not make them, and we would act in some other way, and it would be this other way that would inspire us to have greater or less confidence in the various alternatives.
>
> That a fact *is* or *is not* practically certain is an *opinion*, not a *fact*; that *I judge it practically certain* is a *fact*, not an *opinion*. That I should act according to this opinion is only apparently a corollary, because this opinion only exists in that I think I must govern my action in accordance with it.

What of any of this loses force when we come to quantum mechanics, or when we come to Mermin's 1985 *Physics Today* article? I think almost nothing, except possibly de Finetti's rhetorical question:

> If it is true that "*opium facit dormire*", can we think it true that "*opium habet virtutem dormitivam*"? This is no less difficult and no less deep a philosophical problem! I leave it to the reader's acumen to see whether the comparison is apt.

But even that's up to debate, depending upon which part of the quantum confusion one wants to place alongside the analogy. Thus, if you will allow me to drop those three sentences from the passage, I will boldly declare that nothing whatsoever of de Finetti's point changes when we come to quantum mechanics. In particular nothing changes even when we come to EPR/Bell phenomena.

The issue is just one of mindset. I think Rüdiger said it very nicely yesterday:

Schackcosm 69 : *David approaches him and asks "But aren't you dying to find out how the whole thing can possibly work?" Bob is puzzled. He doesn't understand the question. When he finally thinks he understands, he is even more puzzled. "Why would I be more confident about my predictions if the photons carried instruction sets?", he asks. Clearly, Bob and David speak at cross-purposes.*

Apparently, for David the only acceptable way to understand "how it can possibly work" would be a mechanistic, clock-work type model for it. Bob, Bayesian to the bone, thinks that David meant to ask: "How can you possibly be (almost) certain that tomorrow's outcome will be 0?" And Bob's answer is "What difference would the existence of an instruction set make to my beliefs about tomorrow's outcome?"

So, it is not certainty that is a mystery ever. Certainty is just the expression of someone's state of mind, whether the world will bear it out or not. Certainty is, at best, about internal consistency. But, we would have no right to draw from this that there is no deep lesson to be learned from EPR/Bell phenomena. On the contrary, EPR/Bell is just as important as it ever was—it's only been the conclusion that has been misplaced. David drew from it a mystery that has nagged him (and us) for at least 20 years. Instead, its lesson should be accepted at face value, regardless of the feelings of mystery that motivated Bell's initial analysis: There are no instruction sets. That's the lesson. It is telling us something about (what we believe of) the world.

Wow! What more could one ask for than a precise, well-formulated statement of what one actually believes. Sometimes I have a precise statement of the implicit beliefs that motivate my actions, but not very often. Here's a case where I can actually nail one down.

So, the whole business of understanding quantum mechanics starts to feel even better. It is about understanding one's priors! (I wish I could boom that out in a Ben Schumacheresque fashion! I hope you can hear it.) In accepting quantum mechanics, one is making an implicit statement about one's priors. It is the structure of those priors and not their precise values that is telling us something about what we believe of the world independent of our particular experiences.

In summary, let me just say this. Letting go of the mystery in EPR/Bell is not defeatism. Instead, through it we accumulate a fact for our Bayesian understanding of quantum mechanics, i.e., as a statement about the structure of our priors. There's far more work to be done in that regard. And if one wants metaphysics (or at least a post-positivism, i.e., something more than raw experience), that's where it is to be found.

Thus, let me end Part II. There'll still be a Part III, but now I've got to go to dinner.

30-06-03 *Probabilistic Dialogue* (to R. W. Spekkens)

Spekkensism 17 : *I found myself thinking about probability theory last night. I realized I need to really go deeply into these issues. If radical Bayesianism is the answer, then presumably the only thing preventing me from adopting this view is my ignorance of the problems with the alternative.*

So, what should I read to learn about the arguments for and against both the logical and subjective interpretations of probability? If you could tell me the classic texts for both camps, as well as any good articles providing a modern perspective on the debate, that would be ideal. (I'm not interested in the propensity interpretation.)

I'm glad you're taking a deeper interest in the foundational issues of probability now. I think, as your own toy model shows to some extent, getting the idea of probability straight is a large part of the task of getting quantum mechanics straight. In any case, it is a part of the problem that cannot be ignored.

I think the most devastating critique of the classical and/or logical interpretations is the problem of defining the reference class for any given event. What is the set of possibilities that an event *must* be considered an element of? In the real world of statistical practice and decision, there is never a unique answer. Of course, in fundamental physics, one might think that one is in a holier position: One can always declare a reference class by fiat. But then the onus is on the declarer to show why that reference class and a uniform (or even any nonuniform but otherwise fixed) probability measure over it is actually relevant to statistical practice.

I was hoping to pin down some really good quotes over the weekend to accompany this note. You see, for instance, Richard Jeffrey, the inventor of the idea of "radical probabilism" and a wholehearted supporter of de Finettian ideas, started off in the school of Carnap, where the hope was that logical probability would someday be the new messiah. As I understand it, it was years of watching the project fail that ultimately led Jeffrey in the other direction. But I couldn't find anything quite forceful enough for my tastes.

In any case, here are the things I think you ought to read (in the following order):

1. Leonard J. Savage, *The Foundations of Statistics*. Read all of Chapters 1 and 4, especially Section 4.5.

2. Richard Jeffrey, *Subjective Probability (The Real Thing)*. Read pages 8 to 11. The book can be found at: `www.princeton.edu/∼bayesway/`

3. José M. Bernardo and Adrian F. M. Smith, *Bayesian Theory*. Read all of Chapter 1 and pages 99 to 102. And finally, the very best thing that anyone can read is

4. Bruno de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989). (I have never gotten so much out of any of my reading on probability as I have from this article. But I should warn you that I can already guess you will find Sections 1, 2, and 3 of it unpalatable, at least on first reading. Therefore I hope you will be a little lenient on the man until you get past them. After that (and excepting the very last section on fascism) I think there is so much that is absolutely firm in the article, I don't see how anyone could disagree with it.)

Let me also add three attachments to the present note that I think are directly relevant to you. One is a little thing Joe Renes wrote recently. Despite the free-formness of the note, I think it hits the points related to your present query quite well. The other two are notes that I wrote recently: They have to do with the "mystery" (or lack thereof) of EPR from the Bayesian standpoint, and lean heavily on the concept of "certainty" in the Bayesian sense. I do hope that they'll somehow make something click in you. The best answers are going to be the most trivial, not the most contrived.

I hope you'll let me know what you think of all these things. In particular, it'd be nice if we could start up a dialogue. Can you articulate what troubles you most about the de Finettian version of probability? What looks less than scientific about it to you?

I suspect it all boils down to the fact that you feel that Bayesianism is somehow too arbitrary: "For God's sake, they won't declare *any* probability assignments right or wrong! Science is not just 'anything goes."' … but I don't want to put words into your mouth. If however that is roughly the case, I hope you'll think hard about a point Marcus Appleby likes to make: "It's really hard to believe something you don't actually believe." I.e., Bayesians, from my point of view, think anything but ‘anything goes.’ A probability assignment, when it is made, is one’s best shot at articulating what one believes; and what one believes is not up to one’s whim.

I would have thought that the de Finetti version of Bayesianism should have been quite attractive from the point of view of the cut you want to introduce into physics: the ontic and the epistemic. Try your best to say what the world does or is; that’s what the ontic is about. Fine. I’m OK with that. But then why try to make our fallible beliefs rigidly connected to the ontic world? If you’re going to try to make probability assignments adhere to the concepts of right and wrong (just like those ontic states of yours), why not make them part of the ontic world too? Another way to put it: What’s so blasted wrong about allowing a perfectly rational agent to be ‘wrong’ about the actual state of the world? You want a) that there is a world (with its one true state), and b) that any agent worthy of being called rational or ideal, must by right about it to within some tolerance. But why?

Or maybe it is just the fear of having an agent in the background as the anchor for a probability assignment that bothers you. Didn’t Copernicus teach us that our place in the universe is not the center of the universe? Aha! Maybe that’s it. So, if we could just find a sound notion of objective probability, we could imagine the calculus of quantum mechanics hanging around even when there’s no one about in the quad to make use of it. Is that the deeper of the issues for you?

OK, enough blabbering. I’ll shut up for a little while until you give me some guidance. In the meantime I’ll leave you with some of my favorite Bernardo and Smith quotes. They’re pasted below. [See *Quantum States: W.H.A.T.?*, pp. 19–20.]

30-06-03 *Fear of the Anchorman* (to N. D. Mermin & R. Schack)

Did you notice the ending lines I put in the note to Spekkens?

> I suspect it all boils down to the fact that you feel that Bayesianism is somehow too arbitrary: “For God’s sake, they won’t declare *any* probability assignments right or wrong! Science is not just ‘anything goes.’ ” . . . but I don’t want to put words into your mouth. If however that is roughly the case, I hope you’ll think hard about a point Marcus Appleby likes to make: “It’s really hard to believe something you don’t actually believe.” I.e., Bayesians, from my point of view, think anything but ‘anything goes.’ A probability assignment, when it is made, is one’s best shot at articulating what one believes; and what one believes is not up to one’s whim.
>
> I would have thought that the de Finetti version of Bayesianism should have been quite attractive from the point of view of the cut you want to introduce into physics: the ontic and the epistemic. Try your best to say what the world does or is; that’s what the ontic is about. Fine. I’m OK with that. But then why try to make our fallible beliefs rigidly connected to the ontic world? If you’re going to try to make probability assignments adhere to the concepts of right and wrong (just like those ontic states of yours), why not make them part of the ontic world too? Another way to put it: What’s so blasted wrong about allowing a perfectly rational agent to be ‘wrong’ about the actual state of the world? You want a) that there is a world (with its one true state), and b) that any agent worthy of being called rational or ideal, must by right about it to within some tolerance. But why?
>
> Or maybe it is just the fear of having an agent in the background as the anchor for a probability assignment that bothers you. Didn’t Copernicus teach us that our place in the universe is not the center of the universe? Aha! Maybe that’s it. So, if we could just find a sound notion of objective probability, we could imagine the calculus of quantum

mechanics hanging around even when there's no one about in the quad to make use of it. Is that the deeper of the issues for you?

Now that I've written that, I wonder how much of a problem exactly the latter might be for the larger community. Take, for instance, David Mermin's Desideratum 1 in his original Ithaca interpretation paper: "The theory should describe an objective reality independent of observers and their knowledge." How would he ever fulfill Desideratum 1 if probability must be interpreted in the Bayesian way? Maybe it's that that worries people so much about the Bayesian creed when it comes to quantum mechanics ... even (and particularly) Bayesians!

01-07-03 *Your Papers* (to N. Hadjisavvas)

I have been meaning to write you for some time to thank you for sending me the collection of papers that you did:

- N. Hadjisavvas, "Étude de Certaines Consequences d'une Interprétation Subjective de la Notion d'Etat," Ann. Fond. Louis de Broglie **3**(3), 155–175 (1978).
- N. Hadjisavvas, "Distance between States and Statistical Inference in Quantum Theory," Ann. Inst. Henri Poincaré **35**(4), 287–309 (1981).
- N. Hadjisavvas, "The Maximum Entropy Principle as a Consequence of the Principle of Laplace," J. Stat. Phys. **26**(4), 807–815 (1981).
- N. Hadjisavvas, "Properties of Mixtures of Non-Orthogonal States," Lett. Math. Phys. **5**, 327–332 (1981).

Particularly, I was very pleased to learn that you had toyed with Bayesian ideas within quantum mechanics already in your Master's thesis, so many years ago. I will study all of your papers in detail and cite them duly in my upcoming work.

I wish I knew of which of my papers you found some interest in. In case you haven't found it previously, you can find a body of my writings on quantum foundational things on my personal website (link below). Also, Caves, Schack, and I have an accumulating list of Bayesian-type quantum results posted on the quant-ph archive at `www.arXive.org`.

Nicolas's Preply

> I recently saw (by chance, given that my research interests changed completely since almost 20 years) a couple of your papers on quantum information. Please find enclosed some very old papers of mine on related subjects. Actually, the paper in French was my Master's (DEA) thesis. In it, the idea of a state describing the knowledge of the observer, rather than the system itself, is exploited for (still another) derivation of the projection postulate.

01-07-03 *Rob Spekkens* (to R. D. Gill, K. Mølmer, and E. S. Polzik)

I just wanted to alert you guys to Rob Spekkens, who has just applied to give a talk at the MaPhySto/QUANTOP meeting in August. I hope his talk will be accepted. I think he has truly one of the most interesting quantum foundational / quantum informational results I've seen in quite a while. What he does is explore a toy model that is admittedly not quantum mechanics, but is a certain local hidden-variable theory. What's interesting about the model in spite of this nonsense is that it incorporates a principle that Carl Caves and I were once hoping to found quantum mechanics itself upon: Namely, that in quantum mechanics maximal information is not complete information. The truly surprising result about Spekkens' toy model is just how much of standard quantum-information looking stuff his model contains:

a) a no cloning theorem

b) superdense coding (without entanglement)

c) a no broadcasting theorem

d) a no-go theorem for bit commitment

e) an information-disturbance principle that can be used for secure key distribution

f) something that looks like entanglement monogamy (without entanglement)

and the list goes far beyond this. The point is, one does not (and cannot) recover all of quantum mechanics from his toy model, but one can get a hell of a lot of it even in a hidden variable model (with the principle that maximal information cannot be made complete).

I think we stand to learn a whole lot about real quantum mechanics from this exercise. And it is just a marvelous construction. I hope there is still room in the schedule for young Spekkens.

01-07-03 *Gasp or Shudder?* (to J. W. Nicholson)

Actually was it gasp or shudder? My memory is getting fuzzy on the whole affair. I know that I need to write this story down before it is completely gone. Could I interest you in sending me some notes on what you remember? Of course, when I write it up, I'll probably embellish things a little to try build atmosphere, but I would like to be decently accurate to what actually took place (and how things were said).

01-07-03 *Samizdat* (to A. Fine)

It was good meeting you at the Clifton memorial meeting a few weeks ago; I've long been a fan of your writings. I'm writing to tell you, though, that my quantum samizdat *Notes on a Paulian Idea* has just been published by Växjö University Press, and I have a number of copies to give away. (If you don't know what I'm talking about, you can peruse the file on my webpage (link below).) If you think you might get something out of it, I'll have the publisher send you a copy. All I need is your mailing address.

01-07-03 *My Samizdat* (to W. E. Lawrence)

I've been meaning to write you for a while. Would you be interested in a copy of my quantum samizdat *Notes on a Paulian Idea*? The book has just been published by Växjö University Press, and I have a number of copies to give away. If you don't know what I'm talking about, you can peruse the file in PDF format on my webpage, link below. (Mermin wrote a foreword to the book.)

If you think you might get something out of it, please let me know and I'll have the publisher send you a copy. All I need is your mailing address.

01-07-03 *Objective Chance* (to W. C. Myrvold)

Myrvoldism 1: *It's several projects down the queue, and we may not get to it this summer, but Bill Harper and I plan to write an article on why Bayesians should believe in objective chance. I'll send you a draft when one exists.*

I look forward to it. It's about the biggest thing on my mind at the moment. Particularly, I'm fairly of the opinion that trying to force objective chance (rather than objective indeterminism) onto the world is going to be counterproductive for a good understanding of quantum mechanics. But I'll give you a chance to convince me! (I made the distinction above, by the way, because I rather like indeterminism of a sort, but I would be reluctant to try to ascribe a numerical measure to it.)

In the mean time, anyway, can you give me some pointers to any literature that tries to argue the same point as you'd like. I'll bank on your paper with Harper being a better version of it all, but still I'd like to see some its predecessors.

01-07-03 *Gasp or Shudder?* (to Friend X)

Actually was it gasp or shudder? My memory is getting fuzzy on the whole affair. I know that I need to write this story down before it is completely gone. Could I interest you in sending me some notes on what you remember?

01-07-03 *Making the World Gasp Your Name* (to R. W. Spekkens)

Spekkensism 18: *I had not heard of the Aarhus workshop until now. I am thinking about asking the organizers whether it is too late to attend and whether there are any free slots left. Do you think it will be a good workshop for someone, like myself, who finds himself currently absorbed with foundational rather than practical issues? [...]*

I had a feeling it was something like that. Have a look at this article before it disappears from the NY Times:

`http://nytimes.com/2003/07/01/international/europe/01DENM.html`

"Free Spirits in Their Fortress, the Law at the Gate."

01-07-03 *Samizdat and Potentia* (to A. Shimony)

Shimonyism 1: *About two weeks ago I sent you a note thanking you for your book, but I fear that the address was wrong. If you have already received my note of thanks, no harm is done. I certainly appreciated receiving your remarkable book. You gave good excerpts from emails sent to you, and these together with your replies constituted interesting dialogues. As you know, there are serious differences of opinion between us on the ontology of the wave function, but your arguments always seem to me thoughtful. I shall consult your book often and find stimulation in it.*

Thank you for the kind words.

I also reread the lovely letter you sent me on May 27 of last year. I know that there are differences of opinion between us, but I hope that in the end they will not be so serious. In particular it would be nice if physical theory itself would lead the way to minimizing those differences: I think there is a chance, with a suitable rewriting of quantum mechanics into more Bayesian-like terms. At the very least it'll be a new way to see things, and that may sharpen our real (rather than apparent) points of contention.

You write, "[I]n my opinion the greatest philosophical innovation of QM is the discovery and exploration of a new modality of reality – something in between actuality and logical possibility – which Heisenberg named 'potentiality' in his book *Physics and Philosophy*." But I would like to think that I am quite attracted to the same idea. Throughout *Notes on a Paulian Idea* you'll find image after image of the quantum measurement process as a creative process, something that brings about a transition from the possible to the actual. Indeed it is captured by the Paulian idea itself (the conjunction of the two quotes at the beginning). Here's a slightly longer version of the same:

> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.
>
> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case . . .

Where we part company, I think, is only in a) my resistance to summing up this new category with a numerical measure (i.e., objective probability or objective chance), and b) my desire to make it clear that the quantum formalism is a calculus for manipulating agent-centered probabilities. Sometimes I put the latter point this way: Can a dog collapse a wave function? Dogs don't use wave functions. I myself didn't collapse a wave function until I was 23. But that doesn't mean that the quantum world will disappear without the agent! It only means that (judgmental, personalistic, subjective) quantum probabilities disappear without the agent.

Here's the way I put it (again) to David Mermin the other day:

> I bank on the idea that we will find that they are not correlata or relata at all, but rather creatia. (Simply trying to ontologize or reify the correlations while giving up on the correlata, as you try with your slogan, won't do.) The world is in constant birth. And to the extent that we focus our actions on anything and ask ourselves, "What will come of them?"—i.e., make note of quantum phenomena—we too are involved in that birth.

Thus, the transition from possible to actual I am toying with is of a sort of William Jamesian flavor (now that I know a little bit about James's philosophy).

Does that leave us with an even more gaping trench between us? Or is there some room for finding a common ground?

Abner's Reply

There are some bridges between your Weltanschauung and mine, and there are also some chasms. Among the first are these:

(1) I too am a Bayesian, but in my view of scientific methodology. There are, however, many different versions of Bayesianism – logical probability theory, subjective probability theory, personalist probability theory (similar to subjectivism except for the constraint of consistency), and communalist probability theory. Mine is none of these. The probability theorist whom I most admire is Harold Jeffrey[s], who announces himself as a logical probabilist but modifies that position with what he calls "the simplicity postulate", which is really a strategy. Maybe I should call my rewriting of Jeffreys "strategic Bayesianism." The position is presented at excessive length in "Scientific inference", reprinted in vol. 1 of my *Search for a Naturalistic World View*. An improved and shorter version is in the paper that follows it, called "Reconsiderations on Inductive inference."

(2) I maintain that Bayesian inductive methodology can be employed at the level of generality of metaphysics. It seems to me that Pauli thinks the same, but the metaphysical principles which he derives don't convince me, because of his emphasis on the subject – a residue of Kant. My best expositions of my theses are in the same volume, one called "Search for a world view that can accommodate our knowledge of microphysics," and the other "Reality, causality, and closing the circle."

Another bridge is our common admiration of the pragmatic tradition in American philosophy. You love William James, and I have respect for James but reverence for Charles S. Peirce. If you look at their names in the indices of both volumes of "Search…" you will see my reasons for these attitudes. Something that gave me great satisfaction when I was Wigner's student was pointing out to him an affinity between some of his philosophical papers and those of Peirce, whose name he had never heard of. He then read some Peirce and was excited: "this man has brains and imagination!". And later he cited Peirce several times. It could happen to you, too. I am not a missionary, but I like to share my enthusiasms with my friends [Maybe that's another way of describing a missionary!].

(3) Although Bayesian probability theory does not, in my opinion, suffice to characterize the probabilities implicit in quantum states, it can be a valuable adjunct. I am now in the middle of writing a Bayesian treatment of the problem of probability in ensembles that are both pre- and post- selected – a problem opened in a famous paper by Aharonov, Bergmann, and Lebowitz, Phys. Rev. **134B**, 1410 (1964), anthologized by Wheeler and Zurek, and treated in many papers by Aharonov and his school. There are, in my opinion, systematic errors in their work, easily resolved by carefully using Bayes's theorem. I'd like to present this work if we have the conference on Bayes and QM that we talked about, but in any case I shall send you the paper when it is done. I've written on the topic before, but am dissatisfied with earlier expositions.

This is a beginning. I hope there will be sequels.

Did I tell you how impressed I was by the beautiful photo of you with your daughter? No philosophy can pretend to depth without the message of that picture!

01-07-03 *Endorsing Probabilismo* (to N. D. Mermin)

I do hope by the way, when you come up for air from the wedding (is it one of your children's?) you'll take a shot at reading de Finetti's article "Probabilism." Here are the coordinates:

Bruno de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989).

The article is immediately followed by one titled "Reading Probabilismo" by Richard Jeffrey. It'd be a good idea to take a look at that one too.

I thought I would send you two things to help prod you into taking this homework assignment seriously. One is an endorsement of the paper by Rüdiger that I noticed he hadn't cc'd to you. The other is a quote from within the paper that sounds ever so much like 'correlation without correlata'. (I know that nothing else so whets your appetite.)

First the quote:

> Concerning Aliotta, I think it necessary to report the following passage, to avoid what might be an easy misunderstanding.
>
> "It is necessary to distinguish relativism from relativism. There is one of its forms (the one commonly pointed to when relativism is accused [of skepticism]) that relegates our knowledge to the realm of relativity, opposing to it an absolute reality that will always elude knowledge. In this form relativism has a skeptical and agnostic flavor and often goes together with mysticism. In the blinding light of the absolute our relative world devaluates, degenerating into a vain apparent shadow. We are the dream, the absolute is reality. And life becomes the painful chase of those shadows, vainly trying to become light.
>
> "But there is another form of relativism (and this is mine), in which what is relative is itself the reality and leaves nothing outside itself. What we know is not the shadow, but the light, not a copy, but the true and concrete original" (*Relativismo e Idealismo*, Naples, 1922, p. 92).
>
> This is exactly my opinion, and I wish to note, for more complete rigor, that the sentence "what is relative *leaves nothing outside itself*" must not be understood as saying that the sentence "there exists something outside what is relative" is FALSE, but that it is meaningless, so that it is impossible even to pose the question as to its truth and falsity. This is, after all, the interpretation that conforms to Aliotta's thought, as appears clearly further along in the text, where "*the being in itself and outside any relation of things*" is seen as "one of the many verbal statements to which there correspond no ideas, and which have become true and proper puzzles of philosophy" (ibid.).

02-07-03 *Photographs and Memories* (to A. Shimony)

Thank you again. I will read your note many times over (as is my habit), and I do hope this is only the beginning.

Shimonyism 2: *Did I tell you how impressed I was by the beautiful photo of you with your daughter? No philosophy can pretend to depth without the message of that picture!*

You flatter me … but you might also enjoy two letters in the Charlie Bennett chapter, titled "Emma Jane." They start on pages 52 and 53. The second of the two—[ending with the line "giving her dad more reason to suspect that the world is so much more than a mechanical contraption clinking along"]—was certainly meant to be a philosophical statement.

Abner's Reply

Before answering you I looked at pp. 52–53, as you suggested. The list of eighteen accomplishments at age six months (5 June 1999) is astonishing. I have been a Chomskyan since I first met him forty years ago and listened to the evidence that he had compiled for infantile intelligence, but Emma Jane exhibits muscular and emotional intelligence which Chomsky hardly notices.

She is programmed to elicit the responses that you are programmed to exhibit. Isn't that a wonderful gift. And yes, the last accomplishment in the list is a philosophical lesson of the first order.

You and your wife are fortunate to have such a daughter, and she is fortunate to have parents who properly appreciate her.

03-07-03 *The Dangers of Analogy* (to R. Schack)

Schackcosm 70: *I just discovered that von Mises was right after all! Proof by analogy:*

Einstein: Gravitational mass and inertial mass do not just have the same numerical value—they are the same thing.

Von Mises: Probability and limiting frequency do not just have the same numerical value—they are the same thing.

True enough. But here's one I'm letting guide me ever more often:

$$\text{Hilbert space dimension} \quad \longleftrightarrow \quad \text{gravitational mass}$$

(If the Bekenstein bound is right ... or at least contains a grain of truth ... perhaps it should be mass × area.)

04-07-03 *Solid Ground, Maybe?* (to G. L. Comer)

Comerism 4: *P.S. When we're together, I want to press you somewhat on how spacetime concepts enter, appear, etc either implicitly or explicitly in your information theoretic program.*

Yep, this is what the note is about. Once upon a time I promised to write you something about the information theoretic roots or NONroots of the principle of equivalence if I ever had any thoughts on it. I think I had one thought. Let me try to get it onto paper.

I go up and I go down when it comes to speaking the words gravity and quantum in the same sentence. At times I find myself thinking that general relativity and quantum mechanics express two absolutely incompatible worldviews. The general relativistic universe is a "block universe" in William James's sense: It's just there. One can talk about foliations and dynamics, etc., WITHIN the 4-manifold, but in the largest view—the view from nowhere—the world and all its history is just there. It is a universe without life (in the creative sense). In contrast, the quantum world strikes me as a malleable world—one that is still in formation, and in particular, one for which it is impossible to get such a "view from nowhere" (as Nagel would call it).

At other times, I find myself feeling more lenient: Perhaps the two worldviews are incompatible, but that does not mean we cannot gain insight about one of the theories from the other. And when we find the analogy, maybe it is just at that place where we should start hammering away those inconsistencies.

Suppose you take any two pieces of the universe that your mind is willing to call 'matter.' What can *you* tell me with assurance, even if no further word is said about their constitutions? You would tell me that they attract each other, and the force of attraction (in the Newtonian view) is determined in part by two numbers, one intrinsic to each of the two pieces of matter—their masses m and M.

Suppose now that I take any piece of the universe that my mind is willing to label 'matter.' What can *I* tell you with assurance, even if no further word is said about its constitution? I can tell you that with my free will I can write some number of messages into it. Moreover, I can choose to write them in such a way that that piece of matter will reveal (with some probability) whether anyone else has had a look at my stored message. Both the number of messages I can write into the matter and my best probability of catching an eavesdropper is determined by a single number: The matter's Hilbert space dimension D.

In my paper `quant-ph/0205039` "Quantum Mechanics as Quantum Information (and only a little more)" and in my web samizdat *Quantum States: What the Hell Are They?* (on my homepage), I have argued strenuously that it is ONLY the Hilbert space dimension D that can be taken as a property intrinsic to a quantum system (a piece of matter). The quantum state, nothing to do with entanglement, or even anything to do with a Hamiltonian is intrinsic to it: Just the single, lonely number — the dimension.

It strikes me that we have here a phenomenon of the power and scope of universal gravitation, or maybe the principle of equivalence.

Why did I name this note 'solid ground'? It has to do with something I tried my best to express the last time I was at the Perimeter Institute, where it looks to me like there are so many people flailing about without a clue as to what they should be up to when they speak of combining general relativity with quantum mechanics. Lee Smolin asks me how I could possibly imagine that the linear structure of quantum mechanics will remain when one moves into such a nonlinear regime as that given by the laws of gravity? I say I'm not fazed at all: Most of what he means when he speaks of quantum mechanics as an expression of physics, is for me but a law of thought. A wave function and its evolution are not properties intrinsic to the system for which they are about. Rather, if they are properties of anything at all, they are properties of their user's head—for they capture all his judgments about what might occur if he were to interact with the system of interest.

The quantum foundational task as I see it is to baldly accept that A LARGE PART of the theory is simply not about a world without observers: It is only about our interface with the world. But there is a part that remains, and that part must be given a firm identification. For only once we know how to do that will we know how to move forward when it comes to gravity. Only then will we see that almost all of the ways that have hitherto been considered for combining quantum mechanics with general relativity were far too unconstrained: I am willing to bet that they all essentially boil down to sheer speculation.

So I write this note because I am starting to get a sense that there might be some solid ground in these considerations. Hilbert space dimension is the universal factor for quantum systems that mass is for gravitating systems . . . or something like that. Could they be the same thing? . . . Or something like that?

"What nonsense is this!??!? Any of the simplest 'real world' quantum systems—not these paltry, specialized things you study in quantum information theory—has an infinite dimensional Hilbert space. Just think of the hydrogen atom!" Maybe. But it's caused me to wonder if I should take the Bekenstein entropy bound more seriously than I have in the past. The trouble that I had had with it before—after making my transition to the subjective/Bayesian/Gibbsian school of entropy—was that a bound on the ignorance one can have just doesn't seem to make much sense: entropy is not an objective property. But we have to be careful in these things: there are levels of subjectivity.

What I mean by this is that, though, the cardinality N of a sample space may be subjective in the same sense that a probability assignment is, once one has set it, one is obliged to declare a maximum ignorance, $\log N$, with respect to that setting. And that may be what is really going on the Bekenstein bound.

In other words, maybe in my language, all and only the number ER/h signifies is a Hilbert space dimension. That is one thread of thought, wildly speculative and departing from solid ground though it may be.

But the other thread of thought is in the interpretation of this previous wild speculation. If it's even on the right track, where might it go? Let me give you an example of very bad language: "The Hilbert space dimension signifies the number of potential states a system can inhabit." The lesson of all my (and Bell's, and Mermin's, and many other guys) quantum foundational work is that that is nonsense. Hilbert space dimension signifies something else.

When I get poetic about it, I like to say it signifies something about a system's "sensitivity to the touch", and when I'm getting downright Paulian, I like to say it signifies something about its potential for taking part in creation. With my light touches, I can send it off in ways unknown (to me and to it) and, counter to intuition, the larger it is, the more I can do that. Wojciech Zurek has spent his life making up stories about why big things are "classical." I say he has it all wrong, the bigger the thing, the more Hilbert space it has, the more quantum it is! The more sensitive it is to the touch. And thus the more ignorance an outside observer generally has about it (except with respect to a very small number of features) — not for any particular reason to do with very particular properties of Hamiltonians, but simply because it is big. It is not the system that is classical, but the poor observer's description of it that is.

So, let me leave you with an image for your flight. It's about the fleshpots of creation. Take the Bekenstein bound with more than a grain of salt—I'm not sure that I do yet, but it is fun to play—and take a given region of space. Ask yourself what you should imagine to be there if you want the region to have the most potential for creation. And if you stumble across the answer I'm guessing you will, what on earth could it mean?

PS. Let me append another note I wrote a few months ago to David Mermin [6 January 2003]. Maybe there's a connection in there somewhere.

04-07-03 *Bekenstein Bound Status* (to W. G. Unruh)

Can you fill me in on the latest to do with Bekenstein's entropy bound? Is it still controversial? Have any loopholes been plugged? Is it dead in the water? Things like that. Or can you give me a pointer to something to read on the latest state in the debate?

The reason I ask has to do with a recent *analogy* I've been drawing between (finite) Hilbert space dimension and mass. Maybe it's taking me down heretical lines ...

Thanks!

07-07-03 *Your Letter* (to P. G. L. Mana)

Thanks for the very nice letter of 12 June; it all sounds very interesting. Have you made progress since then? Has Hardy replied with anything of interest?

I apologize for taking so long to reply myself, but many, many administerial distractions have come to me since our nice time in Sweden.

Concerning your first issue, let me ask a naive question. Does your discovery boil down to something other than saying, in the end, one can always embed quantum mechanics in a hidden variable model. (Bohmian mechanics is usually claimed to be proof by explicit example.) Of course, one has to give up some nice features of standard quantum mechanics—like locality—but nevertheless one can embed it into a hidden variable model. If that is what your result is, it may nevertheless be quite a new way of saying it which will lead us to a better understanding of what criteria of aesthetics we are using when we accept the standard quantum model.

Concerning your second issue of generalizing the quantum Bayes rule, I shouldn't think it too hard. Will it not just correspond to taking the matrices V_d in Eq. (95) of my paper to be (nonsquare) isometries rather than full unitaries?

Finally let me address one thing that would be useful for my efforts to raise funds for meetings, postdoctoral positions, and, in particular, sell this direction in quantum foundations that you, Hardy, I, etc., are exploring. In the first note you wrote me you said:

Manalogue 2: *Thank you so much for your letter, and for the words of appreciation for my paper. It is just a little more than a draft, and so its form is far from being nice, and many are the missing references. I already knew "Quantum Mechanics as Quantum Information (and only a little more)"; indeed, your idea, expressed there, that time evolution could be a density operator, stimulated (via Rodriguez' "Unreal probabilities",* `physics/9808010`*) some of the research that led to my paper.*

If that is true, would you mind re-posting a version of your paper that makes some citation to my own. The issue is, I have to make a case here and there that these ideas of mine have some influence, even if it is you young guys out there doing all the nontrivial work. Having proper documentation at those times would definitely help.

How far is Stockholm from Aarhus? If you have the time, you might consider coming to the following conference: `http://www.maphysto.dk/events2/QPFA2003/`.

It'd be great to talk to you again on the short timescale. On the longer timescale, we'll have to look into when you, Schack, and I can all get together in Dublin.

07-07-03 *Bekenstein Bound Status, 2* (to W. G. Unruh)

Unruhism 1: *My take has not changed. It is possible that some such entropy to energy bound exists in the real world. It is certainly not necessary and I can imagine theoretical worlds in which it is not true (the simplest is that the entropy goes up as the number of species of say massless particles goes up, so one can always violate any bound by assuming a large enough supply of species). But it is also not needed for saving black hole thermodynamics, which was what he invented it for.*

That is an almost trivial argument, and I was aware of it — heard it from you long ago. If, however, one could argue that (by fiat) in the GR setting, the assumption of a ER/h value is the *assumption* of a QM Hilbert space of the same dimension, then one would have it. Not a logical necessity, but in essence a new postulate. Still, it would have to be motivated from some considerations: So I guess I was asking about something along the lines of your last sentence above. What is the best thing to read in that regard?

08-07-03 *Slogans, Slogans* (to G. L. Comer)

Comerism 5: *Thinking a little about your idea of black holes and the dimensionality of the Hilbert space, how does the dimensionality of the spacetime enter? Thinking classically for the moment, the degrees of freedom of a Schwarzschild black hole can (at least qualitatively) be understood using the* *quasinormal modes. I imagine that one must sum over more and more l, m etc mode numbers the higher the spacetime dimension gets. If we could 'quantize' those modes, would not the dimension of the Hilbert space change as the spacetime dimension changes?*

Oh, I don't know about any of these things; I hadn't thought about spacetime dimensionality at all.

Mostly it's just my habit of driving research with slogans—you don't know where you're going (or even where you want to go) unless you can make a slogan of it. As I was walking in, I was thinking I could have just as well compacted my last long note into the following little play. Basically I was thinking, wouldn't it be so nice if . . .

> Greg: How much Hilbert space do you think this tin can has?
> Chris: I don't know; let's weigh it.

Don't tell me about its construction, its composition, its history; just weigh it.

It's because I've been having outlandish thoughts like this, that I started wondering about this Bekenstein bound again. Maybe it's all crap. (Overwhelmingly likely it's all crap!)

Does Mr. Bekenstein's bound depend upon spacetime dimension? (I'll dig up some papers when you get here.)

By the way, when do you get here?

08-07-03 *The Common Fear?* (to R. W. Spekkens & N. D. Mermin)

The last three days as I've been walking to work, I've been reading a little book on the thoughts of Jürgen Habermas. I thought of both of you (or, actually, my perceptions of both of you) when I read the following lines. Let me record them.

> Every undergaduate with a unit of behavioural psychology in his or her academic record will have had some experience of the most vulgar example of what Habermas means. In this, as in every other field of postivistic science, we are asked to approach the object world as a disaggregated jumble of discrete objects of perception, as a jumble of 'its'. We are set the task of uncovering the regularities in the behaviour of these atoms of substance by means of an experimental method. The criterion of success lies in the predictive power of the uncovered 'laws' that must produce replicable results. This means results that are *independent* of the author, the inventor, in short of the *thinking subject* who in the first place conceived the problem, the method and the experiment and who thereby created the knowledge. One half of the underlying assumption is that knowledge is always reducible to the totality of discovered properties of the object world. The other half is that the *subject*—the actor, the creator, the knower, the inventor, the scientist—is at worst a pollutant in his own purely objective world, or at best, a ghost in the machine of science and something that must be methodologically controlled and, so far as is possible, eliminated.

09-07-03 *You and PI* (to D. R. Terno)

Things are nifty here and my family—especially my four year old—are quite acclimated already.

It's good to hear that you're liking it at PI. What sorts of things are you working on now? I hope the unhealthy hidden-variablist ideas floating around there aren't infecting you . . .

10-07-03 *The Anointed Snark* (to N. D. Mermin)

Mermintion 115:

> [CAF wrote:] Importantly though—just trying to keep you on the cutting edge—have you read the de Finetti paper "Probabilismo" in your preparation?

Raced through it, probably too quickly, on a very hard bench in the library, after spending 30 minutes hunting it down. Non circulating journal. Too long to Xerox. Has it been reprinted at anybody's web site? It didn't help me much the first time through. The stuff on correlations without correlata seems to be just a footnote.

What has the four page limit of Physical Review Letters done to our brains? Raced through it?! I tell you that it's the best explication/defense of subjective Bayesianism I have ever seen, and you raced through it? I send you evidence that Rüdiger thinks the same, and you raced through it?!? Rüdiger and I both write detailed letters to show how embracing some of the ideas in it may be just the analgesic your 1985 paper needs, and you raced through it?!?!

Tomorrow when I go in to the office, I will Xerox my copy for you and send it across the ocean. Please don't race through it. (But feel free to give honest, open criticism of anything that doesn't make sense in it. With that, we'll all certainly learn something.)

One question in the meantime concerning this:

Mermintion 116: *Glad it's helped you. Hasn't helped me much yet, but I'm still interested in keeping it up.*

Fair enough. But can you tell me this: Did I, in my note "Utter Rubbish and Internal Consistency, Part I" capture what you think is the essential mystery of the subjective view of the quantum state? If so, then I've at least got the right starting point, and I can try to refine what needs to be said.

PS. Yes, the stuff about correlations without correlata was only a footnote with no further mention. I was just trying to use everything at my disposal to lure you into the paper. One thing really of significance though: All those philosophers he mentions near that footnote represent the Italian school of (Jamesian) pragmatism. My love affair with James only deepens and deepens. Some mixture of James, de Finetti, Pauli, and modern quantum information are gonna ultimately rule the day on these issues.

10-07-03 *Solipsism* (to N. D. Mermin)

Mermintion 117: *Thanks, I'd enjoy having a copy.*

I think my problem is not with the subjective view of probability. Between what I've learned from you and Rüdiger, reading around in Jaynes, Cox's famous paper [which I came upon ten years ago — did I ever tell you that I independently derived and solved precisely his functional equation in my "Relativity Without Light" paper (in boojums)], the stuff I read in the Business Library (!) after first meeting Rüdiger, etc., it all makes a lot of sense.

My problem is, and remains, how it ties in with Quantum Mechanics, which appears (among other things) to tell you how to derive precise probabilities under (apparently) precisely defined — if idealized — circumstances. "Quantum Mechanics Is a Law of Thought" is certainly a good slogan for getting started, and your poetry is very soothing, but I can't help feeling there is still an enormous gap, despite nice things like your quantum de Finetti theorem. Declaring the circumstances also *to be subjective judgments appears to be a good (perhaps even necessary) move, but I don't find it convincing and (at best) it seems to lead into some kind of infinite regress in which everything becomes subjective and we're back to solipsism (which is irrefutable and therefore trivial). I haven't been able to articulate what I feel is missing well enough to send it on to you even under the Littlewood-Hardy rules, but I'm still thinking about it.*

Rule 1 definitely applies to the above paragraph.

A) What do you mean by solipsism? And, B) how does the "Paulian idea" I sent you still strike you as falling into that category?

11-07-03 *Wow!* (to J. A. Smolin)

John Smolinism 4: *I am writing up a paper on the lockboxes, and I need to figure out where to send it. I'd prefer a real journal, but perhaps the conference proceedings of Sweden are the best place. In a couple of days I'll send you the paper if you're interested.*

Wow! Excellent. I never imagined that you'd actually write this stuff up. I'll have a look at the actual draft next week. I'll try to give you detailed comments.

The latter part of your note strikes me as really interesting:

John Smolinism 5: *My plan is to actually write two papers. I want to put a paper on the basic idea out there soon, then perhaps later a paper including a discussion of Rob's toy model and some foundational problems. I also have some things I want to work out about Rob's model if he hasn't done it already by the time he write it up. In particular, his love of the so-called underlying ontic states is, I believe, what leads to the disallowing of some mixed states–The rule is that a measurement on one system cannot change the ontic state of another, but the right way to do it is to say that what you can't change is the marginal measurement outcomes of the remote system. This is definitely different under his model and I think leads to a prettier result.*

You know I'm all for nonontonicity when it comes to the "stuff" that quantum states are about.

11-07-03 *THE Paulian Idea* (to N. D. Mermin)

Merminition 118: *Which "Paulian idea"?*

The one for which there are some notes on:

> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case …

Or in more modern language (from 11/4/02):

> 1) The POVM as a function from raw data to meaning.
>
> We generally write a POVM as an indexed set of operators, E_d. Here is how I would denote the referents of those symbols. The index d should be taken to stand for the raw data that can enter our attention when a quantum measurement is performed. The whole object E_d should be construed as the "meaning" we propose to ascribe to that piece of data when/if it comes to our attention. It is important here to recognize the logical distinction between these two roles. The symbol d stands for something beyond our control, something that enters into us from the world outside our head. The ascription of a particular value d is not up to us, by definition. The *function* E_d, however, is of a completely different flavor. It is set by our history, by our education, by whatever incidental factors that have led us to believe whatever it is that we believe when we walk into the laboratory to elicit some data. That is to say, E_d has much the character of a subjective probability assignment. It is a judgment.

11-07-03 *More Solipsism* (to N. D. Mermin)

Still trying to get your worry straight. By your definition, could a solipsist make changes? (The note below makes sense of this question.) [See 21 July 2001 note, "The Reality of Wives," to A. J. Landahl and J. Preskill.]

11-07-03 *One Final Thing in the Wee Hours* (to N. D. Mermin)

Merminition 119: *Solipsism because when I try to look at things your way I find that whatever I try to condition my subjective probabilities on turns out also to be subjective and conditional on further subjective judgments, ad infinitum.*

Then, for you, does Bayesianism in general equal solipsism (independently of quantum mechanical issues)? For every Bayesian has that infinite regress: They ball it all up into something called "the prior" and ask not where it comes from. (As Savage said it very nicely in the passage Rüdiger recommended you read, the prior might simply be a product of Darwinian evolution.)

Good night! (It's after 3:00 AM for me now.)

11-07-03 *Friendship* (to N. D. Mermin)

Your absorbing this de Finetti paper is important for our discussions. Consider it a present from the slush fund Kiki allows me to dip into once a year (for the really important things). It will arrive at your office in Clark Hall sometime Monday. (FedEx tracking number 8410-8544-4295)

Solipsism indeed!

11-07-03 *One Project Done* (to G. L. Comer)

Well I got one project done that I had promised myself to do before your coming to Dublin (even if I finished no others): I finished reading my copy of *Becoming William James*. I just couldn't bear the thought that you might know something about William James that I didn't know! But let me apologize to you: I found that I hated the book, and I'm sorry I burdened you with it. The only thing that comes across in it is that WJ and the whole James family is troubled. One gets no feel for the utter greatness that was developing all along at the same time as these (overemphasized) other bits. And the guy was just too full of it with of his own psychological theories, rather than giving us a rounded glimpse of the lives involved. Anyway, you can see it's on the bottom of my list of James biographies now. I wish I had dug you up a copy of Perry's *The Thought and Character of William James* instead. It carries the message I had wanted you to get, where this other book utterly fails.

See you tomorrow.

13-07-03 *Thanks Again* (to M. Pérez-Suárez)

Thanks for the latest notes. I'll forward them to Joe Renes, who's taking care of the final draft. (The paper will appear in *Foundations of Physics*.)

Concerning the Paulian-Wheelerish compendium, I have finally gotten my computer set up with a scanner and top of the line OCR package (optical character recognition). It works like a charm!! So the compendium will be greatly expanding very soon. I'm hoping to get the final product on the web by Christmastime.

14-07-03 *Unitary Equivalence* (to A. Peres)

Asherism 35: *During the Växjö conference (June 2001) someone (Ben Schumacher?) gave a talk explaining that any dynamical system could be canonically mapped onto a harmonic oscillator and discussed the corresponding quantum property. My comment was that unitary equivalence is not equivalence from the point of view of physics (and I gave a reference to Fong and Sucher). That talk does not appear in the conference proceedings. Do you remember that talk and who gave it?*

It was definitely Ben. He uses that as an example to argue for the inconsistency of the many-worlds interpretation. He's never published the talk as a paper, much to my dismay (I've tried to get him to on a couple of occasions).

What is the Fong and Sucher paper?

Asher's Reply

Fong and Sucher, J. Math. Phys. **5** (1964) 456 — see my book, penultimate paragraph of page 227.

17-07-03 *Utrecht November Conference* (to J. B. M. Uffink)

For your meeting in November, let's try this:

Title: Quantum Information Does Not Exist

Abstract: It is information *carriers* that exist—conceptually both classical and quantum. To confuse the epistemic category (the information) with the ontic (the carriers) is to cause any amount of trouble. Nonetheless, one thing is true when it comes to applications of information theory to classical and quantum phenomena: There is a difference. And, in that difference—this talk will argue—lies quantum theory's most direct statement about properties of the world by itself (i.e., the world without the information processing agent).

The title of the talk is actually a phrase popularized by Ari Duwell; in the talk I will give him proper credit.

17-07-03 *Merminism* (to R. Schack)

Just got the note below from David Mermin. Is this man being a smart ass or what? Or is he being serious?

Merminition 120: *Thanks for sending me Probabilism. It does indeed require careful perusal. I shall bring it along to the sea shore next week.*

Meanwhile I note that near the beginning it offers what I would call a dictionary definition of solipsism. I'm also inclined to recommend it to my constructivist sociologist friends who I suspect would find its point of view highly congenial.

17-07-03 *Seven Pines VIII* (to J. Preskill)

Preskillism 8: *Is this a good thing to do?*

I don't know. Probably. In my own case, I'd be interested in talking to Earman, Howard, Milburn, Unruh, and Wald. So it could be worthwhile.

In particular, I'd like to have the chance to field some questions to the GR people ... as the universal characteristics of Hilbert-space dimension have been on my mind, and I find myself wondering to what extent there might be a (conceptual) analogy to gravitational mass and universal gravitation here. Far-fetched in my usual way ... but maybe not completely stupid.

I think it'd be great if you'd be there.

17-07-03 *Big, Big Favor?* (to R. W. Spekkens)

I wonder if you'd mind doing me a big, big favor? I'd like to get my hands on some pretty wacky articles, and I'm a little embarrassed to use the interlibrary loan services of CNRI to help me in the quest.

All the articles appear in a journal called *Journal of Analytical Psychology* and it looks like the library at Laurier (apparently associated with Waterloo) has the right volumes. Could I ask you to bring copies of the articles with you when you come to the Aarhus meeting. (Did the organizers, by the way, give you a talk slot or a poster session slot?)

Let me know if you can, and I won't pursue any other options I might dream up. I'll put the references I need below.

- B. Zabriskie, "Jung and Pauli: A Subtle Asymmetry," J. Analyt. Psych. **40**, 531–553 (1995).
- D. Lindorff, "One Thousand Dreams: The Spiritual Awakening of Wolfgang Pauli," J. Analyt. Psych. **40**, 555–569 (1995).

- D. Lindorff, "Psyche, Matter and Synchronicity: A Collaboration between C. G. Jung and Wolfgang Pauli," J. Analyt. Psych. **40**, 571–586 (1995).
- V. Mansfield and J. M. Spiegelman, "Quantum Mechanics and Jungian Psychology: Building a Bridge," J. Analyt. Psych. **34**, 3–31 (1989).
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18-07-03 *Solipsism Concerns* (to N. D. Mermin)

Referring to

Merminition 121: *I think my problem is not with the subjective view of probability. Between what I've learned from you and Rüdiger, reading around in Jaynes, Cox's famous paper [which I came upon ten years ago — did I ever tell you that I independently derived and solved precisely his functional equation in my "Relativity Without Light" paper (in boojums)], the stuff I read in the Business Library (!) after first meeting Rüdiger, etc., it all makes a lot of sense.*

My problem is, and remains, how it ties in with Quantum Mechanics, which appears (among other things) to tell you how to derive precise probabilities under (apparently) precisely defined — if idealized — circumstances. "Quantum Mechanics Is a Law of Thought" is certainly a good slogan for getting started, and your poetry is very soothing, but I can't help feeling there is still an enormous gap, despite nice things like your quantum de Finetti theorem. Declaring the circumstances also to be subjective judgments appears to be a good (perhaps even necessary) move, but I don't find it convincing and (at best) it seems to lead into some kind of infinite regress in which everything becomes subjective and we're back to solipsism (which is irrefutable and therefore trivial). I haven't been able to articulate what I feel is missing well enough to send it on to you even under the Littlewood-Hardy rules, but I'm still thinking about it.

and

Merminition 122: *Solipsism because when I try to look at things your way I find that whatever I try to condition my subjective probabilities on turns out also to be subjective and conditional on further subjective judgments, ad infinitum.*

and

Merminition 123: *Thanks for sending me Probabilism. It does indeed require careful perusal. I shall bring it along to the sea shore next week.*

Meanwhile I note that near the beginning it offers what I would call a dictionary definition of solipsism. I'm also inclined to recommend it to my constructivist sociologist friends who I suspect would find its point of view highly congenial.

I guess I am seriously concerned by the charge of solipsism you have made of our program. It is a serious charge. (All one has to do is look into the daily news to see the dangers of it. See the Salon article pasted below about G. W. Bush's shenanigans for a particularly moving example.) Why is it that we fail to communicate on this point?

The world is not what I will it to be. And there is nothing in this view of quantum mechanics that our group is trying to construct that hints of this.

Solipsism would come about if from the quantum formalism one could prove

1. that there are no "instruction sets," and
2. that the outcomes of all interventions (measurements) could actually be controlled by the agent instigating the intervention.

But that is not the case. I was thinking harder about this last night as I was doing a little editing on my CG Fire Series, Vol. II. The frontispiece contains a quote from John Wheeler that I'll paste below because it emphasizes precisely the right point. The radical constructivism or solipsism that you fear is blocked for each and every quantum agent by his own stark admission that the outcomes of his interventions are beyond his control. If they are beyond his control, then there is no need to suppose that they are products of his mind. What more needs to be said?

Let me leave it at that for this round. Rüdiger is hoping to construct a note for you within the next three hours or so—hopefully before you leave for the seashore—making explicit reference to some of the dangers we are starting to perceive in the opening sections of Probabilismo … probably precisely the ones that are worrying you. So, stay tuned to your email before leaving for vacation! (You're our most valued customer.)

First the Wheeler quote:

> The Universe can't be Laplacean. It may be higgledy-piggledy. But have hope. Surely someday we will see the necessity of the quantum in its construction. Would you like a little story along this line?
>
> Of course! About what?
>
> About the game of twenty questions. You recall how it goes—one of the after-dinner party sent out of the living room, the others agreeing on a word, the one fated to be a questioner returning and starting his questions. "Is it a living object?" "No." "Is it here on earth?" "Yes." So the questions go from respondent to respondent around the room until at length the word emerges: victory if in twenty tries or less; otherwise, defeat.
>
> Then comes the moment when we are fourth to be sent from the room. We are locked out unbelievably long. On finally being readmitted, we find a smile on everyone's face, sign of a joke or a plot. We innocently start our questions. At first the answers come quickly. Then each question begins to take longer in the answering—strange, when the answer itself is only a simple "yes" or "no." At length, feeling hot on the trail, we ask, "Is the word 'cloud'?" "Yes," comes the reply, and everyone bursts out laughing. When we were out of the room, they explain, they had agreed not to agree in advance on any word at all. Each one around the circle could respond "yes" or "no" as he pleased to whatever question we put to him. But however he replied he had to have a word in mind compatible with his own reply—and with all the replies that went before. No wonder some of those decisions between "yes" and "no" proved so hard!
>
> And the point of your story?
>
> Compare the game in its two versions with physics in its two formulations, classical and quantum. First, we thought the word already existed "out there" as physics once thought that the position and momentum of the electron existed "out there," independent of any act of observation. Second, in actuality the information about the word was brought into being step by step through the questions we raised, as the information

about the electron is brought into being, step by step, by the experiments that the observer chooses to make. Third, if we had chosen to ask different questions we would have ended up with a different word—as the experimenter would have ended up with a different story for the doings of the electron if he had measured different quantities or the same quantities in a different order. Fourth, whatever power we had in bringing the particular word "cloud" into being was partial only. A major part of the selection—unknowing selection—lay in the "yes" or "no" replies of the colleagues around the room. Similarly, the experimenter has some substantial influence on what will happen to the electron by the choice of experiments he will do on it; but he knows there is much impredictability about what any given one of his measurements will disclose. Fifth, there was a "rule of the game" that required of every participator that his choice of yes or no should be compatible with *some* word. Similarly, there is a consistency about the observations made in physics. One person must be able to tell another in plain language what he finds and the second person must be able to verify the observation.

— *John Archibald Wheeler*
Frontiers of Time, 1979

And, now from Joe Conason's Journal in *Salon*: "President Bush's Astonishing New Reason for the War with Iraq: Saddam Wouldn't Let Weapons Inspectors In."

> July 15, 2003 — A "darn good" quote that almost nobody quoted "We gave him a chance to allow the inspectors in, and he wouldn't let them in."
>
> George W. Bush uttered that amazing sentence yesterday to justify the war in Iraq, according to the Washington Post.
>
> What? Yes, I promise that's what the man said. (And by "him," the president clearly meant Saddam Hussein – not Kim Jong Il, who actually has refused to let international inspectors into North Korea.)
>
> Now a presidential statement so frontally at variance with the universally acknowledged facts obviously presents a problem for the White House press corps. He wasn't joking, and he didn't sound disoriented or unwell. Although Dana Priest and Dana Milbank wrote the story as delicately as they possibly could, they couldn't make it seem less weird:
>
> "The president's assertion that the war began because Iraq did not admit inspectors appeared to contradict the events leading up to war this spring: Hussein had, in fact, admitted the inspectors and Bush had opposed extending their work because he did not believe them effective."
>
> Appeared to contradict the events leading up to war? Indeed, that's an exceedingly mild description of what Bush said. There's no plausible explanation, unless the president suddenly flashed back to his Yale sophomore philosophy seminar, grappling with the argument that everything we perceive is mere illusion.
>
> For the moment, however, let's just assume reality does exist. What possessed the president to make an assertion that everyone on the planet knows to be untrue? . . .

18-07-03 *Most In One Place* (to N. D. Mermin & R. Schack)

As I've told both of you, I'm starting to put together another samizdat—this one to contain the last year's discussions. Let me drop it off with you as it stands at the moment. Even though it's not complete you might find it a little useful. I think it already contains all of the emails from my side in the latest round of discussions … if that's useful.

Parts of it are certainly diatribic … and needlessly so. But on the other hand, there are some notes—the note of 10 January 2003, "Filth Under the Rug," comes to mind—that I think express a truly heartfelt frustration.

David is right: "'Quantum Mechanics Is a Law of Thought' is certainly a good slogan for getting started, and your poetry is very soothing, but I can't help feeling there is still an enormous gap, despite nice things like your quantum de Finetti theorem." There is so very much that needs to be done by way of technical work. But that should not stop anyone from seeing that we are moving absolutely in the right direction. Just precisely what is it that is blocking the vision?

19-07-03 *Definitions from Britannica* (to N. D. Mermin)

Merminition 124: *As far as I know solipsism is the claim that there is nothing more than my own sense impressions. It does not imply that I can control what those sense impressions are. They could be like a film I am doomed to keep watching — or an uncontrollable dream.*

I believe that's the correct use of the term, but if I'm wrong, give my "serious charge" another name. Perhaps this clarification makes it less serious? Hope not.

I looked up several definitions for the heck of it. The first set below comes from the 2001 *Encyclopedia Britannica*. Following that, I place my search results from several dictionaries for the word solipsism in particular. I guess the definition from *Wikipedia* best captures what I thought the word meant.

In any case, regardless of the labels, David's biggest worry seems to be this:

Merminition 125: *A) Solipsism because when I try to look at things your way I find that whatever I try to condition my subjective probabilities on turns out also to be subjective and conditional on further subjective judgments, ad infinitum.*

Let me try to tackle this one directly, now that maybe I understand your worries better and after having talked to Rüdiger yesterday. Take a good solid physicist like Steven Weinberg who stakes his career on the search for a grand unified field theory. Suppose he finds it. To find it (I presume) is to declare: The world's Lagrangian is L. Now suppose I were to ask Mr. Weinberg, "Why L? Why not M?" I know for sure his answer will be of the form, "L just is. It is the starting point. It is an ultimate fact of nature; it calls for no explanation. In any case, if it calls for an explanation, its answer must come from outside the realm of science—religion? theology?—but I see no reason to go to such lengths."

Would that make Weinberg a solipsist? A sensationalist? A phenomenalist? The point is Weinberg's stance has nothing to do with any of these labels.

Similarly for the Bayesian (even of the de Finettian variety, despite the mumbo jumbo in the opening sections of Probabilismo). For him, "the prior" on any event space is treated as an ultimate fact—an ultimate fact about the agent. There is no infinite regress because, just as with Weinberg, ultimate facts call for no further explanation.

Bayesian practice—and Rüdiger and I would claim the formal structure of quantum mechanics too—is all about what to do once a prior is established. It is not about what to do before the prior is established. In the quantum mechanical case, establishing "a prior" is 1) to write down a quantum state for all systems considered, and 2) to write down a (conditional) quantum operation for all measuring devices considered.

If there are two agents, there may well be two priors in the sense above —i.e., two ultimate facts (with respect to this level of inquiry). In that sense, the priors are "subjective", but that does not take away their status as ultimate facts in this treatment. It only calls for a recognition that the facts are about the agents.

The role of the separate system and measuring device—now specializing on quantum mechanics—is that when the two are combined they give "birth" to a new ultimate fact: The "click." There is no sense, however, in which this new ultimate fact is *about* either of the agents: It has a life of its own. (In fact, it is because of these lines of thought that I sometimes call my view "the sexual interpretation of quantum mechanics.")

Does this clarify anything? Does this in any way address your fears of solipsism/sensationalism/phenomenalism?

Definitions below.

> **Solipsism:**
>
> in philosophy, formerly, moral egoism (as used in the writings of Immanuel Kant), but now, in an epistemological sense, the extreme form of subjective idealism that denies that the human mind has any valid ground for believing in the existence of anything but itself. The British idealist F. H. Bradley, in *Appearance and Reality* (1897), characterized the solipsistic view as follows:
>
> "I cannot transcend experience, and experience is my experience. From this it follows that nothing beyond myself exists; for what is experience is its (the self's) states."
>
> Presented as a solution of the problem of explaining human knowledge of the external world, it is generally regarded as a reductio ad absurdum. The only scholar who seems to have been a coherent radical solipsist is Claude Brunet, a 17th-century French physician.
>
> **Subjective Idealism:**
>
> a philosophy based on the premise that nothing exists except minds and spirits and their perceptions or ideas. A person experiences material things, but their existence is not independent of the perceiving mind; material things are thus mere perceptions. The reality of the outside world is contingent on a knower. The 18th-century Anglo-Irish philosopher George Berkeley succinctly formulated his fundamental proposition thus: Esse est percipi ("To be is to be perceived"). In its more extreme forms, subjective idealism tends toward solipsism, which holds that I alone exist.
>
> **Sensationalism:**
>
> in epistemology and psychology, a form of Empiricism that limits experience as a source of knowledge to sensation or sense perceptions. Sensationalism is a consequence of the notion of the mind as a tabula rasa, or "clean slate." In ancient Greek philosophy, the Cyrenaics, proponents of a pleasure ethic, subscribed unreservedly to a sensationalist doctrine. The medieval Scholastics' maxim that "there is nothing in the mind but what was previously in the senses" must be understood with Aristotelian reservations that sense data are converted into concepts. The Empiricism of the 17th century, however—exemplified by Pierre Gassendi, a French neo-Epicurean, and by the Englishmen Thomas Hobbes and John Locke—put a greater emphasis on the role of the senses, in reaction against the followers of René Descartes who stressed the mind's faculty of reasoning. Locke's influence on 18th-century French philosophy produced the extreme sensationnisme (or, less often, sensualisme) of Étienne Bonnot de Condillac, who contended that "all our faculties come from the senses or more precisely, from

sensations"; that "our sensations are not the very qualities of objects [but] only modifications of our soul"; and that attention is only the sensation's occupancy of the mind, memory the retention of sensation, and comparison a twofold attention.

Phenomenalism:

a philosophical theory of perception and the external world. Its essential tenet is that propositions about material objects are reducible to propositions about actual and possible sensations, or sense data, or appearances. According to the phenomenalists, a material object is not a mysterious something "behind" the appearances that people experience in sensation. If it were, the material world would be unknowable; indeed, the term matter itself would be unintelligible unless it somehow could be defined by reference to sense experiences. In speaking about a material object, then, reference must be made to a very large group or system of many different possibilities of sensation. Whether actualized or not, these possibilities continue during a certain period of time. When the object is observed, some of these possibilities are actualized, though not all of them. So long as the material object is unobserved, none of them is actualized. In this way, the phenomenalist claims, an "empirical cash value" can be given to the concept of matter by analyzing it in terms of sensations.

Positivism:

in philosophy, generally, any system that confines itself to the data of experience and excludes a priori or metaphysical speculations. More narrowly, the term designates the thought of the French philosopher Auguste Comte (1798–1857). The basic affirmations of Positivism are (1) that all knowledge regarding matters of fact is based on the "positive" data of experience, and (2) that beyond the realm of fact is that of pure logic and pure mathematics, which were already recognized by the Scottish Empiricist and Skeptic David Hume as concerned with the "relations of ideas" and, in a later phase of Positivism, were classified as purely formal sciences. On the negative and critical side, the Positivists became noted for their repudiation of metaphysics; i.e., of speculation regarding the nature of reality that radically goes beyond any possible evidence that could either support or refute such "transcendent" knowledge claims. In its basic ideological posture, Positivism is thus worldly, secular, antitheological, and antimetaphysical. Strict adherence to the testimony of observation and experience is the all-important imperative of the Positivists. This imperative is reflected also in their contributions to ethics and moral philosophy, and most Positivists have been Utilitarians to the extent that something like "the greatest happiness for the greatest number of people" was their ethical maxim. It is notable, in this connection, that Auguste Comte was the founder of a short-lived religion, in which the object of worship was not the deity of the monotheistic faiths but humanity.

Further definitions of Solipsism:

- **Merriam-Webster Online Dictionary:** a theory holding that the self can know nothing but its own modifications and that the self is the only existent thing.
- **Cambridge International Dictionary:** the belief that only one's own experiences and existence can be known with certainty.
- **American Heritage Dictionary:** 1. The theory that the self is the only thing that can be known and verified. 2. The theory or view that the self is the only reality.

- **Encyclopedia Britannica, 1911 edition:** a philosophical term, applied to an extreme form of subjective idealism which denies that the human mind has any valid ground for believing in the existence of anything but itself. It may best be defined, perhaps, as the doctrine that all "existence is experience, and that there is only one experient. The Solipsist thinks that he is the one!" (Schiller). It is presented as a solution of the problem of explaining the nature of our knowledge of the external world. We cannot know things-in-themselves: they exist for us only in our cognition of them, through the medium of sense-given data. In. F. H. Bradley's words (Appearance and Reality):

  "I cannot transcend experience, and experience is my experience. From this it follows that nothing beyond myself exists; for what is experience is its (the self's) states."

- **Wikipedia:** Solipsism is a metaphysical belief that one is like a God, creating the reality in which one exists. Solipsism is logically coherent, but not falsifiable, so it cannot be established by current modes of the scientific method.

  The classic objection to solipsism is that people die. However, you have not died, and therefore you have not disproved it.

  A further objection is that life causes pain. Why would we create pain for ourselves? There may be some reason which we have decided to forget, such as the law of Karma, or a desire not to be bored.

  Solipsism is a common theme in eastern philosophy. Various interpretations of Buddhism, especially Zen, teach that the entire universe exists only in one's mind.

24-07-03 *Subjective and Objective, Precursor* (to A. Sudbery)

I promised to say something a little more scientific this afternoon, rather than administrative. But time slipped away, and I ended up doing some mathematics with Chris King at the chalkboard. Consequently, I didn't get Nagel's article finished, which I started on the train yesterday.

But let me build up to a discussion on it (and then after it, on your paper 'Why Am I Me?' in the coming weeks). Mostly, I just want to say that, so far, it's not looking so good for Mr. Nagel in my eyes: I'm finding that I'm not liking the paper as much as I was hoping to. Of course, I could have guessed that I would get tangled in a sparring match with it . . . but I didn't think I would get tangled in disagreement even with the motivation it sets out in the first couple of pages.

Let me try to show you why. Attached is an excerpt, relevant to the present subject, from my smaller samizdat "Quantum States: What the Hell Are They?" posted on my webpage. [See notes to Wiseman starting on pages 210 and 217 respectively.] I hope it shows why I would be resistant to an 'external view' at the very outset. I will certainly read Nagel thoroughly before this is all over with, but already on the very first pages it looks that we come from very different worlds.

More eventually . . .

25-07-03 *Agendas and Rubrics* (to R. W. Spekkens)

Spekkensism 19: *These observations led me to wonder whether it might be impossible to achieve a HVT with non-contextuality for the preparations for any dimensionality of Hilbert space. I believe I have now proven such a no-go theorem using nine states from a 2d Hilbert space. I'll try to write up the proof tonight.*

If it stands, I hope that this result may serve as another clue in the mystery, another aid in determining the nature of the conceptual innovation that needs to occur before one can devise a proper realist interpretation of quantum theory, if such an interpretation can be had.

What a novel idea! I like the technical part.

Question: What does it take to be a proper realist interpretation of quantum theory?

I never did finish that note on black holes to you; I'll paste the point I got up to in it below. The only point I wanted to make (but I wanted to do it in style!) was that, in my thinking about quantum mechanics, Hilbert-space dimension plays a conceptual role that is a bit like black-hole mass. I.e., Hilbert-space dimension is the ontic state of a quantum system.

Now, why wouldn't that fall under the rubric of "proper realist interpretation" in your eyes? I'm guessing that it won't. In particular, I'm guessing that it has something to do with the "Common Fear?" I wrote you and Mermin about a couple of weeks ago. But, I don't know, you tell me.

— — — — — — — — — — —

From just before the IBM meeting:

I spent a little while digging around in my email archive this morning and was finally able to pull out this piece from June 7, 1991. It was in a letter to Greg Comer.

> The other night I had a dream in which I was trying to calculate how many black holes would fit in my ice chest. I could already see one in there and I was trying to figure out how many more would fit. They're really beautiful, you know—contrasting against the white of the ice chest and the glistening of the ice. So, I pulled out another beer and watched for a while.

Consider a Schwarzschild black hole, to the exclusion of all else in the universe. What is the black hole's ontic state?

There is only one thing to pin it on: It is the black hole's mass, M. Do you balk at that? Does the starkness of the black hole's characterization specify a dearth or emptiness of phenomena associated with this lovely physical object? Not at all, once the black hole is embedded within the wider context of general relativity, the orbits of various test bodies, and so forth.

Especially in light of the immense physics that comes from such a single, simple number M when ... [Never finished.]

25-07-03 *Short Replies* (to G. L. Comer)

Comerism 6: *I like that word "interlocutor."*

I'm not so taken with it. It's probably because I'm not so taken with the "asking questions of nature and getting answers back" imagery any more. I'm more taken with the imagery of "push on nature and see how it reacts." We never ask of nature, we only push on it—or at least that's my take at the moment. (It's the reason I'm calling my CG Fire Series, Vol. II, project "The Activating Observer".) But like my philosophy, I can be malleable.

25-07-03 *No, Thank You!* (to J. Honner)

Thanks for the nice compliments/complements. Why did you leave academia? Not only did I enjoy your Bohr/Derrida article, but many years before that, I enjoyed your book on Bohr—reading it was one of my formative experiences in the field. I'd love to get hold of it again, as now in working on my "The Activating Observer: Resource Material for a Paulian/Wheelerish Conception of Nature" project (partially posted at my webpage) I'm going through many old things once again, but with my own fine-toothed comb. (Though, I guess I have to admit, I did make a bit of fun of your book and its genre in my paper `quant-ph/0104088`. You can see what I mean in endnote 5 and the text leading to it.)

By the way, if you think you might get something out of `Notes on a Paulian Idea`, I'll have a copy sent your way. Växjö University Press published 100 copies (in proper book form) for my use, and I've still got about 30 copies to give away.

Anyway, it was really great hearing from you, and you can bet I'll probably consult with you again when I'm in the stages of tidying up "The Activating Observer" project.

25-07-03 *Question* (to V. Kargin)

Karginism 1: *I am trying to find out who said: "Probability theory is measure theory with a soul". It appears that some attribute this quote to Mark Kac and some to Kolmogoroff. I noticed that you used a couple of times this quotation in your papers. Can you help me in locating the source?*

Indeed it was Mark Kac; I'm sorry if I caused any confusion in my writings. In my review of Holevo's book, I wrote:

> It is a probabilistic model that differs radically in character from the classical model first laid down by A. N. Kolmogorov in 1933 as "measure theory with a soul."

but the way that sentence should be parsed is the following:

> It is a probabilistic model that differs radically in character from [the classical model first laid down by A. N. Kolmogorov in 1933] as ["measure theory with a soul."]

I didn't mean to imply that the phrase should be attributed to Kolmogorov. Basically I was leaving the attribution of the phrase blank so as not to complicate the sentence too much. Maybe that was a mistake.

Anyway, here is the exact quote attributed to Mark Kac that I found in Holevo's book:

> "Probability theory is a measure theory – with a soul."

I wish I knew the exact source. Perhaps Alexander Holevo would know.

Slava's Reply

> Thank you for your reply. I didn't mean to imply that the phrase in your review is misleading. I have seen the attribution to Kolmogoroff somewhere else.
>
> I actually located the source of this quote. It is from a preface by Mark Kac to a book by Luis Santalo *Integral Geometry and Geometric Probability*. Here is how it goes:
>
> > Above all the book should remind all of us that Probability Theory is measure theory with a "soul", which in this case is provided not by Physics, or by games of chance, or by Economics but by the most ancient and noble of all mathematical disciplines, namely Geometry.

25-07-03 *Relative Onticity* (to R. Schack)

There are two points I like in your last note:

Schackcosm 71 : *What makes all this exciting for me is the fact that there appears to be a straightforward logical argument from your "Is the Moon There?" article to de Finetti's position. Particles don't carry instruction sets. Hence $|x\rangle$ is subjective.*

and

Schackcosm 72 : *I believe that you get an infinite regress only if you try to find the objective ground behind your probability assignments. This is futile. Your infinite regress argument is an argument FOR the subjective viewpoint. The latter is not solipsism, but simply the realization that our description in terms of maths is not the same thing as the external reality.*

Let's see what kind of impact they'll have on Mr. Mermin now.

I thought a little more about our phone conversation on my walk to work this morning—in particular, your point about, "Why not let Mr. Caves (under the appropriate conditions) act AS IF his Hamiltonians are real? What's to be lost by it?" I think the thing to be lost is our hard won category distinction between A) the probability function $P(x)$ and B) the values x of its argument. I.e., the beliefs and the facts. Even when the Bayesian gives an "as if" theorem, like with the de Finetti theorem, he never drops this category distinction. I.e., he does not confuse the data he is gathering (which with the assumption of exchangeability will allow him to asymptotically settle on a product probability distribution) with the updated belief.

At best, the "as if" theorems can give us something like the new semi-ontic category that Shimony seeks: the potentia. That is to say, there are "actualities" represented by the CLICKS, but then there are the "potentia" represented by the probabilities of which we can act AS IF they are objective.

I continue to think that it is of ultimate importance for this program to not let Mr. Caves go only halfway. Where we are heading, I think, is toward a wholly new kind of ontology—despite what looks to me like your present attraction to sensationalism. For that task, we just shouldn't stop halfway. I see us discovering and finding convincing explanation for the idea that the world is truly on the make. To stop with the "as ifs" that we can all agree upon (presumably after inculcation by the right physics community)—let us say they really are Hamiltonians—strikes me overpoweringly as an effort to once again introduce an overriding stasis to the universe. The real, unchanging ground of the universe is *the* Hamiltonian, full stop. And what is a Hamiltonian? In effect, it is the potentia, or even a propensity. (I.e., it is the generator of a conditional probability which has been given an objective status by fiat.)

Here's maybe another way of putting what I fear. If we stop with identifying the conditions under which one can act AS IF, then at best we will draw a new picture of existing quantum mechanics. We will give new names to old prejudices. It is simply not radical enough for my tastes; when we solve the old problem of quantum interpretations, we must get something new. At the very least a new world view, but I suspect something much more substantial and technical than that.

But, anyway, why am I any better off with my Hilbert-space dimension? Surely there is a sense in which it too is a subjective judgment. True enough. But then, why do I not see myself as falling into the same trap as Mr. Caves? I want to claim that in opposition to the Hamiltonian which is on the A) side of the category distinction above, Hilbert-space dimension should be more rightly thought of as on the B) side. Hilbert-space dimension should be thought of as taking the role of cardinality of the sample space in classical probability theory. And as you and I have already discussed, the setting of a sample space (classically) is certainly a subjective judgment. But nevertheless the elements within the sample space play the role of (potential) FACTS, not BELIEFS, after the setting is made. So, it is a relative onticity of a sort, but that is all I have ever been trying to capture with my "objective with respect to the theory" (or whatever phrase I used to use).

Enough. I'll say more when I can say more. Whereof one cannot speak, thereof one must be silent ...

28-07-03 *Popescu-Rohrlich Correlations, Gleason, Our Stuff* (to H. Barnum)

Barnumism 5: *Actually, maybe Chris and/or Joe's work, if I recall correctly, has to do with perverted Gleason's theorems in which nonorthogonal sets of vectors (with specific angle-sets) are identified with measurements having mutually exclusive outcomes.*

You're right, but that was mostly work with Nurit Baytch. Sadly, I never got off my duff to publish it, and now it's not particularly timely (at least in my own head). Also the CG Fire put an end to the records of the calculation I had, which involved a lot of nasty stuff about spherical harmonics.

I think the result was the following. Consider qutrits. Suppose for whatever weird reason you wanted to consider three-outcomed "measurements" whose outcomes are associated with vectors that are angles α, β, and γ (all fixed) away from each other. In the usual notion of von Neumann measurement $\alpha = \beta = \gamma = 90°$. Now, try to build a concept of frame function for this kind of measurement and try to prove a Gleason theorem. The upshot was that the *only* frame function existing in such cases were the uniform ones (i.e., flat functions), except in the particular case that $\alpha = \beta = \gamma = 90°$ in which one recovers the usual Gleason theorem. [The style of problem is captured in my big samizdat, `quant-ph/0105039`, pages 86 through 88.]

I guess I'm not as interested in the result as before. It came from a time when POVMs looked like particularly arbitrary structures to me. Now though that I think I understand the deeper connection between the structure of measurements and Bayes' rule, I guess I wouldn't be going down such lines.

28-07-03 *Tickles and Wiggles* (to H. Barnum)

By the way, in the evenings before bed, I've been finally reading Thomas Nagel ... since both you and Tony Sudbery have pushed me toward it. I hope to give you a report of my thoughts on it in the decently near future.

28-07-03 *Understanding Buridan* (to R. Schack)

Schackcosm 73: *I think this is usually quoted in discussions about free will versus determinism. I like it because it illustrates the absurdity of indeterminism.*

Does this mean that deep underneath it all you're a determinist who believes in instruction sets? This worries me a bit.

28-07-03 *New Twenty Questions* (to R. Schack)

Schackcosm 74: *Presently I am very much under the antirealist spell of de Finetti. The way I understand de Finetti, indeterminism is just as meaningless as determinism. But maybe I am taking the first three paragraphs of Probabilismo too seriously…*

The lines below are from my paper `quant-ph/0204146`. Part of them, even I am in disagreement with now. But the part that gives a definition of indeterminism, I am still OK with. Are you OK with that, or do you still contend that it is meaningless?

> In choosing one experiment over another, I choose one context over another. The experiment elicits the world to do something. To say that the world is indeterministic means simply that I cannot predict with certainty what it will do in response to my action. Instead, I say what I can in the form of a probability assignment. My probability assignment comes about from the information available to me (how the system reacted in other contexts, etc., etc.). Similarly for you, even though your information may not be the same as mine. The OBJECTIVE content of the probability assignment comes from the fact that *no one* can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation *rules* from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments *within* each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!

28-07-03 *New Twenty Questions, 2* (to R. Schack)

Different question, but again referring to this:

Schackcosm 75: *Presently I am very much under the antirealist spell of de Finetti. The way I understand de Finetti, indeterminism is just as meaningless as determinism. But maybe I am taking the first three paragraphs of Probabilismo too seriously…*

What do you say about Mermin's anti-instruction-set derivation? Is that now contentless (at worst) or superfluous (at best) from this anti-realist view?

28-07-03 *Your Newest Turn* (to R. Schack)

I did truly get into quite a bit of trouble tonight, not getting home until 7:30. In general it's just been a very bad day for me.

Let me give you an example of something I don't think you can allow yourself to say in your newest turn.

When I wrote my ill-fated note "Got It!," I wrote in Section 4 of it:

> 4) POVMs and radical pluralism.
>
> Now let me go into a bit of the metaphysics of this. Here's a point of view that I'm finding myself more and more attracted to lately.

I think it is safe to say that the following idea is pretty commonplace in quantum mechanical practice. Suppose I measure a single POVM twice—maybe on the same system or two different systems, I don't care—and just happen to get the same outcome in both cases. Namely, a single operator E_d. The common idea, and one I've held onto for years, is that there is an objective sense in which those two events are identical copies of each other. They are like identical atoms ... or something like the spacetime equivalent of atoms. But now I think we have no warrant to think that. Rather, I would say the two outcomes are identical only because we have (subjectively) chosen to ignore almost all of their structure.

That is to say, I now count myself not so far from the opinion of Ulfbeck and Bohr, when they write:

> The click ... is seen to be an event entirely beyond law. ... [I]t is a unique event that never repeats ... The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety .... [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

For though I have made a logical distinction between the role of the d's and the E_d's above, one should not forget the very theory-ladenness of the set of possible d's. What I think is going on here is that it takes (a lot of) theory to get us to even recognize the raw data, much less ascribe it some meaning. In Marcus Appleby's terms, all that stuff resides in the "primitive theory" (or perhaps some extension of it), which is a level well below quantum mechanics. What quantum mechanics is about is a little froth on the top of a much deeper sea. Once that deeper sea is set, then it makes sense to make a distinction between the inside and the outside of the agent—i.e., the subjective and the objective—as we did above. For even in this froth on the top of a deeper sea, we still find things we cannot control once our basic beliefs—i.e., our theory—are set.

Without the potential d's we could not even speak of the possibility of experiment. Yet like the cardinality of the set of colors in the rainbow—Newton said seven, Aristotle said three or four—a subjective judgment had to be made (within the wide community) before we could get to that level. If this is so, then it should not strike us as so strange that the raw data d in our quantum mechanical experience will ultimately be ascribed with a meaning E_d that is subjectively given. (I expressed some of this a little better in a note I wrote to David last month; I'll place it below as a supplement.) More particularly, with respect to the EPR example above, it should not strike us as odd that the phenomenon comes about solely because of an interpretive convention we set: All quantum measurement outcomes are unique and incomparable at the ontic level. At least that's the idea I'm toying with.

to which you replied at the time:

Schackcosm 76 : *Section 4), I like. It highlights the chasm that exists between our approach and the many-worlders and decoherence people. We all agree that a click is not an elementary phenomenon. They want to reduce it to something more fundamental. We say it's irreducible (which does not mean that we cannot analyze a measurement apparatus or decoherence of a quantum register in as much depth and detail as anybody else). Your metaphysical bit is nice in that it makes clear that "irreducible" is not the same thing as "elementary". As you say it,*

> *All quantum measurement outcomes are unique and incomparable at the ontic level.*

In your newest turn, such discussion would be "meaningless"—correct? You would no longer allow yourself to contemplate what the CLICK d might be in its own essence. You would no longer say that "successive clicks with a single value d are truly the same," nor would you contemplate that "successive clicks with a single value d may actually be truly different."

That is to say, in this new philosophy you are toying with, if something (i.e., an event, a fact, etc.) is not a "hook" upon which a probability can be conditioned, you are not willing to speak of it. It is meaningless—I believe you say. You don't even let yourself conjecture about the stuff that is out there independently of us. (Or at least I don't see how you're going to be able to do this with such a strongly positivistic line.)

As I tried to express today, and as I'm trying harder to articulate now, I guess I don't like that.

Pragmatists are not positivists, but more opportunists. James allowed personal religions to be parts of reality, and I guess I'm inclined to that.

The part of de Finetti's introductory section that I do like is his organicist take on scientific theories. I'm inclined to view the PRESENT scientific theory of any type as part of the specification of our species. Such theories are part of the account of our being at the moment; they express our possibilities and our limitations. By this account, for instance, Hamilton and Lagrange belonged to ever slightly different species than you and me. Silly, huh? I don't think so: For what's important about this is that one sees by it that a theory (and the entities within it) are every bit as real or unreal as biological species. Whatever they are, they have a temporary hold on our description of things that cannot be denied. Are they all and only AS IF statements?

I guess I don't think so. In other words, I guess I'm saying that I think you're going too far with your ASIFism. As far as I can see, all a representation theorem (say of the de Finetti type) can give us is the conditions under which we can act AS IF our impredictability is coming from a true but unknown PROPENSITY. In particular, what the AS IF theorems do not give are the sample spaces. They are always set before these representation theorems can be posed at all. And, in that way, I say sample spaces obtain a status—though subjective in the character of how they are set—that is somehow different from what a representation theorem can give.

My goal continues to be ultimately realist in tone. All I have ever wanted to do in these last couple of years is strip away the objective character of all probability statements and any part of quantum mechanics that smells of being a probability statement in disguise. I have never imagined (nor do I think we have any warrant from anything technical within probability theory) that the whole structure of QM will go up in smoke in the process.

By the way—on a slightly different subject—let me readdress one of the points in your Buridan's Ass note again:

Schackcosm 77: *Well, Buridan's ass stands in front of two buckets of hay, neither of which looks to him any better than the other. Hence the poor ass remains standing there and eventually dies. I think this is usually quoted in discussions about free will versus determinism. I like it because it illustrates the absurdity of indeterminism. The state* $|R\rangle + |L\rangle$ *is symmetric with respect to left and right. If nature really IS in a symmetric state with respect to left and right, then what breaks the symmetry? If it is our state of belief, however, that is symmetric, there is no problem. The ass chooses one or the other, it's me who has no clue which one it will be. Either one or the other detector goes click, it's me who has no clue which one it will be.*

I would say the very lesson of Bell and Kochen-Specker is that we cannot (or rather should not) act AS IF nature is not at a juncture in the quantum measurement setting. "Unperformed Measurements Have No Outcomes."

29-07-03 *Wiggles and Bait* (to H. Barnum)

More about Nagel coming soon. Sudbery has prodded me further out of my slumber (yesterday) ... and I can feel those natural defense mechanisms kicking in! The email was starting to form in my head as I was walking in to the office today!

29-07-03 *Please Do* (to J. Honner)

Honnerific 1: *In 1995-7 I started writing what I thought was a half interesting book* [Bohr, Einstein, Bell, and the Feminist Critique of Physics] *... on physics and the unconscious basically ... but after three rejections of the core article I pulled my head in and changed my life. If you can bear reading the feminist critique of physics and a fairly devastating set of reflections on Einstein, I'd be happy to dig out a file and send it to you.*

Yes, please do send it! Chances are I won't read it immediately, but it'll be in the queue then (and it may get a citation in the "Activating Observer" compendium). When I do get to it, chances are I'll give you some feedback.

Also, by the way, if you could give me a complete list of all your quantum publications that would be most helpful.

30-07-03 *Title* (to C. King)

"Two Characterizations of Complete Positivity that Evoke No Imagery from the Everett Interpretation of Quantum Mechanics"

30-07-03 *Convictions, Courage, Clear Thinking* (to R. Schack)

Your note was so chock-full, I had quite a sleepless night. How I wish I had time to reply to you right now in great detail—it is something I need internally, there is no better therapy for me than composing a note—but it is going to have to wait most likely until I am in Germany.

I think you were right on most (or at least many) counts. In particular, I think this discussion represents a lack of courage on my part, along with a small lack of clear thinking: The former likely caused the latter. Funny how many times I have accused Carl (and sometimes you) for not having the courage of your convictions. Stones and glass houses.

Also there is a slight problem of emphasis and particular choices of words that might be getting in our way—i.e., that may be one of the things that helped shunt us from complete and immediate agreement. These things need fleshing out.

Mostly I want to say for the moment that your note inspired me, and I think (maybe unseen to you) it gives me a way to have my cake and eat it too.

I will be back with a vengeance in a few days (for hopefully an adequate mix of email and verbal communication) ...

In the meantime, though, would you think about the tension in these three statements of yours:

Schackcosm 78: *Well, Buridan's ass stands in front of two buckets of hay, neither of which looks to him any better than the other. Hence the poor ass remains standing there and eventually dies. I think this is usually quoted in discussions about free will versus determinism. I like it because it illustrates the absurdity of indeterminism. The state* $|R\rangle + |L\rangle$ *is symmetric with respect to left and* *right. If nature really IS in a symmetric state with respect to left and right, then what breaks the symmetry? If it is our state of belief, however, that is symmetric, there is no problem. The ass chooses one or the other, it's me who has no clue which one it will be. Either one or the other detector goes click, it's me who has no clue which one it will be.*

and

Schackcosm 79: *NOOOOOOO! You are joking, aren't you? The direct lesson of Bell and Kochen-Specker is indeed that we should not act AS IF unperformed measurement had outcomes. To conclude that nature is at a juncture is adding a lot of baggage, however. I am sure de Finetti would be rotating in his grave if he read this!*

and

Schackcosm 80: *What de Finetti's anti-realism attacks is the prejudice that to the terms of our description literally corresponds some real stuff out there in nature. Isn't it ridiculous, he asks, to conclude that nature is in the situation of Buridan's ass, only because we aren't certain about what will happen next? Does that mean de Finetti believes in instruction sets? I think this would do him injustice.*

I'm with you on the middle one at the moment. But the three together sure look like a tense mix to me. What actually would do de Finetti justice on this issue? It is not clear to me at all.

There's a lovely passage in an 1872 diary entry of William James: I wish I had it here to get the exact wording. It was something of the order, "my first act of free will shall be to believe in free will ... I will give it a shot for one year and see where that gets me."

01-08-03 *Mortified, Yes* (to N. D. Mermin)

Merminition 126: *SHPMP Sept 2003 just arrived.*

Well I knew you hated Copenhagen Computation*, but making it the one article in the collection that you don't say a word about in the Introduction seems a bit much, particularly since your theme is the vindication of Bohr over Einstein. Putting it last underlines the omission. It will be clear to readers that I foisted it on you. (As I remember I didn't want to send in anything, but you kept insisting...)*

I love you anyway. Actually I assume it was an accident — Freudian, of course — and trust that you are properly mortified.

I very, very, very much apologize. I feel like utter crap. How I could let that happen I do not know. Jeff wrote the first draft, and I jiggled some things in it, but that I did not notice the omission is inexcusable.

All I can say now is that I apologize, and I've got to find a way to make it up to you in some other aspect of life.

It is not true that I did not like your paper. I liked it a lot, and I especially liked your presentation at the Bennettfest. It seems to me that you get to the essential point when you point out the non-problematicity of the ultimate readoff: it is no more or less mysterious than reading off of a piece of information from a computer screen. Either they are both deep problems, or neither of them are. Showing that that is the best way to teach quantum computing to CS people makes a deep statement ... and you're in tune with it.

Please do forgive me. It wasn't intentional, and it wasn't even Freudian.

01-08-03 *Done* (to A. Peres)

Thanks! What will you speak on in Aarhus? Do you know if Petra has prepared a poster for the poster session? I would recommend to you, by the way, Rob Spekkens' talk. I think it is particularly deep, and the work has not been posted on `quant-ph` yet. Here is what I wrote to the organizers in my efforts to get him a speaking slot (at a late date): [See 01-07-03 note "Rob Spekkens" to R. D. Gill, K. Mølmer, and E. S. Polzik.]

01-08-03 *Spekkens and Letter* (to A. Peres)

Asherism 36 : *I believe I saw something like you told me about Spekkens in* `quant-ph`*, or in* Found. Phys. Lett.*, or elsewhere. I can't remember. I regret I have no patience for these games.*

But you should. The whole point of Spekkens' toy model is to bolster our point that a quantum state is a state of knowledge. The tactic is to show the extreme similarities between quantum mechanics and Liouvillian mechanics—a point I first learned from you. The thing that is new in Spekkens' game is that he not only uses Liouville mechanics, but he adds a further restriction that the distributions cannot be too peaked. With that he gets a theory that looks ever so much more (but not completely) like quantum mechanics.

The point to be made for people like Gisin and Jozsa who don't find quantum mechanics as palatable as they should: Quantum mechanics is not so very weird and different from plain old Liouville mechanics. That community should quit beating their heads trying to find nonlinear evolution equations and such.

I doubt you saw something like this (or at least of the prettiness of Spekkens' result) in *Found. Phys. Lett.*

Give it a chance. It is really very good and very creative work.

12-08-03 *Me, Me, Me* (to N. D. Mermin & R. Schack)

Me, me, me; it's always about me! —Yes. But nonetheless it is simply not solipsism. Let me explain.

I guess I was actually fortunate today: For the second time in a month, I was called a solipsist by one of my friends. (This time the accuser was Howard Wiseman.) On top of that, Asher Peres gave a talk this morning that made me cringe, saying things like, "When no one performs a measurement, nothing happens [in the world]." The combination of these two bad experiences caused me to wander the streets of Aarhus this afternoon in spite of the horrible heat. I suppose I needed to find a way to sweat the poisons from my body.

The fortune in this is that it caused me once again to strive for a clearer and more consistent form of expression. I want to try to capture some of that in this note. Mostly it is about not allowing oneself to get hung up in someone else's (inconsistent) expectations for what quantum theory ought to be.

In our 2000 opinion piece in Physics Today, Asher and I wrote:

> The thread common to all the nonstandard "interpretations" is the desire to create a new theory with features that correspond to some reality independent of our potential experiments. But, trying to fulfill a classical worldview by encumbering quantum mechanics with hidden variables, multiple worlds, consistency rules, or spontaneous collapse, without any improvement in its predictive power, only gives the illusion of

> a better understanding. Contrary to those desires, quantum theory does *not* describe physical reality. What it does is provide an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions. This strict definition of the scope of quantum theory is the only interpretation ever needed, whether by experimenters or theorists.

But that is misleading and trouble-making. In the second to last sentence—with the experience of three more years of thinking on this subject—I so wish we had said something more to the tune:

> What quantum theory does is provide a framework for structuring MY expectations for the consequences of MY interventions upon the external world.

At least that is what the formal structure is about. There is no "we," there is no "our." At this level of consideration, quantum theory has nothing to do with intersubjective agreement. (By the way, I'm not fooling myself: Of course we could not have said what I said above without restructuring the whole article—it would have opened a can of worms! I just want to try to do the idea better justice right now.)

Here it is: Any single application of quantum THEORY is about ME, only me. It is about MY interventions, MY expectations for their consequences, and MY reevaluations of MY old expectations in the light of those consequences. It is noncommittal beyond that. This is not solipsism; it is simply a statement of the subject matter.

Is there any contradiction in this? I say no, but how do I get you into a mindset so that you might say the same? Maybe the best way to do this is to run through a glossary of quantum terms as I did once before ... but now with all the latest slant.

- SYSTEM: In talking about quantum measurement, I divide the world into two parts—the part that is subject to (or an extension of) my will, and the part that is beyond my control (at least in some aspects). The idea of a "system" pertains to a part beyond my control. It counts as the source of my surprises, and in that sense obtains an existence of its own external to me. (Point 1 against solipsism, but I will return for another.)

- POVM: In the theory, this counts as an extension of my will. It counts as a freely chosen action on my part. The whole concept of a "measuring device" as something distinct from me—I am now thinking—just gets in the way. It is a point that Pauli made, but I am coming ever more to appreciate it. A "measuring device" is like a prosthetic hand; its conceptual role is for the purpose of recovering from our natural incapacities and, thus, might as well be thought of as part of ourselves proper. I perform a POVM on a system—captured mathematically by a set—and one of its elements comes about as a consequence.

- QUANTUM STATE: As usual, the catalog of MY expectations for the consequences of MY actions (i.e., POVMs) ... but now with absolute, utter emphasis on the MY.

- UNITARY READJUSTMENT: I'm talking here about the readjustment appearing in Eq. (95) of my paper `quant-ph/0205039`. This, like a quantum state, also captures a belief or expectation. Its purpose is to quantify the extent to which I feel the need to deviate from Bayes' rule after learning the consequence of my action. This is what takes account of the nonpassive nature of MY interventions.

- QUANTUM DYNAMICS: This is the unitary readjustment (or mixture of decompositions and unitary readjustments) that I judge I ought to apply if my action on the system is passive, i.e., if my POVM is the singleton set. It is how I readjust my expectations when I am learning nothing.

Summing up the glossary, I would say quantum theory in its single user implementation is about ME. I act on the world and it reacts in a way unpredictable to me beyond the expectations I build from MY quantum state (about the system).

Why is this not solipsism? Because quantum theory is not a theory of everything. It is not a statement of all that is and all that happens; it is not a mirror image of nature. It is about me and the little part I play in the world, as gambled upon from my perspective. But just as I can use quantum theory for my purposes, you can use it for yours. Thus, if I had not been seeking dramatic effect above, I should have more properly said, "Any single application of quantum THEORY is about the ME who applies it." (Don't correct my English.) When David Mermin is a practitioner of quantum theory, what the theory does is provide a framework for structuring HIS expectations for the consequences of HIS interventions upon HIS external world. ... And that is Point 2 against solipsism.

Recall the definition of solipsism I dredged up from the Encyclopedia Britannica:

> in philosophy ... the extreme form of subjective idealism that denies that the human mind has any valid ground for believing in the existence of anything but itself.

It seems to me we have plenty of valid ground for believing in the existence of something besides ourselves: It comes from all the things we cannot control. Indeed, as already emphasized, for those things we can control, we might as well think of them as extensions of ourselves. Thus, to my mind, quantum theory already gives a karate chop to solipsism because of the indeterminism it entails: With each quantum measurement there is immediately something beyond my control.

Beyond Point 1, though, there is Point 2. It is a question of finally getting straight what should and should not be in the purview of the theory. In this account, quantum theory is a theory of personal action (and reaction). The law-of-thought aspect of it comes out with respect to each individual who uses it. The textbook poses an exercise that starts out, "Suppose a hydrogen atom is in its ground state. Calculate the expectation of ... blah, blah, blah." One might think it is asking us to calculate some objective feature of the world. It is not. It is only asking us to carry out the logical consequences of a supposed state of belief and a supposed action that one could take upon the system. And here's the clincher about Bayesianism. Just as no student in his right mind would find it worthy to ask why the textbook writer posed the problem with the ground state rather than the first excited state, no quantum theorist should make a big to-do about it either. It is simply an assumed starting point. An agent in the thick middle of a quantum application can no more ask where he got his initial beliefs from, than a pendulum can ask where it got its initial conditions from. The cause of bottom-level initial conditions is ALWAYS left unanalyzed. If such was not a sin in Newtonian mechanics, it should not be a sin in a Bayesian formulation of quantum mechanics.

So, it seems to me, if anything, the Bayesian account of quantum theory is essentially the opposite of solipsism. Rather than a unity to nature, it suggests a plurality. An image that might be useful (but certainly flawed) comes from Escher's various paintings of impossible objects. The viewer would initially like to think of them as two-D projections of a three dimensional object; but he cannot. Now imagine how much worse it would get if we were to have two viewers with two slightly different paintings, each purporting to be a different perspective on "the" impossible object. Since neither viewer can lift from his own two-D object to a three-D one, there is no way to unify the pictures into a single whole.

Yet we live in one world, you say. Maybe. But, you should remember that these quantum states we speak of are not perspectives. They are personal possessions. To paraphrase Tilgher's quote at the beginning of de Finetti's Probabilismo,

> A quantum state is not a mirror in which a reality external to us is faithfully reflected; it is simply a biological function, a means of orientation in life, of preserving and enriching it, of enabling and facilitating action, of taking account of reality and dominating it.

"Are there other minds beside your own?," Howard Wiseman asks. If a mind is what it takes to write down a quantum state, then why not? "If you leave the origin of the quantum state unanalyzed, why would two minds ever agree on anything?" That is the issue of intersubjective agreement—something thankfully we can study within the context of quantum theory. But the first thing to get straight is why the single user of quantum theory uses the very structure. What is it precisely that he is believing of the world and his place in it that leads him to the choice of quantum theory as his law of thought?

That is, it is about ME and what I believe. But what do I believe? That's the research program!

13-08-03 *The Tense Mix* (to R. Schack)

Now that I'm back in Munich, I've tried to become re-engaged with the conversation we left on, but I'm having difficulty. I've read your 30/7/03 note "levels" three times over since yesterday, and I'm not sure what I can add at the moment. Clearly I've absorbed some of it into my mentality. In particular, I like the scolding you gave me:

Schackcosm 81: *NOOOOOOO! You are joking, aren't you? The direct lesson of Bell and Kochen-Specker is indeed that we should not act AS IF unperformed measurement had outcomes. To conclude that nature is at a juncture is adding a lot of baggage, however. I am sure de Finetti would be rotating in his grave if he read this!*

You are right; the best I can say is that the lesson of quantum mechanics is that we should not act AS IF unperformed measurements had outcomes. And that is good enough for me. Like with William James, I will act as if nature is undetermined and see where that leads me. I can't do more than that. (In fact, I have said it many times that one may never be able to disprove either many-worlds or Bohmian mechanics. I only bank that those trains of thought will not lead in any productive directions. I suppose I suspended such carefulness as my passions started to flare in our discussion.)

But as you might guess from the paragraph I just wrote, I still see something of an uncomfortable tension in what you've written. For instance, I don't even understand your very next sentences:

Schackcosm 82: *I think de Finetti was both anti-realist and anti-indeterminist. The Buridan's ass passage illustrates this beautifully. Your position is clearly not anti-indeterminist. Be careful who you recommend Probabilismo to!*

Also I guess I don't like this emphasis of yours on the MEANINGLESSNESS of various statements. It doesn't ring true to me, especially with the determinist/indeterminist issue. It strikes me as perfectly useful to take a stance on the issue (of course in the AS IF sense that you have emphasized); and in that way it is not meaningless. That, I think, is why I brought up religions in an earlier note; religions are never verifiable or falsifiable, but faith in them is not meaningless.

It seems to me clear—and I don't think your note has changed this—that when we practice quantum mechanics, we are implicitly stating some beliefs about the world (as it is independently of the agent). You are right that we cannot move from that step to saying that that is actually the way the world is. Nonetheless we are expressing *beliefs* about the world without our presence. The issue is to make those beliefs explicit—and that is your program of AS IF. I am OK with that. To put so much emphasize on the meaningless of this or that, though, repels me slightly. That may be what caused me to swing the pendulum too far in the opposite direction.

I'm really looking forward to your visit to Munich. I hope we can find a way to get together. Aarhus was a useful meeting: Klaus Mølmer, Richard Gill, Ray Streater, Vladimir Bužek, Mauro D'Ariano, Hideo Mabuchi, and Howard Wiseman showed a some decent interest in the program. But I must say, the divergences between us and our desires and Rob Spekkens are becoming clearer . . . and more disappointing. I see him as having the potential to cause real damage. It really would be useful to have a more thoughtful critique of his "classical interpretation of probability over a fundamental event space" position. Bernardo and Smith's dismissal is simply too curt.

Somehow over the course of this meeting, I've decided the time is right to try to put together a paper explicitly and solely on the Penrose argument. The little title I've toyed with in my head has been "On Quantum Certainty." What would you think? Might we throw in on it together? (I guess I was partially induced to this by Spekkens' assertion that we still have no adequate reply to Penrose without the picture he is trying to construct . . . based on ignorance of an ontic state.) Anyway, I started thinking that that might be riper and more ready to go than a critique of the Horodeckis. Also it strikes me as paving the ground for all the other discussions, and worthy of its own thorough treatment.

This is the first of a couple of notes I'll be writing you today. Another one will be coming down the tubes in an hour or so.

13-08-03 *Renouvier* (to R. Schack)

I'm back a little faster than I thought I would be. In this note, I just want to send you a passage that I scanned into my computer from a book on Charles Renouvier—the philosopher who convinced William James to try out free will. There is something about the argument in the last two paragraphs of page 2 that takes me—but I want to see it made more rigorous, or stated better. Also I hope you are as impressed by Footnote 56 as I was. If those passages are not too far out of context, they may show that he is going down the same lines as de Finetti and us.

I'd love to get my hands on anything more technical to do with Renouvier, but apparently there is almost nothing written on him in the English language. The only article I have been able to unearth that might possibly be in some library in your university is: S. Hodgson, "M. Renouvier's Philosophy," Mind **6**, 31–61, 173–211 (1881). If you have time, could you check on that before Munich?

From: William Logue, *Charles Renouvier, Philosopher of Liberty*,
(Louisiana State University Press, Baton Rouge, 1993), pp. 86–92.

> Under the influences of discussions with his friend Jules Lequier, he [Renouvier] became convinced of the reality of human free will and its central importance for the understanding of everything else.[45] This conviction came to Renouvier while he was still deeply under the influence of his first contact with the Saint-Simonians. He experienced, not an overnight liberation from their deterministic viewpoint, but a more gradual readjustment of his views, which became a complete detachment from them only after 1851. Perhaps the failure of the socialist movements in 1848, rooted as they were in the

[45] Renouvier's account of his "conversion" to free will is in the last part of Vol. II of the *Esquisse*. Lionel Dauriac ("Les Moments de la philosophie de Charles Renouvier," *Bulletin de la société francaise de philosophie*, IV [1904], 23) defined the high point of Lequier's influence—the writing of the *Deuxième essai*—as one of four "moments" in Renouvier's philosophical development.

would-be scientific philosophies of the preceding three decades, finally persuaded him of the dangers of rejecting free will.[46] Alienated from political life during the Second Empire, he would spend nearly two decades in the construction and elaboration of his philosophy of liberty, establishing its foundations and exploring its consequences.

Renouvier was aware that for a long time the question had been of mainly religious significance: whether man's salvation depended on free will or on predestination.[47] This debate had reached its peak, in both vehemence and subtlety, in the famous exchange between Erasmus and Luther in the sixteenth century. The emergence of a secular debate over free will was a result of the rise of the scientific worldview in the seventeenth century. The ascendancy of the idea that the world was governed by invariable laws, taking the role previously occupied by an all-powerful, all-knowing God, seemed to leave less and less room for the view that man was somehow an exception to the general rule. The most heroic task for the modern philosopher was to find a means of validating science and free will simultaneously, and the most heroic effort of the eighteenth century was that of Immanuel Kant. But for many in the next century, it seemed that Kant had saved free will only at the cost of making it irrelevant.[48] Fichte tried to rescue Kantian philosophy from this unhappy outcome, but in the general opinion (only recently challenged by Alexis Philonenko, Luc Ferry, and Alain Renaut) his effort led to the fairyland of absolute idealism, denying reality to the material world.[49]

Against the rising tide of determinism, Renouvier would try to show that Kant could be the launching pad for a defense of free will that would maintain its practical relevance and demonstrate its compatibility with natural science, properly understood. He did not claim to be presenting any new arguments in favor of free will; he felt they were in any case unnecessary.[50] Renouvier's reasons for coming to the defense of free will were partly shared with Kant and partly his own. As we have seen in the previous chapter, the shared part was the most familiar: a concern for the connection between free will and moral behavior. Free will was for Kant the essential basis of *practical reason*; without it, the whole idea of moral obligation ceased to have meaning. For Renouvier, this consideration remained central. Without moral responsibility, man would not be distinct from the rest of the animal kingdom, and the whole of civilization would be meaningless. But this was not the sole basis for his concern with free will, and this additional concern moved Renouvier beyond Kant and Fichte, bringing him closer to our own time.[51]

[46]Mouy (*Idée de progrès*, 43) argues that the disappointments of 1848 played a key role in shaping Renouvier's idea of liberty. For Renouvier, free will came to be seen as the ultimate basis of political liberty (*Deuxième essai*, 551). See *Histoire*, IV, 431, and especially *Esquisse*, II, 382, and *Deuxième essai*, 371*n*1.

[47]Renouvier saw free will as one of the basic concepts of both philosophy and Christian doctrine (*Histoire*, IV, 277).

[48]What does it matter to man if he has freedom in the world of the noumena if his world of phenomena is entirely determined? Renouvier later felt that Kant held on to free will solely for the sake of morals while not really believing in it (*Quatrième essai*, 35–36).

[49]Renouvier praised Fichte as a defender of freedom and criticized him as a mystic (*Quatrième essai*, 46). See Alain Renaut, *Le Système de droit: philosophie et droit dans la pensée de Fichte* (Paris, 1986); Luc Ferry, *Le Système des philosophies de l'histoire* (Paris, 1984), Vol. II of Ferry and Renaut, *Philosophie politique*; Alexis Philonenko, *La Liberté humaine dans la philosophie de Fichte* (Paris, 1966).

[50]Renouvier was concerned to establish a rationalist and not an empiricist view of science. He saw free will as perhaps the main issue dividing the rationalists and empiricists (*Histoire*, IV, 262). He indicated that there had been no new arguments in favor of free will since Kant and Rousseau (*Esquisse*, I, 280).

[51]Renouvier saw Kant's German disciples as having abandoned liberty for determinism, optimism, and pantheism (*Histoire*, IV, 467).

It is not just the moral aspect of civilization that hangs on the reality of free will, in Renouvier's opinion, but the whole of our intellectual life. Free will is also the foundation on which philosophy and the natural sciences rest.[52] Without free will, our ability to know anything, whether about man or about nature, is fatally undermined. Scientists do not need to believe in free will, and as he knew, they prefer to avoid this sort of question. In practice, they can legitimately do so because in their narrow spheres of inquiry they have developed techniques of investigation that work even when the scientist is unconscious of the fundamental assumptions on which his method rests. But without free will, the certainty of scientific truths becomes illusory; a consistent determinism must lead to a profound skepticism.[53] Renouvier would never despair of convincing the scientists that just as our concepts of right and wrong depend on free will, so do our concepts of true and false. Indeed, without free will, we could not even talk sensibly about things being true or false.

If, as he pointed out, I hold such and such a view to be true and I am determined by forces outside my control to hold this view, the person who disagrees with me is equally determined by outside forces in his position. If these mutually contradictory positions are equally necessary, what grounds can we have for the certainty that either view is the correct one?[54] If our belief that our ideas are determined is itself determined, so is the other person's belief in free will determined. Under these conditions how could it make any sense to speak of one view as "right" and the other as "wrong"? If, on the other hand, our choices are free, I may freely choose to believe in free will or in spite of the apparent contradiction, to believe in universal determinism. Of the four possible positions revealed by this analysis, the only one that can serve as a foundation for a rational certainty in the truth of our beliefs is to freely believe in freedom.[55] But as Renouvier insists, this means that we must give up any pretension to the absolute certainty of our beliefs.[56] The truth of free will cannot be proved so that no rational person can doubt it. It is a relative truth, like all our other truths, but more important because it plants a relativism at the very core of our thought.[57]

Scientists, Renouvier thought, should have no difficulty understanding and accepting this because science is built on an awareness of the conditional character of our knowl-

[52] See *Deuxième essai*, 227.

[53] *Histoire*, IV, 399; *Deuxième essai*, 327.

[54] *Histoire*, IV, 399; *Deuxième essai*, 306–307 (according to Hamelin, *Système de Renouvier*, 242). Necessity destroys truth: "If everything is necessary, error is necessary just as much as truth is, and their claims to validity are comparable" (*Deuxième essai*, 327). For a restatement of his argument that freedom is essential to the certainty of our knowledge, see *Esquisse*, II, 270–74; see also, *Science de la morale*, II, 377.

[55] The four are (1) we are determined to believe in freedom; (2) we are determined to believe in determinism; (3) we freely believe in determinism; (4) we freely believe in freedom. See *Deuxième essai*, 478; *Histoire*, IV, 399; Hamelin, *Système de Renouvier*, 273–74.

[56] "Certitude is not and cannot be an absolute. It is, as is too often forgotten, a condition and an action of man: not an action or a condition where he grasps directly that which cannot be directly grasped—that is to say, facts and laws which are outside or higher than present experience—but rather where he places his conscience such as it is and as he supports it. Properly speaking, there is no certitude; there are only men who are certain" (*Deuxième essai*, 390). For Renouvier's battle against the idea of evident truths, see *Histoire*, IV, 75, 261; certitude is a sort of "personal contract," "a real contract that a man makes with himself" (Lacroix, *Vocation personnelle*, 114).

[57] *Deuxième essai*, 309–10 (according to Hamelin, *Système de Renouvier*, 242). There were, however, "great probabilities in its [free will's] favor" (*Deuxième essai*, 475). "It ought to be a universally accepted maxim that *everything that is in the mind is relative to the mind*" (*Deuxième essai*, 390). Philosophy needs to take into account the existence of disagreement among philosophers (*ibid.*, 414). Renouvier's approach to the existence of these disagreements is one of the distinctive features of his philosophy.

edge, an openness to the discovery of new truths and the abandonment of old ones.[58] In fact, he had to admit, many scientists were still under the sway of older metaphysical conceptions of truth, except in the conduct of their personal research, and were unaware of any inconsistency in their position.[59] Some who were aware were evidently afraid that to admit that an act of belief was at the base of scientific knowledge would risk undermining the claim of science to objectivity and, even worse, open the way to the proliferation of pseudoscientific beliefs.[60] In reality, pseudoscientific beliefs were already proliferating under the aegis of the belief in determinism. Without a critical analysis of the nature and limits of scientific knowledge, however, our intellectual life is subject to a constant abuse of the name and prestige of science.

The abuse of science takes many forms: the application of research methods to fields where they do not apply, the application of particular concepts to areas other than those where they originated, the confusion of "Science" with the operations of particular sciences. One of the main intellectual trends of the nineteenth century, which Renouvier called *scientisme*, usually rendered as "scientism" in English, was the product of this abuse. Renouvier's relativism does not justify believing in whatever we want to believe.[61] It insists on submitting our opinions to every possible test of logic, experiment, and experience. But we have to admit that our logic, the hypotheses on which our experiments are based, the schemas of thought by which we interpret our experience, all rest ultimately on acts of belief and not on absolute certainties.[62]

If free will is thus essential to both morals and science, just what does he mean by it? Over the centuries, most of the debate over free will has failed to advance our understanding because of the lack of agreement about what is meant by the term.[63] I cannot solve that problem, but I think we will see that Renouvier's view makes the issue more comprehensible.

Free will, for Renouvier, is a capacity possessed by human beings, and only by human beings, that enables them to choose whether to accept one idea or another, whether to perform one act or a different one. It is thus a rejection of the doctrine that holds that all events, mental or physical, are absolutely determined and cannot be other than what they are.[64] Free will is also a rejection of the doctrine of chance, for it is an active power and not the "liberty of indifference" so belabored by determinists.[65] Chance is also hostile to liberty, since it denies man a real power of decision.

The existence of free will requires a measure of indetermination in the universe but

[58]On the use of hypotheses in science, see *Premier essai*, 200.

[59]Renouvier credited the English empiricists, following Hume, with freeing science from the metaphysical concept of cause (*Histoire*, IV, 273).

[60]This is the concern of Parodi (*Du positivisme à l'idéalisme*, 184–85), who finds in Renouvier a dangerous fideism. So does Brunschwicg (*Progrès de la conscience*, 625). For Renouvier's praise of Boutroux's argument that the contingency of the laws of nature is not a threat to science, see *Histoire*, IV, 673–74.

[61]Dauriac ("Moments de la philosophie," 30–32) strongly makes this point. It would be interesting to compare Renouvier's conclusion on this point with the similar view expressed by Richard Rorty, coming from a rather different direction.

[62]*Histoire*, IV, 692; on the need for faith in reason, even though such faith is in itself not a rational act, see Popper, *High Tide of Prophecy*, 218–19.

[63]Adler, *Idea of Freedom*.

[64]See definition of free will in *Histoire*, IV, 337; on liberty as choice, see *Deuxième essai*, 466; on real alternatives, see *ibid.*, 339. Renouvier is rejecting a causal necessity, not analytic necessity, as in the syllogisms of logical operations (*Premier essai*, 232–36).

[65]*Deuxième essai*, 330–34, 336, 337; Hamelin, *Système de Renouvier*, 242–43,249.

could not exist if nature were essentially indeterminate.[66] Our acts of free will are the beginnings of chains of consequences and would have no meaning if their consequences were not subject to cause and effect. "Free acts are not effects without causes; their cause is man, the ensemble and fullness of his functions. They are not isolated, but are always closely attached to the preceding condition of the passions and of knowledge. *A posteriori* they seem henceforth indissoluble parts of an order of facts, although a different order was possible *a priori*."[67] The laws that permit us to say this is followed by that do not admit of an infinite regression into the past, according to Renouvier. Therefore, every series of phenomena—and indeed the existence of any phenomena—must have a beginning that we cannot explain in terms of antecedents.[68]

The act of creation of the universe is thus replicated (in a much smaller way!) in every act of free will. Every act of free will is the creation of a new series of phenomena, a series that would not otherwise have existed.[69] These new chains of cause and effect are not simply the product of the intersection of existing but independent series, as A.-A. Cournot argued, for such intersections, though they appear random from the point of view of any one of the colliding series, would be necessary from a higher viewpoint.[70] They must be new beginnings, arising from a conjuncture in which, given the antecedents, more than one consequence was possible: "ambiguous futures," Renouvier called them.[71] Free will is the capacity to opt for one or another of those futures.

13-08-03 *Pragmatism and QM* (to G. Valente)

Valente-ism 1: *I hope [to be] finishing my dissertation in a couple of months and taking my degree in November. I think the title will read "Probability and quantum meaning: Chris Fuchs' pragmatism in quantum foundations". I've been trying to understand the development of your idea, especially in a philosophical light, but I'd also like to understand if I've been understanding . . .*

Very roughly, I recognized three instances (however connected) of pragmatism in your analysis:

- *one due to the mere acceptance of the theory; that implies, for example, the tensor product rule to represent a basic trail of the formalism and the quantum states to be pragmatic commitments (following a Bayesian approach to quantum probabilities and assuming Dutch-book as [the] criterion of consistency)*

- *one peculiar of the preceding itself in quantum foundations, which banishes any metaphysical interpretation and leads operatively to identify a core of "reality" as un-veiling ("reality is in the difference") and to drop important but not fundamental axioms of the theory (for example the time evolution)*

[66] "Liberty does not require the complete indetermination of particular future events, even of those that are directly connected to it" (*Deuxième essai*, 459); see also *ibid.*, 357; Hamelin, *Système de Renouvier*, 244.

[67] *Deuxième essai*, 359; see also *Science de la morale*, II, 361–62.

[68] *Premier essai*, 237; *Science de la morale*, II, 360–61. Most scientists today reject the idea that infinite regression is an absurdity; *Esquisse*, II, 378–79. We cannot explain beginnings because they are by definition at the limits of our possible knowledge.

[69] *Esquisse*, II, 196-97.

[70] A.-A. Cournot, *Considérations sur la marche des idées et des événements dans le temps modernes* (Paris, 1973), 9–10, Vol. IV of Cournot, *Oeuvres complètes*, ed. André Robinet.

[71] *Deuxième essai*, 210. "The real indetermination of various phenomena envisaged in the future" (*Premier essai*, 240). "[A determinist] would renounce everything called reflection and reason, for these functions do not work without the consciousness of a *representative self-motivation*, which is itself linked to an awareness of the *real ambiguity of future conditions* before it takes action" (*Histoire*, IV, 769). See also *Troisième essai*, xlvii; Hamelin, *Système de Renouvier*, 230.

- *one "deeper" inspired by William James, that suggests a notion of reality as construction.*

Thank you for your note, and I am flattered about your dissertation. I very much hope you will send me a copy when it is complete.

I think your three points are roughly on the mark, and I will enjoy seeing them developed. Concerning the last two points, I will add a little material for your thought. The first is an out-take from my samizdat *Quantum States: What the Hell Are They?* posted at my website. It delineates a little more directly than usual what I see as the great hope in melding quantum theory with Jamesian and Deweyan kinds of thoughts. (It is a file I put together for several other people, called `Construction.pdf`.) The other piece is pasted below. It is a letter written to David Mermin and Rüdiger Schack over the last three days (and sent off yesterday evening). I think it best captures my newest way of explaining things, and I hope it clarifies some points. You can feel free to use the thoughts in it in your development, even though it is not yet posted on my webpage.

In general, I would say the most encompassing source of the thoughts I've already gone public with is the document *Quantum States: What the Hell Are They?* (whose last entry is dated 29 June 2002). Since then, some ideas have changed but not too many; mostly I've spent my time developing the Darwinism issue . . . and that is somewhat uncoupled from the older material. Anyway, I am hoping to finally compress the whole *Quantum States: W.H.A.T.?* document into a proper paper before December, but we shall have to see. Unfortunately it will almost certainly not happen before you write your dissertation.

Concerning your very last question:

Valente-ism 2: *Please, can I ask you any bibliographical indication about this issue or something new about Bayesian probabilities?*

I would say the very best thing to read is Bruno de Finetti's paper, "Probabilismo." A month ago, I read it for my fourth time in six years, and this time I have to say it really, really sunk in . . . and I was ashamed that I had not so much absorbed it before. It is a masterpiece (except for the fascist statements at the very end) and has played a very deep role in my latest collaborations with Rüdiger Schack. Here is the English translation: Bruno de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989). Immediately following that is an article by Richard Jeffrey titled "Reading Probabilismo" which it would also be good to have a look at, for it sets the context of the paper. In case it is more accessible to you, here is the original reference in Italian: Bruno de Finetti, "Probabilismo," Logos **14**, 163–219 (1931).

Good luck with everything!

14-08-03 *Uncertainty* (to W. E. Lawrence)

I'm glad you got my book. I hope you'll get some bits and pieces of insight from it. The development of the idea (the Paulian one) still goes on and seems to be gaining some momentum in the wider community. (I am a decently good salesman, even if not a decently good physicist.) Have you seen the September issue of *Studies in History and Philosophy of Modern Physics* devoted to quantum information? A lot of good articles there; Jeff Bub and I were editors. I'm excited that in some small number of years, we're really going to tie this up and move on to the next step of physics.

14-08-03 *Behind Again* (to P. G. L. Mana)

I'm in Munich at my in-laws' house, with some moments to finally catch up on old mails. Thanks for your insightful letter of 16 July.

I wish I had more to say at the moment; I just give a few nods of "yes" to you.

Manalogue 3: *With regard to my paper, I wonder if I can ask your advice on a point. I'd like to make there the bold statement that the concept of probability table and its decomposition offer a simple point of view to look at Gleason's theorem (and also at the one with general POVMs). The point of view is the following (sketched):*

1) The concept of preparations and effects can very intuitively (and precisely, I think) be expressed through the idea of a 'probability data table'.

2) The assumption that effects are to be represented by vectors implies that the only simple way to pass from a probability table to these vectors is through the table decomposition in the manner I do in the paper. Note that the 'effects = vectors' assumption is implicit in Gleason's premises, since the starting objects used as effects (projectors or POVMs) are additive. Note also that the table decomposition explains why the preparations are also to be vectors, and why linearity comes apparently for free in Gleason's theorem.

3) The decomposition and the fact that we are dealing with probabilities (bounded within $[0,1]$*) also leads easily to the fact that the 'shape' of the set of preparation-vectors determines the shape of the set of effect-vectors, and vice versa. Now, Gleason doesn't just make the assumption 'effects = vectors', but the stronger one 'effects = projectors' (or 'effects = POVMs'), which corresponds to assuming also a definite shape of the set of effects – thus it is also clear why the theorem gets almost for free the geometry of the set of quantum mechanical preparations.*

Do you think that these points make some sense?

Yes, I like this line of thought very much. Ever since first drawing the picture that became Fig. 2 in my `quant-ph/0205039`, I've been thinking that Gleason's theorem is almost superfluous. If you could dot the eyes and cross the tees in that in an even more general setting, that would be great!

Manalogue 4: *In connexion with point 3) above, there is a very interesting and non-trivial thing about the sets of effects and states in quantum mechanics: they are the same, modulo a renormalisation of effects. This, for a generic imaginary data table, doesn't happen; in fact, in general it is even meaningless to speak of a 'renormalisation'. For example, if the set of states is a simplex, the set of effects is a cube, which projects onto an hexagon; if the set of states is a semicircle, the set of effects is a bi-pyramid which projects onto a pointed ellipse ... Only in special cases does one have that symmetry (e.g., a circle, a sphere, etc.). This puzzles me, and I wonder if this symmetry has some physical meaning or if it just hides some assumption we make in our concepts of 'preparation' and 'outcome'. Because it amounts to saying, more or less, that there is a correspondence between a preparation and an outcome – which, in general, I cannot see.*

I think the deep reason there is precisely that quantum measurement corresponds to an application of Bayes' rule. In a measurement, one simply "refines" one's knowledge: i.e., one moves from the barycenter of a convex decomposition of a density operator to one of the outlying points in the decomposition.

Manalogue 5: *In Bayesian updating, I update the probability distribution that I assign to some (mutually exclusive and exhaustive) propositions – call them hypotheses – in view of new data. The point is that both my old and my new probability distributions refer to the same set of hypotheses.*

The quantum "updating", instead, can bring me from one kind of system to another kind of system, which can be completely different (in particular, of different dimensionality, eg, when the V_d are unitaries). From a Bayesian point of view, this looks more like going from a set of hypotheses to another set (which need not have something to do with the former) ie from a probability distribution $\{P(A_i|L)\}$ to another $\{P(B_j|DL)\}$.

Yes, you are right there, and I don't quite have my head around the point yet. But you might also note in the Fig. 2 that I mentioned above, one could interpret the extra unitary readjustment in exactly the same way: i.e., as readjusting the hypothesis set rather than the final state. The problem is in part that the hypothesis set (i.e., the standard quantum measurement device at the Bureau of Standards) is just a scaffold and not real. The main point of importance for me is the invariant piece: for *any* standard measurement chosen, quantum updating looks like an application of Bayes' rule + linear readjustment.

Manalogue 6: *Finally, I'd like to ask you something about hidden-variable theories and non-locality (I wanted to write and ask Mermin long ago, but doubted to receive a reply). From a Bayesian point of view, isn't the concept of "correlation" in the mind of the agent, and is it something non-physical, just like quantum states? In Bell's theorem, the assumption is made that the settings of the measuring devices are uncorrelated (between each other, and between different times for any one device). But can this assumption really be proven or disproven by experiment, for the Bayesian reason above? What happens if one drops it? Can one get a local deterministic hidden-variable theory that says that the devices' settings are pre-determined, together with the states of the two particles (of course, since the theory is deterministic), and that they are pre-determined in such a way to give the observed outcomes – whatever degree of "correlation" they have? In order to determine the behavior of all the pieces (particles, devices, experimenters, etc.) wouldn't one only need to set up appropriate initial conditions in a space-like slice of the past light-cone which contains all the pieces and all the events?*

Yes, and you are right: Several people have made that point. (Not the Bayesian point, but the latter point.) Unfortunately I can't think of any references—though, it may have been Bell himself first.

17-08-03 *Tearing Off the Duct Tape* (to N. D. Mermin)

Merminition 127: *Please take the damn tape off your mouth. If I'm going in the wrong direction I don't wish to waste anybody's time.*

I apologize for keeping you waiting for another two days. Once I signed off Friday at Beer:30, I wasn't able to get back to email until this morning. The whole family ended up in Munich proper yesterday (rather than a leisurely day here in Zorneding), and then by the evening I was exhausted.

So, let's get to it.

Merminition 128: *Quantum mechanics says (a) If two gates are measurement gates and (b) if no other unitary gates are in the circuit but those two then (c) if a qubit is sent through the circuit then with probability 1 both gates will give the same reading.*

This appears to be an assertion that under certain conditions any rational person will strongly believe that both gates will give the same reading. Furthermore, the conditions are not that in the past million runs only 00 and 11 have been registered and never 01 and 10 (as in the story you tell *me about how Bob comes to his strongly held belief). They are conditions about the structure of the circuit, through which no qubit may ever have been sent. If, under these conditions, all rational people must agree that the gates will give the same reading, some might worry that it was nitpicking (rather than deeply insightful) to nevertheless insist that this must be viewed as a subjective judgment rather than an objective property of the circuit.*

Yes, exactly; your last sentence would certainly be my own take if it were to stop at that. I doubt you remember this, but it was the BFM paper that started pushing me down the radical Bayesian path precisely because I perceived its message to be something like your point above. (Of course you didn't say "subjective judgment" but rather "knowledge," but for me it had the same effect.)

I just took a walk down memory lane rereading my correspondence to you, Brun, Finkelstein, Caves, and Schack between 7 August and 2 September 2001 to see if I could find any good quotes along those lines. Kind of depressing really: The sad thing was that I seemed to be far more lucid then than I was by the end of our Montréal meeting in November 2002 (capped off by my fatal mistake on 4 November). For instance, when I look back at my 22 August 2001 note to Caves and Schack "Identity Crisis," I see that it had a perfectly good discussion of CERTAINTY—the very starting point of this latest email conversation, as rekindled by Rüdiger's 18 June note this year. Traveling in circles and circles. Reread some of that stuff and tell me whether you don't think it is dead on the mark for today's very discussion. Let me give you an outtake, from a note to you 2 September 2001:

> The point of separating the categories "knowledge" and "reality" (or "subject" and "object" for that matter) is not to make knowledge an objective reality in its own right or, even worse, to make it the sole reality. Rather it is to say that there is a distinction and that that distinction should be recognized. ...
>
> What I have ultimately NOT been able to stomach about your wording of the whose-knowledge "answer", and Jerry's wording of the whose-knowledge "answer"—some of Todd's versions would actually survive—is that you say, under certain circumstances, two scientific agents (observers, or what have you) MUST assign "consistent" quantum states to a given system. In the case of pure states, the two agents MUST assign the *same* pure state to the system. ...
>
> What I object to is the word MUST. Todd once wrote it this way,
>
> **Brunism 9:** *We have been describing a consistency criterion.* If *one wishes to combine two state descriptions of a single system into a* single *state description, the criterion tells one* when *it is consistent to do so (i.e., when the two descriptions are not actually contradictory).*
>
> *I agree that nobody is holding a gun to Alice's head and forcing her to incorporate Bob's information.*
>
> and to this way of speaking I can agree. But if you take away Todd's "*If*", then everything collapses in my mind. Enforcing that two agents MUST make the same state assignment if they are going to be "right" at all reinstates the very objectivity, the very agent-independence of the quantum state that the Mechanica-Quantica-Lex-Cogitationis-Est program has been working so hard to exorcise. ...
>
> It is much like the old debate. Is materialism right? Or is it Berkeley's idealism that is right? Who cares, I say. Both philosophies are just simple samples of realism: They only disagree on the precise concept which ought to be taken as real, mundane matter

> or sublime consciousness. The way you characterize it above, one would think that the only fruit of the Mechanica Quantica program would be the RENAMING of a material reality into an ideal one—a shift more of emphasis, rather than anything of grit.

So, let me get to your better formulation straight away:

Merminition 129: *I believe your answer to such worriers would be something like this: that* (a) *and* (b) *imply* (c) *is not an objective fact about the world of circuits, but a law of thought (though it remains a part of the research program to show precisely how it emerges as a law of thought). In isolation it cannot affect anybody's degree of belief — for example it cannot guide betting behavior — unless* (a) *and* (b) *can be established. But* (a) *and* (b) *are subjective judgments. Bob requires a million runs not to establish the law of thought* $(a) + (b) \Rightarrow (c)$*, but to build up his strongly held belief in* $(a) + (b)$*.*

Yep, you're roughly on track here. The law-of-thought (or maybe better, law-of-ideal-thought) character in the implication $(a) + (b) \Rightarrow (c)$ is due to Gleason's theorem plus Dutch-book coherence. It's the kind of point we tried to make in `quant-ph/0106133`. The definition of your first measurement gate in (a) is that it gives "maximal information" about a reapplication of itself, i.e., it gives certainty for the click (as judged by any agent for which it is actually a measurement gate). DB coherence turns that certainty into a $p = 1$ assignment, and Gleason's theorem further turns that into a unique pure state. The judgment of (b) allows this pure state to be reconverted into a second $p = 1$ assignment for the outcome of the later measurement gate.

The goal of the research program is indeed to show that all of this together is a kind of Dutch-book coherence or, at least, Dutch-book coherence modulo some particular assumption about the properties of the (contingent, physical) world. (Rüdiger and I may still have slightly different goals here; it's hard to tell at this point.)

Merminition 130: *In isolation it cannot affect anybody's degree of belief — for example it cannot guide betting behavior — unless* (a) *and* (b) *can be established.*

Yes. In fact, it is just as normal Dutch-book coherence sets no probability assignments at all. (Emphasizing, of course, that even in the case of "certainty" we have had to recognize the subjectivity of the judgment.) If DB coherence is taken to be akin to a dynamical law in classical physics, (a) and (b) should be taken to be akin to the initial conditions. (I don't know if that analogy helps.)

Let me only take point with one issue:

Merminition 131: *But* (a) *and* (b) *are subjective judgments.*

You should stop your development right there. Conceptually, the origins of (a) and (b) should be left unanalyzed (just as with initial conditions in classical mechanics). You can of course do what you do in the next sentence (to somehow make it all more palatable),

Merminition 132: *Bob requires a million runs not to establish the law of thought* $(a)+(b) \Rightarrow (c)$*, but to build up his strongly held belief in* $(a) + (b)$*.*

but you should realize that in doing this you expand the context of the problem without adding anything of deeper significance to it. The data from those million runs are of no significance without still a further prior judgment. (Rüdiger introduced exchangeability to bring the language to the turf of your 1985 paper, but exchangeability is still nothing more than a judgment.) To know that a million runs (rather than a billion runs or a trillion runs) is of significance requires still more to be said: In particular one must choose a particular exchangeable assignment rather than another. So at some level, one is always stuck with making a quantum state (or quantum operation) assignment just to get the ball rolling. Thereafter, everything is empirical data plus law of thought applied to that initial judgment.

For (a) and (b) to be "subjective judgments" in our sense means (1) that their origins need not be analyzed, and (2) there is nothing in the world that *requires* two agents to have the same such judgments. The value of the game foundation-wise is that it tells you that if you are looking for the "objective" or "intersubjective" in quantum mechanics, you should look elsewhere.

I hope you'll send the second installment soon! Rüdiger and I are now set to meet late morning Tuesday, and it would be great to discuss your note face to face.

18-08-03 *The Big IF* (to A. Sudbery & H. Barnum)

I'm running far, far behind in all the things I've wanted to do this summer, but maybe there's a chance I'll catch up. In particular, I have been trying to give Mr. Nagel a concerted effort during my vacation here in Munich. I went out and bought *The View from Nowhere* and am a little ways into it. When I finish that, I'll re-approach your article "Why Am I Me?" and give you a detailed appraisal.

It's probably too early in my reading to tell, but my troubles with Nagel may all boil down to "The Big IF." That is, they may boil down to the religion that lies behind this passage plucked out of his article "Subjective and Objective." (I'll capitalize the big IF and a couple of other appropriate words so that you'll know what I'm talking about.) Here goes:

> Since a kind of intersubjective agreement characterizes even what is most subjective, the transition to a more objective viewpoint is not accomplished merely through intersubjective agreement. Nor does it proceed by an increase of imaginative scope that provides access to many subjective points of view other than one's own. Its essential character, in all the examples cited, is externality or DETACHMENT. The attempt is made to view the world not from a place within it, or from the vantage point of a special type of life and awareness, but from nowhere in particular and no form of life in particular at all. The object is to discount for the features of our pre-reflective outlook that make things appear to us as they do, and thereby to reach an understanding of things as they really are. We flee the subjective under the pressure of an assumption that everything must be something not to any point of view, but in itself. To grasp this by DETACHING more and more from our own point of view is the unreachable ideal at which the pursuit of objectivity aims.
>
> Some version of this polarity can be found in relation to most subject matter—ethical, epistemological, metaphysical. The relative subjectivity or objectivity of different appearances is a matter of degree, but the same pressures toward a more external viewpoint are to be found everywhere. It is recognized that one's own point of view can be distorted as a result of contingencies of one's makeup or situation. To compensate for these distortions it is necessary either to reduce dependence on those forms of perception or judgment in which they are most marked, or to analyze the mechanisms of distortion and discount for them explicitly. The subjective comes to be defined by contrast with this development of objectivity.
>
> Problems arise because the same individual is the occupant of both viewpoints. In trying to understand and discount for the distorting influences of his specific nature he must rely on certain aspects of his nature which he deems less prone to such influence.

> He examines himself and his interactions with the world, using a specially selected part of himself for the purpose. That part may subsequently be scrutinized in turn, and there may be no end to the process. But obviously the selection of trustworthy subparts presents a problem.
>
> The selection of what to rely on is based partly on the idea that the less an appearance depends on contingencies of this particular self, the more it is capable of being arrived at from a variety of points of view. IF THERE IS A WAY THINGS REALLY ARE, which explains their diverse appearances to differently constituted and situated observers, then it is most accurately apprehended by methods not specific to particular types of observers. That is why scientific measurement interposes between us and the world instruments whose interactions with the world are of a kind that could be detected by a creature not sharing the human senses. Objectivity requires not only a departure from one's individual viewpoint, but also, so far as possible, departure from a specifically human or even mammalian viewpoint. The idea is that if one can still maintain some view when one relies less and less on what is specific to one's position or form, it will be truer to reality. The respects in which the results of various viewpoints are incompatible with each other represent distortions of the way matters really are. And if there is such a thing as the correct view, it is certainly not going to be the unedited view from wherever one happens to be in the world. It must be a view that includes oneself, with all one's contingencies of constitution and circumstance, among the things viewed, without according it any special centrality. And it must accord the same DETACHED treatment to the type of which one is an instance. The true view of things can no more be the way they naturally appear to human beings than the way they look from here.
>
> The pursuit of objectivity therefore involves a transcendence of the self, in two ways: a transcendence of particularity and a transcendence of one's type. It must be distinguished from a different kind of transcendence by which one enters imaginatively into other subjective points of view, and tries to see how things appear from other specific standpoints. Objective transcendence aims at a representation of what is external to each specific point of view: what is there or what is of value in itself, rather than *for* anyone. Though it employs whatever point of view is available as the representational vehicle—humans typically use visual diagrams and notation in thinking about physics—the aim is to represent how things are, not *for* anyone or any type of being. And the enterprise assumes that what is represented is DETACHABLE from the mode of representation, so that the same laws of physics could be represented by creatures sharing none of our sensory modalities.

The two key ideas in this passage that I think quantum mechanics plays the most havoc with are:

1. the DETACHED agent (observer, scientist, etc.), and

2. IF there is a way things really are ...

I honestly believe one can take the Nagel worldview seriously—I suspect there is no logical flaw in it. One can legitimately try to make quantum mechanics fit that worldview with more or less success. My only point is the strong personal suspicion that with such a project one forces quantum mechanics into shoes it does not fit. And, as I see it, what bunions that will cause in the future!

The whole subject matter of my *Notes on a Paulian Idea* is in toying with the idea that the cleanest expression of quantum mechanics will come about once one realizes that its overwhelming message is that the observer cannot be detached from the phenomena he HELPS bring about. I capitalize the word HELPS because I want you to take it seriously; the world is not solely a social construction, or at least I cannot imagine it so. For my own part, I imagine the world as a seething orgy of creation. It was in that orgy before there were any agents to practice quantum mechanics and will be in the same orgy long after the Bush administration wipes the planet clean. Both of you have probably heard me joke of my view as the "sexual interpretation of quantum mechanics." There is no one way the world is because the world is still in creation, still being hammered out. It is still in birth and always will be—that's the idea. What quantum mechanics is about—I toy with—is each agent's little part in the creation (as gambled upon from his own perspective). It is a theory about a very small part of the world. In fact, I see it as a theory that is trying to tell us that there is much, much more to the world than it can say. I hear it pleading, "Please don't try to view me as a theory of everything; you take away my creative power, my very promise, when you do that! I am only a little theory of how to gamble in the light of a far more interesting world! Don't shut your eyes to it."

The question is, how to get one's head around this idea and make it precise? And then, once it is precise, what new, wonderful, wild conclusions can we draw from it? That is the research program I am trying to define.

Is it a SCIENTIFIC research program? I think so, and in the usual sense. There will be lemmas, theorems, and corollaries. (I would like to think that my work and the work of the fellows I've drawn down this path already evidences this.) Ultimately there will be calls for experiments. There will be technologies suggested and money to be made from the program's fruits. Failure of nerve? Anything but!:

Sudberyism 1: *Maybe you and [Rorty] can shift me from my instinctive reaction to pragmatism, which is that for a scientist it represents a failure of nerve, a failure of imagination, and most seriously a failure of curiosity. Being useful cannot, for a scientist, be the end of the story about a statement or a theory; we immediately want to know* why *one theory is more useful than another. That "why?" leads us to an external world of some kind, maybe very strange (the stranger the better, i.e. the more interesting, I would say) and to refuse to follow where it leads seems to me to be a scientific copout.*

I see it as anything but a failure of curiosity or a copout! What you wrote me above reminds me of a conversation I had with Chris Timpson in a pub one night. I made the mistake of mentioning William James, and Chris quickly intoned, "ALL James was about was the nonsense that truth resides in what is useful." The word ALL just boomed! A man's whole life was dismissed in a single sentence. I cut him short, "William James was about many things, ONE OF WHICH was that the correspondence theory of truth holds no water." Similarly I will say to you, there is far more explored by the pragmatist thinkers than that which is delimited by their ideas on truth and warranted belief. Pragmatism is not positivism; it is not that there is nothing to be sought in science beyond the connections between sense perceptions. I see the classical pragmatists (and myself) as ultimately realists, but honest realists—ones who have realized that our theories are not mirror images of the underlying reality, but rather extensions of our biological brains.

But that is going in a direction I don't want to go down at the moment. In any case, don't read Rorty first! Read James' little book Pragmatism to start off with. More immediately, with respect to the present Nagelian discussion, read "Genesis and the Quantum" on pages 122–123, the dialogue between Adam and God on pages 118–120, "Evolution and Physics" and "Precision" on pages 267–270, and some of Jeff Bub's expressions on the idea in Chapter 9, most notably pages 139–140 and 141–142—all these things in the samizdat I sent you. The game of ASSUMING the possibility of a detached observer, as Nagel does, is just that: a game of assuming. Thereafter, Nagel tries to make sense of our more personal worlds in spite of this. The pages I've just referred to in my samizdat try to sketch what quantum mechanics might be talking about if one does not make such an assumption. In fact, they try to justify NOT making the assumption at all. I hope from these readings you will get the impression that though there may be a fundamental disagreement between Nagel and me at the outset, such a disagreement does not necessarily amount to a copout on my part.

Finally, let me paste in a note below that I sent to David Mermin and Rüdiger Schack the other day. It's titled "Me, Me, Me!" and gives my very latest attempt to express the content of quantum mechanics in these lights. I think it does an adequate job ... but experience tells me I am always over-optimistic. In any case, it is directly related to all that was said above.

I'll be back again after completing Nagel and your paper.

PS. I'll also attach the note on Rorty-ish stuff that I sent you previously. That way Howard will have a better handle on why I find excitement in dismissing the starting point "IF there is a way things really are ..."

21-08-03 *Everywhere and Nowhere* (to H. Barnum)

Thanks for the note in reply to my pre-Nagel thoughts. You've given me courage in a way: I feel more strongly than before that reading Nagel is worth my time. I will follow it through to completion.

A couple of very little points.

Barnumism 6: *I'm not sure if "religion" is supposed to be a bad thing in this instance, but couldn't we just as easily call it a "research program"? Even a SCIENTIFIC one? I guess my point here is that I don't think quantum mechanics can yet be considered decisive against such a program... and the theorems, lemmas, technologies you mention (below) are coming from quantum mechanics taking a wide variety of interpretational positions (or none at all)... sometimes very similar theorems from people having (I think) different points of view.*

Fair enough. I suppose my choice of the word was conditioned on the number of times I've had my own research program depicted as "ignoring the results of science" or as "antithetical to the very idea of science" (D. Deutsch and D. Albert, two examples). The point I wanted to get at with the term "religion" was only that the possibility of satisfying "The Big IF" is a *faith*, not a self-evident truth.

(By the way, while I'm thinking of Deutsch and Albert and pointing out the depths of their religiosity, let me go the rest of the way and tell you the remainder of the thought. These are religions, yes, but worse than that, they are *easy* religions. Evidence: Deutsch and Albert both have already found *the* answers, whereas I still strive away.)

Barnumism 7: *Again, I tend to think this "assuming the possibility" may just amount to "attempting to coordinate the various perspectives into a coherent whole". Quantum mechanics suggests difficulties with this, enough to make it worthwhile to pursue your vision of "assuming the impossibility of a detached observer" ... but to me, it would be a failure of scientific nerve/imagination to think quantum mechanics justifies abandoning the attempt to coordinate. Let both approaches bloom ...*

Life is short!

22-08-03 *Chance and Paper Letters* (to W. C. Myrvold)

Funny you mentioned getting a paper letter from Abner. Just yesterday when I returned from Munich I found a letter from Abner that had been forwarded to me from Bell Labs: It was dated June 3!! I was so delighted with it I scanned it into my computer this morning for my permanent records. If Abner had only emailed it, it would have been in my computer three and a half months ago!

Concerning a conversation I'd ultimately like to start up with you, though—namely, subjectivist or personalist probability in quantum mechanics—a better source is my samizdat *Quantum States: What the Hell Are They?* posted at my webpage. In particular, you might enjoy skimming though it in connection with your project with Bill Harper. Most of the fun in the document starts up on pages 19 and 35 (and thereafter). That's where I take the turn of giving up on "objective certainty," having realized that through quantum mechanics its logical derivative is ineluctably "objective chance."

Rüdiger Schack and I will be condensing much of that subject matter into a proper paper starting in the middle part of this month.

I'm actually surprised to hear what you say in this:

Myrvoldism 2: *I don't know of a good source on why Bayesians should believe in objective chance (or, better: at least regard the notion as intelligible), and neither does Bill Harper.*

I recall the words in David Lewis's paper, 'A Subjectivist's Guide to Objective Chance': "As philosophers we may well find the concept of objective chance troublesome, but that is no excuse to deny its existence, its legitimacy, or its indispensability. If we can't understand it, so much the worse for us." I would have thought a lively discussion would ensue in the subjectivists' camps thereafter.

Anyway, I'm very much looking forward to this paper of yours.

25-08-03 *Coordinate Systems* (to G. L. Comer)

I read two things this morning that put me in the mood of coordinate systems. The first was a remarkable letter from Pauli to Bohr, dated 11 March 1955. [See 29-03-01 note "There's At Least One More" to H. J. Folse.] The second was your latest draft of "linear structures." I'm just trying to get these things clear in my head and put them into a common language. (By the way, I'm sorry I've been so absent lately. Unfortunately, after this brief return I'll probably be absent again for a while; I've got to get a book review to *Physics World* by September 4, and I am far, far behind on my ARDA roadmap duties. Also I've got to finally get my application off to the Bohr Institute this week—a much more difficult process than you might imagine. In total I'm going to be inundated through mid-September.)

Anyway, I want to attach a PDF file containing two letters on quantum mechanics that I've written recently (one to Mermin and Schack, and one to Sudbery and Barnum), along with the Pauli letter mentioned above. Strangely enough—you must get tired of my saying this!—I feel like I've had another epiphany in expression. My two letters try to capture that.

Now for the common language business. What I found remarkable upon rereading the Pauli letter—I last read it maybe a year ago—was his discussion of coordinate systems and the way he alludes to calling them "actions on the part of the observer." Or, at least in my cherry-picking mood, that's what I would like to glean from the letter (ignoring that he did also use the word "knowledge"). You'll see why that takes me when you read the letter I wrote to Mermin and

Schack. There I explicitly identify the set of POVMs (i.e., the full set of quantum measurements upon a system) as the set of ACTIONS one's *will* can take upon the system.

Clearly, Pauli sees the coordinate system as the classical analog of the POVM (and vice versa, the POVM as the quantum analog of the coordinate system). They are both mathematizations of "the observer," or better "the agent," in the respective theories. But there are differences. For one, classical coordinate systems are completely passive—this Pauli recognizes. Equipping a manifold with a coordinate system does not change the manifold or any of its contingent properties (i.e., any of the fields living on it). On the other hand, POVMs are anything but passive. This was a major point for Pauli. A POVM elicits the world to do something it would not have otherwise done. The combination of the action of one's will (the POVM) with a system external to it gives rise to a birth of sorts—the flash, the event which we usually call (in older language) the measurement outcome.

What is funny, however, is Pauli's discussion of Einstein. As far as I can tell, what he intends by the word "consequent" would better be expressed by "consistent." (Actually, upon looking up the word in my dictionary, I find that this is its very meaning!) Consequently I find Pauli's contention that Einstein was being inconsistent puzzling. The very reason Einstein felt no qualms about introducing coordinate systems when necessary was because in relativity theory the underlying observer-free description—i.e., the manifold equipped with its fields—had been achieved. In contradistinction, this is something quantum theory has yet to achieve (and may never be able to). In fact, it was this that troubled Einstein ... saving him from inconsistency.

Let me belabor the similarity and distinction. I think you are certainly right to point out one of the similarities via the mutual use of projections in the two theories. In the case of relativity theory, a projection is used to tell what one WILL "see" from one's perspective—you gave energy as an example. The projection is an operation used for expressing an aspect of a preexisting reality, e.g., a tensor field. In the case of quantum mechanics, one projects one's state of knowledge (i.e., one's density operator for a system) onto the potential consequences of one's action (i.e., onto the POVM elements) to obtain a probability distribution. This is just the Born rule in a generalized setting. The probability distribution so obtained expresses one's expectations for the consequences of one's action, not one's expectations for a preexisting reality.

So, projections both, but they encode very different things.

Let me close with another distinction that I'm wondering what to make of. It's connected to a point in the Anti-Växjö paper I sent you and a point in your latest draft. As far as I can tell, I believe both:

> **Me:** The OBJECTIVE content of the probability assignment comes from the fact that *no one* can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation *rules* from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments *within* each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!
>
> **You:** Even if the observers are at the same place, at the same time, when they make their measurement, they will in general not record the same energy because their motions will in general be different. So how can they ever agree on anything? Relativity tells them how to "translate" one measurement into the other so that a consistent description

> results. What is objective about the energy? Nothing. Its value depends on the motion of the observer. Where is the objectivity? Only in the rules of translation. The only objective thing to me seems to be how one object is to be compared with another.

I.e., both theories make use of transformation groups in a crucial way. But what different ways! In the case of relativity, I guess I would have said that the translations are for external purposes. They tell me how to connect one observer's potential observations to another's. Or another way to say it, they tell me how to translate from one aspect of the preexisting reality to another. In the case of quantum theory, however, the translation is all internal: What should I believe of this, given that I believe such-and-such of that? The quantum transformation rules I am thinking of here say nothing of how to translate one person's beliefs into another person's. In fact, the whole point of personalistic probability is that there is IN PRINCIPLE no way to connect someone's beliefs to anyone else's: Probabilistic statements are personal betting strategies.

Thus I guess I find no warrant in what Pauli says when he tries to make the point: "That the situation in quantum mechanics has a deep similarity with the situation in relativity is already shown by the application of mathematical groups of transformation in the physical laws in both cases."

But then again, I start to wonder that if I took Pauli's starting point completely seriously—i.e., if I were to start to think of coordinate systems as ACTIONS of an individual observer—all my troubles would melt away and maybe I'd start to see a deeper analogy as he saw it. I guess I'm still very much in the process of thinking.

Enough for now. (Don't forget the attached files—the one already mentioned plus one further one relevant to the Sudbery discussion.)

31-08-03 *Acceleration is Intervention!* (to G. L. Comer)

What is it about Sundays that sets off the thought muscles? Anyway, I hope my title says it all. That was the thought that hit me as I was washing the dishes this morning. Maybe coordinate systems are interventions after all, when considered in the quantum context. [Cf. last Sunday's discussion.] What happens when a "particle detector" is accelerated? You've told me before that it goes click. The usual explanation goes that it has something to do with the perceived quantum state of the field – namely it becomes something like a thermal density operator. But maybe instead, we should think of the process as a change in the character of the measurement device. That is, maybe an accelerated particle detector is a distinct POVM from its inertial cousin. I.e., it intervenes on the field in way different than the inertial detector. (In older language, that is to say, it "measures" a different observable.)

At least that's the thought that hit me.

Look at me getting pulled down the path of GR: Analogies between Hilbert-space dimension and gravitational mass, analogies between coordinate systems and measurement devices. What's happening to me?!?!

28-08-03 *Update* (to C. King)

Kingism 2: *What are you working on now? Any more neat additivity conjectures to throw my way?? You had mentioned the possibility of coming to Boston some time – now that Peter Shor is here, perhaps you have even more reason to pop over?*

Mostly I'm trying get my part of the ARDA roadmap finished. It's a big annoying project. And I may have to go to DC Sept 14-16 for it.

Mathematically, mostly I'd like to see whether or not there is a set of quantum states with $d^2 - 1$ elements which achieves the quantumness of the Hilbert space (i.e., $2/(d+1)$).

Coming to Boston definitely sounds nice! By the way, would you have any to get (or get me) into the Harvard library? There's an old PhD thesis that I'd like to get a copy of:

- W. H. Long, *The Philosophy of Charles Renouvier and Its Influence on William James*, Ph. D. thesis, Harvard University, 1927.

and it is not available through University Microfilms, etc.

01-09-03 *Answering Correspondence with Correspondence* (to S. Savitt)

Savittism 1: *I've always (though perhaps incorrectly) thought of solipsism as the view that my mind was the only mind that exists (or that I knew exists, or that I could know exists). This way of looking at it is, of course, compatible with materialism.*

The predominant view seems to be that it connotes an extreme form of idealism in which (one) self is the only reality, or at the very least, the ground of reality. I have a correspondence with Mermin in which I compile several definitions for the word pulled from various sources; I'll attach that rather than pull it out of context.

Savittism 2: *I didn't get your remarks about Unitary Readjustment. Is the kind of updating of your beliefs that you do as the result of a measurement in QM supposed to be incompatible with standard Bayesian updating. (I thought one theme of your "Quantum Information" paper is to show that these processes were at root the same.) Alas, the details of your massive paper have begin to blur in my mind.*

Yes it is generally incompatible; the deviation represents part of the "only a little more" in the paper's title. In Sections 6 and 6.1 of my paper I deemphasized the incompatibility (or rather, "deviation from Bayes") because I wanted to first clinch it in the minds of the readers that quantum collapse has a large component that is simply Bayesian conditionalization. The unitary readjustment quantifies the "deviation from."

There is an interesting case where quantum collapse is nothing at all more than Bayesian updating: That is, when there is no physical interaction with the system being considered—for instance in the standard EPR scenario. By making a measurement on one of an entangled pair of particles, one updates one's description for the other particle. The interesting thing one sees by writing collapse in the way I endorse is that, in this case, the unitary readjustment is just the identity operation . . . which makes collapse precisely Bayesian conditionalization.

The idea is that, when there is a deviation from Bayes, what one is seeing is a mild trace of the much deeper statement: The observer cannot be detached from the phenomena he helps bring about.

Savittism 3: *Finally, despite your protest and because many will read your writing, I will make the odd remark about grammar. It's not the one you forestalled, though. It's that 'like' is a preposition, introducing a clause. So on page one of the 12 August note, I think you really should say "Maybe* *the best way to do this is to run through a glossary of quantum terms as I did once before. . . " And there's a similar mis-use on p. 2. Obviously, I don't think that Winstons carried the day on this one.*

Much appreciated. I only started learning grammar about ten years ago, and it shows. I will readjust the sentences as soon as I send off this email. Please explain the Winstons remark though. What does it refer to?

Savittism 4: *Well, maybe I have another thing to add. I have read only little of Shimony's technical work in QM, but here's someone who worked on personal or subjective Bayesianism and doubtless had some ideas as to how it applied to QM. Looking at your brief email correspondence with him in the samizdat, it occurs to me that* perhaps *your difference (if there really is a difference) as to whether an interpretation of QM must be "ontological" or not obscures the fact that his Bayesianism is like yours.*

Oh, there's quite a difference between Shimony and me on this score. But there is enough mutual understanding that progress may be had in the future. In the attachment, I'll also include some correspondence to and from Shimony. The way I would characterize Abner is that he is Bayesian (of a flavor) *about* theories, but, within a theory, he is objectivist about its uses of probability (or at least this is so for him with respect to quantum theory).

To me, making that move—i.e. giving up on the Bayesian account *within* quantum theory—is the greatest impediment to understanding. The empirical evidence I would give for this is simply the number of years Abner spent taking as serious and reasonable a search for collapse theories (say of the GRW variety). Also the lengthy discussions he's had about the "surprising" consistency between special relativity and wave-function collapse—an effect he dubbed "passion at a distance" to contrast it with "action at a distance" (i.e., trying to make it clear that one could not use wave-function collapse for signaling superluminally).

The thing Abner does not yet appreciate is the immense leading power of taking Bayesianism seriously *within* quantum theory. The way I would characterize the present stage of my research program is, "Seek and ye shall find." An awfully good example comes from the thing I pointed out above: The EPR scenario actually represents pure Bayesian conditionalization if written in the right way. Far from being anything mysterious—like passion at a distance—it simply represents the usual conception of conditionalization. In fact, it is only the method of updating one's probabilities for systems with which one makes physical contact that has any mystery or calls for an explanation. (I.e., the situations where one must insert an extra unitary update.)

So, seek and ye shall find: Look for a Bayesian reason for this or that within quantum mechanics, and one finds—my experience tells—that the theory is a much tighter package than maybe first imagined. It's only in the slight deviation from Bayesianism that things really get interesting.

01-09-03 *One Further Thought* (to S. Savitt)

One further thought. I retumbled the note I wrote you earlier this morning in my head as I was walking to work, and it dawned on me that maybe I went too far in the other direction in trying to clarify things for you. I wrote:

> Yes it is generally incompatible; the deviation represents part of the "only a little more" in the paper's title. In Sections 6 and 6.1 of my paper I deemphasized the incompatibility (or rather, "deviation from Bayes") because I wanted to first clinch it

in the minds of the readers that quantum collapse has a large component that is simply Bayesian conditionalization. The unitary readjustment quantifies the "deviation from."

I want to emphasize that this deviation only concerns *updating*. However, I show in the fat paper that there is another role Bayes' rule plays in quantum measurement (just prior to the update), and in that role it is indeed "Bayes' rule full stop" that is the crucial idea. Namely, I show that there is a one-to-one correspondence between the full set of quantum measurements (i.e., the POVMs) and applications of Bayes' rule.

So, when you wrote

Savittism 5: *Is the kind of updating of your beliefs that you do as the result of a measurement in QM supposed to be incompatible with standard Bayesian updating. (I thought one theme of your "Quantum Information" paper is to show that these processes were at root the same.)*

you were more right than I was giving you credit for. There is a one-to-one correspondence between possible applications of Bayes' rule and measurement devices (or, as I expressed it in "Me, Me, Me," actions that my will can take). But, whereas in classical measurement the updating stops there, in the quantum case there is a little more.

01-09-03 *Fragmentation and Wholeness* (to N. D. Mermin)

I hope you are enjoying all the wholeness in well-being Japan and the bodhisattva can afford.

As for your fragment, I am having quite some difficulty understanding it: There was only one thing I could latch onto enough to dislike. Namely, I object to this phraseology:

Merminition 133: *It's terrible for teaching computer scientists quantum mechanics. The subject can't get off the ground with warnings that you can't be sure a measurement gate is a measurement gate, a cNOT is a cNOT, that no gates act except the ones you set up, etc.*

You still slip into a mode of language that acts as if there is some magical fact out there that is going to confirm that a cNOT is really a cNOT or not. The point is, I would just never talk this way. Let me give an example. Suppose I am talking about a single toss of a coin, and I say, "For me, personally, the probability that it will land heads is 50/50." In making such a statement, should I worry myself that maybe my personal probability is not really 50/50 after all? Or to put it another way, and borrow your sentence structure, would one ever say of classical probability theory:

> Things I don't like.
> 1. It's terrible for teaching gamblers how to gamble. The subject can't get off the ground with warnings that you can't be sure a 50/50 probability assignment is a 50/50 probability assignment, ...

In our language, a measurement gate is a prior. A cNOT is a prior. Neither are open to empirical verification beyond soul-searching. The only thing that ever needs be said to the student is, "IF your judgment of this is 'measurement gate', and your judgment of that is 'cNOT', and that is 'phase gate', etc., etc., then for coherence you must judge the outcomes of the computation with such-and-such a kind of probability." Notice the single quotes; they are important. I did not say "If your judgment of this is that IT is a measurement gate, and your judgment of that is that IT is a cNOT, ..." The latter kind of phraseology would lead one to think that the words 'measurement gate' correspond to a FACT of which one's judgment can either be correct or incorrect.

I'll leave it at that for now. Honestly, I got quite confused on everything else you said. Perhaps Rüdiger will fair better than I did.

As you once told me, don't eat the fugu! (At least until after you clean up your fragment a bit.)

02-09-03 *Peres Festschrift* (to 48 colleagues)

Dear Colleague,

Asher Peres is turning 70 years old next year! On this important occasion we would like to honor him with a festschrift for his seminal contributions to quantum theory, quantum foundations, quantum information, and relativity theory. The festschrift will be published as a number of reserved issues in the journal *Foundations of Physics*.

It is our pleasure to invite you to submit a research paper of about 20 to 30 pages, or even longer if the subject matter requires. The manuscript would be due at the end of February 2004, leaving a reasonable time for reviewing and revision purposes if necessary.

Please let us know at your earliest convenience whether you will be able to contribute. Your contribution would certainly be valued, and we hope for a positive reply!

With best regards,

Chris Fuchs and Alwyn van der Merwe

02-09-03 *'Gleason' Paper* (to P. Busch)

Buschism 1: *Unfortunately I haven't made any progress on the problem you proposed. Have you got anywhere with it in the meantime?*

We've made progress in better understanding the quantumness function—that's recorded in `quant-ph/0308120`—but I've made essentially no progress in the question I asked you ... if I recall correctly the question I asked you. I think I do. Namely, to prove (or find a counterexample) that if the cardinality of a set of states is $d^2 - 1$ or less, then the quantumness of the set is bounded away from the minimum value $2/(d+1)$.

For Holevo's festchrift, I'm going to write a little paper proving the minimum value just mentioned and make a few statements about sets of states that achieve it. Unfortunately, I'm just going to have to conjecture the converse theorem above.

02-09-03 *The Archivist* (to I. Pitowsky)

Thanks for sending those pieces. The Maxwell one was especially nice in that Rüdiger and I are presently working up a paper on "On Quantum Certainty." Some of the phrases used in the letter fit it well! Anyway, it's kind of nice being the historian of the field.

Itamar's Preply

> From: *The Life of J. C. Maxwell* – L. Campbell and W. Garnett, London: Macmilian and Co., 1882.
>
> A letter from Maxwell to Campbell probably from June 1850:

"...I was thinking to-day of the duties of (the) cognitive faculty. It is universally admitted that duties are voluntary, and that the will governs understanding by giving or withholding Attention. They say that Understanding ought to work by the rules of right reason. These rules are, or ought to be contained in Logic; but the actual science of Logic is conversant at present only with thing either certain, impossible of *entirely* doubted, none of which (fortunately) we have to reason on. Therefore the true Logic for this world is the Calculus of Probabilities, which takes account of the magnitude of the probability (which is, or which ought to be in a reasonable man's mind). This branch of Math., which is generally thought to favour gambling, dicing, and wagering, and therefore highly immoral, is the only "Mathematics for Practical Men," as we ought to be. Now, as human knowledge comes by the senses in such a way that the existence of things external is only inferred from the harmonious (not similar) testimony of the different senses, Understanding, acting by the laws of the right reason, will assign to different truth (or facts, or testimonies, or what shall I call them) different degrees of probability. Now, as the sense give new testimonies continually, and as no man ever detected in them any real inconsistency, it follows that the probability and *credibility* of their testimony is increasing day by day, and the more a man uses them the more he believes them. He believes them. What is believing? When the probability (there is no better word found) in a man's mind of a certain proposition being true is greater than that of its being false, he believes it with a proportion of faith corresponding to the probability, and this probability may be increased or diminished by new facts. This is faith in general. When a man thinks he has enough of evidence for some notion of his he sometimes refuses to listen to any additional evidence *pro* or *con*, saying, "It is settled question, *probatis probata*; it needs no evidence; it is certain." This is knowledge as distinguished from faith. He says, "I do not believe; I know". "If any man thinketh that he knoweth, he knoweth yet nothing as he ought to know". This knowledge is a shutting of one's ears to all arguments, and is the same as "Implicit faith" in one of its meanings. "Childlike faith", confounded with it, is not credulity, for children are not credulous, but find out sooner than some think that many men are liars."

03-09-03 *Stuff* (to A. Plotnitsky)

Plotnitskyism 10: *It's good to hear from you–and not without some synchronicity, as I was in fact reading your "correspondences," now in print, of course. [...] Although I have of course read most of it earlier, there are parts of it, to which I continue to return, since they deal with truly essential and as yet (for me at least) not quite resolved points, plus it's a real pleasure to read. So I keep a copy handy.*

Thanks for all the flattery. Actually, you know something that would be really useful to me, and maybe more entertaining for you, would be if you'd have a look at my newer samizdat *Quantum States: What the Hell Are They?* posted at my webpage. It's probably much harder reading than the older one, but also—by my own reckoning—it's much deeper: It can't quite be nibbled on in the same way. Anyway, Rüdiger and I are going to try to condense a good piece of that document into a single paper this fall. The working title is "On Quantum Certainty." After we post our paper on `quant-ph`, I plan to post the said samizdat too. So it'd be useful to get any feedback I can on it. For instance, if you notice any missing cross-connects that make it difficult to read, etc., it be great to know about that!

By now that's old stuff for me, but it might be new stuff for you. Rüdiger counts it as where we make our most decisive break with Bohr. Here's the way he once put it to me. He said, "Bohr would always draw measuring devices as these heavy pieces of equipment, firmly bolted to table tops to emphasize that they are classical matters of fact. But you don't that. You bolt your measurement devices to the tops of turtles! And then it's turtles all the way down."

The next stage of my development is captured in still another samizdat, titled "Darwinism All the Way Down (and probabilism all the way back up)"—about 100 pages—but unfortunately Mermin hasn't given me permission to share that with anyone yet. I guess I need to edit him out a bit more first.

Plotnitskyism 11: *I feel, though, a bit intimidated by the list of potential contributors and I am worried that whatever I'd write may not be technical enough.*

You shouldn't be intimidated at all. I know that Asher respects you; that's why I put you in the list. Also you met Asher's approval: Believe me, not everyone in my initial list survived! So, just write us an interesting paper, and everything will be OK.

Plotnitskyism 12: *P.S. I was thinking of you in yet another context the other day. Are you aware that both formulas $E = mc^2$ (no surprise here of course) and $E = h\nu$ apparently occur for the first time in Einstein's articles, on special relativity and on photoeffects, both in 1905? Planck, it appears, did not write $E = h\nu$, although it is of course implicit in his articles on the black body radiation. He must have pondered their connections, especially given Lorentz's work, although photons are of course not electrons in terms of their (quantum) mechanics or electrodynamics.*

I thought $E = mc^2$ first come up in a 1907 paper. You might check into that. As far as $E = hv$, I guess I wasn't aware of that. However, I do seem to recall that in Planck's original paper he had an incorrect *derivation* of the (correct) black-body radiation formula. And I believe it was Einstein who fixed that up in his PhD thesis (in 1903 or 1904). This is not a fact emphasized in the physics textbooks. But it must have put Einstein on the map with respect to Planck's attention. I think a good source of these stories might be Kuhn's book on black-body radiation, but I've never read it.

By the way, I finally met Paula's friend Jane Nordholt at a quantum cryptography meeting in Virginia in June. She said that she hadn't heard from Paula in quite a while (and I think maybe didn't know how to get in touch with her since her move to NY). I said I'd write you about this—but then promptly forgot to! Tell Paula I apologize. Is everything going well with her? Has she settled into NY fine?

03-09-03 *Pronouncing Fuchs* (to R. Schack)

If you are around, could you send me a pronunciation guide (perhaps using LaTeX) symbols for how to pronounce the German words for "book" and "fox." I might want to capture how Gell-Mann mispronounced my name in this *Physics World* book review I'm writing (when describing the author's own discussion on the subject). If it do it, I'm going to have to do it with tact. But still, I would like to know how to write these things anyway.

Rüdiger's Reply

> Here is your pronunciation guide, with explanations of the symbols. The phonetic alphabet used is that of the International Phonetic Association (IPA), the explanations come from the Langenscheid Großwörterbuch Deutsch-Englisch.

[**u:**] Buch [bu:x] long, resembles English oo in boot, but closer and more retracted than this

[**x**] Buch [bu:x] similar to Scottish ch in loch

[ʊ] Fuchs [fʊks] short, resembles English u in bull, but closer and more retracted than this

Good luck!

04-09-03 *Letting You Down, but Bringing You With* (to G. Brassard)

I had to make a tough decision yesterday: I decided not to come to the Wye River Meeting. My travel schedule just got too hectic again, and Richard Hughes had written me, "if you would be coming from Ireland especially for this I would recommend saving the wear and tear unless you have other business in the US." So, I took him up on it: I didn't really have any other business in the US. But you probably don't need me anyway; you'll have plenty of fun with Charlie if you go.

Anyway, that's the bad news. But let me follow through with a proposal that we've discussed before—you might find some good news in it. Let us DO write a paper together for Asher's festschrift. "Brassard and Fuchs, together for the first time," the show bill could say. I'd like to call the paper "Quantum Foundations in the Light of Quantum Cryptography" (or some variation thereof) and I'll do almost all the work for it. The only thing you'd have to do is decide whether you can agree with most of the outlandish statements I'll be making. I'll have plenty of equations, though, so that it'll look respectable. In this paper, I want to emphasize an information-disturbance principle (akin to the mechanism behind secure QKD ... thus the title and your involvement) more than I have in the past with my "Quantum Foundations in the Light of Quantum Information" papers. It'll be a good quality paper. Will you join me?

More immediately, after I finish the book review I'm writing for *Physics World* today, I plan to immerse myself in the ARDA roadmap business starting tomorrow or Monday latest. I sure hope we'll be able to put our heads together for this. I'm gonna need a lot of guidance!!

04-09-03 *Nietsnie dna Rhob* (to R. Schack)

Schackcosm 83: *In Munich, I visited a small Einstein exhibition in the Deutsches Museum, where the appended small letter* [[from Bohr to Einstein]] *(my translation) was displayed. Isn't that exactly the Buridan's ass point?*

> *Dear Einstein,*
>
> *Many thanks for your kind lines. It gave us all great joy to express our feelings on the occasion of your birthday. To continue in the same playful tone, I can't help saying about the unsettling questions that, in my opinion, the issue is not whether or not we should stick to a reality amenable to physical description, but to follow the path shown by you and to identify the logical conditions for the description of the realities. In my bold way I would even like to say that nobody—not even the good Lord—can know the meaning of an expression such as "playing dice" in this context.*
>
> *With best wishes Niels Bohr*

You and your ass! As always, I have trouble deciphering Bohr. There is something about the language pattern that never sinks in to me. I know you want to say that he is saying that it is *meaningless* to discuss whether the world itself is either determined or undetermined. But why? I don't see why it is meaningless. It may not be fruitful to ask. It may not be answerable from within our inside or finite perspectives. Both of those things I would be willing to grant you, and I regimen my life appropriately taking them into account. But why is it meaningless? I don't think Bohr ever gave an answer, and that is why it is pretty hard to find him convincing.

And don't forget to tell me how to pronounce Fuchs.

07-09-03 *correspondentx.com* (to Correspondent X)

I have gone to your website and looked at your open letter. You seem like a sincere person—one who practices a healthy amount of self-criticism, which is one of the most important assets of a scientist.

Concerning the content of your letter however, I am not the best person to write. Unfortunately I have gathered the reputation of an enthusiastic correspondent: No doubt it comes about predominantly from my having published reams of my own correspondence. In the last year alone, I count 14 unsolicited letters from people unknown to me offering either a) to derive quantum mechanics on the cheap, or b) to demonstrate that quantum mechanics is completely wrong. This is not an exaggerated number; I keep all these letters in a single folder within my email program.

What I mean by "deriving quantum mechanics on the cheap" is a magical combination of English words—very rarely an equation—that makes the whole edifice (i.e., all those equations, relations, and mathematical structures) of quantum mechanics make sense or even, all of a sudden, become obvious.

You've got to realize it is a difficult call for me. On the one hand, you might be the one who really has hit that magical combination—one that conveys an idea that has never before been conveyed, or at least, of an old idea, says it in a way that finally makes it become crystal clear and undeniable. Who am I to stifle that? On the other hand, of your letter, I can say it didn't "feel right" to me.

Now, I could give you reasons for that, but if I were to do so I would have another correspondent on my list. And that would mean more Saturdays and Sundays away from my family as I plug away on letters to another complete stranger who hasn't yet entertained me. It's harsh, but I have to make a judgment call or I would be inundated—I already am inundated! You ask is there a difference between POVMs and q-numbers somewhere in the middle of your letter, and I think, "If he couldn't get this straight from my paper, what good is more correspondence going to do?"

All I can say is, if you are confident of your work, publish it or post it on `quant-ph`. Someone will eventually take the bait and give you feedback, and if no one does, that is feedback in itself.

As I say, and I am serious about this, you strike me as a sincere person. I apologize if I have hurt your feelings, but I really can't take on another correspondent right now.

09-09-03 *The Missing Word* (to A. Plotnitsky)

The missing word was "leads."

> It is surely a significant feature of the theory that consideration of impossible outcomes and very little else leads, without any invocation of "the uncertainty principle" or "maximal information", to the fact that pure state assignments must be unique, as well as the more general constraint on mixed-state assignments.

Thanks for catching that!

09-09-03 *Nonperiodic Penrose Tiles* (to R. Schack)

Below is the transcription of the notes from our conversation. They're pretty holey—the e is pretty important at this stage, even if it might not appear in the end product.

- "On Quantum Certainty"
- Brief mention of Bayesian program.
- Penrose seems to rely on correspondence theory of certainty. (unwarranted assumption)
- If we could act "as if"—Martin Gardner—then why not the above?
- But why we can't act is because of Bell.
- Must reassess certainty.
- Draw attention to our own bad ways? A footnote. Simply say we were less noncommittal in our meaning of certainty previously.
- (emails) Objective Certainty + QM $\Longrightarrow$ Objective Probability
- (Montréal) Objective Operations $\Longrightarrow$ Objective Quantum States
- Repeat key arguments for subjectivity of states.
- Argument for subjectivity of POVM and preparation operation.
- Very careful to state(ment) what we mean by subjective judgments. It does not mean one can believe anything one wants to believe. One issue is to decide when intersubjective agreement can be had.
- Spekkens!
- What means quantum randomness? It means that if I make a pure state assignment, I am also certain that no one else has more privileged information than me. (Rüdiger's point against Gisin crew + anti-Växjö paper)
- Once one has got the concept of certainty straight, what would having an underlying instruction set add? (Rüdiger's point.)

Unfortunately, that's all I have written down. It seems like we said so much more. Also, I'm not even sure what some of the notations above mean.

09-09-03 *Warm Hearts* (to H. J. Kimble)

Thanks! I think there'll only be one more application going off soon: [. . .]
But now this

Kimble-ism 1 : *I remain a big fan of your work, including your recent manuscript that explored the question of the "quantumness" of states (even if you should be burned at the Quantum Optics stake!).*

really warmed my heart! Thanks. I'll post another paper soon on the quantumness of a Hilbert space, which I can now prove is $2/(d+1)$ for dimension d. In it I want to say some fantastical things about the Eötvös experiment ... how quantumness plays the role of mass in these new considerations. And I'll say some things along these lines in my talk at Preskill's group meeting Oct. 16. Hope you can come. (And don't miss Rob Spekkens' talk Oct. 14; it's really really really good work.)

09-09-03 *The Big Man Speaks* (to M. Sasaki)

We just got a little more positive feedback on our paper. This sentence just came from Jeff Kimble:

> I remain a big fan of your work, including your recent manuscript that explored the question of the "quantumness" of states (even if you should be burned at the Quantum Optics stake!).

Do you understand the joke? In any case, it is a compliment to us. He is saying that we should be set to fire and burned alive as religious heretics have been in the past. A stake is what people are tied to when they are burned this way. The reason he is saying this is because we said in our paper (essentially): "[Coherent states] cannot be cloned, and this holds whether the quantum optics community calls such states 'classical' or not." In other words, Fuchs and Sasaki are heretics!

09-09-03 *Feynman's Rainbow* (to M. Durrani)

Durrani-ism 1: *In the final paragraph you say: "I know that Hilbert space makes my heart flutter, but does an atom?" Do you mean that your current work (on atoms?) doesn't really interest you and that you would rather be working on Hilbert space? Or that you are glad of the field you're working in.*

Does an atom make my heart flutter? Not really. But the Hilbert spaces of quantum mechanics do. The implication is the question: Would Feynman think of me as a physicist or not? Should I think of myself as a physicist or not? In other words, this is just like the self-examination Feynman gave Mlodinow. Also, the word "self-examination" refers back to the third paragraph in the review, which is the running theme of the essay.

I read the paragraph to three guys down the hall, and they all got it. I say, let's leave it as written. Or how about this: Just to make it absolutely clear, let me change "Hilbert space" to "quantum-mechanical Hilbert space":

> Still, the book contains a nub of wisdom here and there. Near the end of it, Mlodinow tells how Feynman challenged him to think hard about the emotions he finds upon viewing an electron-microscope image of an atom. "Does it make your heart flutter?" I have found myself thinking about this all week. I know that quantum-mechanical Hilbert space makes my heart flutter, but does an atom? Sometimes the self-examinations of Caltech life are hard to leave behind.

10-09-03 *cond-mat/0309188* (to S. H. Simon)

Simonism 1: *I've heard some buzz about the quantum measurement paper* `cond-mat/0309188`*. Have you guys looked at it? Any comments?*

Well, I see I'm depicted as one of the good guys in their reference 8. Still I doubt they've absorbed what Caves, Schack and I have been writing about.

The way I approach the issue of quantum measurement is to first forget about quantum mechanics and talk about undergraduate probability theory. The textbook gives you a probability distribution $P(h,d)$ over two variables h and d. From it you can derive a marginal distribution over h alone; namely $P(h)$. Now suppose you gather an explicit piece of data d. That may be some information you can use for updating your expectations about h. The mechanism is simply to use Bayes' rule: You update from $P(h)$ to $P(h|d)$.

No big deal, right? So, let me ask you this: Might I have pulled the wool over your eyes any? In particular, shouldn't I tell you about the precise mechanism the brain uses for updating from $P(h)$ to $P(h|d)$? Shouldn't I give a physical explanation for the process? Isn't the undergraduate textbook treating the problem incompletely by not requiring that I fill in those missing steps?

I think you'd be crazy if you said "yes" to any of those things. The axiom of updating is primitive within probability theory. It calls for no physical mechanism behind it, and none should be sought.

With that out of the way, let us go back to quantum mechanics and quantum measurement. One can show formally that quantum measurement is precisely the process above—i.e., Bayes' rule in action—modulo an extra readjustment that takes into account the idea that in quantum measurements one might actually have to touch the system one is interested in (and hence cause some kind of disturbance). This is worked out in Sections 4.2, 6 and 6.1 of my paper `quant-ph/0205039`. (Have a look at it: I'll bet money—real money—that my paper is clearer than theirs.)

Anyway, from this point of view, collapse calls for no explanation in the same way that the process of updating from $P(h)$ to $P(h|d)$ calls for no explanation. In fact, you would have to give a physical explanation for latter in order to give a physical explanation for the former—i.e., you would have to give a physical mechanism for Bayes' rule itself. And we've already agreed (or at least we should have) that that would be silly.

Now concerning the paper you've asked about: They clearly think they've done something. But I'll bet, in the last analysis, they've done essentially nothing ... except lead the reader down an infinite regress that they haven't had the guts to analyze.

Anyway, that's my take after giving the paper not more than a three-minute skim. After thinking about these issues for years, one realizes that papers like this aren't so different than the latest proposal for a perpetuum mobile. It's a question of finding the flaw, and then—and this is always the hard part—convincing the authors that it is a flaw. It's almost never worth the time.

Now, my sarcasm aside: Why has there been some buzz about this paper in the community? What's been found exciting in it?

10-09-03 *Careful Reading Sir* (to D. M. Appleby)

... will be rewarded.

Thanks for the long note and the self-esteem course. I needed the latter!

And I was happy for the former too. However, I think most of the fight you fight in those pages has to do with a Chris that was too sloppy with his words in his youth.

A better Chris to fight with is the one represented in the "Me, Me, Me" note. You might think you read it carefully, but all the evidence shows that you didn't.

In particular, I would localize a good bit of your troubles in your paragraph:

Applebyism 3 : *You say somewhere that Alice has the right to assign any state she wants, just as Bob has the right to assign any state he wants. So what is to stop Bush saying he has the right* *to assign any probability he wants to the proposition "every person held at Guantanamo Bay is a terrorist murderer"?*

But how does that mesh with the following lines from "Me, Me, Me":

> The textbook poses an exercise that starts out, "Suppose a hydrogen atom is in its ground state. Calculate the expectation of . . . blah, blah, blah." One might think it is asking us to calculate some objective feature of the world. It is not. It is only asking us to carry out the logical consequences of a supposed state of belief and a supposed action that one could take upon the system. And here's the clincher about Bayesianism. Just as no student in his right mind would find it worthy to ask why the textbook writer posed the problem with the ground state rather than the first excited state, no quantum theorist should make a big to-do about it either. It is simply an assumed starting point. An agent in the thick middle of a quantum application can no more ask where he got his initial beliefs from, than a pendulum can ask where it got its initial conditions from. The cause of bottom-level initial conditions is ALWAYS left unanalyzed. If such was not a sin in Newtonian mechanics, it should not be a sin in a Bayesian formulation of quantum mechanics.

In the words of the great Marcus Appleby, "It's really hard to believe something you don't actually believe." When—long ago!—I said that Alice had the "right" to assign, I meant that there is nothing about the system itself that determines her state for it. I did not mean that she had the right to believe something she does not actually believe.

Here's another way that I've put it more recently, in a different excerpt from the same samizdat:

> Take a good solid physicist like Steven Weinberg who stakes his career on the search for a grand unified field theory. Suppose he finds it. To find it (I presume) is to declare: The world's Lagrangian is L. Now suppose I were to ask Mr. Weinberg, "Why L? Why not M?" I know for sure his answer will be of the form, "L just is. It is the starting point. It is an ultimate fact of nature; it calls for no explanation. In any case, if it calls for an explanation, its answer must come from outside the realm of science—religion? theology?—but I see no reason to go to such lengths."
>
> Would that make Weinberg a solipsist? A sensationalist? A phenomenalist? The point is Weinberg's stance has nothing to do with any of these labels.
>
> Similarly for the Bayesian (even of the de Finettian variety, despite the mumbo jumbo in the opening sections of Probabilismo). For him, "the prior" on any event space is treated as an ultimate fact—an ultimate fact about the agent. There is no infinite regress because, just as with Weinberg, ultimate facts call for no further explanation.
>
> Bayesian practice—and Rüdiger and I would claim the formal structure of quantum mechanics too—is all about what to do once a prior is established. It is not about what to do before the prior is established. In the quantum mechanical case, establishing "a prior" is 1) to write down a quantum state for all systems considered, and 2) to write down a (conditional) quantum operation for all measuring devices considered.
>
> If there are two agents, there may well be two priors in the sense above—i.e., two ultimate facts (with respect to this level of inquiry). In that sense, the priors are "subjective", but that does not take away their status as ultimate facts in this treatment. It only calls for a recognition that the facts are about the agents.
>
> The role of the separate system and measuring device—now specializing on quantum mechanics—is that when the two are combined they give "birth" to a new ultimate fact:

> The "click." There is no sense, however, in which this new ultimate fact is *about* either of the agents: It has a life of its own. (In fact, it is because of these lines of thought that I sometimes call my view "the sexual interpretation of quantum mechanics.")

Now, I will make the bold claim (because I don't have three days to spend on writing a new note), that if you look carefully at the words above and carefully back at "Me, Me, Me," you will see a lot of your other troubles melt too. For instance, look harder at the glossary. Is there not something in there that corresponds to your "a way things really are"? (Though I would prefer, "a way things really come out.") And further, notice the explicit dualism in the system/POVM definitions. I'm going to claim that that dualism saves me from being *either* an idealist or a materialist.

I believe in FACTS just as much as the next guy: Quit accusing me of not. I only stress that facts are made, they do not preexist. And it is because of this that there is no one way the world is. (The capitalization in Nagel, by the way, was all my addition.)

Finally, here are two excerpts from your note that I really, really liked. I tend to agree with the first (whether you agree with it or not) and I tend to like the sound of the second.

Applebyism 4: *One final point. You say that probability assignments float above the world (or words to that effect). This is true (on your principles) however much they are updated. Rationality resides purely in the updating procedure. The assignments themselves are never rational. Not only are they not rational at the outset, before any updating has been performed. They are no more rational at the finish, after all the evidence has come in. In short, they float. They tell us nothing at all about "the way things really are".*

and

Applebyism 5: *I am tempted to say that the ordinary human world essentially CONSISTS of probability assignments. That probability, so far from floating, is actually constitutive of the world (what we normally and humanly take to be the world).*

10-09-03 *Facts* (to D. M. Appleby)

One final thought, to complete my last note. Looking back at this passage:

Applebyism 6: *One final point. You say that probability assignments float above the world (or words to that effect). This is true (on your principles) however much they are updated. Rationality resides purely in the updating procedure. The assignments themselves are never rational. Not only are they not rational at the outset, before any updating has been performed. They are no more rational at the finish, after all the evidence has come in. In short, they float. They tell us nothing at all about "the way things really are".*

though I generally liked it, I would not use the word "evidence." Facts and data are just facts and data. To say that they are "evidence" already presupposes a (subjective) probability assignment.

11-09-03 *Peres Festschrift* (to R. Omnès)

That is too bad. If you decide to reconsider at any time, please just let me know. I think philosophy of science would be just fine. And certainly you would not have to commit to 20 pages; that was listed only so that contributors would realize that they could use this as an opportunity for a long paper if they wished. But it is not necessary.

Speaking of your philosophy of science, I was so impressed with some of the lines in one of your papers that I copied them into my computer. They are beautiful. (I'll paste them below so that you can see the ones I am speaking about.)

Things go well for me, though I could use some mathematical help!

> R. Omnès, "Consistent Interpretations of Quantum Mechanics," Rev. Mod. Phys. **64**, 339–382 (1992).
>
> Perhaps the best way to see what it is all about is to consider what would happen if a theory were able to offer a detailed mechanism for actualization. This is, after all, what the advocates of hidden variables are asking for. It would mean that everything is deeply determined. The evolution of the universe would be nothing but a long reading of its initial state. Moreover, nothing would distinguish reality from theory, the latter being an exact copy of the former. More properly, nothing would distinguish reality from logos, the time-changing from the timeless. Time itself would be an illusion, just a convenient ordering index in the theory. ... Physics is not a complete explanation of reality, which would be its insane reduction to pure unchanging mathematics. It is a *representation* of reality that does not cross the threshold of actuality. ... It is wonderful how quantum mechanics succeeds in giving such a precise and, as of now, such an encompassing description of reality, while avoiding the risk of an overdeterministic insanity. It does it because it is probabilistic in an essential way. This is not an accident, nor a blemish to be cured, since probability was found to be an intrinsic building block of logic long before reappearing as an expression of ignorance, as empirical probabilities. Moreover, and this is peculiar to quantum mechanics, theory ceases to be identical with reality at their ultimate encounter, precisely when potentiality becomes actuality. This is why one may legitimately consider that the inability of quantum mechanics to account for actuality is not a problem nor a flaw, but the best mark of its unprecedented success.

11-09-03 *Bayesian Beginnings* (to G. M. D'Ariano)

D'Ariano-ism 1 : *I really enjoyed our discussions in Aahrus on Quantum Mechanics, and when I got back to US, I careful read your long paper "Quantum Mechanics as Quantum Information (and only a little more)" published on the Proceedings of the Växjö Conference, keeping also notes for myself (something that I do with quite few papers). I should say that I completely agree with your point of view, and I was happy to see that in many considerations that I made recently, I'm not alone. You know, my school was Masanao Ozawa, and in the last years I taught quantum mechanics of measurements using the Bayes principle. However, after reading your paper I really became a strenuous Bayesian (including tomography).*

Thank you so much for your heartwarming letter. I'm really glad you got something out of my paper. I've been really enjoying your new ones on the structures of POVMs too: But I already told you that.

Yes, it would be great to visit Pavia. Maybe we could work out a good time in the Spring.

D'Ariano-ism 2 : *Sorry if I bothered you. My problem is that now I became too much involved in such re-considerations, that I can hardly think to anything different.*

You didn't bother me at all. This is what I live for! They say every parent wants a better world for their children—a better world than they themselves grew up in. This is true: I want to see a complete understanding of the structure of quantum mechanics developed before my daughters go to college.

We're going to do it!

16-09-03 *Two Typos, One Disagreement* (to A. Peres)

Two typos:

1) One day, I came into Rosen's office, and I found sorting out his papers.

2) The EPR article was not wrong, but it had be written too early.

One disagreement:

Asherism 37 : *Information is not an abstract notion. It is a physical object which requires a physical carrier, and in particular is localized.*

Let me lodge my disagreement with this language, by pasting in two old notes. [See 25-04-02 note "Short Thoughtful Reply" to C. H. Bennett and 02-02-02 note "Colleague" to C. G. Timpson.] The first explains my disagreement I think fairly well. The second is there mostly for the purpose of relating an anecdote about Landauer.

Where your language above and [Charles Bennett's] language below come closest to each other is when he talks about the "information you just ate."

17-09-03 *More Than Semantics, I Think* (to A. Peres)

Asherism 38 : *Information is not an abstract notion. It is a physical object which requires a physical carrier, and in particular is* localized.

Our disagreement is localized in your phrase (effectively) "information is a physical object." I would say the thing that we have learned from quantum information is that we cannot, even in abstracto, ignore the physical properties of information CARRIERS. That was something ignored in the original Shannon information theory. But recognizing that is a far cry from saying "information is a physical object" . . . which, to me at least, conveys the idea that information is something that can exist independently of the agent possessing it.

At the end of your article, you write: "Quantum states are not physical objects: they exist only in our imagination." True enough. But, by the view we expressed in our *Physics Today* Opinion, a quantum state is nothing beyond one's information about a system. Here's one of the ways we said it then:

> From this example, it is clear that a wavefunction is only a mathematical expression for evaluating probabilities and depends on the knowledge of whoever is doing the computing.

It seems to me that if you are going to claim that information is a physical object you will be stuck in an inherent contradiction (at least in phraseology).

17-09-03 *Went on to Organize* (to W. M. Fuchs)

Dear Mike,

I've been thinking about what I'd like to write you for your birthday. 50 is quite an important one. On the one hand, I wanted to say, "Well, you're half way there." But that would have been a lie. Life is much, much bigger than that: Even a hundred years isn't the half of it.

There's a song that's been tumbling in my head the last few days that Joan Baez sings. It's called *Joe Hill*, and it's about a labor organizer who was active in the 1910s. It's an important song. It conveys the idea that an honest and true life never dies. With every smile you give your nieces, your brothers, your sisters, your mom, your friends, your coworkers, you'll never die. With every piece of wisdom you pass on, you'll never die. Every bit of science I do stems from the starlit conversations we had in the 70s. Life propagates in a lot more ways than the material.

Keep up the good work!

Happy birthday brother,

Chris

Joe Hill

I dreamed I saw Joe Hill last night,
alive as you or me.
Says I "But Joe, you're ten years dead"
"I never died" said he,
"I never died" said he.

"The Copper Bosses killed you Joe,
they shot you Joe" says I.
"Takes more than guns to kill a man"
Says Joe "I didn't die"
Says Joe "I didn't die"

And standing there as big as life
and smiling with his eyes.
Says Joe "What they can never kill
went on to organize,
went on to organize"

From San Diego up to Maine,
in every mine and mill,
where working-men defend their rights,
it's there you'll find Joe Hill,
it's there you'll find Joe Hill!

I dreamed I saw Joe Hill last night,
alive as you or me.
Says I "But Joe, you're ten years dead"
"I never died" said he,
"I never died" said he.

18-09-03 *Instrumentalism* (to A. Peres)

After reading your new article, I looked up the word "instrumentalism" in my *Encyclopedia Britannica*. Here is what I found.

> **Instrumentalism:** also called Experimentalism, a philosophy advanced by the American philosopher John Dewey holding that what is most important in a thing or idea is its value as an instrument of action and that the truth of an idea lies in its usefulness. Dewey favoured these terms over the term pragmatism to label the philosophy on which his views of education rested. His school claimed that cognition has evolved not for speculative or metaphysical purposes but for the practical purpose of successful adjustment. Ideas are conceived as instruments for transforming the uneasiness arising from facing a problem into the satisfaction of solving it.

19-09-03 *de Finetti vs. Jaynes* (to D. Poulin)

Poulinism 11: *In thermodynamics, the hypothesis of exchangeability seems well motivated, for noninteracting systems at least. Therefore, if one was to measure a complete set of observables, one would be left with a product state of the form $(\rho \otimes \rho \otimes \rho \otimes \cdots)$, i.e. the "probability distribution over states" appearing in the de Finetti representation would have converged to a delta. But if one measures less observables, e.g. a single one, his state assignment should not be a product space unless his "prior distribution over state" has a very special form, which I wouldn't know how to justify. On the other hand, Jaynes tells us that after having measured say the average energy, our state assignment should be a product state as above with $\rho = \exp(-\beta H)$. As you know very well, this is done by maximizing the entropy given the constraint observed: the product emerges as a consequence that the systems are not interacting.*

I understand your problem, and it's a mistake a lot of people make. The main point to keep in mind is that a max-ent assignment is a SINGLE system assignment. One cannot extend it to a multi-system assignment unless one is confident that further measurements will reveal no further information useful for updating.

There is a fairly thorough discussion of this issue in `quant-ph/0010038` by Brun, Caves, and Schack, though in a different context than the one you bring up above. The substance of the issue is the same, however. The Horodecki's had made the assumption that max-ent gives a multi-system assignment and it caused them to get a nonsense result. But rather than thinking hard about where they might have gone wrong, they just plowed ahead and declared the max-ent program to be "wrong" in quantum mechanics.

A more important thing to read is Jaynes' own discussion of the issue. You can find that in reference 13 of the paper above.

Poulinism 12: *How are you? Here, things are going very well, many interesting research projects on the way. As always, I am preaching for the epistemic interpretation of quantum mechanics. Believe it or not, I have almost convinced Valentini that this is the right way to think about states.*

Now this is very interesting!! The last I remembered, you were still fairly agnostic on the issue. I'm really glad to hear of this.

I've been doing some fairly technical things lately. In particular, I've been trying to shore up my old idea that quantum mechanics looks the way it does because it is predominantly about how we should manipulate and update our information in light of the fact that quantum systems have a kind of "sensitivity to the touch." Thus I've been playing with this measure of quantumness that Sasaki and I posted on `quant-ph` a while ago. In the coming month I'll put another paper on `quant-ph` showing how the quantumness of a Hilbert space (by this measure) connects up to doing quantum mechanics on a simplex (remember the pictures I incessantly drew in Montréal?).

The deeper thing on my mind, though, has been how this "sensitivity to the touch" is universal ... in a weird analogy with gravitation. An 18 dimensional Hilbert space, for instance, does not care whether it is embedded in copper or gold. Just like a given amount of gravitational mass does not care if it is embedded in copper or gold. (I use copper and gold because I think those were the metals used in the Eötvös experiment; I should look it up.) This intrigues me to no end. I'm going to give a talk at Caltech on it in October; I wish you could be there to keep me on my toes.

Also Schack and I are finally distilling my big thing "Quantum States: What the Hell Are They?" into a single paper. The tentative title is "On Quantum Certainty." In it we'll tackle the Penrose argument head on and also how quantum operations are epistemic in exactly the same way as quantum states. Your revelation of non-agnosticism, by the way, was the second piece of good news I read today. Concerning this new paper of Schack and mine, David Mermin wrote me this:

Merminition 134: *I think we're closer than it might appear (but I doubt that it's close enough to write a joint paper about it with you and Rüdiger, much as I would enjoy doing it (and I really would, in spite of my fulminations against collaborating in "writing physics".)*

I was absolutely floored! Now that signaled progress! That he would even contemplate joining in on the project signaled that something is really going right here.

Workers of the world unite!, I say. Good hearing from you, and I hope the max-ent references given above will make the answer to your question clear—I think they will.

22-09-03 *Cosmology* (to C. H. Bennett)

Here's something I've been wanting to tell you about for a while, but I kept forgetting. Do you recall the conversation we had after our dog Wizzy died? You thought briefly that I had beat around the bush with Emma, not quite telling her the truth about the event. (I'll place the old note below in case you don't remember.)

Anyway, about six months to a year after that—I wish I could remember precisely when—Emma was hanging out in my office one day, kind of swinging back and forth with the door, and out of the blue asked, "Dad, will you die one day?" That one was a tough question: I knew I had to say yes, but I didn't want to say yes. In the end, I told her, "Yes, but you shouldn't worry about it so much; it won't happen until you're grown and have children of your own." Now, that WAS beating around the bush—of course I couldn't know when I'm going to die. Funny though how I thought the promise of having one's own children at the time would soften the blow.

The reason I'm telling you this story after all this time is because Saturday seemed to mark another, perhaps not unconnected, event in Emma's development. For the first time she asked me a cosmological question. She's about 4 and 2/3 years old now. She asked, "What was here before the dinosaurs?" I explained a little about the earth before life, and about how the earth was formed in the formation of the sun. Then she asked, "What was here before the sun?" I said, "Ah, you see the pattern: You can always ask a 'before' question, can't you?" She said, "Wow!" (Not a lie.) Then I said, "Well I don't really know to any extent what was here before the sun," deciding to skip a discussion about the big bang and all that. I guess she decided to have some fun with me because then she came back with, "You don't know what happened before the sun??!" I said, "Nope, no one does really," deciding to be metaphorical for the moment—i.e., switching the sun and the big bang in my mind. She questioned in surprise, "Not even William James?" I said, "Not even William James."

22-09-03 *More General Relativity* (to C. H. Bennett)

Bennettism 20: *Reading your paper on quantumness of a set of states, I see that you define quantumness of a set of states by pessimizing the QCQ channel's I/O fidelity over all choices of probabilities of states in the ensemble, while optimizing it over measurements and preparations of the states resp. before and after they are forced through the C part of the channel. In this sense you view a set of states as more fundamental than an ensemble. But isn't a mixed state ρ even more fundamental than either a set of states or an ensemble? What would be wrong with defining the quantumness a density operator ρ as the least expected I/O fidelity for any ensemble realizing ρ; or if that diverged, for any ensemble consisting of r linearly independent pure states, where r is the rank of ρ?*

I don't think it would diverge because the maximum eigenvalue function (which appears in a higher order expression for accessible fidelity) is a convex function. So, the definition is mathematically sensible. I just hadn't seen a motivation for a quantity like that. It kind of reminds me of a quantity that Oliver Cohen once wrote about ... just digging it up, `quant-ph/9907110`.

Actually if you want to go down a route like that, I think it is better to think of accessible fidelity as a function of an entanglement-breaking channel—it is the channel that is the fundamental thing. As Audenaert, King, Winter and I show in the other paper on quantumness, for each ensemble, one can make a channel of particular interest. However then, for any ensemble that gives rise to the same channel, one gets the same accessible fidelity.

In any case, what I'm most interested in at the moment is the quantity I called "quantumness of a Hilbert space." I can now prove that it is numerically equal to $2/(d+1)$, and that to achieve that value on a tensor-product Hilbert space one must use an ensemble of entangled states. I'm writing that paper right now.

What intrigues me most is the universality of some of these concepts like quantumness (as a potential characterization of Hilbert space itself). I guess you've been saying something similar for a lot of years, talking about the fungibility of quantum information, but it had never really taken me until I started thinking about analogies with general relativity. Here's the way I put it to David Poulin the other day:

> The deeper thing on my mind, though, has been how this "sensitivity to the touch" is universal ... in a weird analogy with gravitation. An 18 dimensional Hilbert space, for instance, does not care whether it is embedded in copper or gold. Just like a given amount of gravitational mass does not care if it is embedded in copper or gold. (I use copper and gold because I think those were the metals used in the Eötvös experiment; I should look it up.) This intrigues me to no end. I'm going to give a talk at Caltech on it in October; I wish you could be there to keep me on my toes.

If you were there, maybe I'd be dancing a ballet!

22-09-03 *Lockbox Louder* (to J. A. Smolin)

Attached are my remarks. They're almost all trivial and about presentation.

I enjoyed the paper a lot. In fact, once you touch the paper up, I wouldn't mind reading it again. I was particularly taken with your real toy model: Mainly by just how much it resembles an EPR pair ... or correlation without correlata as David Mermin would say. (Well, the correlata are there but completely hidden—they almost might as well not be there.) That is intriguing.

I'm sure I did a lot of things to the draft that will annoy you. Certainly feel free to ignore any and everything you wish.

John Smolinism 6: *A* lockbox*, the basic unit of matter in this theory, is an object akin to a physical box locked with a combination lock, which can contain a bit value b. The value cannot be read out of the lockbox except if a particular string of bits C—the combination—is presented to it. The bit b and combination C are chosen by the lockbox's creator at the time of its creation. If the lockbox is presented with an incorrect combination, the bit value is destroyed. This box need not be allowed by physics, it is instead the building block of the toy theory.*

Such a box cannot exist in classical mechanics. It is often said that one way in which quantum mechanics differs from classical mechanics is that it cannot be represented by a local hidden variable theory. This statement hides a common oversight about classical mechanics. Classical mechanics also is not correctly represented by a local hidden variable theory, but by a local unhidden *variable theory—in principle every possible property of a classical system can be measured perfectly.*

You might cite the "nonlocality without entanglement" paper here. Apparently also, Bell himself somewhere along the way made this distinction. There's a nice discussion of it in Marcus Appleby's paper `quant-ph/0308114`, pages 14–16. Why don't you cite him too? He, or Bell, called them "exposed variables" interpretations.

John Smolinism 7: *Thus a lockbox explicitly mimics the quantum property that unknown non-orthogonal states cannot be cloned (copied) or even measured without disturbance.*

Have I ever told you how much I hate it that physicists have taken it upon themselves to start a new rule in English: Thou shalt hyphenate all words beginning with the prefix non. And they do this in spite the fact that almost every dictionary writes nonabsorbent, nonabsorptive, nonadaptive, nonaddictive, nonadhesive, nonadjacent, nonadsorbent, nonadsorptive, nonaged, nonagenarian, nonaggressive, nonalcoholic, nonaligned, nonappointive, nonarbitrable, nonarbitrary, nonarboreal, nonassertive, nonassociative, nonastringent, nonautonomous, noncitizen, noncombatant, noncommissioned, noncompliance, nonconductor, nonconformance, nonconformist, nonconformity, nondepository, nondescript, nondevelopment, nondirectional, nondiscretionary, nondisjunction, nondrinker, nondriver, nonentity, nonequivalence, nonessential, nonevent, nonexistence, nonfat, nonfeasance, nonfiction, nonfictional, nonflowering, nonfluent, nongonococcal, nonindulgence, noninterference, nonintervention, nonlinear, nonmalignant, nonmember, nonmetal, nonobservance, nonoccurrence, nonparametric, nonparticipant, nonparticipation, nonparticulate, nonpartisanship, nonpayment, nonperformance, nonrapid, nonreader, nonreligious, nonremittal, nonresistance, ... This is a habit that has got to be squashed.

John Smolinism 8: *It is straightforward to implement secure key distribution using lockboxes. As in quantum key distribution, Alice and Bob are assumed to share an ordinary classical channel, which is unjammable and authenticated, and to have the ability to create and send states, in this case lockboxes, to each other.*

We've had this discussion before: One sends systems not states. There is a difference in my eyes and an important difference. One never speaks of sending the color red. One never speaks of sending happy down the channel. Lockboxes are not states, but systems.

22-09-03 *A Thought of You* (to A. Shimony)

I thought of you when I wrote a little story about my daughter Emma and cosmology to Charlie Bennett earlier this morning. In particular, I was thinking about how you wrote me this:

Shimonyism 3: *Another bridge is our common admiration of the pragmatic tradition in American philosophy. You love William James, and I have respect for James but reverence for Charles S. Peirce.*

Clearly, you and Emma have attuned to the same vibe!

I'll attach the story (for your enjoyment) and the previous obituary of our dog Wizzy (so you'll understand the complete context). Everything below is copied in reverse chronological order. [See 22-09-03 note titled "Cosmology" to C. H. Bennett, 17-02-02 note titled "The Process" to C. H. Bennett, and 16-02-02 announcement titled "Some Things Should Not Pass".]

No need to reply. I was just thinking of you.

Abner's Reply

> Thank you for the beautiful letter concerning Wizzy and Emma. I was very moved by your conversation with her, both with her questions and remarks and with your psychological difficulties in telling her some painful truths. You were certainly right to meet her fear of your death by the supposition that she would have children of her own. Why not share with your child the thoughts that give you the most consolation – namely, that something you care infinitely for will live after you? Most of all you care for your daughter living after you, but why not generalize somewhat and tell her that you care very much that there will be a better world following you. Even a small child can understand the idea of a surrogate for immortality in the continuation of things one is devoted to. I feel that my sons have internalized this idea, and I hope that it helped them when they lost their mother eight years ago.
>
> Her elevation of William James to the status of prophet is both amusing and inspiring.

23-09-03 *The Trivial Nontrivial* (to S. Savitt)

Savittism 6: *Secondly, I realized (as I had not before) that a good way to think about the issue I'm currently struggling with (in philosophy of time, of course) would be to see it as trying to reconcile subjective (or, at least, perspectival) and objective views of time. So, I am using that slender excuse to send you a copy of my paper-in-progress. If you do look at it, you won't see the connection till you get to the second half, the constructive argument.*

Well, I tried to give your paper a shot over the weekend, but it was pretty tough going for me. At times like this, I really feel the lack of a philosophical training! It can be a real impediment. Probably the best critique I could muster for you would be to point out two typos ("Temporal Ontolgy" and "simpliciter, on might say") and tell you to take away the hyphens behind your nons. (Except for possibly "non-usual," which is a non-usual word anyway.)

More seriously, I wonder if you could help me classify the view of George Herbert Mead presented in his little book *The Philosophy of the Present*? For the moment it strikes me as neither a species of presentism nor a species of eternalism (to the extent that either of those terms are coherent, or more to the point, to the extent that Mead is coherent!). I'll attach a copy of a passage I scanned out of Arthur Murphy's introduction to the book; I just happen to have it in my computer (for my "Resource Material for a Paulian-Wheelerish Conception of Nature" project)—you've probably long since forgotten about Mead.

Weirdly I'm starting to take this view more seriously than I had before—even though I have previously used a version of it via John Wheeler's slogan "the past exists only insofar as it is recorded in the present" for dramatic purposes. (See the "Postpartum" chapter, pp. 680–681, at the end of my samizdat.)

Of course, the reason I'm taking it weirdly seriously has to do with quantum measurement theory. It has to do with the Bayesian updating I wrote you about previously. One of the things I emphasized there is that quantum updating (upon the recognition of a measurement result) can be viewed as Bayes' rule simpliciter—thanks for teaching me that word—as long as the quantum system whose quantum state is being updated is causally disconnected from the measurement action. That is true. One thing I did not emphasize, however, is the nature of the argument, x, for such a probability function, $P(x)$—i.e., the one we are talking about using Bayesian conditionalization upon.

The value of the random variable x cannot be viewed as a pre-existent fact of which we are ignorant. Rather it must be viewed as a potential consequence of our actions (i.e., measurements, in older language). Our ignorance is about what will be the actual consequence of our actions, not about what is pre-existent. Well, the same holds true when we are using the outcomes of our quantum measurements here and now for updating our quantum states for something there and then. The updating does not update our ignorance of what was existent there and then, but rather updates our predictability of what would come about *were* we to interact with that system there and then. Unfortunately, the latter we cannot do. The best we can do is to wait for the system to move into our present and interact with it then.

I think this is an important point. Let me try to put it another way: Our quantum measurements in the present never tell us about the past. They only tell us about the consequences of the past for any of our other measurements in the present.

I wish I could put it more clearly than that, but that's the best I can do for now. Despite first appearances, I don't think this is a species of presentism: It does not say "only things in the present exist" (unless it is going to be in your non-usual way). It just says, the quantum states I write down are always ultimately about the present: They gauge my expectations for what *would* happen as a consequence of my reaching out and touching my systems *now*.

Maybe I'll let it go at that for the moment.

From: "Introduction" to George Herbert Mead's *The Philosophy of the Present*,
by Arthur E. Murphy

> The present is to be taken as the locus of reality. This means, I take it, that to consider anything as real is to consider it as existing in, or in relation to, a present. Now what, in relation to any present, is the status of its past? This is not to ask what it was when it was present, for then it was not past and did not stand in that relation by virtue of which it acquires the status of pastness. The past of an event is not just an antecedent present. This is Mr. Mead's main thesis throughout, but it does not often get as clearly expressed as in the following statement. "When one recalls his boyhood days he cannot get into them as he then was, without their relationship to what he has become; and if he could, that is, if he could reproduce the experience as it then took place, he could not use it, for this would involve his not being in the present within which that use must take place. A string of presents conceivably existing as presents would not constitute a past."

The distinctive character of the past in its relation to the present is manifestly that of irrevocability. As conditioning the present, as making its occurrence possible, the past must have been of a determinate character. It expresses the settled condition to which the present must conform and without which it could not have been what it is. And this means not merely antecedent occurrence, it means causal determination or, as Mr. Mead tends to put it, the "carrying on of relations." The past is that out of which the present has arisen and irreversibility—the appeal might here have been made to Kant—has its critical value in terms of such conditioning.

Yet this carrying on of identical relations is never the whole story. The doctrine of emergence asks us to believe that the present is always in some sense novel, abrupt, something which is not completely determined by the past out of which it arose. A present, if it is really new at all, will have in it an element of temporal and causal discontinuity. Recent quantum physics has taught us to believe that such indetermination is quite consistent with rigorous physical analysis. But how is it possible to reconcile this novelty with scientific determinism?

The answer to this question supplies the basic principles of the theory. Before the emergent has occurred, and at the moment of its occurrence, it does not follow from the past. That past relative to which it was novel cannot be made to contain it. But after it has occurred we endeavor to reconstruct experience in terms of it, we alter our interpretation and try to conceive a past from which the recalcitrant element does follow and thus to eliminate the discontinuous aspect of its present status. Its abruptness is then removed by a new standpoint, a new set of laws, from which the conditions of our new present can be understood. These laws could not have been a part of any previous past, for in the presents with relation to which those pasts existed there was no such emergent element. To assume a single determinate past to which every present must wholly conform is to deny emergence altogether. But at the same time, to treat the emergent as a permanently alien and irrational element is to leave it a sheer mystery. It can be rationalized after the fact, in a new present, and in the past of that present it follows from antecedent conditions, where previously it did not follow at all. As the condition of the present, the past, then, will vary as the present varies, and new pasts will "arise behind us" in the course of evolution as each present "marks out and in a sense selects what has made its own peculiarity possible."

Is there any contradiction between this novelty of the past and its essential irrevocability? None at all, for the two apply in different senses. The irrevocable past is the past of any given present, that which accounts for its occurrence. Its determining conditions will be ideally if not actually fully determinable in the present to which it is relative. But when a new present has arisen, with emergent facts which were really not contained in the former present, its determining conditions, hence its past, will of necessity be different. The determinism then holds of the past implied in any present, the emergence in the relation of one such present, with its past, to another.

This hypothesis, in Mr. Mead's opinion, has two main advantages. In the first place it accounts for the attitude of the research scientist toward the data he is describing, an attitude otherwise highly paradoxical. The laws of any science do in a sense reconstruct the past out of which its given elements have arisen. So much is assumed in the establishment of determinate laws, and for the scientist to suppose that the present did not follow from the past in terms of the laws he had established would be to deny their adequacy to the data they interpret. So far as it goes in any field science tends to be deterministic. Yet this "following" of present from past is wholly relative to the data on which the interpretation is based, and the scientist looks forward with equanimity to a new interpretation, and hence a new past, relative to the emergent data which the future will supply. And this combination of relative determinism and future reconstruction which holds for the research scientist, holds also, on this theory, for the nature he is describing.

Secondly, this view is in harmony with the emergence of novelty in experience, and the reorganization of experience in terms of it. This is the theme of the first Supplementary Essay. Even those who "bifurcate" nature most relentlessly must admit that in experience data may appear as intrusive elements in a world which has, in its present constitution, no place for them. They stand in contradiction to that world as currently interpreted and set a problem for reconstruction. To interpret the world exclusively in terms of the conditioning objects which a given period has isolated as the permanent background of becoming is to relegate novelty to a merely subjective experience. But in the case of data relevant to his own problems a scientist makes no such bifurcation. Rather does he treat the data as provisionally isolated in a world that does not now account for them, but as candidates for admission to a reconstituted world which may make the facts previously rejected the very center of its interpretation. So it was, for example, in the status of the Michelson-Morley experiment, first in its relation to classical mechanics, then in the theory of relativity. Within experience new objects are continually arising and a new present reorients the settled conditions of an older era in the light of its discoveries. And if the past is this orientation of settled conditions with respect to present data, the past does empirically change as evolution proceeds. This empirical description has been a part of Mr. Mead's philosophy for many years. The novelty of the present account arises from its correlation with the structure of temporal reality as such, in the relation of a determining past to an emergent present.

At this point the reader will be all too likely to object that it is clearly only our viewpoint or interpretation of the past that has altered here. The past in itself has surely not been changed by the new way in which we have come to look at it. This however is just the distinction that Mr. Mead's whole analysis attempts to supersede. For a temporalist philosophy the past "in itself" is not a past at all—its relation to the present is the ground of its pastness. And this relation is empirically a causal one. If becoming is real that causal relation is never such as to exclude emergence. When emergence occurs a new perspective of the past, a new relatedness, will ensue—a relatedness which is a natural fact about the new situation, though it could never have occurred in the old. And what is here new is precisely the way in which what, in the older present, was merely novel and abrupt has become a part of the world of causal objects, hence a part of the past through which they are supposed to operate. The relatedness is real, and the perspective past it generates, the past of the new present, is the real past of that present, and only for a present can the past be real at all.

Mr. Mead's most objective version of his thesis occurs in Chapter Two, in the contrast between the past as relative to a present and the past as absolute. He holds, especially in criticizing Alexander, that the past which physics requires is simply the expression of identical relations in nature, not an antecedent environment, existing in itself and giving rise, in its isolated being, to all subsequent reality. Space-Time in Alexander's metaphysic, seems to be a mathematical structure taken out of relation to the physical data it interprets and transformed, in all its abstract independence, into a metaphysical matrix from which all the complexities of nature are somehow to be derived. This, on Mead's view, is just what the past "in itself" would be, a conditioning phase of natural process turned into a metaphysical substance. The search for such a substance is not ruled out for those whom it may concern. But the research scientist cares for none of these things.

We seem, then, to have discovered in temporal transition itself a unique sort of relativity, and a set of what we are now to describe as "temporal perspectives" or "systems." Each such system is distinguished by the temporal center from which its relation to past events is organized, and they differ primarily in this, that what is external, contingent, hence "emergent" for one such standpoint will "follow from" and hence be reflected in the past of another. How are such perspectives related, and how does the transition from one to another take place? The answer can be given only when we have inquired into the nature of relativity, and into its social implications.

and

The argument returns at the end, as it should, to its point of departure. It is in a present that emergent sociality occurs. And we can now see that such a present is no mere moment of time, arbitrarily cut out from an otherwise uniform "passage of nature." A present is a unit of natural becoming; it is the period within which something temporally real can happen. What has been and what may be have their focus and actualization in a present standpoint and it is from such a standpoint that creative intelligence, transforming the novelty of emergence and the fatality of mere repetition into a measure at least of meaningful development, brings to articulate and self-conscious expression the pervasive form of natural process. It is as the scene of such process that the present is the locus of reality.

So original a hypothesis will naturally raise doubts and generate formidable problems.

24-09-03 *The Helping Hand* (to R. W. Spekkens)

Spekkensism 20: *This allegory is especially apt for the situation in a toy theory universe, but it misses part of what is going on in quantum theory, specifically, the contextuality of quantum theory: the involvement of the observer in what is observed. My sense is that contextuality is telling us that we need to abandon part of the traditional conceptual framework of the realist, specifically, the notion of a primary quality. This notion dates back at least to Galileo and was refined after the advent of Newtonian mechanics by John Locke. It is meant to be a quality that is inherent to a system and independent of its relation to an observer. I think that many of Berkeley's criticisms of this notion are valid, but whereas his idealism seems to me to have little content or guidance for a theoretical physicist, the relationalism espoused by Leibniz may be a useful conceptual alternative. My hope is that if one takes seriously the idea that the paradigm of systems and properties should be replaced by some sort of paradigm of relations and relations among relations, then the quantum state will be found to have a natural interpretation as a state of knowledge about these relations.*

Quantum theory is whispering something important to us, and these vague ideas are the best I have come up with so far in trying to make out the subject of the conversation.

I was reading a book as I was walking home the other day, as is my habit here in Dublin, trying to avoid running into street lamps, etc., when I came across a passage that seemed perfect for this desire of yours. It was actually something I had read before but never thought to bring up to you.

Anyway, I scanned the passage in this morning and will attach it as a PDF file to this note. Let me know if it helps you articulate what you're trying to get at.

For myself, it still doesn't feel sexual enough, but I have to admit it feels like a move in the right direction. It'll be interesting to hear your reaction.

> **NOTE**: The following passage was taken from Richard Rorty's article "A World without Substances or Essences"—pp. 47–71 in R. Rorty, *Philosophy and Social Hope* (Penguin, London, 1999)—but it is *not* copied verbatim. In particular, I have removed all instances of the words "Dewey," "James," "Peirce," "pragmatism," "human purpose," and a few other words of the same ilk. Also I deleted some whole sentences and paragraphs and, at least once, substituted the word "antiessentialist" for "pragmatist." The goal was to see how the passage would be received with such an ever so slightly different slant. Of course, I didn't reveal this to Spekkens at the time; I revealed it only after the experiment was complete.

We need to break down the distinction between intrinsic and extrinsic—between the inner core of X and a peripheral area of X which is constituted by the fact that X stands in certain relations to the other items which make up the universe. The attempt to break down this distinction is what I call antiessentialism. For antiessentialists, there is no such thing as a nonrelational feature of X, any more than there is such a thing as the intrinsic nature, the essence, of X.

In the rest of this essay I shall be trying to sketch how things look when described in antiessentialist terms. I hope to show that such terms are more useful than terminologies which presuppose 'the whole brood and nest of dualisms' which we inherit from the Greeks. The panrelationalism I advocate is summed up in the suggestion that we think of everything as if it were a *number*.

The nice thing about numbers, from my point of view, is simply that it is very hard to think of them as having intrinsic natures, as having an essential core surrounded by a penumbra of accidental relationships. Numbers are an admirable example of something which it is difficult to describe in essentialist language.

To see my point, ask what the essence of the number 17 is—what it is *in itself*, apart from its relationships to other numbers. What is wanted is a description of 17 which is different in kind from the following descriptions: less than 22, more than 8, the sum of 6 and 11, the square root of 289, the square of 4.123105, the difference between 1,678,922 and 1,678,905. The tiresome thing about all *these* descriptions is that none of them seems to get closer to the number 17 than do any of the others. Equally tiresomely, there are obviously an infinite number of other descriptions which you could offer of 17, all of which would be equally 'accidental' and 'extrinsic'. None of these descriptions seems to give you a clue to the intrinsic seventeenness of 17—the unique feature which makes it the very number that it is. For your choice among these descriptions is obviously a matter of what purpose you have in mind—the particular situation which caused you to think of the number 17 in the first place.

If we want to be essentialist about the number 17, we have to say, in philosophical jargon, that *all* its infinitely many different relations to infinitely many other numbers are *internal* relations—that is, that none of these relations could be different without the number 17 being different. So there seems to be no way to define the essence of seventeenhood short of finding some mechanism for generating *all* the true descriptions of 17, specifying all its relations to *all* the other numbers. Mathematicians can in fact produce such a mechanism by axiomatizing arithmetic, or by reducing numbers to sets and axiomatizing set theory. But if the mathematician then points to his neat little batch of axioms and says, 'Behold the essence of 17!' we feel gypped. There is nothing very seventeenish about those axioms, for they are equally the essence of 1, or 2, of 289, and of 1,678,922.

I conclude that, whatever sorts of things may have intrinsic natures, numbers do not—that it simply does not pay to be an essentialist about numbers. We antiessentialists would like to convince you that it also does not pay to be essentialist about tables, stars, electrons, human beings, or anything else. We suggest that you think of all such objects as resembling numbers in the following respect: there is nothing to be known about them except an initially large, and forever expandable, web of relations to other objects. Everything that can serve as the term of a relation can be dissolved into another set of relations, and so on forever. There are, so to speak, relations all the way down, all the way up, and all the way out in every direction: you never reach something which is not just one more nexus of relations. The system of natural numbers is a good model of the universe because in that system it is obvious, and obviously harmless, that there are no terms of relations which are not simply clusters of further relations.

To say that relations go all the way down is a corollary of psychological nominalism: of the doctrine that there is nothing to be known about anything save what is stated in sentences describing it. For every sentence about an object is an explicit or implicit description of its relation to one or more other objects. So if there is no knowledge by acquaintance, no knowledge which does not take the form of a sentential attitude, then there is nothing to be known about anything save its relations to other things. To insist that there is a difference between a nonrelational *ordo essendi* and a relational *ordo cognoscendi* is, inevitably, to recreate the Kantian Thing-in-Itself.

For psychological nominalists, no description of an object is more a description of the 'real', as opposed to the 'apparent', object than any other, nor are any of them descriptions of, so to speak, the object's relation to itself—of its identity with its own essence. Some of them are, to be sure, better descriptions than others. But this betterness is a matter of being more useful tools—tools which accomplish some purpose better than do competing descriptions. All these purposes are, from a philosophical as opposed to a practical point of view, on a par. There is no over-riding purpose which takes precedence.

Common sense—or at least Western common sense—has trouble with the claim that numbers are good models for objects in general because it seems counterintuitive to say that physical, spatiotemporal objects dissolve into webs of relations in the way that numbers do. When numbers are analyzed away into relations to other numbers, nobody mourns the loss of their substantial, independent, autonomous reality. But things are different with tables and stars and electrons. Here common sense is inclined to stick in its toes and say that you cannot have relations without things to be related. If there were not a hard, substantial autonomous table to stand in relation to, e.g., you and me and the chair, or to be constituted, out of hard, substantial, elementary particles, there would be nothing to get related and so no relations. There is, common sense insists, a difference between relations and the things that get related, and philosophy cannot break that distinction down.

The antiessentialist reply to this bit of common sense is pretty much the one Berkeley made to Locke's attempt to distinguish primary from secondary qualities. The contemporary, linguistified form of Berkeley's reply is: All that we know about this hard, substantial table—about the thing that gets related as opposed to its relations—is that certain sentences are true of it. It is that of which the following statements are true: It is rectangular, it is brown, it is ugly, made out of a tree, smaller than a house, larger than a mouse, less luminous than a star, and so on and on. There is nothing to be known about an object except what sentences are true of it. The antiessentialist's argument thus comes down to saying that since all sentences can do is relate objects to one another, every sentence which describes an object will, implicitly or explicitly, attribute a relational property to it.[72] We antiessentialists try to substitute the picture of language as a way of hooking objects up to one another for the picture of language as a veil interposed between us and objects.

Essentialists typically rejoin, at this point, that psychological nominalism is a mistake, that we should retrieve what was true in empiricism, and not admit that language provides our only cognitive access to objects. They suggest that we must have some prelinguistic knowledge of objects, knowledge that cannot be caught in language. This knowledge, they say, is what prevents the table or the number or the human being from being what they call a 'mere linguistic construct'. To illustrate what he means by non-linguistic knowledge, the essentialist, at this point in the argument, usually bangs his hand on the table and flinches. He thereby hopes to demonstrate that he has acquired a bit of knowledge, and a kind of intimacy with the table, which escapes the reach of language. He claims that that knowledge of the table's *intrinsic causal powers*, its sheer brute *thereness*, keeps him in touch with reality in a way in which the antiessentialist is not.

Unfazed by this suggestion that he is out of touch, the antiessentialist reiterates that if you want to know what the table really, intrinsically, is, the best answer you are going to get is 'that of which the following statements are true: it is brown, ugly, painful to banging heads, capable of being stumbled over, made of atoms, and so on and on'. The painfulness, the solidity, and the causal powers of the table are on all fours with its brownness and its ugliness. Just as you do not get on more intimate terms with the number 17 by discovering its square root, you do not get on more intimate terms with the table, closer to its intrinsic nature, by hitting it than by looking at it or talking about it. All that hitting it, or decomposing it into atoms, does is to enable you to relate it to a few more things. It does not take you out of language into fact, or out of appearance into reality, or out of a remote and disinterested relationship into more immediate and intense relationship.

The point of this little exchange is, once again, that the antiessentialist denies that there is a way to pick out an object from the rest of the universe *except* as the object of which a certain set of sentences are true. With Wittgenstein, he says that ostension only works against the backdrop of a linguistic practice, and that the self-identity of the thing picked out is itself description-relative.[73] Antiessentialists think that the

[72]The properties usually called 'nonrelational' (e.g., 'red', as opposed to 'on the left-hand side') are treated by psychological nominalists as properties signified by predicates which are, for some purpose or another, being treated as primitive. But the primitiveness of a predicate is not intrinsic to the predicate; it is relative to a way of teaching, or otherwise exhibiting, a use of the predicate. The putative nonrelationality of a property signified by a predicate is relative to a certain way of describing a certain range of objects having the predicate. One way of putting the lessons taught by both Saussure and Wittgenstein is to say that no predicate is intrinsically primitive.

For a firm statement, of the contrasting view, see John Searle, *The Rediscovery of the Mind* (Cambridge, Mass.: MIT Press, 1992), p. 211.

[73]On the fundamental importance of this latter Wittgensteinian point, see Barry Allen, *Truth in Philosophy*

distinction between things related and relations is just an alternative way of making the distinction between what we are talking about and what we say about it. The latter distinction is, as Whitehead said, just a hypostatization of the relation between linguistic subject and linguistic predicate.[74]

Just as the utterance of a noun conveys no information to people who are unfamiliar with adjectives and verbs, so there is no way to convey information except by relating something to something else. Only in the context of a sentence, as Frege told us, does a word have meaning. But that means that there is no way of getting behind language to some more immediate nonlinguistic form of acquaintance with what we are talking about. Only when linked up with some other parts of speech does a noun have a use, and only as the term of a relation is an object an object of knowledge. There is no knowledge of the subject without knowledge of what sentences referring to it are true, just as there is no knowledge of a number without knowledge of its relations to other numbers.

Our sense that we can know a thing without knowing its relations to other things is explained away by antiessentialist philosophers as a reflection of the difference between being certain about some familiar, taken-for-granted, obvious relations in which the thing stands and being uncertain about its other relations. Seventeen, for example, starts out by being the sum of 17 ones, the number between 16 and 18, and so on. Enough such familiar statements, and we begin to think of 17 as a thing waiting to get related to other things. When we are told that 17 is also the difference between 1,678,922 and 1,678,905 we feel that we have learned about a rather remote, inessential, connection between it and something else, rather than more about 17 *itself*. But when pressed we have to admit that the relation between 17 and 1,678,922 is no more or less intrinsic than that between 16 and 17. For, in the case of numbers, there is no clear sense to be given to term 'intrinsic'. We do not really want to say that 17, in the secret depths of its heart, *feels* closer to 16 than to numbers further down the line.

Antiessentialists suggest that we also brush aside the question of whether the hardness of the table is more intrinsic to the table than its color, or whether the atomic constitution of the star Polaris is more intrinsic to it than its location in a constellation. The question of whether there really are such things as constellations, or whether they are merely illusions produced by the fact that we cannot visually distinguish the distance of stars, strikes antiessentialists as being as bad as the question of whether there really are such things as moral values, or whether they are merely projections of human wishes. They suggest we brush aside all questions about where the thing stops and its relations begin, all questions about where its intrinsic nature starts and its external relations begin, all questions about where its essential core ends and its accidental periphery begins. Antiessentialists like to ask, with Wittgenstein, whether a chessboard is *really* one thing or 64 things. To ask that question, they think, is to expose its foolishness—its lack of any interesting point. Questions which have a point

(Cambridge, Mass.: Harvard University Press, 1993).

[74]It is useful to think of this Whiteheadian criticism of Aristotle (a criticism found in other early twentieth-century philosophers—e.g., Russell—who tried to formulate a non subject-predicate logic) as paralleling Derrida's criticism of logocentrism. Derrida's picture of a word as a node in an infinitely flexible web of relationships with other words is obviously reminiscent of Whitehead's account, in *Process and Reality*, of every actual occasion as constituted by relations to all other actual occasions. My hunch is that the twentieth century will be seen by historians of philosophy as the period in which a kind of neo-Leibnizian panrelationalism was developed in various different idioms—a panrelationism which restates Leibniz's point that each monad is nothing but all the other monads seen from a certain perspective, each substance nothing but its relations to all the other substances.

are those which meet the requirement that any difference must make a difference.

The residual essentialism of common sense may rejoin to all this that antiessentialism is a sort of linguistic idealism: a way of suggesting that there was really nothing there to be talked about before people began talking—that objects are artifacts of language. But this rejoinder is a confusion between the question, 'How do we pick out objects?' and, 'Do objects antedate being picked out by us?' The antiessentialist has no doubt that there were trees and stars long before there were statements about trees and stars. But the fact of antecedent existence is of no use in giving sense to the question, 'What are trees and stars apart from their relations to other things—apart from our statements about them?' Nor is it of any help in giving sense to the sceptic's claim that trees and stars have non-relational, intrinsic, essences which may, alas, be beyond our ken. If that claim is to have a clear meaning, we have to be able to say something more about *what* is beyond our ken, what we are deprived of. Otherwise, we are stuck with Kant's unknowable Thing-in-Itself. From the antiessentialist's point of view, the Kantian lament that we are for ever trapped behind the veil of subjectivity is merely the pointless, because tautologous, claim that something we define as being beyond our knowledge is, alas, beyond our knowledge.

The essentialist's picture of the relation between language and world drives him back on the claim that the world is identifiable independently of language. This is why he has to insist that the world is initially known to us through a kind of nonlinguistic encounter—through banging into it, or letting it bounce some photons off our retinas. This initial encounter is an encounter with the very world itself—the world as it intrinsically is. When we try to recapture what we learned in this encounter in language, however, we are frustrated by the fact that the sentences of our language merely relate things to other things. The sentences, 'This is brown', or 'This is square', or 'This is hard', tell us something about how our nervous system deals with stimuli emanating from the neighborhood of the object. Sentences like, 'It is located at the following space-time coordinates' are, even more obviously, sentences which tell us about what the essentialist mournfully calls 'merely relational, merely accidental, properties'.

Rob's Reply

Sorry I didn't respond to your last email sooner. I really enjoyed the excerpt on relationalism. This is definitely along the lines I've been thinking. We will have to discuss this at Caltech.

For now, I have a more mundane question. [...]

25-09-03 *The Immediate* (to M. Pérez-Suárez)

I am intrigued by your proposal, but I am also a little frightened by your proposal. A student—especially one in a foreign land far away from his home for any length of time—can be an immense responsibility. I'll try to indicate the sorts of thoughts on my mind, by attaching a letter I wrote to (an undergraduate) Gabe Plunk when he inquired about working with me one summer. [See 26-02-02 note "A Tired Old Man" to G. Plunk.]

Do you feel of yourself that you can fulfill the criteria I laid out for him? Also, do you think of yourself that you can skirt the worries I expressed to David Mermin? Beyond that, what I think *the* program (i.e., the Bayesian quantum information program) *needs* is tireless criticism from withinside—that is the only way it will ever achieve consistency. Do you think you could fulfill that role? The last thing the program needs is a "yes man" who will take much of it on faith.

Think a little bit about it and write me back.

26-09-03 *The Austin Interpretation of Quantum Mechanics* (to A. Fine)

Chris Timpson (a grad student in the philosophy of science at Oxford) brought up the idea of suggesting a 'Quantum Information and Foundations' symposium at the Philosophy of Science Association meeting in Austin, Texas in November 2004. I think that's an absolutely great idea. Would you be interested in throwing in with us on this? (Or maybe just him, if I myself don't have the time.) In any case, do you have any advice on the matter?

I'm picking on you because I read somewhere that you had been a president of the PSA, and also I noticed you seemed to have a developing interest in quantum information.

Here's a link Timpson gave me to look at:

`http://web.archive.org/web/20040511031253/http://`
`www.temple.edu/psa2004/Call_for_Symposia_and_Workshops.htm`

There's evidence that someone on the program committee might already find this an interesting idea.

28-09-03 *PI Workshop on QC-QI-QG* (to F. Girelli & E. R. Livine)

The idea of the meeting sounds great. You can count on my being there; just give me the exact dates as soon as you can. In the meantime, I've marked my calendar tentatively for February 25 through March 3. Strangely enough, lately my work in quantum cryptography has been showing a significant analogy between (finite) Hilbert-space dimension and gravitational mass; I'll be posting a paper on the subject on the LANL archive in the coming month. I have this little desire that it's more than just an analogy, though that's probably just a wild thought. Anyway, the meeting will be a good opportunity for me to get some feedback on this stuff (outside my usual circles).

28-09-03 *Rainy Dublin* (to A. Fine)

Thanks for the reply; I've forwarded it on to Chris Timpson. It'll be nice if something gets organized: I think the time is definitely ripe.

Fine-ism 1: *I see you are in Dublin. Never been there. Is it nice?*

I like Dublin a lot. The people are about the friendliest in the world, and the population in general is quite educated (in comparison with America's). For instance, it's quite easy to strike up an *interesting* conversation with a taxi driver. Not a lie: Three times now, I've met taxi drivers who knew something about quantum cryptography or quantum computing (at the level of *Scientific American* sort of stuff).

But boy does it rain here: Seattle ain't got nothin' on us!

29-09-03 *Equiangular Lines* (to H. König)

I am a physicist at Bell Labs, and I have been working on a problem in quantum measurement theory that requires me to construct d^2 equiangular lines in a complex vector space of dimension d. By this I mean, can one find d^2 unit vectors x_k such that $|(x_k, x_l)|^2 = 1/d$ whenever $k \neq l$? It seems to be a much more difficult problem than I had initially suspected.

Anyway, in searching on the subject, I came across your paper "Aspects of the Isometric Theory of Banach Spaces" with Alexander Koldobsky. Just preceding your Proposition 18—if I understand you correctly—you seem to say that such sets have only been found in dimensions 2, 3, and 8.

Does that remain the state of the art? Also, can you give me a reference for where to find that result? (I could not find a direct reference to it in your paper.) I have been able to work out the $d = 2$ and 3 cases myself, but I would be interested to see the $d = 8$ case. Finally, do you know if this is a problem with a long history? Is it a well-known difficult problem and maybe I shouldn't spend too much time on it? (I.e., I am not a mathematician, and don't want fool myself if the problem will not be reasonably within my reach.)

Thank you for any help you can give.

H. König's Reply

> It is only known for $n = 2, 3, 8$ that there are n^2 equiangular complex lines, probably there are no more cases of existence. In the real case the situation is explained very much in Lemmens-Seidel's paper which is referred to in my paper which I attach. Actually, in the real case, only $O(n^{3/2})$ equiangular lines are known to exist. For $n = 8$, I believe the construction is mentioned in Hoggar's or Delsarte-Goethals-Seidel's paper. It uses quaternions for simpler representation, as far as I remember. In the paper I attach ["Cubature Formulas on Spheres"] I constructed for $n =$ prime power $+ 1$, N equiangular vectors, where $N = n^2 - n + 1$, so it is almost the maximal number n^2 but not quite (see page 6). I attach the TEX as well as the dvi-file of the paper.

29-09-03 *Quotes That Bugged Me* (to A. Sudbery)

I give up: My backlog of email that needs replying to has grown too enormous, and I'm going to have to make some cuts.

Let me just lodge a complaint about two of your quotes that bugged me. These are:

Sudberyism 2: *So I should keep to myself my enthusiasm for ambitious all-embracing theories of reality and not go around insulting people who don't need them, accusing them of copping out and God knows what. But I'm afraid it's still true for me that not to be curious about what makes QM work would be to stop doing science, to switch off what made me want to learn about QM in the first place.*

and

Sudberyism 3: *I have to say that I don't find the existence of objective reality so easy to dismiss as just a faith, which one could simply choose to abandon. The reason that the tension between subjective and objective is a real philosophical* problem *is that there seem to be good reasons for holding the objective view.*

Curt Replies:

1)

Let me try to reiterate the goal of the program Caves, Schack and I are developing. It is not to say that there is no world external to us; it is only to say that there are no quantum states external to us, and then to see where that leads us in understanding quantum mechanics. There is a difference; why can't you see it?

I think it would be hard to call my 59 page paper, `quant-ph/0205039`, a "lack of curiosity about what makes QM work." The whole point of the paper was that we don't have a decent handle on what makes quantum mechanics work. It then tries to systematically explore a particular line of thought—that the quantum state represents information, rather than material property. It does not eschew the very existence of material properties. It simply says that among them, the quantum state is a very bad candidate.

Here is the way I would caricature where you seem to stand in relation to me. Imagine a young student who first learned classical electrodynamics solely in terms of the vector potential. Then one day someone pointed out to him that all the physical phenomena he could see actually depended only upon the fields, not the complete vector potential after all—there is a gauge freedom. Well, flabbergasted, it felt to this student that he was stripped of something he ought to have. So he spent the rest of his life doggedly trying to find a justification for the TRUE gauge. Of course he had to give umpteen ad hoc reasons for why the true gauge could not be measured, but that was the price to pay to do science. (For if you're not trying to see the vector potential as a real property of the world, you're not doing science.) What a pity.

Likewise I would characterize where you want to go and where I want to go with quantum mechanics. You want to see the quantum state simpliciter as a representation of reality. Whereas I think we are more likely to find reality in the "differential." I.e., in the support structure in which quantum states live and in the rules for changing those states in light of how we are stimulated (by the world external to us). The structure of those rules represent something we are assuming of the world as it is independently of us.

And wouldn't we like to know what we are assuming of the world as it is independently of us?

2)

Why do I go to pains to say things like this:

> SYSTEM: In talking about quantum measurement, I divide the world into two parts—the part that is subject to (or an extension of) my will, and the part that is beyond my control (at least in some aspects). The idea of a "system" pertains to a part beyond my control. It counts as the source of my surprises, and in that sense obtains an existence of its own external to me.

as I did in my "Me, Me, Me" note, when you nevertheless respond with things like this:

> I have to say that I don't find the existence of objective reality so easy to dismiss as just a faith, which one could simply choose to abandon.

In any case, my issue with Nagel—the big IF—is not about whether there is a world external to or beyond all agents—objective reality if you will. But whether it is sensible to assume that there can be a REPRESENTATION of it. Like the boy fixated on his vector potentials, one can always act as if there is such a REPRESENTATION from the outside—the view from nowhere—but also like the boy, one might be wasting one's time in doing so. It is my sense that quantum mechanics hints at the latter.

03-10-03 *IC POVMs* (to G. M. D'Ariano)

I just finished reading your new paper on informationally complete measurements. It is fascinating and very nicely done; I learned a lot.

I wonder however if I could ask to cite my paper `quant-ph/0205039` (the one you referred to above) in a reposting of yours—only the `quant-ph` number please, not the Växjö version. I ask this, not because anything in your paper depends in a technical way on my paper, but because I want to spread the word as far as possible. As I see it, it is an issue of getting a large enough workforce to move in these directions if we're going to make long-lasting progress. The picture of doing quantum mechanics on a simplex and the "standard quantum measurement device" that I try to make attractive in that paper depends crucially on the existence of minimal informationally complete POVMs, which I emphasize. For our colleagues to see the deeper connection between your work and mine, I think, can only be healthy.

Also, by the way, the existence of minimal informationally complete POVMs was crucial for our proof of the quantum de Finetti theorem (nonminimal IC POVMs could not be amended to that proof technique): C. M. Caves, C. A. Fuchs and R. Schack, "Unknown Quantum States: The Quantum de Finetti Representation," *Journal of Mathematical Physics* **43**(9), 4537–4559 (2002). [Reprinted in *Virtual Journal of Quantum Information* **2**(9).] `quant-ph/0104088`.

It is interesting that your paper can be used to connect the de Finetti theorem to group theory.

Anyway, if you could make a citation or two, I'd much appreciate it.

03-10-03 *A Plea* (to R. Blume-Kohout, J. M. Renes, & C. M. Caves)

I want to make a plea to you guys to write up what you and your collaborators have on SIC-POVMs and get it out on the web in a hurry. What I would suggest is that you write it up in a style appropriate for the journal *Linear Algebra and Its Applications* and then ultimately submit it there. For that journal, all you need to do is state the raw mathematical problem—that is enough motivation—and then state what you've got (including numerics). I estimate that it would not take more than a week to write such a paper, at least to the quality of posting it on `quant-ph`.

The reason I am writing this plea now are fourfold.

1. I have only recently come to appreciate how old and well respected this problem is, and it appears to me you guys actually know more than anyone else. I'll put a list of references with annotations below. The upshot is that the problem is about 30 years old and has been hit by some *real* mathematicians. The most telling tale comes from Hermann König (a mathematics professor in Kiel) who wrote me: "It is only known for $n = 2, 3, 8$ that there are n^2 equiangular complex lines, probably there are no more cases of existence."

2. Have a look at `quant-ph/0310013` posted this morning by D'Ariano and collaborators. You'll see that they're already working with your beloved group $\mathbb{Z}_d \times \mathbb{Z}_d$ for generating IC-POVMs. I deem that it can't be long before they realize the interest in adding an S to that. And, look, if they write a paper posing this problem you guys will lose a lot of credit for a lot of work: It is not worth it to hold on to it privately anymore.

3. I myself would like to talk more openly about the sensitivity of SIC ensembles with respect to eavesdropping, and I don't feel completely comfortable doing this while you guys continue to sit on your paper. In particular I am writing something for the Holevo festschrift that I'll

call “On the Quantumness of a Hilbert Space” showing that these ensembles (if they exist) are maximally quantum and some related facts. It would be nice to have something solid of yours to cite.

4. The most important reason: You’re holding up science! What I suspect is, if you put your paper in the right audience, someone will be able to run with your $\mathbb{Z}_d \times \mathbb{Z}_d$ conjecture and prove it to completion. Simply be happy that you’ve contributed this much, and give someone else a turn.

I hope I’ve made my case strongly enough.

1. P. W. H. Lemmens and J. J. Seidel, “Equiangular Lines,” Journal of Algebra **24**, 494–512 (1973). This seems to be the father paper of the field, even though they do give some references to earlier work studying equiangular lines in the terminology of “elliptic geometry” (whatever that means). This paper, however, is devoted solely to real vector spaces.

2. P. Delsarte, J. M. Goethels, and J. J. Seidel, “Bounds for Systems of Lines and Jacobi Polynomials,” Philips Research Reports **30**, 91–105 (1975). This paper studies both real and complex spaces. They make it clear that they know of the existence of maximal equiangular sets (i.e., the ones that achieve the “Gerzon bound,” namely n^2 in the complex case) in dimensions 2 and 3. Looking at their references, these cases might have been known as early as 1914 but I have not confirmed that.

3. H. König and N. Tomczak-Jaegermann, “Norms of Minimal Projections,” `arXiv:math.FA/9211211`. I don’t think this paper proves anything of use to the SIC-POVM problem—though I don’t know—but it does show that these questions are intimately related to other issues in mathematics that people take seriously. I.e., this is another reason your paper should be out there.

4. S. G. Hoggar, “64 Lines from a Quaternionic Polytope,” Geometriae Dedicata **69**, 287–289. Apparently the problem considered in this paper proves the existence of a SIC-POVM in $d = 8$.

5. A. Koldobsky and H. König, “Aspects of the Isometric Theory of Banach Spaces,” in *Handbook of the Geometry of Banach Spaces*, Vol. 1, edited by W. B. Johnson and J. Lindenstrauss, (North Holland, Dordrecht, 2001), pp. 899–939. Available online at: `http://www.math.missouri.edu/~koldobsk/`. I’ll repeat what I said above: I don’t think this paper proves anything of use to the SIC-POVM problem, but it does show that these questions are intimately related to other issues in mathematics that people take seriously. You can find an indication of the SIC-POVM problem on page 19 of the web-based manuscript.

6. T. Strohmer and R. Heath, “Grassmanian Frames with Applications to Coding and Communication,” `arXiv:math.FA/0301135`. Not much of interest here, except a rederivation of the Gerzon bound.

7. H. König, “Cubature Formulas on Spheres.” Available online at: `http://analysis.math.uni-kiel.de/koenig/pub.shtml`. This paper is interesting and getting close to the real thing. He derives the existence of $d^2 - d + 1$ equiangular lines when $d = p^m + 1$, p a prime number and m a natural number.

8. B. Et-Taoui, “Equiangular Lines in C^r,” Indagationes Mathematicae **11**, 201–207 (2000). Despite the title, this paper is not particularly interesting (I think): The problem he addresses is much more particular than the title lets on. But, again, it does show that people are thinking about these things.

9. B. Et-Taoui, “Equiangular Lines in C^r (Part II),” Indagationes Mathematicae **13**, 483–486 (2002). Ditto.

10. G. M. D’Ariano, P. Perinotti, and M. F. Sacchi, “Informationally Complete Measurements and Groups Representation,” `arXiv:quant-ph/0310013`.

06-10-03 *Keep It Up!* (to J. M. Renes and C. M. Caves)

I skimmed over your draft this morning [of what eventually became J. M. Renes, R. Blume-Kohout, A. J. Scott and C. M. Caves, “Symmetric Informationally Complete Quantum Measurements,” J. Math. Phys. **45**, 2171 (2004)]. I think it is starting to look good. (However, when I suggested *Linear Algebra and Its Applications* as a target, I was imagining something even more skeletal than that. The whole point of that journal is to leave the words behind.)

Unfortunately I’m starting to think there is even more need for speed than I had thought before. For instance, Koenraad Audenaert wrote me the following last night (I opened up the note this morning):

Audenaertism 1: *I may have something interesting here! (and then again, maybe not ☺) In Dublin you told me about the problem of constructing a SIC-POVM in any dimension. Now I have been thinking about this now and again, but without further results. Last Friday, however, there was this preprint by Mauro d’Ariano et al about the construction of IC-POVMs (not necessarily symmetrical ones). Upon reading it I was struck by their first example (in section 4), where they constructed an IC-POVM starting from an initial pure state and applying what seems to be all possible displacement operators in the associated phase space of the Hilbert space. Well, my left toe immediately started to itch. Can one come up with SIC-POVMs in this way, starting from an appropriately chosen pure state? I wrote a MatLab program that did a brute force numerical search (simplex search) and, believe it or not, for all dimensions I tried (2, 3, 4, 5 and 6) it did come up with a SIC-POVM (the inner products being correct up to 12 decimals!).*

But maybe this is precisely the “numerical evidence” you mentioned.

I asked Koenraad to make sure if he finds something, he would not post before you, but I am not going to be able to ask everyone that.

Now, a couple of notes on your draft.

1. I’m not an author. I wish I had contributed enough to the project to be in the list, but unfortunately I haven’t. (I would be very happy, however, if you would find a way to cite my papers 0205039 and *0302092*.) In general for this paper, you should be careful not to let the author list get out of hand. I would say that hours spent working privately on the project should not be a criterion of authorship.

2. I didn’t understand your argument for linear independence on page 1. The Gram matrix for an SIC ensemble (i.e., the matrix of inner products), will not be a real matrix; there will be phases everywhere. I don’t see why you don’t use Carl’s simple straightforward argument from his web notes to establish linear independence.

3. "However, it is known that for $t = 2$, the minimum number is $n = d^2$, so the SICPOVM is the smallest spherical 2-design." Where is this established?

4. For Section III, I think you should exhibit the form of the expansion of a fiducial projector in terms of the displacement operators D_{jk}. Then comment that the problem of existence boils down to any satisfying assignment for all the phases, and exhibit how nonlinear of a problem that looks to be.

I'll send any further comments as they come to me, but this will probably be my last note for today: I've got to give a talk at Trinity College this afternoon.

06-10-03 *Agreement/Disagreement* (to J. M. Renes and C. M. Caves)

Renesism 10: *OK, I'll try to tighten it up. But Carl also suggested Journal of Mathematical Physics, which in the end is probably what I was going for. (Actually, I don't know so much about that journal either, so what I was going for was the archive. I treat POVM as if the reader already knows what it is, for instance.)*

I agree that `arXiv.org` is the very most top priority. However, I disagree with J Math Phys. It's a pure maths problem, and I deem it will have a much better chance of being solved if the paper ends up in Lin Alg App. In the end it is a problem about equiangular lines, full stop; and the LAA audience is an audience hungry for those kinds of problems. The mathematical-physics community, I suspect, will quickly forget about it if it's not easily tractable ... whereas the LAA audience will plug away at it year after year. They won't denigrate it just because it is a finite-dimensional vector-space problem, where the JMP crowd might—at least that's the sort of reaction I personally have had from them before. Finally, especially if it does turn out not to be so easily tractable, I think you'll ultimately generate more citations (outside of the `quant-ph` crowd) with LAA.

07-10-03 *Congratulations!* (to A. J. Leggett)

Congratulations on the Nobel prize!! Now, almost for sure, I'm not going to get you to write something for the Peres festschrift—who would have the time?!

Anyway congratulations; from what David Mermin tells me about your work, I know it is well-deserved.

07-10-03 *More Covariance* (to G. M. D'Ariano)

Thanks for your long letter, and certainly thanks for the citations in your "informationally complete" paper. I printed out your "Extremal Covariant Quantum Operations" paper today, but now that one I cannot finish off in a lunchtime reading (as I did the last)! It will take me sometime to digest it all.

In fact it will take me some time to digest your long note. Unfortunately, I don't have any answers to your questions.

I think a visit to Pavia would be great as I said before. Two weeks, in fact, would probably be optimal. Unfortunately though, I really can't make any new plans until after the new year. (I will be in Dublin until April 15.)

Finally, let me ask you: Would you like a copy of my book *Notes on a Paulian Idea* (quant-ph/0105039)? It is a samizdat, as David Mermin calls it, more for perusing than systematic reading, but I think it does capture some of the philosophical flair of what we are mutually shooting for in quantum foundations. I have some copies of the Växjö University Press edition for giving away, if you think you or your students might get something out of it. (Kluwer will be reprinting it in a more proper edition, in their *Fundamental Theories of Physics* series, but that will probably be a year away; and I won't have many copies to give away then.) Anyway, if you want a copy, just tell me which mailing address to send it to.

08-10-03 *Heart of the Matter* (to M. Pérez-Suárez)

Pérez-Suárezism 4: *And, finally, on your note about "You, you, you ...". I usually enjoy your papers and notes, and this is no exception. I have never thought of your approach as implying solipsism. I don't even understand how someone might have thought it did. The only point I haven't been able to come to terms with yet is that sort of "unitary readjustment". I do still think that it would have been "nice" being able to get Bayes' rule without any further modifications. Is this thought an indication that I am missing or misunderstanding something which is fundamental?*

No, I don't think it would have been nice at all. I think the modification to Bayesian conditionalization that we see here is one of our clearest (and most quantitative) indications yet that quantum measurement should not be viewed as a passive process by which the agent's mind comes to mirror a pre-existent reality. If the agent were not changing his external reality in the process of having a look, his update rule would have been the regular Bayesian one. The mantra is: The quantum rule only reduces completely to the Bayesian rule when the agent believes he has no causal influence upon the system he is updating his beliefs about. A good case in point for this is to consider making a measurement on one half of an entangled pair. The update rule that one uses on the other half (subsequent to the data gathered on the first half) is precisely the Bayesian rule.

Open question: Why is the readjustment unitary, rather than some more general linear or nonlinear transformation?

08-10-03 *EnNobelization* (to N. D. Mermin)

That was great news hearing about Tony Leggett winning the Nobel prize yesterday. Strangely though, it made me feel very nervous. I guess I had always seen Tony as "wasting a perfectly good mind" (I dug almost exactly that phrase in a note I had written to Asher Peres in July 2000). As I wrote to my friend Greg Comer in reply to one of his points:

> Yep, he's definitely a bread and butter physicist. And clearly very smart and rigorous and—if you could see the equations in his talks—you'd understand that he must be absolutely single-minded when it comes to calculating. It is only that all his later work is predicated on the obstinate belief that the quantum state *must* correspond to an objective property. The Nobel prize was for the sort of work you are talking about. I've only seen him in his later incarnation, i.e., worrying about (and proposing experiments for) quantum foundations. He always expresses surprise and confusion that quantum mechanics has not been seen to break down yet.
>
> Anyway, I just sent him a note of congratulations. It is a very impressive feat to so change the world as he (and the other two) have done.

Well, I guess his Nobel prize taught me! A man shouldn't be judged by his interpretation of quantum mechanics alone!

08-10-03 *Can't Resist* (to H. Barnum & A. Sudbery)

I know that I said I would not write again until I had finished reading Nagel's book, but I came across a passage yesterday in an essay of Richard Rorty's that I could not resist scanning into my computer. It deals somewhat with something Howard wrote us:

Barnumism 8: *During the "Bohmian dialogue" years ago at Hampshire (organized by Herb Bernstein), I came to a couple of important realizations mostly in the process of defining my views against "what we've all learned over the last twenty years" (which included things like "you can choose your own myth," as I recall).*

One of them was that, though I am not explicitly religious, I value "transcendence" (the term was being used as a putdown, I think). That is perhaps a nicer word for what Nagel is calling "detachment"... getting beyond your own limited point of view to an expanded point of view.... even though of course that expanded point of view is still gotten to by you, interacting with others, using more of the different modalities and apparatuses available to you. It is <u>part of</u> *"variety and freedom," (the truth will set you free, dontcha know) and "growth," for me.*

What I dislike most in some strains of "antirealist" modern thought, is their disdain for the value of transcendence, their desire to make everything a useful, folksy, comforting tool for humans.... not that useful, folksy, comforting are not good, but so is getting outside oneself and recognizing the vast unbelievableness of what becomes apparent as one does so....

I don't necessarily think your approach will end up negating that value, in end..... our views may be closer than it seems. I'll read on....

In the present passage, Rorty is talking about culture and politics, but he might as well have been talking about views of physical theory. In fact, in much of the rest of the book *Philosophy and Social Hope* he was.

I will return on Nagel (sooner rather than later).

Here's the passage:

> Insofar as 'postmodern' philosophical thinking is identified with a mindless and stupid cultural relativism—with the idea that any fool thing that calls itself culture is worthy of respect—then I have no use for such thinking. But I do not see that what I have called 'philosophical pluralism' entails any such stupidity. The reason to try persuasion rather than force, to do our best to come to terms with people whose convictions are archaic and ingenerate, is simply that using force, or mockery, or insult, is likely to decrease human happiness.
>
> We do not need to supplement this wise utilitarian counsel with the idea that every culture has some sort of intrinsic worth. We have learned the futility of trying to assign all cultures and persons places on a hierarchical scale, but this realization does not impugn the obvious fact that there are lots of cultures we would be better off without, just as there are lots of people we would be better off without. To say that there is no such scale, and that we are simply clever animals trying to increase our happiness by continually reinventing ourselves, has no relativistic consequences. The difference between pluralism and cultural relativism is the difference between pragmatically justified tolerance and mindless irresponsibility.

So much for my suggestion that the popularity of the meaningless term 'postmodernism' is the result of an inability to resist the claims of philosophical pluralism combined with a quite reasonable fear that history is about to turn against us. But I want to toss in a concluding word about the *un*popularity of the term—about the rhetoric of those who use this word as a term of abuse.

Many of my fellow philosophers use the term 'postmodernist relativism' as if it were a pleonasm, and as if utilitarians, pragmatists and philosophical pluralists generally had committed a sort of 'treason of the clerks', as Julien Benda puts it. They often suggest that if philosophers had united behind the good old theologicometaphysical verities—or if James and Nietzsche had been strangled in their cradles—the fate of mankind might have been different. Just as Christian fundamentalists tell us that tolerance of homosexuality leads to the collapse of civilization, so those who would have us return to Plato and Kant believe that utilitarianism and pragmatism may weaken our intellectual and moral fibre. The triumph of European democratic ideals, they suggest, would have been much more likely had we philosophical pluralists kept our mouths shut.

08-10-03 *Can't Resist, 2* (to A. Sudbery)

Sudberyism 4: *Curiously enough, I am toying with the idea of writing in defence of quantum-mechanical pluralism for my contribution to Asher's festschrift. Where does the Rorty quote come from?*

It comes from the last essay in the book, *Philosophy and Social Hope*. Unfortunately I don't have the book with me at the moment to see what the title of the essay is. I suggest you run to the bookstore and get a copy just as fast as you can. The best things to read are the introductory essay (spurning Platonism), along with the three essays:

- "Thomas Kuhn, Rocks and the Laws of Physics"
- "A World without Substances or Essences"
- "Truth without Correspondence to Reality"

08-10-03 *Jaynes Stuff* (to D. Poulin)

Thanks for the long letter, and thanks for the invitation to work on a project with you and Raymond. I've printed out your draft, and I'll see what I can understand from it. But I fear you might have to give me a lecture in person before much will sink in! (At the moment I am preoccupied with trying to get off to Caltech this weekend and making a talk that won't make me a laughingstock.)

I liked your way of putting things in the Jaynes' principle debate: "One can use Jaynes' principle to assign probabilities at the price of treating it like it wasn't a probability distribution." It seems to be of the right flavor ... though I can't say for sure that I have completely made up my mind on the value of the principle. Since I am now drawn more to de Finetti's personalistic Bayesianism than Jaynes's version of it all, I suspect I would not put nearly as much stock in the "principle" as I used to.

Poulinism 13: *I have also had a look at Jaynes' 1986 paper. I don't see what Brun et al. refer to when they say that "Jaynes [13] has given a thorough discussion of this problem." Maybe he did, but not in this reference.*

Yeah, 1986 would be much too late. Unfortunately, I don't have Jaynes' book to figure out which is the right reference. It had to do with an issue brought up by Friedman and Shimony in the 1970s. I did do a little web search and found this nice paper by Jos Uffink:

`http://www.phys.uu.nl/~wwwgrnsl/jos/mep2def/mep2def.html`.

I think it is dead on the mark with respect to the issue you're interested in. Also, Jos is a very thoughtful person, so I think it is probably a very important one to read.

I'll try to write you again during my California stay.

08-10-03 *Renormalization* (to F. Girelli)

I apologize for the delay in responding to your note. I didn't realize until this morning that it had been eight days since I received it.

Girelli-ism 1: *My question is about renormalization. It is well known that there is an interpretation of renormalization in terms of information: eg when renormalizing we coarse grain so we loose some local information. The ideas of renormalization started with some spin systems, so it seems that the de Finetti representation is a right tool to use.*

Are you aware if the interpretation in terms of information has been pushed forward? And is it possible?

When you do the successive measurements in the context of the de Finetti representation you refine your knowledge, which can be seen as the opposite operation of the coarse graining. It would make sense as well to see how the Bayesian approach is implemented in the renormalization scheme. For example if you deal with only the bare coupling constants (I am taking the Polchinski point of view, or Wilson's), it is not working, you have to measure some of them (the relevant ones) and then update your measurement and everything gets fine.

Do you have any references, ideas, intuitions about those things? I would be very glad to hear about it.

I wish I could say something intelligent on the subject, but I don't think I can at the moment. I had never thought about renormalization in information theoretic terms before reading your note. I did a google search on the words "renormalization group information theory" and the only thing that caught my eye was Robert Tucci's paper "An Application of Renormalization Group Techniques to Classical Information Theory" posted on `quant-ph` ... but I doubt it can be relevant.

Maybe when I come visit Perimeter during your meeting, you can fill me in in more detail what sort of thing you have in mind. I have a hard time formulating a precise question at this stage. It's probably the sort of thing we need a chalk board for. You'd probably have to give me a few lessons before I'd have any good input.

Maybe for this problem—seeing that you are talking about throwing away information or disregarding information—it is best to scrap considerations to do with the de Finetti theorem. Keep in mind that in quantum mechanics, the general expression (or mechanism) for information loss is the trace preserving completely positive map. Which brings me to this point of yours:

Girelli-ism 2: *I am working with David Poulin on the notion of time that we can extract from information theory, in order to understand the notion of time in quantum gravity.*

On this track, I have had some thoughts on recovering the general expression for quantum time evolution (i.e., CP maps) from a Gleason type theorem for the probabilities of outcomes of sequential measurements upon single systems. In a way, that would be an information theoretic accounting of it. What I had hoped to do was something along the lines of what I did in Section 5 of my paper `quant-ph/0205039`, the difference being that the two "systems" should be separated in time rather than in space. The hope was, enforcing causality on the joint probabilities of outcomes (referring to measurements at two different times) would be enough to recover the general form of a completely positive map. I made some progress, but I could never quite get it to work out. Maybe this is another thing we could talk about when I visit.

08-10-03 *The Push* (to J. M. Renes and C. M. Caves)

I'll try to have a look at the new draft tonight. One thing I'm just noticing is that you should put the $d = 2$ case in there. It's the only damned case anyone can visualize; put it in Bloch sphere language. You can crib it directly out of Carl's notes.

Another thing: I think it would be slightly cleaner to write the unitary operators in your Conjecture 1 as D'Ariano does in Eq. (21) of `quant-ph/0310013`. Finally (for the moment): I did not understand your equation 18 nor the discussion right around it.

Why am I giving you more push? Because Koenraad just wrote me that he has now numerically confirmed the conjecture out to $d = 24$ (and counting). He really has come up with all this independently, but I don't think you deserve to add another author and water down the list that much more.

What is a Gabor frame? What is a Weyl-Heisenberg frame?

I've got to run home now. I'll probably be online later.

09-10-03 *Pauli Back* (to K. Gottfried)

Thank you for warm letter; it was quite nice of you.

Gottfriedism 1: *First, from where are those great quotes from Pauli on p. v1? The two words in italics – are they yours – are so central to the enigmas posed by quantum mechanics.*

The italics are Pauli's. The two passages on that page come from his article "Matter", which is included in *Writings on Physics and Philosophy*. There are more Pauli quotes that intrigued me recorded on pages 192 through 195 of the samizdat, in the "Greg Comer" chapter. It was in February and March 1995 that the Paulian Idea really took hold of me, while reading the collection you mention.

Gottfriedism 2: *I am a life-long Pauli devotee, and as a student in the early 1950s, taking advantage of my childish knowledge of German, devoted a large effort to studying his superb Handbuch article, which except for Dirac, was the only serious text then available. Last year David M steered me to "Writings on Physics and Philosophy" but I have not been able to find the quotes there in an admittedly cursory search.*

I understand that Charles Enz has just published a biography of Pauli, but I haven't read it yet. I have a much more extensive compilation of Paulian quotes in a document I'm putting together—including some amazing letters between Pauli and Bohr—called "Resource Material for a Paulian-Wheelerish Conception of Nature." It's sitting at 112 pages (and 452 references), but it needs to be twice that size before I'll call it complete and post it. (I.e., a lot of the references don't have quotes inserted into them yet.) I will let you know when it's ready for perusal if you wish. (Or if you'd like to see a sneak preview, I'm always looking for people who might catch a typo.)

Gottfriedism 3: *This summer the completely new second edition of my text finally appeared and I'd like to send you a copy if you'll tell me your mailing address.*

I'd love to have it! Thank you. One of these days I'd even love to teach a quantum mechanics course. As long as the post will arrive before April 15, 2004, the mailing address to use is the one below.

09-10-03 *Mocking Bird* (to N. D. Mermin)

Merminition 135: *So don't underestimate him* [Tony Leggett]*. He's one of the most impressive and thoughtful theoretical physicists I've ever met. Have you ever looked at his little book "The Problems of Physics"?*

I hope you understand that the note I wrote to you yesterday was written self-mockingly. That was its whole purpose.

I hadn't seen or heard of the book before. I'll try to have a look at it while I'm at Caltech next week.

09-10-03 *Parsing for the Uninitiated* (to J. M. Renes and C. M. Caves)

Now I've skimmed the latest version of your paper. At first I thought I'd read it very, very carefully this time around and give you detailed comments. But then I found myself pulling my hair out with you first paragraph and ultimately gave up: It's your paper not mine.

Take your first two sentences for instance: "A quantum measurement is termed informationally-complete if its statistics completely determine the input quantum state. An aesthetically appealing and potentially useful measurement is one which also possesses the symmetry that all pairwise inner product magnitudes are equal."

What are these inner products taken between? States? Positive operators? Complex vectors? If that's not bad enough already, in the next sentence one starts to feel schizophrenic: "In the full regalia of quantum information jargon, such a set of states is a 'symmetric informationally-complete positive operator-valued measure' ... " In one half of the sentence you talk about states, in the other half you talk about positive operators. Clearly you're thinking of a POVM as a set of state vectors (rather than positive operators adding up to unity), as you make more explicit in the next paragraph, but that is quite a nonstandard definition. Finally, you talk about "the existence of it" as if there is only one.

Contrast this with your use of language in Section IV: There you tell the reader that are only 2 SIC-POVMs when there are clearly a continuous infinity. Presumably what you mean is that there are only two possible fiducial vectors (for this particular method of generating SIC-POVMs). But what can it hurt to be precise?

I could keep going on. For instance, just after Theorem 1, you invoke a nonstandard Gram matrix—I say it is nonstandard because previously you had only talked about sets of state vectors, but now you are considering sets of operators (in their own linear space), and you haven't even told us about the inner product you're using. (You don't introduce that inner product until Section III.)

The point is, you should be have mercy on your reader, who, by definition, will not be sitting in your head. Try to anticipate his questions and potential frustrations. It's a worthy exercise and will help keep people reading your papers.

Enough. I guess I'm just voicing frustration because I had wanted to give you detailed comments and you thwarted me—it became clear that it wasn't worth my while.

Here is one thing, however, that is worth my while. Would you change your sentence, "In quantum information theory such measurements are relevant to foundational issues

`\cite{fuchssasaki03a,fuchssasaki03b}`,

and useful for creating classical representations of quantum states, doing quantum state tomography, and quantum cryptography," to something more along the lines of the following:

> In quantum information theory such measurements are relevant to quantum state tomography `\cite{A}`, quantum cryptography `\cite{B}` and to foundational studies `\cite{C}` where they would make for a particularly interesting "standard quantum measurement".

A) C. M. Caves, C. A. Fuchs and R. Schack, "Unknown Quantum States: The Quantum de Finetti Representation," J. Math. Phys. **43**, 4537–4559 (2002).

B) C. A. Fuchs and M. Sasaki, "Squeezing Quantum Information through a Classical Channel: Measuring the 'Quantumness' of a Set of Quantum States," Quant. Info. Comp. **3**, 377–404 (2003).

C) C. A. Fuchs, "Quantum Mechanics as Quantum Information (and only a little more)," `quant-ph/0205039`, and private communication.

You can throw out the reference to the other paper by Sasaki and me.

While I'm here I might as well make a couple of other trivial points.

1. Your title: No journal is going to accept POVM in it. You might just make it "Symmetric Informationally-Complete Measurements".

2. I think it is worthwhile to mention in the introduction that one of the results of Lemmens/Seidel is that symmetric informationally-complete measurements do not generally exist for real Hilbert spaces. Therefore it is somewhat surprising (and very intriguing) that they seem to for the complex case.

3. I noticed that Theorem 1 has changed. The upper bound is gone (maybe it shouldn't have been there in the first place?); and a state vector is not a subset. Also you haven't given a proper citation to Benedetto et al.

4. Definition of f_t. Missing a subscript t on $\mathcal{H}$. Also f_t is NOT equivalent to a choice of F_t, only on the symmetric subspace. Also equations 6 and 7 are bad notations, if you're going to first write equation 5.

5. Why introduce terms like Gabor frame if you don't use them?

6. I still didn't get equation 18 and its surroundings.

I'll desist at that.

10-10-03 *Letters and Quantum Canaries* (to P. Hayden)

Of course I'll send off a letter for you. I'll even spruce up the old one a bit before doing so. I seem to recall you've published some decent papers since the last time I wrote for you. ☺

And I'll take you up on a dinner at your house—it won't count as a bribe. But here's what I'd really like in return for my efforts: Solve this problem. You're probably just the man to do it anyway.

Fix a given set of pure states Π_i on a Hilbert space of dimension d with some associated probabilities π_i. Imagine drawing from that ensemble just once and handing off a state to a very reserved eavesdropper. She's very reserved in that she's not interested in gathering all the (expected) information she can; rather she settles for $I = \epsilon$. Finally she gives back the system you originally gave her, and you check the yes-no question associated with the original state you had prepared. Tabulate the disturbance in terms of fidelity F, just as Sasaki and I did in `quant-ph/0302092`.

The preliminary question is what is the functional dependence between I and F in that limit? The answer should depend upon the ensemble—for instance one might get $F = 1 - I^\alpha$, where α depends upon the details of the ensemble. Thus, more to the point, I would like to know what is the most sensitive ensemble in this sense, and how does the ultimate sensitivity depend upon d? I.e., what are the best canaries for sniffing out the fumes in these mines?

In the case of the earlier stuff considered by Sasaki and me, I can now show that (along with the uniform ensemble) a symmetric informationally complete ensemble consisting of d^2 elements is a maximally sensitive ensemble. I.e., in the terminology there, it achieves the "quantumness of the Hilbert space." (I only kind of semi-conjectured it in the earlier paper.) [[BTW, we must have talked about SIC-POVMs at some point? It's a set of d^2 rank-one positive operators for which all pairwise Hilbert-Schmidt inner products are identical. We still don't know for sure whether they exist, but that shouldn't stop one from using their formal properties. In any case, Renes and company will post a paper next week showing numerical evidence that they exist at least up to $d = 39$. So I think it's darned likely they really do exist for all d. And they even have a very nice symmetry that's been uncovered (again numerically)—namely, $\mathbb{Z}_d \times \mathbb{Z}_d$.]] So a natural guess is that the SIC ensembles will also be good canaries in this sense, but I wouldn't have a clue about whether it's really true or how to solve it.

You solve that or give me some good ideas about how to do it, and you can bribe me for a lot of things.

12-10-03 *Torture and Sleepless Nights* (to C. King)

Well, after a lot of self-torture and sleepless nights since I last wrote you, I decided to give up on the SIC-POVM problem (for the time being). It had become an obsession. Then I turned my attention to prodding Renes and company to quickly get their conjecture out on the web before someone else did. (A paper by the D'Ariano group a couple of weeks ago made it clear that he might be only one step away from the conjecture himself. So promptness seemed called for!)

The upshot is, their paper will appear on `quant-ph` Tuesday. Have a look at it if you get a chance.

12-10-03 *Moving On* (to C. M. Caves)

If I can muster the strength, I'm going to plunge ahead on my Holevo festschrift "On the Quantumness of a Hilbert Space." Hopefully I'll have it finished by the end of the week. There isn't much that it doesn't mimic from your paper, but at least I found some of it on my own (though maybe through a subliminal suggestion from you guys).

If I can figure out the words to say it with, I'd like to make a metaphysical point in its introduction. It is that Hilbert-space dimension plays a role in our Bayesian considerations that has an eerie conceptual similarity to rest mass in relativity theory. When an agent hypothesizes a Hilbert-space dimension for an object, he is hypothesizing something *it* possesses. (In contrast, when he hypothesizes a quantum state for the object, he is hypothesizing something *he* possesses.) Also I want to make the point that the fungibility—a word I really dislike—of quantum information should maybe more properly conjure up images of the Eötvös experiment and the equivalence principle than monetary considerations. I hope that if I can say it in the right way in the end you'll approve (at least that this is a worthy direction to explore), but I have a long history of such hopes.

14-10-03 *quant-ph/0310075* (to J. M. Renes, R. Blume-Kohout, A. J. Scott, and C. M. Caves)

Good to see its appearance. I think this is going to be a very important paper.

19-10-03 *Fidelity* (to A. Peres)

Good to hear from you. I am visiting Caltech at the moment. Rob Spekkens is also visiting. Through our combined forces, we are trying to make a full frontal assault on John Preskill's quantum foundations sensibilities: The goal is to make him see that the best way to think of the quantum state is as a state of knowledge.

Concerning fidelity, the first time I ever heard the term was in Ben Schumacher's talk in PhysComp '92 in Dallas, TX. The term subsequently appeared in

- Benjamin Schumacher, "Quantum Coding," *Physical Review A* **51**(4), 2738–2747 (1995)

(which was based on the PhysComp talk) but the article had actually been submitted about two years previous to that—you can check the submission date. By the way, this is also the article where the word "qubit" made its first appearance.

Finally, let mention this article:

- Richard Jozsa and Benjamin Schumacher, "A New Proof of the Quantum Noiseless Coding Theorem," Journal of Modern Optics **41**(12), 2343–2349 (1994).

It rederives the result in Schumacher's original (and also used the terms fidelity and qubit), but it actually appeared in print earlier!

19-10-03 *SIC POVMs* (to A. Sudbery)

Sudberyism 5: *We had our first weekly QIT meeting on Thursday. I talked about mutually unbiased bases, which Sam picked up and is running with. And my new student is full of ideas (but too shy to talk to anyone but me so far). It's looking good!*

Good. If you're interested in that, you might also be interested in `quant-ph/0310075` by Renes and company on "symmetric informationally complete POVMs". I think it's a very important problem, and as opposed to the MUBs, it looks like a solution exists in all dimensions.

19-10-03 *Question* (to A. W. Harrow)

Which postmodern writer was it that made some sense to you? And what was the book? I was telling what I could remember of the story to some people here at Caltech tonight, but I want to make sure I get my facts right in the future.

Aram's Reply

> Believe it or not, there have been a few postmodernists who have made some sense to me at some point or other. In particular, I liked Foucault and Zizek.
>
> However, the story I told you was about Jacques Derrida's "Of Grammatology," specifically part II, chapter 1. He was writing about "On the Origin of Language" by Rousseau and "Tristes Tropiques" by Levi-Strauss, I think.
>
> Glad this is still making the rounds.

22-10-03 *Upside-Down Quantumness* (to S. J. van Enk)

van Enkism 4: *He thought your measure of quantumness had the wrong "sign", with which I agreed. Namely, your measure of quantumness is smaller when the set is more quantum ... You agree?*

Yes, I agree. But here's the justification I used in the paper, for whatever it's worth:

> Finally the *quantumness* of the set $\mathcal{S}$ is defined by
>
> $$Q(\mathcal{S}) = \inf_{\{\pi_i\}} F_{\mathcal{P}} \ . \tag{42}$$
>
> This definition has the slightly awkward property that the *smaller* $Q(\mathcal{S})$ is, the more quantum the set $\mathcal{S}$ is. This, of course, could be remedied easily by subtracting the present quantity from any constant. However, if we wanted to further normalize the quantumness so that, say, its value achieves a maximum when no set of states has a higher quantumness, we would have to make use of a (presently) unknown constant in our definition. Thus, it seems easiest for the moment to simply remain with Eq. (42). This does, however, raise an important point—indeed one of paramount concern for the ultimate use of quantumness. Just how quantum can a set of states be in the most extreme case?

30-10-03 *Minor Correction* (to C. G. Timpson)

I quickly skimmed your proposal. It looks pretty good.

One minor point about my biography: [...]

Now, no need for a change on this one—it's not so important at this stage—but I'm not very comfortable with the language that "measurement induces an uncontrollable disturbance in the object system." The reason for this is that I would not characterize the process of giving birth as an "uncontrollable disturbance" to one's wife. I'll attach two excerpts from the latest samizdat that I think best characterize my present broader thoughts on quantum mechanics—the more key issue is the nondetachableness of the observer. BTW, you actually played a bit part in one of the notes. (Looking back at it, I realize I probably characterized you too harshly, but I hope you'll forgive me.)

04-11-03 *Slow Draw McGraw* (to R. W. Spekkens)

Spekkensism 21: *I forgot to ask you while you were here: where did you quote van Enk as saying that there is nothing inherently nonclassical about a 2d Hilbert space? I'd like to throw that reference into the contextuality paper.*

The discussion you're thinking of about van Enk is in my paper `quant-ph/0205039`. It went like this:

> There are two things that are significant about this much of the proof. First, in contrast to Gleason's original theorem, there is nothing to bar the same logic from working when $D = 2$. This is quite nice because much of the community has gotten into the habit of thinking that there is nothing particularly "quantum mechanical" about a single qubit. Indeed, because orthogonal projectors on $\mathcal{H}_2$ can be mapped onto antipodes of the Bloch sphere, it is known that the measurement-outcome statistics for any standard measurement can be mocked-up through a noncontextual hidden-variable theory. What this result shows is that that simply is not the case when one considers the full set of POVMs as one's potential measurements.

But actually that was a dirty trick on Steven. (I thought I was being cute, but he probably hates me for it!) The reason is because I was partly responsible for Steven's having put a statement like that at the end of his anti-Meyer paper—I remember advising him on it, it was definitely the sort of thing I was thinking at the time. Anyway, I looked for my email to him on the subject but couldn't find it; maybe it was something that came out of our discussions. I'll cc this note to Steven to see what he says.

04-11-03 *Belle and Beau* (to S. L. Braunstein)

Braunsteinism 2: *Do you have explicit forms of Wootters' "belle" and "beau" mutually unbiased bases? I am trying to pin down an error in something I am doing and I suspect it occurs here.*

Actually I don't even know what the belle and beau bases are. They do sound interesting though. Can I infer from your question that complete sets of mutually unbiased bases come in at least two flavors? What is the criterion?

This intrigues me mostly, because it dawned on me the other day that symmetric informationally complete POVMs may come in many different flavors. For instance, the character of the "triple products"

$$\mathrm{tr}(\Pi_i \Pi_j \Pi_k)$$

can make all the difference in the world for certain questions.

07-11-03 *Airline Tickets* (to J. B. M. Uffink)

Since I see Jean Bricmont is among the speakers, I will have to do my homework: I.e., I'll read his book with Sokal, *Intellectual Impostures*, before coming. I have a sinking feeling that he'd label any attempt to Bayesianize quantum probability—and thus me—in the same way.

By the way, a couple of weeks ago when I had a little leisure time at Caltech I discovered your paper "The Constraint Rule of the Maximum Entropy Principle." It's great!

07-11-03 *Quantum Pragmatology* (to G. Valente)

Thank you for sending me your thesis "Probability and Quantum Meaning: Chris Fuchs' Pragmatism in Quantum Foundations". Unfortunately it came through my emailer as a "corrupted file," so I had to do a good bit of reconstruction on it before I could get the LaTeX to compile properly. But when I did finally get it to print this morning, what a nice thing to behold! I am very flattered by it all. I never imagined that you were embarking on a work of such scope (140 pages!). Is this a kind of "senior honors thesis" or rather is it a "master's thesis"? Somehow I had gotten the impression before that you were an undergraduate, but maybe I was wrong?

Anyway, at this point, I've read the first 21 pages and also Chapter 5, and it seems to flow very nicely. I especially enjoyed your discussion in Section 2.1.2. I think you're the first person to ever show any appreciation of that idea. Also your analysis on pages 124 and 125 were quite enjoyable. I have had some connected thoughts myself recently (i.e., that dimensionality would be a pretty worthless 'property' unless all Hilbert spaces are, in fact, finite) . . . which leads me to some rather fantastic thoughts about the various entropy bounds cropping up in black hole theory and quantum gravity. I'll report some of this in my upcoming paper "On the Quantumness of a Hilbert Space" (which I'm working on at the moment). If you would like, I'll send you a copy when the crucial pieces are in place.

Already seeing this much of your thesis—I hope the rest of it won't make me reassess!—I hope you will consider posting the final version of it on the `quant-ph` archive. At the very least it may stir up some discussion and get more people involved.

By the way, I've caught a few typos. Maybe the most important is in your Musil quote: You write, "It is reality that awakens possibilities, and nothing would more perverse than deny it." Is it missing a "be" and a "to"? Maybe you should check the whole passage very carefully:

> To pass freely through open doors, it is necessary to respect the fact that they have solid frames. This principle, by which the old professor had lived, is simply a requisite of the sense of reality. But if there is a sense of reality, and no one will doubt that it has its justifications for existing, then there must also be something we can call a sense of possibility. Whoever has it doesn't say, for instance: Here this or that has happened, will happen, must happen; but he invents: Here this or that might, could, or ought to happen. If he is told that something is the way it is, he will think: Well, it could probably just as well be otherwise. . . A possible experience or truth is not the same as an actual experience or truth minus its "reality value" but has—according to its partisans, at least—something quite divine about it, a fire, a soaring, a readiness to build and a conscious utopianism that does not shrink from reality but sees it as a project, something yet to be invented. After all, the earth is not that old, and was apparently never so ready as now to give birth to its full potential. . . It is reality that awakens possibilities, and nothing would be more perverse than to deny it. Even so, it will always be the same possibilities, in sum or on the average, that go on repeating themselves until a man comes along who does not value the actuality above idea. It is he who first gives the new possibilities their meaning, their direction, and he awakens them. . . And since the possession of qualities assumes a certain pleasure in their reality, we can see how a man who cannot summon up a sense of reality even in relation to himself may suddenly, one day, come to see himself as a man without qualities.
>
> — Robert Musil, *The Man without Qualities*

(I've never actually read Musil myself. In fact, I had never heard of him until last year when Frank Verstraete told me that his book seems to express my views on quantum mechanics! The quote you used has convinced me that I should buy the book and try to read it!) Another important mistake: Look closely at Eq. (2.8). Also the explanation "moreover, the passage in the fifth line ..." following the equation array on page 20 is incorrect. G will not necessarily commute with the Π_d. Rather it is the general property that for any two operators A and B, the products AB and BA have the same eigenvalues.

I am glad to hear that you will take a PhD in the philosophy of physics, and I hope you will continue to pursue this pragmatic line of thought. Where to go? Most of all, I'd recommend to stay away from the UK! Pragmatism is certainly not viewed particularly positively there! A few places to consider in the US might be: 1) the University of Maryland, Jeff Bub is there and he is really getting taken with this stuff, 2) University of Washington, Arthur Fine is there and seems to have quite an interest in it too, and finally 3) Princeton University, Bas van Fraassen is there and he seems to have some affinity (or at least understanding) of Bayesian ideas. Unfortunately Richard Jeffrey died a few months ago, he would have probably been an excellent advisor at Princeton. Those three choices definitely follow my personal preferences, so you should compensate for that in your calculation. The are plenty of very strong Philosophy of Science programs in the US—like the University of Pittsburgh—but I doubt those places are as prepared for your line of thought as the three places I listed above.

I'll leave you with that. As I wind my way through your thesis, I may write you more. Oh, you asked about a visit to Dublin. Sounds great; but my calendar won't be free until at least mid-January.

10-11-03 *Ever Vigilant* (to S. Aaronson)

Where are you sending that lovely paper `quant-ph/0311039`??

10-11-03 *Peres Theme* (to V. Buzek)

Buzekism 1: *I am honoured to write a paper to the Festshcrift for Asher Peres. Do you have any specific underlying theme in mind?*

That's great! The underlying theme is Asher ... and all that entails. There will be some submissions on relativity theory, but most will be on quantum information and quantum foundations. Take your pick.

10-11-03 *Who Knows What I Am?!* (to G. Valente)

Valente-ism 3: *Finally, let me know something I have been wondering since I started reading your ideas. Isn't what I called "un-veiling the reality" with respect to your program a suggestion you interiorized from your interest for Schopenhauer (dropping Maya's veil)?*

About Schopenhauer, I really haven't read much of him ... only a little (actually very little) about him from secondary sources. Effectively the only thing I really know about him is the quote below that I had put into my computer. [See 08-08-01 note titled "The First Eye" to C. M. Caves.] So, your "un-veiling"—I think—is new to me.

11-11-03 *Working Title* (to G. Brassard)

We can't have a paper without a prospective title. We've got to settle first things first. How about, "Prospects for Quantum Cryptographic Interpretation of Quantum Mechanics"?

Flying to Canada on a 747 tomorrow. I hate those things.

11-11-03 *Budget Butter* (to R. E. Slusher)

I wish I could say I had some exciting news, but mostly I'm just plugging away. These symmetric POVMs have turned into an interesting and long-lasting problem. Have you looked at Caves and Co's paper `quant-ph/0310075`? They had originally had me on the author list of that, but in the end I withdrew thinking I hadn't done enough to merit it. I'll probably kick myself in the butt for years to come: I'm starting to feel that the conjecture there may remain open for quite some time. I discussed it a lot when I was visiting Caltech and even the great Kitaev thought it nontrivial.

Anyway, Patrick Hayden and I do have some results on the accessible information of such an ensemble (i.e., no longer thinking of the states as elements of a POVM, but instead a signaling ensemble). I'm in the process of writing that part of the work up. I hope I'll put it on `quant-ph` soon.

How is Bernie's patent coming along? And how about your experiment of the same?

13-11-03 *Pearle, Hampton, Hampshire* (to R. W. Spekkens)

While it's on my mind, I've confirmed that Phil Pearle was at Hamilton College in New York State. He retired in 2000. Last night I said it was Herb Bernstein who was at Hampton College, but that was a glitch: Herb is at Hampshire College in Massachusetts (with a name like that, you might have thought it'd be in New Hampshire). And finally, on further reflection I don't know that I know of a Hampton College. The only thing I can think of is Hampton Inn, but that's a hotel chain.

13-11-03 *Variety and Freedom* (to L. Hardy)

Attached is the Rorty passages I sent to Howard Wiseman. [See note titled, "The World is Under Construction."] The phrase was actually "variety and freedom"—that's the only goal. Reading the passages again I still like them. I think they nicely capture what would make a lawless world a better world than a law-fixed one.

13-11-03 *Elephants* (to L. Hardy & F. Girelli)

Below I place some excerpts I dug out of my paper `quant-ph/0204146`, "The Anti-Växjö Interpretation of Quantum Mechanics." They certainly do a better job of what I was on about last night than I did ... though in my sober reading this morning they only seem slightly less drunk!

To Lucien's remark that scientific theories are cumulative—which he wanted to use as an indication that succeeding theories are better and better representations of reality—my reply might be that, from this point of view which I'm trying to develop, the more relevant concept is that succeeding theories have better "feedback mechanisms." That is, one might say that the human species is more developed than the elephant species—even though neither species was foreordained by nature—simply because humans can better adapt to the changing conditions around them. The human species has more feedback mechanisms for adjusting to the environment around it. Similarly, let us say for general relativity and Newtonian gravity. The former can survive more experimental onslaughts than the latter because its structure is more malleable and less rigid than the latter's. But I'm just shooting from the hip at the moment.

15-11-03 *TOE Jam* (to C. Snyder)

I very much enjoyed listening to your excitement last night about a TOE-less world. I think we're kindred spirits in this project. Let me give you a pointer to two of my writings that you might enjoy in this regard.

1) Look at Sections 4 and 5 of my paper "The Anti-Växjö Interpretation of Quantum Mechanics"

and

2) the little essay "The World is Under Construction" on pages 210–215 in my samizdat "Quantum States: What the Hell Are They?".

Both documents can be found on my webpage in PDF format. If you have trouble viewing the files, let me know and I'll send you excerpts in plain text.

By the way, the samizdat that Kluwer Academic will be publishing for me is *Notes on a Paulian Idea*. It's posted on the same webpage.

Hope to see you again later this week.

15-11-03 *The Artist* (to C. Snyder)

I was just flipping through "Quantum States: WHAT?" after telling you about it, and came across the birth announcement I wrote for my second daughter. It's on page 121. [See 19-12-01 note "Katie Viola Fuchs" to the world.] Maybe it captures a little bit (in a single sentence) of what moves you as an artist—that your painting will change the world.

15-11-03 *Conjugalation* (to G. L. Comer)

I bet you've never used the words "lascivious" and "conjugal" in a talk abstract before. Call me crazy, but I did. (See below.) Somehow it seemed like crazy words were called for at a crazy place. I'm visiting the Perimeter Institute for a week and a half [. . .]

Maybe one fortunate thing has come from this visit to Perimeter: I've been reading loads of review material on quantum gravity. I have particularly liked the stuff I have read by Jacob Bekenstein and Raphael Bousso. I'm definitely tipping toward the entropy-bound side now, which I am convinced can only mean "assign a finite dimensional Hilbert space to what was once thought to be a continuous system." I think this stuff is very likely revolutionary, and I am so sorry it took me 15 years to appreciate it.

Another thing that has hit me is that with this Bayesian view of quantum operations developed in *Quantum States: What the Hell Are They?* is that there is simply no information-loss "paradox" associated with black holes. From my point of view, as long as a time-evolution map is linear, completely-positive there is nothing to keep one from assigning it as long as it is actually one's firm belief for a system. In particular there is no requirement that the map be derivable from a unitary evolution on some composite physical system. Thus the paradox, like the black hole itself, evaporates. I'm going to try to write this up a little more clearly and pass it by John Preskill soon. I know, of course, that he won't buy it—he could only do that if he had bought the starting point, i.e., that quantum states and quantum operations are not ontic, but subjective—but it'll still be a good exercise, and I know I'll learn from his reaction (as long as it is not silence).

> Title:
>
> What is the Difference between a Quantum Observer and a Weatherman?
>
> Abstract:
>
> Not much. But where there is a difference, there lies quantum theory's most direct statement about properties of the world by itself (i.e., the world without observers or weathermen). In this talk, I will try to shore up this idea by writing quantum mechanics in a way that references probability simplexes rather than Hilbert spaces. By doing so, the connection between quantum collapse and Bayes' rule in classical probability theory becomes evident: They are actually the same thing up to a linear transformation depending upon the details of the measurement method. Looking at quantum collapse this way turns the usual debate in quantum foundations on its head: only local state changes look to be a mystery. State changes at a distance (as after a measurement on one half of an EPR pair) are completely innocent—they simply correspond to applications of Bayes' rule itself, without the extra transformation; that is, collapse-at-a-distance is nothing more than the usual method of updating one's information after gathering data. Thus the idea develops that if a quantum reality is to be found in the quantum formalism, it will be found only in the formalism's *deviations* from classical probability theory: Reality is in the difference. Time permitting at the end of the talk, I will try to sketch, without getting too lascivious, how such a reality may be best thought of in conjugal terms.

John's Reply — Yes, Really, John's Reply

> I was disappointed to hear that you won't learn from my silence. I may need to rethink my method of communicating ...

15-11-03 *Elementary Quantum Phenomena* (to R. Laflamme)

I looked in my email for that dinner or beer you said I promised you, but I couldn't find it. You know, with my weird beliefs about quantum mechanics, a quantum phenomenon may not be an actual phenomenon until it is written in an email somewhere! (Or was it John Wheeler who said that?)

17-11-03 *Tossing and Turning* (to L. Smolin)

Well you caused me quite a sleepless night of tossing and turning with your "needling" question yesterday. When I snapped that your question is like my asking you why you haven't quantized gravity yet, maybe I should have bitten all the way through the flesh: Maybe our two projects are the same damned thing! And if you can't do it by yourself, why should I be expected to? More seriously, I wonder whether identifying the ontic piece of quantum theory—i.e., the research program I am trying to build—might not just amount to (a large part of) getting at a quantum theory of gravity.

You said, "Come back to me when you've got the ontic piece." (Well, you didn't quite say ontic but something of the same flavor.) The part I see most clearly at the moment is Hilbert space *dimension* … and maybe that's all there is. At present, I am toying with the idea that D quantifies a kind of "nervous creative energy" on the part of a quantum system. More speculatively, I find myself wondering if it is too far-fetched to think that that creative energy gravitates much like its less metaphorical cousin.

So I was really quite excited last night on the bus ride home when I came across your appendix on Relational Quantum Cosmology in the review article you gave me! I had never heard of Crane before, and I didn't know about your work with Fotini on what you call the weak holographic principle. I almost melted when I read the sentence, "The only property a screen has beyond its place in the causal network is the dimension of its Hilbert space."

Anyway, in good faith to this excitement, let me place below some excerpts from my newest email samizdat, *Darwinism All the Way Down (and Probabilism All the Back Up)*. The connections to your sentence above should be evident, and I hope the passages convey a little of why I am thinking in these directions. Of course the mumblings are not science yet (or they would be appearing in papers, not samizdats), but rather dreams of a solid direction to follow. The real science strikes me as just around the corner.

18-11-03 *Don't Forget Rorty* (to R. W. Spekkens)

Don't forget to bring my Rorty back before I fly out.

Oh bring back, oh bring back, oh bring back my Rorty to me, to me …

25-11-03 *Philosophy Sauce* (to A. Valentini)

Valentini-ism 1: *Sorry about the testosterone-fueled rant last night. It did come out with an aggressive edge. Hope you have a good return trip.*

No bothers; it just made for a saucier night.

For the record though:

The anti-Växjö paper was passed by (at least): Jeff Bub, Henry Folse, Bas van Fraassen, Wayne Myrvold, and Chris Timpson.

The "World is Under Construction" note was passed by (at least): Richard Healey, Wayne Myrvold, Chris Timpson, and Steven Savitt.

To my knowledge `quant-ph/0205039` "Quantum Mechanics as Quantum Information (and only a little more)" has been read at some level by: Guido Bacciagaluppi, Jeff Bub, Bill Demopoulos, Armond Duwell, Arthur Fine, Bas van Fraassen, Stephan Hartmann, Richard Healey, Wayne Myrvold, Itamar Pitowsky, Steven Savitt, Abner Shimony, Chris Timpson, and Giovanni Valente.

The large samizdat *Notes on a Paulian Idea* has been distributed to: Guido Bacciagaluppi, Jeffrey Bub, Jeremy Butterfield, Arthur Fine, Henry Folse, Bas van Fraassen, Alexei Grinbaum, Hans Halvorson, Stephan Hartmann, Richard Healey, David Malament, Wayne Myrvold, Itamar Pitowsky, Steven Savitt, Abner Shimony, Chris Timpson, Giovanni Valenti, and Steve Weinstein.

Of the people above: Amit Hagar has written a critique of my stuff in the journal *Philosophy of Science*, Giovanni Valente has just submitted a Master's thesis in philosophy at the University of Padua on it, and Stephan Hartmann is including it as a component in his course at the London School of Economics.

The point: I don't know whether you would be willing to call these guys philosophers, but it can be verified that they are all employed by philosophy departments. The last thing I see myself doing is hiding from critique—a better word would be "test."

The only real goal is in understanding quantum mechanics. And on this, we are in it together.

25-11-03 *The Unfortunate Phrase* (to A. Valentini)

Let me follow up on your question about my view of the phrase appearing in one of my papers with Caves and Schack: "Gleason's theorem can be regarded as the greatest triumph of Bayesian reasoning."

The place to look is my web samizdat *Quantum States: What the Hell Are They?*. See:

- page 75, "The Stopgap"
- page 84, Cavesism 27 and below
- page 118, "Trumps and Triumphs".

My own view of what the paper accomplishes can be found in the letters

- "Identity Crisis" on pages 35 to 38,
- "Unique Assignment" on pages 53 to 54.

I thought I had said it somewhere more clearly in there, but I don't have the time at the moment to keep digging for it.

25-11-03 *Worthwhility* (to J. E. Sipe)

I just wanted to say I'm sorry I didn't get a chance to talk to you longer at PI. Your reaction after my talk made my day; in fact, it alone was enough to make my giving the talk worthwhile. You wouldn't believe how much strength and stamina to go on I derive from reactions like yours.

Anyway, there's so much to do in this program and so many subtleties that still need hammering out, I welcome your interest.

25-11-03 *I Dream of Everything* (to C. Snyder)

I just reread the note you sent me and enjoyed it again indeed. Thanks for the great conversations last week.

Snyderism 1: *As artists we are subjective in the extreme ... (there are no wrong descriptions (or models), only personal ones.*

This you might say is the program of "Quantum States: What the Hell Are They?": It is to make the physicist realize that he has more in common with the artist than he might have thought. The quantum state, in particular, I say, represents a subjective ascription. Mimicking your words, there are no wrong quantum states, only personal ones. I did a quick search and found some discussions of this on pages 39 (right after Merminition 9), 49–50, 54–56 (the underappreciated point), and 159–166 (parts of the letter to Hardy) of the document you printed for me. You might enjoy reading about this overlap of our concerns—it is quite real.

I hope we'll have an infinite time in the future to build on this.

Christian's Preply

> Thanks for directing me to the various articles ... it will take some time to digest (I'm afraid I did a little more reading then perhaps I should have.)
>
> I slept last night (got to bed late, as per always) and was pushed into a rather awkward dream (or state that resembled a dream). In this dream I was able to observe many things: myself, the universe, the lines of the forever.
>
> Now I must apologize because I just spent 30 minutes trying to explain what it was that I saw in this dream, and realized I couldn't do it without some sort of pen and paper ... so I've erased it. Let's just say there was an x, y and z axis, a reflected sine wave and a bad morning hangover where everything I observed on the inside of my eyelid kept changing from a particle to a wave. It is a bad way to wake up when you have a soccer game an hour later.
>
> Anyway, I was thinking, artists and you physics guys aren't really so dissimilar you know. We are both observers of the world, we both try to describe the world, but our models of the world are so fundamentally different it makes me wonder about you guys sometimes. As artists we are subjective in the extreme ... (there are no wrong descriptions (or models), only personal ones). As physics guys (and gals of course) you always want to be objective and your models (or descriptions) are always wrong. You know this going in ... you might get closer to the truth (assuming of course there is an objective overriding truth, which you don't believe in) but you guys do it with *your* data, with *your* machines.
>
> Why is it that we expect objective data from machines we build?
>
> *LONG BREAK*
>
> Sorry, the clock on the wall says 3 AM, I'm going to bed. I'll write more later.
>
> *LONGER BREAK*
>
> Ahem, looks like I forgot to send this last night. Well here's to the early morning forgetfulness. Now I must promise not to re-read this thing before I send it 'cause otherwise I'll never send it.

26-11-03 *Bartleby and What I Really Mean* (to C. H. Bennett)

I'm off to the Utrecht meeting tomorrow morning to declare "Quantum Information Does Not Exist."

Title: Quantum Information Does Not Exist

Abstract: It is information *carriers* that exist—conceptually both classical and quantum. To confuse the epistemic category (the information) with the ontic (the carriers) is to cause any amount of trouble. Nonetheless, one thing is true when it comes to applications of information theory to classical and quantum phenomena: There is a difference. And, in that difference—this talk will argue—lies quantum theory's most direct statement about properties of the world by itself (i.e., the world without the information processing agent).

02-12-03 *Alive and Well, but back in Dublin* (to A. Peres)

I am just back in Dublin from a few days of email blackout in Utrecht. Kiki accompanied me to this conference, and we left the kids in the care of their grandmother (who flew to Dublin from Munich just for the purpose).

I gave a talk with the belligerent title "Quantum Information Does Not Exist", but it was a variant of the one you saw me give in Aarhus. I chose the title that I did to give Ari Duwell's nice paper of the same title some advertisement, but also because I wanted to emphasize de Finetti's version of Bayesian probability more this time. For most of the audience though, I think the talk was a disaster. The main thing was that I did not enter the arena with enough forethought of the "crowd control" I would need. Roger Balian and Theo Nieuwenhuizen, in particular, heckled me quite a bit. They would not accept that a POVM could actually be measured (since it contains noncommuting operators). It was silly and only demonstrated that they just were not listening. In the tradeoff at least, I could see the lights flashing in the eyes of Jos Uffink and Michał Horodecki. Michał, for one, seemed to finally understand how you and I view the wavefunction ("Quantum Theory Needs No Interpretation"), and how we are not "inconsistent"—something that he apparently thought of us all these years. So, that was very nice to see such a change in him.

02-12-03 *You Really Must Hurry* (to R. W. Spekkens)

I'm just back from the Utrecht meeting and pretty exhausted, but I've got to tell you about a conversation I had with Michał Horodecki. He was telling me about how Robert Alicki has been pushing them to think of quantum mechanics as just a restriction on knowledge. And thus they had developed some "toy model" that, for instance, exhibits a kind of no-broadcasting theorem on top of a no-cloning theorem. Prompted by that, I figured I had better tell him (a very little) about your toy model. You could actually see the panic in his face. He said something like, "We must post our paper soon." I told him that priority shouldn't be an issue because your work has already been cited three times on `quant-ph` and gazillions of people have heard your talk on the subject. But I doubt I fazed him much with that: These Horodeckis are wham-bam-thank-you-ma'am kind of guys when it comes to getting their ideas out there.

I know that you understand the point: You are walking on thin ice with this result. It's a beautiful piece of work, however the time is starting to become ripe for its re-appearance out of sheer Darwinism. The pressures are mounting for the quantum state to be viewed epistemically and science is responding.

02-12-03 *Now, For the Important Stuff* (to L. Henderson)

Here's the PhD thesis I was talking about:

- W. H. Long, *The Philosophy of Charles Renouvier and Its Influence on William James*, Ph. D. thesis, Harvard University, 1927.

There's another one along the same lines that I also want to get my hands on:

- H. C. Sprinkle, *Concerning the Philosophical Defensibility of a Limited Indeterminism: An Enquiry based on a Critical Study of the Indeterministic Theories of James, Renouvier, Boutroux, Eddington, Bergson and Whitehead*, Ph. D. thesis, Yale University, 1929.

but that's beside the point. (I just wanted to have a record of both in the same email; I store most of my information in emails.)

Anyway, it would be lovely if you could check out the Long thesis before coming to Maryland conference next April 30. Or maybe I could figure out a way to get to Cambridge before then.

Thanks so much for this help!

02-12-03 *Paper and Visit* (to R. Schack)

I'm just back from Jos Uffink's meeting in Holland. Among other things, I got to meet Stephan Hartmann there; he's quite a Bayesian. Also I had a fruitful time talking to Jos himself. I asked him his views on the interpretation of probability and got quite a surprising answer: He has not made up his mind, but he leans 1) mostly toward radical von Mises type frequentism (with kollectivs, randomness and place selections and all that), but 2) to a smaller extent toward radical subjectivism. He sees those as the only two consistent alternatives. In particular, he rejects propensitism, logical interpretations, objective Bayesianism, etc. Strangely he also knows (and admits) that frequentism has no empirical content, but that doesn't seem to sway him.

Have you ever read his paper on the MaxEnt principle and thought about whether it is valid? It's actually quite nice, and I think an indication that he is a thorough thinker. You can find it here: `http://www.phys.uu.nl/~wwwgrnsl/jos/mep2def/mep2def.html`.

02-12-03 *Scrap Paper* (to L. Hardy)

I just sent the attached picture to my brother so that he could print it out for my mom. It's a drawing of Emma's from last week. Upon reviewing it before sending it, its relevance to Perimeter dawned on me: You'll see what I mean. Whenever I come home from a trip, Emma asks me if I have any scrap paper for her. I usually give her whatever was in the conference packet, etc. This time it was a Perimeter Institute pad. Her drawing skills are getting better and better; I'm pretty proud of her.

03-12-03 *Holism, Nonholism, or Nihilism?* (to M. P. Seevinck)

I had a chance to look at your paper as I walked in to work today. So, I absorbed about 45 minutes worth of it. I don't think I have anything substantial to say at the moment, other than I think I like your approach based on LOCC and operationalism. It seems to be moving in the right direction.

I am a little curious to know how this train of thought I've been trying to hammer out in quantum foundations fits into one or another holism/nonholism distinction. Tentatively I call my (forming) view "The Sexual Interpretation of Quantum Mechanics". Its beginnings are in `quant-ph/0205039` "Quantum Mechanics as Quantum Information (and only a little more)" and `quant-ph/0204146` "The Anti-Växjö Interpretation of QM" (Sections 4 and 5), but it only really takes on a kind of force in my document "Quantum States: What the Hell Are They?" posted on my webpage. For only there do I come to realize that I fairly explicitly drop what you call the eigenvalue-eigenstate link in these newest trains of thought.

Is it holism? Or, is it not holism? On the one hand I wouldn't think my view a kind of holism, as the only *property* I find myself willing to suppose of a quantum system is its Hilbert-space dimension. But then on the other hand, I find myself wondering how to interpret the following statements that I find myself saying:

page 48 of "QS: W.H.A.T.?":

> Excellent! This is only a joke partially, but lately I've been so taken with the idea that unions can give rise to things greater than those contained in the parts—thinking of quantum measurement, in particular, from this angle—I've thought about calling my view on QM "the sexual interpretation of quantum mechanics."

page 49 of "QS: W.H.A.T.?":

> Lately, I've been jokingly calling my view (as it stands) the "sexual interpretation of quantum mechanics." (Most people turn red and become uncomfortable when I do that and explain why. I suspect the same will be true even in your reading of this note. So, brace yourselves.) The essential idea is that something new really does come into the world when two of its pieces are united. We capture the idea that something new really arises by saying that physical law cannot go there—that the individual outcome of a quantum measurement is random and lawless. The very fact that the consequence of the union is random signifies that there is more to the sum than is contained in the parts. But I promise you I won't reflect the licentious details of this view in the glossary below. I'll leave the missing terms to your imagination.

page 190 of "QS: W.H.A.T.?":

> I suppose if I were to start to label things, then this thing I was telling you about the other day—"the sexual interpretation of quantum mechanics (SIQM)"—would be a kind of dualistic theory. I said it metaphorically this way: When things bang together, something is created that is greater than the sum of the parts. Or again: When things—that's the materialistic aspect—bang together, something is created—that's the mentalistic aspect, for it is like an act of the will or a decision. But that's just a thought that's hitting me at 4:00 in the morning. (So trust it less than even the usual things that come out of my mouth.) I hadn't thought about it in this way before, and I'm not sure I want to continue to thinking about it this way. In general, I don't like dualisms. (Though even saying that is paradoxical; for I think I like "pluralisms" in the sense of James.)

If you need more of the surrounding conversation to make sense of these statements, please go to my webpage and look at "QS: W.H.A.T.?" directly.

Also here are some pieces from my more recent correspondences: [See 19-07-03 note "Definitions from Britannica," to N. D. Mermin and 18-08-03 note "The Big IF" to A. Sudbery & H. Barnum.] (In fact, I'll just attach a pre-packaged thing related to the Sudbery/Barnum correspondence. Maybe you'll find it of interest in its own right.)

Anyway, all these statements certainly feel like a kind of holism as you define it early in your paper, except for possibly the "creative" aspect of a quantum measurement and "subjective" aspect of the quantum state that I keep emphasizing.

Is it holism, nonholism, or nihilism? The last part of that is a joke, but you have piqued my interest in the question.

So, this note was hardly a comment on your paper, but I hope you see that you've gotten me thinking.

03-12-03 *Recovery* (to M. Pérez-Suárez)

Pérez-Suárezism 5: *Well, the point is that I thought there was a one-to-one correspondence between quantum tests (by which I mean, basically, a certain number of possible outcomes, with a probability assignment for each one, as well as an updating for any quantum state assignment after having gathered the actual data) and POVMs (by which I mean a set of nonnegative operators comprising a resolution of the identity). But there is not. Due to the dependence of the quantum state updating on the particular Kraus representation for the effects (up to a unitary relation) in the POVM, it turns out that one and the same POVM corresponds to different tests, all of which, nonetheless, share the number of outcomes (of course) and the probability distribution for them. Thus, the different tests corresponding to the same POVM differ only in the updated quantum state assignment. The reasoning behind this is so simple that I don't think I have made any mistake, but I felt surprised at this, though I guess it is a well-known result I had simply not read about. I'd like to know what you think this is telling us (if anything) about the relation between those tests and, maybe, by the way, about the nature of a quantum state assignment.*

I've already tried to tell you what I think about it. It is in my explanation of the extra "mental readjustment" in Section 6 of `quant-ph/0205039`. Namely, one's (subjective) estimate of one's influence on a system in the process of measurement is something over and above simple Bayesian updating. I think there is still a whole lot more that needs to be said to flesh this out properly, but maybe that should wait until you arrive.

03-12-03 *Växjö Contributions* (to H. Barnum)

By the way, here are the titles that have been promised on the Växjö proceedings. Also I'm going to try to write something small. I don't recall ever seeing a wink from you on whether you'll submit your big paper. Are you gonna do it?

Scott Aaronson:: short, recreational paper about what goes wrong if you replace $|\psi|^2$ by $|\psi|^3$ or change QM in various other ways.

Guido Bacciagaluppi:: "Classical extensions, classical representations and Bayesian updating in quantum mechanics"

Paul Busch:: "Less (Precision) is More (Information): Quantum Information in Classical Embeddings"

Bob Coecke and Keye Martin:: "Partiality in Physics"

Piero Mana:: "Why can states and measurement outcomes be represented as vectors?"

Rüdiger Schack:: Titleless at the moment.

Alexander Wilce:: "Probability: classical, quantum, and otherwise", or "Symmetry and compactness in quantum logic"

03-12-03 *A Question of Condensation and Time, 2* (to J. I. Rosado)

Because of the new paper that Rüdiger Schack and I are constructing, tentatively titled "On Quantum Certainty", I was thinking of you the other day. At first it was quite hard to find our correspondence in my files because I could not remember your name, but after some work I found it. [See 21-04-03 note "A Question of Condensation and Time" to J. I. Rosado.]

I don't think you made a further reply on the note below, but I just wanted to make sure. In any case, if you have ever written anything on the subject of your note, Schack and I should cite you. Please let me know.

Where are you located? Are you a student, postdoc, professor, etc.? Tell me a little about yourself, so that I can better remember your name in the future.

04-12-03 *Incompleteness* (to R. Plaga)

Plaga-rism 1: *I read with great interest you paper "Quantum Mechanics as Quantum Information" (*`quant-ph/0205039`*). I really like your idea that QM is in a similar state as special relativity before 1905.*

One question about your discussion of Einstein's criticism on p. 11. You write "the world has seen much in the mean time" and quote the theorems Bell and Gleason. Why do these theorems refute Einstein's unwillingness to accept incompleteness?

Thanks for your interest in my paper! I do not think that Bell and Kochen-Specker *refute* Einstein's goal of finding a more complete theory. In fact, I do not think it can be refuted. Rather, I would say that the theorems I refer to better delimit the costs of such a venture. And by my own scale, the costs are now high enough that I find it worthwhile pursuing the opposite extreme: I.e., to search for a sound, palatable, and instructive reason for why such an Einsteinian-type completion need not be sought. Can we imagine a model of the world in which such a completion is blocked by the world's very properties? That's the sort of thing I am after.

04-12-03 *Degrees of Belief* (to C. Snyder)

Wow, you lot from the Jane Bond (this is my new name for the residents of Waterloo that I did not meet explicitly at the Perimeter Institute) are some pretty serious scholars.

Snyderism 2: *So ... Why this word BELIEF? (Please don't ask me to read de Finetti, Bernardo or Smith ... I'm more interested in your own words.)*

I don't think any of the Webster's definitions capture the particular meaning I had in mind. What I meant was something particularly abstruse. Let me give you an example, that'll maybe clarify my usage a little.

Will it rain here in Dublin tomorrow? I don't know. At best I might have some kind of internal degree of belief p (p is a number between 0 and 1) for whether it will rain. What does that mean? Its operational meaning is this: If anyone were to offer me a wager of S dollars, then I should have no qualms about laying down (p times S) dollars on the condition that IF it rains, he will return S dollars to me. Otherwise, he will keep my (p times S) dollars for his own profit.

In this sense my degree of belief p is experimentally testable: It is manifested in how many dollars I would put down on some uncertain event. But there is no sense in which my degree of belief can be right or wrong. It will either rain or not in Dublin tomorrow, and it will do that independently of my beliefs (i.e., which bets I will accept). To mimic you words a last time: there are no wrong degrees of belief, only personal ones.

When I say that a quantum state for a physical system is a belief, what I really mean is that it is a SET of degrees of belief for the outcomes of all the different kinds of experiments I might imagine performing on that system.

Hope that helps.

Snyderism 3: *I would expect most quantum guys (this is my new name for your lot) to not be terribly keen on the word BELIEF (even if you do define it in the context of quantum discussion.)*

Yep, but my gut and my logic both tell me that I'm pushing the community in the right direction. Some of them are only coming kicking and screaming at the moment, and some of them are fighting with all their lives, but that's slowly changing.

Christian's Preply, "To BELIEVE or to KNOW"

> Sorry about the delay in responding … it has been quite a busy spell here for me. Listen … I was reading your last email, and reading the things you asked me to read in your last email about subjectivity, but I had trouble getting beyond your "Note on Terminology." I found your term BELIEF a very interesting one. You, of course, define it in the context of quantum discussions. And in that context define BELIEF as a quantum state. Good enough … however Mermin would rather talk about knowledge, not belief.
>
> Granted the game being played is one of definition, not entirely unlike the the white on black illusion you refer to on the previous page (you're both talking about the same thing, just defining/seeing it differently.) So to my question …
>
> I would expect most quantum guys (this is my new name for your lot) to not be terribly keen on the word BELIEF (even if you do define it in the context of quantum discussion).
>
> The original form (definition) of the word (outside of your context) is an assent to a proposition or affirmation, or the acceptance of a fact, opinion, or assertion as real or true, without immediate personal knowledge; reliance upon word or testimony; partial or full assurance without positive knowledge or absolute certainty; persuasion; conviction; confidence; as, belief of a witness; the belief of our senses. (That would be Webster's not Oxford … but whatever.)
>
> So is it the last line you like? (The belief of our senses and therein the subjective.) But there is almost a contradiction in the word isn't there … the assertion as real or true, without immediate personal knowledge, and yet can also be the belief of our senses. It is interesting that Webster's also lists opinion as a synonym.
>
> It is also interesting that there is also a definition for ultimate belief (last in a train of progression or consequences; tended toward by all that precedes; arrived at, as the last result; final.) I wonder if there is a definition for ultimate knowledge … if there is, it would no doubt be identical.
>
> So … Why this word BELIEF? (Please don't ask me to read de Finetti, Bernardo or Smith … I'm more interested in your own words.)

10-12-03 *Girelli's Workshop* (to W. G. Unruh)

I've been meaning to write you for some time, but I kept forgetting to. Seeing the line-up at the Seven Pines thing in my email today finally reminded me. Anyway, in particular, I had said to

Florian Girelli that I would write you to encourage your participation in his workshop Feb 23–29 on quantum information and gravity at the Perimeter Institute. I hope you will come. The time does seem ripe to put these two things together (and knowing that you'll be there will help me justify in my mind my own participation!). I'm hoping he'll also be able to get Raphael Bousso to come; it'd be interesting what comes out of the mix of you two. In any case, the bigger, the better the crowd, the more interesting it'll be all around.

10-12-03 *Thanks* (to J. B. M. Uffink)

By the way, I've been wanting to thank you for inviting me to your meeting. I learned some things, and it certainly helped me discover that the Netherlands is every bit as nice as my wife had been telling me!

I conveyed a report of your unique view on interpretations of probability to Rüdiger Schack. (Unique, because you could see the merit in *both* strict-von-Mises frequentism and radical subjectivism.) I hope I got the details right! Anyway, I think it would be good for us to talk this through at length one of these days. If Rüdiger and I can just pull off our "Being Bayesian in a Quantum World" conference (to which you would be invited), that'd be the perfect place.

10-12-03 *Slogans* (to A. Duwell)

I've been meaning to write you for a couple of weeks to tell you a little story. Sorry I didn't get a chance before now. Anyway, it is about Jos Uffink's and Dennis Dieks' conference in Utrecht a couple of weeks ago. Since they wanted me to talk about the meaning of quantum information, rather than quantum states per se, I knew that there was nothing more accurate I could title my talk than with your slogan, "Quantum Information Does Not Exist." I hope you don't mind! In particular, I opened my talk with some lines that went something like this:

> Unfortunately, I have to start my talk with a horrible admission: You have a thief among you! For I have stolen the title for my talk from a nice paper of Ari Duwell that I would encourage all of you to read. [Then I put up a slide with the title, your name, and the paper's publication details.] At the end of my talk, I hope you will see a connection between what I am about to say and this slogan. However, for most of my time I will be talking about a formalism that might help you see the interpretational problems of quantum mechanics in a new light. Though I stole the title, I hope you won't think I stole the contents of my talk!

It got a good laugh all around, and I hope it did encourage some in the audience to pay attention to your paper.

Which reminds me of something else I've been wanting to say to you for some time: You really ought to post your paper on the `quant-ph` archive. A lot of people in the quantum information community are going astray. If you don't post your paper there, a lot of people in that community are never going to see it . . . and never get a chance to get their thoughts straightened out from it.

10-12-03 *Sneaky Ways to See You* (to C. H. Bennett)

I'm writing you to encourage you to go to Jeff Bub's meeting April 30 through May 2 in Maryland. See this link for details: `http://carnap.umd.edu/philphysics/newdirections04.html`. If you're going to go, could you drop him a line to let him know that you'll be there? (And if you're not going to go, could you let him know that too?)

But I do hope you'll go. The argument that you didn't actually settle the Maxwell demon problem seems to be gaining some momentum in that crowd, and I think if you'd just face them one-on-one for once, it would do a world of good. In particular at the Utrecht meeting last week, a fellow named Owen Maroney—who will most surely be at the upcoming meeting too—gave what appeared to be a decently reasonable talk trying to pinpoint where you had gone wrong. (I'll put his abstract below.) If you had just been in the audience to ask the right question at the right time—I hate to say it again—it would have made a world of difference.

Given that you never wrote your planned review article for RMP, this is probably the best thing you could do as a substitute for the time being. And besides, you and David Mermin and I could all have some fun. Certainly David and I will have a lot more fun if you're around than not.

Please write me to let know if you'll be coming. But more importantly, do please write Jeff with your decision.

> **Owen Maroney: "The (absence of a) relationship between logical and thermodynamic reversibility"**
>
> Landauer erasure seems to provide a powerful link between thermodynamics and information processing (logical computation). The only logical operations which require a dissipation of energy are logically irreversible ones, with the minimum dissipation being $kT \ln 2$ per bit of information lost. Nevertheless, it can be shown that logical reversibility neither implies nor is implied by thermodynamic reversibility. By examining thermodynamically reversible operations which are logically irreversible, it is argued that information and entropy, while having the same mathematical form, have significant conceptual differences.

10-12-03 *City of Light* (to D. B. L. Baker)

Do you know it's been over a year since I've written you a proper letter? (You ought to know by now that a letter from me is not proper unless it contains at least 5K of raw text.)

I'm sure you've guessed from the title of this note that I'm in Paris. What a charming city ... even if it is full of surly waiters! I haven't been here since 1994, and then only for one day in a not-particularly-interesting part of the city. So really my memory has to reach back to 1991 for anything that remotely compares to this weekend. However even then, I would say I've grown so much and learned to appreciate Europe that it is altogether quite something new. Frankly I'm carried away in a romantic/bohemian mood. It probably also helps that I earn a lot more money now, and so don't really mind dropping 70 euro on a meal, as I did last night. It opens up exponentially more of the city, and even takes some of the edge off the waiters. Last night, a colleague and I dined next to a Picasso. It didn't make the food or the ambiance any better, but maybe *they* gave Picasso the extra something he needed to finally (after all these years) draw my attention more than fleetingly.

The brick streets, the cheese shops, the wine shops, the cafés, the life of the bohemian. I am having a great time. And I'm reawakening slightly.

... The long weekend has come and gone, and I'm now at the airport, waiting on a delayed flight. I thought I'd have a chance to write you much more from the Parisian cafés, but a proper flux of writing juices never materialized. Looking back, what can I tell you? What do I feel like telling you? Maybe the thing that will interest you most had to do with an Italian student, Giovanni Valente, who just finished his master's degree in philosophy in Padua, writing a thesis titled, "Probability and quantum meaning: Chris Fuchs' pragmatism in quantum foundations." He'd been wanting to meet me for a while, so a month ago I sent him my travel schedule, saying that he could drop by Dublin one of the times in between. Out of the blue, I got this from him Friday:

Valente-ism 4: *What about meeting in Paris? You wrote me that you have planned to go there (6-9 December) and I could be there. I've been taking a little rest after my viva and the GRE and my parents own a little flat in front of the Moulin Rouge, so it may be a good occasion to meet. Since I haven't been there for a while, I feel I could spend a few days in my favorite place. Anyway, if you think we may meet and drink something, let me know quite soon: I need to book a flight very quickly.*

Well, he wasn't lying! He showed up in Paris the next day, and he actually did have a flat just across the street from the Moulin Rouge! He made me a nice spaghetti dinner and we had a couple of bottles of wine there one evening. The flat was in an old bordello. I certainly never imagined on that infamous New Year's Eve at your house so many years ago that I'd one day get an opportunity like that. Apparently the boy's father was a socialist politician in Italy for some years. Who said socialism was the equalizer of mankind?[75]

Monday I lectured at École Polytechnique. It went well. But, the really nice thing was that they put me up in a hotel nearby. So I was in the middle of the Latin Quarter for the stay. That put a few thousand restaurants within my reach, even if only to walk by and look at (quite an experience in its own right). And I was just a mild walk away from the Pantheon, Notre Dame, the Louvre, Musée d'Orsay, Luxembourg Gardens, the Sorbonne, etc. I walked into the courtyard of the Louvre today around 4:00 PM, just after all the bustle had died down and just as the sun was moving into twilight. (The sun sets early this far north.) One could hear just a tinge of traffic noise coming over the top of the building. What a profound aloneness: I savored every second of it. I just kept saying over and over to myself "Le Grand Palais du Louvre." It is quite a monument to the power and creative force of mankind! Big buildings always do me in.

10-12-03 *First Meeting* (to M. Bitbol)

It was great meeting you the other day. As I told you then, I have greatly enjoyed finally reading some of your papers.

Let me paste below the passage from my samizdat *Quantum States: W.H.A.T.?* that refers to you. The text between the `bjb` and `ejb` symbols are quotes of Jeff Bub. Now that I understand that Jeff's "neo-Kantian" might just mean "somewhat pragmatic," I am much more intrigued than I was before. Have you written anything in English on the "blinding closeness" that Jeff mentions?

If you get a chance to have a look at it, I would greatly appreciate hearing your thoughts on sections 5 and 6 of my paper "The Anti-Växjö Interpretation of Quantum Mechanics". The paper can be found at my web page; there is a link to it below.

Michel's Reply

> Better late than never (I hope this faithfully translates the French "Mieux vaut tard que jamais").

[75] In the editorial stage of this samizdat, Giovanni pointed out that I "still don't fully get the difference between socialism and communism."

I thought I had to answer your mail one day, but many other tasks piled up. However, I finally found a free sunday morning and I read some of your stuff with pleasure. The "Anti-Växjö interpretation" sounded very relevant since Andrei Khrennikov is coming to visit us in CREA tomorrow!

I must say I was delighted to discover that, in this paper written by a physicist (you!), there is more excellent philosophy of quantum mechanics than I had ever seen before. As you already know, I am disposed to agree on most of what you say. I was drawn independently to very similar conclusions by combining a radical version of Copenhagen interpretation (plus some insights from Schrödinger) and a network of philosophical readings ranging from neo-kantianism to pragmatism. Here is, to begin with, a short list of points of agreement (with some nuances):

1) Quantum mechanics is a theory of contextual probabilities: Kolmogorovian probabilities in each context, several such probability spaces being "glued" in a unified Hilbert space structure. To summarize this idea, I called QM a "Meta-contextual theory of probabilities".

2) The main terms of the theory refer to certain gambling attitudes. With some stringent constraints however: coherence and intersubjectivity (or rather inter-situationality, invariance of the formal tools with respect to changes in situations/contexts), etc. When this constraint of inter-situationality or intersubjectivity is emphasized, one lands into a central concept of Kant's transcendental philosophy: constitution of objectivity.

3) I also agree that physical theories express the conditions for our successful life within the world rather than a faithful picture of the world. Your insistence on Darwinian evolution, on viability rather than mirroring, is very close to the ideas I developed after a long contact with Varela's autopoietic theory of cognition. Here is a short statement of this theory I recently wrote:

> In the autopoietic theory of cognition, the relevant concept is not inputs provided by the external world, but only local environmental conditions for maintaining an operationally closed unit. The invariants of this type of unit do not represent any feature of the world, but rather identify with steady aspects of its own internal dynamical organisation. As for the appropriate changes of an operationally closed unit, it does not prove that the unit possesses a faithful picture of the world, but only that its internal working is viable in relation to environmental disturbances. One must redefine the "cognitive domain" of the operationally closed unit accordingly. This domain is no longer some fraction of a pre-existing world liable to representation, but a region of the environment which has co-evolved with the closed unit and in which the latter's organization may persist, develop, and reproduce despite the disturbances.

As you rightly pointed out, the structure of an elephant (a special case of autopoietic unit) does not represent its environment, but is the end product of a long history of environmental alterations and adaptative changes: it is a summary of the adaptative moves the species had to do. One interesting (and very relevant) features of this example is that the adaptation to the environment is not purely passive. The behavior of elephants, their search for better-suited environments (by migration) or even their own transforming activity, is also part of the adaptative process. The adaptation then becomes somehow mutual. The organism and its environment can be said to "co-emerge". In the same way, our theories are likely to be an elaborated and stabilized byproduct of a cognitive history (made of "conjectures and refutations", gambling rules and failures) which massively involves our experimental activity (our "interventions" by means of apparatuses).

The latter general overview was partly expressed in my "blinding closeness of reality", which unfortunately exists only as a book in French. But the basic idea is easy to summarize: Bernard d'Espagnat (my master in this field) told that reality is quite difficult to picture (actually, in the micro-domain, even impossible to describe) because it is somehow remotely distant from us, because it is separated by a "veil" from us. This was only a metaphor as I soon understood by discussing with B. d'Espagnat, but the happy effect of this was to trigger a diametrically opposite metaphor. My idea was that the reason why reality is impossible to describe as such is that we are so deeply and intricately immersed in it, that we do not have the opportunity of creating the objectifying distance. My motto, inspired from Wittgenstein, can be translated thus: "The subject is not facing the world; it is so much committed in it that this does not allow description." But this does allow orientation in it, or anticipation of part of what may happen for us in it.

I thus warmly approve your "oceanic" picture!

4) Reality manifests itself by its "unpredictable kicks". This is perfectly true. But, as you know, there exists another, very popular, conception of how reality occurs to us. According to many people, we reach some real structure when we have extracted an invariant from phenomena. Since it is invariant, they say, it does not depend on particular points of view, or on particular subjects, and "therefore" (I am very reluctant when I read this "therefore"), it faithfully describes some independent feature of the world "in itself".

Scientists usually combine (in various proportions) these two conceptions of reality in the very dialectic of elaboration of their theories. But they are basically wrong. And, I believe, you are completely right: the only manifestations of something like reality beyond the narrow boundaries of our little persons are unpredictable kicks. Why is it so? Because there is a logical fallacy in the usual inference from invariance to "independent" reality. Any feature of a pre-structured "independent" reality would take for us the form of an invariant; but invariants are not bound to express features of some "independent" reality. We are not even sure that "independent" reality, if this sequence of words is meaningful at all, has already a structure in store, ready to be disclosed (Rom Harre calls the micro-world "the glub" to express the idea that it is likely to be a plastic and dispositional stuff rather than a pre-structured network of actual properties). So, if the cognitive invariants do not disclose the structure of "independent" reality, what do they do? They disclose stable, viable, and intersubjective forms of our anticipations (our gamblings).

Instead of opposing two conceptions of the manifestation of reality (as unpredictable variations and as structural invariants), we should only keep one of them: yours, namely unpredictable variations. The other conception only deals with what Kant calls "objectivity" in his consistently anti-metaphysical acceptation. Reaching objectivity in Kant's sense is by no means identical to mirroring reality. This is what Kant meant when he carefully distinguished objectivity from faithfulness to the "thing in itself".

I applied this remark to the status of QM in several papers (including "Some steps towards a transcendental QM", published by Philosophia Naturalis in 1998 and available on my website). In a paper in French about "Laws of Nature", I concluded thus:

> The boundary between what is transcendentally necessary and what is irreducibly contingent has moved beyond recognition. All the structural features of QM, including its law of evolution, are transcendentally necessary in so far as they express the conditions of possibility of any coherent (probabilistic) anticipation of the contextual phenomena obtained by means of a systematic experimental investigation. And the only element which remains definitely non-contingent is non-structural; it is the very occurrence of isolated experimental outcomes. Hence the aphorism: in QM, nothing structural is contingent, and nothing contingent is structural.

5) I also agree with you that the many-worlds picture of reality is amazingly daring and much more anthropomorphic than its supporters think. However, there are other possible meta-interpretations of Everett's interpretation than the many-worlds, and some of them proved very close to your (to our!) view. An example of them is the indexical view supported by Simon Saunders (and by me in "L'aveuglante proximité du réel"). Here, the universal state vector is not bound to describe the world as a whole, but rather to display the set of possible particular situations we may occupy within it, obtaining this or that unpredictable isolated outcome as a result of an experiment.

6) At this stage, I have to state my major point of disagreement with you: I suspect the remark you made about the "many-worlds interpretation" applies quite well to some of what you say. Let me explain this.

You write: "What I find egocentric about the Everett point of view is the way it purports to be a means for us little finite beings to get outside the universe and imagine what it is doing as a whole." I deeply agree with you.

But, then, you begin to do essentially the same as the naive Everettian. You try to describe our situation in the world from a sort of vantage point. Here is the sentence in your paper that made me suspicious: "I think the solution is in nothing other than holding firmly—absolutely firmly—to the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettist as holding to it—we have to recognize that what we're doing in the game of science is swimming in the thick middle of things."

Of course, once again, I am delighted by the Pascalian metaphor of "swimming in the thick middle of things". But you seem to take it as more than a metaphor: a definite belief about (not to say a faithful description of) the world and our position in it. You write seriously that "we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world". Isn't this a way of extrapolating one of our pragmatic - adaptative concepts, one of the concepts we need to swim with some success in the midst of the "glub" (here, the concept of a "physical system"), in order to describe everything including us as if it were seen from outside? I hear you saying something like "the world as I see it from my cosmic exile is made of physical systems and each one of us is one of these physical systems". But if we are "swimming" in the deep ocean of whatever we call "reality", we have absolutely no context-independent concept at our disposal, not even the very general meta-concept of "physical system". We must say that we ignore everything of the "thing in itself", including whether it is organized or not in a plurality of "physical systems". And we must therefore content ourselves with stating the formal conditions of our cognitive aptitudes (within it). This latter attitude is typical of the Kantian and neo-Kantian

> lineage of philosophy (when one gets rid of the foundational aspect of Kant himself and hold a pragmatic variety of Kantianism, as I do). I hope you'll recognize it as a radical variety of your view… I wonder whether you'll become a member of our radical club or rather decide to stick to your position as it stands.

10-12-03 *Thoughts in France* (to J. Bub)

I'm just back from a few days in Paris. I had a great thoughtful time reading your paper "Why the Quantum?" during a long lunch and during my morning and afternoon coffees in the Latin Quarter yesterday. I read it as I threaded in and out of the Louvre and Notre Dame. There is much I like about the paper, but there is also much I disagree with (especially in the last third). I owe it to you to get those thoughts down solidly. At the moment, I'm shooting to get them to you before the end of the weekend. Just a heads-up so you'll have a chance to stock up the liniment oil!

10-12-03 *Down with Frequentism* (to K. R. Duffy)

Sorry I forgot to contact Schack for a title to his talk Monday. (Are you out there Rüdiger?) Let's title it this tentatively:

> Defining (Carefully) a Quantum-Mechanical Relative Frequency Operator.

Schack's affiliation is below.

11-12-03 *My Communication Skills* (to R. Balian)

It was very good meeting you in Utrecht two weeks ago. I was dismayed a little bit by your reaction to my talk, however. Without a doubt, I must have presented something in a confusing manner—for in the end all the things I said were rather trivial and elementary. I.e., they were not the sorts of things that could even be controversial, much less between two Bayesians.

In any case, I hope that the paper associated with my talk "Quantum Mechanics as Quantum Information (and only a little more)" will do a better job than I did in person. For your reference, you can find it here: `http://www.arxiv.org/abs/quant-ph/0205039`. If you find anything in it that is still unclear after your reading, I would much appreciate your feedback.

11-12-03 *Thomas, Pauli, Lubańaski, and Enz* (to A. Peres)

Asherism 39: *You are an inexhaustible source for references: where is the Pauli-Lubańaski vector? I asked Charles Enz who wrote a scientific biography of Pauli, but got no answer (Enz, who is not young, was a student of Pauli). Thanks.*

- L. H. Thomas, "The Kinematics of an Electron with an Axis," Phil. Mag. **3**, 1–22 (1927).
- J. K. Lubańaski, "Sur la théorie des particules élémentaires de spin quelconque. I.," Physica **9**, 310 (1942).

I'm not sure how Pauli came into the game though. If you find out, please let me know.

About Enz, I had some contact with him soon after the fires in Los Alamos: He replenished my supply of historical papers on Pauli that he had written. His last note came to me 6/20/2000.

Since then I have written him on two occasions, the last being 6/10/2003, and have not gotten a reply. This year or so, his book *No Time to be Brief: A Scientific Biography of Wolfgang Pauli* appeared, but I found no reference to Lubańaski in it. I understand the book to be good, but I have not read it myself.

15-12-03 *Alchemy* (to G. Valente)

I have been meaning to write you since my departure from Paris. I hope you will accept my apologies for taking so long. Mostly, I just want to say that I had a fabulous time discussing philosophy/politics with you. Thanks also for the nice pasta meal.

Let me share one of my more secret projects with you as a little recompense. I'll attach a document I'm compiling called "The Activating Observer: Resource Material for a Paulian/Wheelerish Conception of Nature". It's still very far from complete, but I think you'll enjoy it even at this stage.

At times, I have dreamt fleetingly that the ancient alchemists may not have been so very far off course. You have rekindled that dream in me again.

16-12-03 *BIPM* (to R. Balian)

Thanks so much for your notes. I am pleased and excited to hear that you have printed out some of our papers. For myself, I think quantum mechanics is at a crucial time. The issue as I see it is that if we all can just muster the stamina and courage to be absolutely consistent in following through with a Bayesian interpretation of quantum probabilities some of the greatest physics may be just around the corner.

Roger's Preply

> When printing your paper on qm and information, I noticed that you locate the "Bureau International des Poids et Mesures" in Paris. It is actually in a suburb, close to the one where I live, in a nice park on a hill just above the famous china manufacture of Svres. Its address, that is mythical since we learn it at school when we are taught the metric system, is: "Pavillon de Breteuil, Sèvres".
>
> The kilogram of the SI system of units is indeed in deep caves below this small end of XVIIIth century castle, but the meter, which is also there, has lost its official status . . .

16-12-03 *Reading Assignment* (to M. Pérez-Suárez)

I've been thinking a little bit on what we might work on. There's a cluster of problems I've been thinking about to do with symmetric informationally-complete sets of states. We'll probably pick up there after you get settled in here.

But let me give you a reading assignment as you suggested.

Take a look at:

- Symmetric Informationally Complete Quantum Measurements
  `http://www.arxiv.org/abs/quant-ph/0310075`

- Quantum Key Distribution Using the Trine Ensemble
  `http://www.arxiv.org/abs/quant-ph/0311106`

And the draft of a paper I am writing, that I will attach. I won't have any time to correspond about the ideas in these, but that's the sort of thing you can familiarize yourself with.

17-12-03 *Pragmatism for the Holidays* (to L. Henderson)

Hendersonism 1: *And by the way, have you sent me a copy of your book yet? I would like one.*

Yes ma'am. ... Well, actually I've sent your address to VUP. But I don't know if the Swedish holidays will get in the way of a prompt delivery.

Hendersonism 2: *I have also been eyeing my roommate's books on pragmatism with a certain curiosity since our train conversation—it's not something I know much about. Maybe in the holidays I will get a chance to have a look. What do you suggest of William James? We have* Pragmatism *and* The Meaning of Truth *on the shelf.*

Definitely *Pragmatism*. It's the book that first hooked me. I think *The Meaning of Truth* causes more trouble than it mends. When or if you're ready for that stage, I'll try to dig up some selections of Putnam, Dewey, or Rorty for you. I might also suggest some very pleasant leisure reading in Louis Menand's book *The Metaphysical Club: A Story of Ideas in America*. Here's the Amazon.com link in case you want to read about it.

17-12-03 *State of the Union* (to R. E. Slusher)

The world of quantum information remains firm. I'm trying to get three papers out before February. One on my quantumness measure for the Holevo festschrift, one with Schack on quantum-state tomography from a Bayesian view, and one with Brassard on the fundamental nature (for specifying quantum mechanics itself) of quantum cryptographic tasks. Also, Bennett, Brassard, Preskill, and I just turned in our section for the ARDA quantum crypto roadmap. So that's finished for a while!

Was there any progress putting Bernie's tetrahedral measurement box together? I've got a PhD student joining me in January from Spain, and my plan is to get him working on some theory to do with these measurements. Also I've started up a collaboration with a statistician, Richard Gill from Utrecht, to continue the search for things such a measurement would be optimal for. One of these days we're bound to find something it's good for!

18-12-03 *Finally Some Answers* (to A. Sudbery)

Sudberyism 6: *I wanted to make some general points about the nature of our disagreement and also some particular (to the point of being finicky) points about* 0205039*. I'll probably have to stop before I've finished, so I'll take the general points first.*

Sudberyism 7: *I want to write a piece called "Quantum interpretation needs no mechanics", but perhaps "polemics" would be a better last word.*

I try to use every trick in the bag to motivate the community to think about the connections and contrast between quantum mechanics and general probabilistic reasoning. Sometimes some of the tricks work—and when they do they sometimes work spectacularly—and sometimes they just grate on the reader's nerves. If you wait long enough before giving up hope on me, I might just find a trick that'll work on you. The reason I believe this is I see *truth* here, and the truth will ultimately prevail (no matter how bad a prior one's earlier education may have drilled into them).

Sudberyism 8: *I'm sure I'm not alone in seeing your dissolving of the "violent" state change (on p. 34) as a cheat. Calling it a "mental readjustment" doesn't make it any less violent. Incidentally, there's something wrong with eq. (97); these U_i are not unitary.*

Fair enough. It is violent—every bit as violent, discontinuous, and irreversible as a usual application of Bayes' rule. But the point is that it is on the inside of the agent's head, and no one cares what is going on in the agent's head. What physicists should be worried about is the physics of the external world (and not the agent's head). And who knows what really happens there ... especially if physics itself is only really about the interface.

About the U_i's you are correct. I'm sorry, I've prepared an updated version with a lot of the typos, etc., fixed, but I haven't reposted it yet. Here's what the new edition says:

> That is to say, there is a sense in which the measurement is solely disturbance. In particular, when the POVM is an orthogonal set of projectors $\{\Pi_i = |i\rangle\langle i|\}$ and the state-change mechanism is the von Neumann collapse postulate, this simply corresponds to a readjustment according to unitary operators U_i whose action in the subspace spanned by $|\psi\rangle$ is $|i\rangle\langle\psi|$.

Sudberyism 9: *There's something I'm failing to understand on p. 20. You seem to be arguing that equiprobable events must be identical. Is that what you mean, or am I being obtuse? (Also, in para 3, why do you think the word "measurement" belongs only to quantum mechanics?)*

Looking back, I don't know how to say it any more clearly than I said it there:

> The consequences (m_i and n_j) of our disparate actions (M and N) should be labeled the same when we would bet the same on them in all possible circumstances (i.e., regardless of our initial knowledge of S). To put this maybe a bit more baldly, the label by which we identify a measurement outcome is a subjective judgment just like a probability, and just like a quantum state.

You might contemplate that God can see the difference between m_i and n_j, but we can't. And to that extent, we should call them the same thing.

Sudberyism 10: *I also, last weekend, enjoyed some more skimming through your* Paulian Idea*, particularly the glimpse of the smithy where you and Asher forged your* Physics Today *article, but I haven't got my notes with me. I remember only your impatience with your commentators. I repeat, we all have to learn tolerance. But not of interfering APS editors; fight with David Mermin under the boojum banner!*

The worst example I have ever seen of this came from the *Physics World* editor I had to deal with for the thing I wrote for the November issue. Here is a sample of my exchange with him. (Note what an annoying character I appear to be in the first part of this, but then read my second exchange to see why I was furious.)

ME: Thank you; that was a good and well-reasoned change, and I appreciate it. However for most of the rest I am annoyed. Could your copy-editor not see that I was in complete control of my writing? All my words were chosen for reasons, and it would have paid him to spend a little time trying think of what those reasons might be. There is a difference between "mustered" and "muttered", for God's sake. In any case, the article will be written in my style (as long as it is grammatically sound), or it won't be written at all. Luckily I have not yet released the copyright to *Physics World*. I will spend much of my morning reinstating a lot of my old word choices—ones that reflect my writing style and personality, not his—and send you a revised draft before the day is out.

HIM: I am sorry you did not like the subediting, which was done entirely by me. Please rest assured that it was not done to annoy you, but — I hope — to make sure your article is as readable as possible. I'm sorry also for the mustered/muttered confusion, which we can of course change.

ME: Changing almost every sentence in the text—19 out of 23 in the first column alone—is not acceptable.

In the end, that fraction had carried through to the whole article: He had literally changed 83% of the sentences! I spent almost a whole day at Caltech changing almost all of his changes back, and writing an explanation for why I preferred my wording over his. The final product is almost exactly as I had originally written it. Tell me, does it look so bad to you? Did it look worth an 83% change?

Sudberyism 11 : *However, all the modern dictionaries I have give an overwhelming majority of hyphens with "non" – it must be a transatlantic thing.*

I had a similar encounter with John Smolin on this issue. Here is what he wrote me:

John Smolinism 9 : *Regarding the "non" prefix. I agree with you for the most part. At first I didn't. I considered your extensive list of non words as evidence that you DO need the hyphen, since if the dictionary lists so many words, it must consider all the ones it doesn't list as nonwords. And far be it from me to make up a new word, so I preferred the hyphen. But after looking around the net I found basically one style guide that agreed with me and many that more or less tell you to never use the hyphen. So I'm changing sides. I still find it hard to write "nonsecret" as I'd never accept that as a scrabble word. Of course, the solution there is to use "public" instead of making up the word "nonsecret" anyway. Oh, and "nonagenerian" should not have been in your list. Cut-and-pasting from the dictionary is a nongood idea.*

18-12-03 *Finding Gleason* (to R. Schack & C. M. Caves)

Just wanted to say how much I'm starting to like your work on the frequency operator stuff. At first—though I never said so aloud—I thought, why beat a dead horse? Eventually the ugliness and mystery of the construction would make it disappear on its own right, ably assisted by the simplicity of the POVM-Gleason construction. But after Rüdiger's talk here, I started to reassess. What I was particularly impressed with is how you were able to uncover a hidden application of Gleason's very starting point. And that's nice.

This leads me to pose a challenge, whose solution I would relish that much more. Find a hidden use of Gleason's assumptions or theorem in Wojciech's "envariance" argument for the quantum probability rule.

You see, what more could there be to the quantum probability rule than Gleason? Anyone who finds it by some other supposed means has to be faking something.

18-12-03 *General Summary* (to R. W. Spekkens)

Spekkensism 22: *Howard mentioned that you got the news today about your candidacy at PI. A disappointing outcome. How are you feeling about the whole affair? What are your plans?*

Spekkensism 23: *I am still quite bummed out about the outcome of the discussions on your candidacy. You should know that there were many strong advocates for you here.*

Thanks. I feel quite dicked over by this, you know. One night at the Jane Bond, Valentini said jokingly (not so jokingly), "We need to bump you off before you influence any others." That must have been the consensus in the mind(s) of whoever it was that actually quashed me.

Well, I haven't been bumped off. And this is too powerful of a turn in quantum foundations that it won't live by itself. But my life has been made immensely inconvenient by this decision. And I am so pissed off that I won't now be able to be fully engaged in the birth of this baby. It does quite sicken me.

It's just not a good thing to be up at 4:00 AM ...

18-12-03 *Invitation* (to H. J. Briegel)

Congratulations again on the big move. I suspect you are quite enjoying your new life. Or, in any case, the promise of directing great progress must be very satisfying.

I would love to visit Innsbruck and am flattered by your invitation. However, at the moment, my travel schedule is so full, I dare not make any more commitments for the foreseeable months. Perhaps I will take you up on your offer of a visiting position at the new institute in (late) Autumn 2004 or Spring 2005. However, even then, I hesitate to make a firm commitment at this point. The truth is, I am quietly seeking an academic position outside of Bell Labs. And if that materializes somewhere, I know that I will have to learn new responsibilities with respect to my travels.

But oh, how I would enjoy a discussion with you on some deep quantum thoughts. I saw a very good talk by Andrew Childs recently in which he showed a couple of distinct ways to systematically generate states of the cluster-state type or variety (though they appeared to be inequivalent to your own cluster states). It was quite fascinating, and I was once again struck by the depth of the approach. I really want all of the most fascinating stuff in quantum mechanics to reduce to the structure of measurement, and your computational model seems to be a move in that direction (modulo the initial state)—I've told you that before. But the opportunities look great with just the right thinking.

19-12-03 *Time to Think about Time* (to G. L. Comer)

Comerism 7: *What's that line of Wheeler's about the past? Is it something like "... the past exists insomuch as at is consistent with the present"? I've had this crazy idea about closed timelike curves, information theory, no-cloning theorems, and time travel. The craziness is this, can I perform a whole suite of information-gathering exercises today, that would in effect change the past so that it looks like I actually travelled back in time and effected the changes directly? Can nature be so cockeyed as to allow something like that? I mean, can I squeeze and prod nature in such a way that I can extract a present that is consistent with some other past? In effect, could I then "travel" back to the "past", and "kill" my grandparents? Surely nature is not this screwy.*

It was never quite clear what Wheeler was saying on this subject, i.e., whether we can change the past with our quantum measurements, or rather just change what we can say about the past. I think he flip-flopped from time to time. My own present take is that it is neither. A quantum measurement here and now cannot actually change the past there and then (whatever that might mean). That is just as a quantum measurement here cannot actually change anything physical there (i.e., a spacelike separation away). Rather with regard to time, it is all and only about the present. I would say this is inspired by Wheeler's take on the subject, but not quite the same. (Though who can say, maybe it is). The closer alliance is to the thought of the pragmatist philosopher George Mead. I'll attach a note that hopefully makes some sense of this. [See note to Savitt, dated 23 September 2003.] Reading back over it, I still like my quantum discussion, but there's no doubt it needs fleshing out. Tell me if my discussion or the Mead passage provokes any thoughts.

19-12-03 *Prizes, Hippies, and Vectors* (to A. Peres)

Congratulations on the Rothschild prize. What was the particular citation for? Years of theoretical service? Or was it for a particular paper or series of papers that you wrote? Did you get any money with the prize?

Thanks for the cheer-up note. It certainly helped! I just couldn't stop laughing about the commune-of-hippies remark:

> I must confess that I am not well impressed by the research being done at PI. It looks more like a commune of hippies, very pleasant to be there, but not fruitful for physics.

(I read the whole thing to Kiki, who had just woken up.)

Asherism 40: *You gave me a reference to a paper by Thomas, about the Pauli-Lubańaski vector (the paper is in Phil. Mag. 1927). There is in it no mention of anything like the P-L vector. What I am looking for is a statement that it actually is a classical vector, indep of quantum mechanics. The reason is that Czachor has sent to PRL a Comment on a paper by Danny, Petra and me, and I have now to prepare a rebuttal.*

All I did was a google search on Pauli-Lubańaski vector, and those were the references I found. They appeared in a few papers, so I felt confident that they must be right. But maybe one cited the incorrect reference, and the effect just snowballed as one author simply went to another's paper rather than the original reference.

Anyway here is the passage I find in Enz's biography of Pauli that refers to Thomas:

> Frenkel did the only sensible thing: that is, use the analogy with the electromagnetic field to enlarge the magnetic moment vector $\vec{\mu}$ into a covariant antisymmetric tensor by combining it with an electric dipole moment and to develop the associated equation of motion [24d]. This work, which confirmed a Thomas factor of 2, was submitted for publication on 2 May 1926. In January of the following year a second paper by Thomas appeared in which essentially the same formalism was developed [24b]. But Pauli had been convinced of the correctness of Thomas's result already before.

Reference [24b] is the one I've already given you. Reference [24d] is:

- J. Frenkel, "Die Electrodynamik des rotierenden Elektrons," Zeit. fur Phys. **37**, 243 (1926).

Maybe I am not quite understanding the vector you speak of. I found this essay on "Relativistic Angular Momentum" on the web http://panda.unm.edu/Courses/finley/P495/TermPapers/relangmom.pdf

that makes a distinction between the "spin vector" and the Pauli-Lubańaski vector. It is written by an undergraduate, Nick Menicucci, who was Carl Caves' student for a while. I know that Carl has much respect for him, calling him the best undergrad physics student he had ever had. (They have one or two papers together.) Maybe he's the right person to contact with your question. He is now a graduate student at Princeton. In any case, there is no quantum mechanics in Menicucci's essay, and it certainly defines something called the Pauli-Lubańaski vector (in section 5).

22-12-03 *Tail Tuckers* (to D. B. L. Baker)

Sorry to hear about the decline of your pool skills. But, to mimic Einstein, "Whatever your troubles with pool, I assure you that mine are worse."

Bakerism 1 : *I'm glad to hear about your latest book. Do you get any money out of the deal?*

With the VUP edition, I didn't make anything monetarily. The gain was purely the satisfaction that comes with spreading the word. (They printed 200 copies and financed it with a Swedish science grant.) For the Kluwer edition (initial printing 450 copies), I'll get 6% of the net receipts and 20 free copies. That's effectively making nothing again, except the satisfaction of spreading the word even further.

Bakerism 2 : *I suppose your title refers to Linus Pauling? Can't see how any of my thoughts could have been any help. Maybe my comment about wearing a beret? I thing the coolest pictures of any "respectable" person wearing a beret would be him and Leonard Bernstein.*

The title refers to Wolfgang Pauli. The "Paulian Idea" is something I made up: It's effectively the conjunction of these three quotes, which I took out of one of his papers.

> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case.
>
> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

The book as a whole is mostly a study in the sociology of science—it is quite literally only a collection of my emails—but strangely they have been emails with quite an effect on quantum foundations. For instance, they've been responsible for at least two Bohmians leaving the fold! (That alone probably made the writing worth it, I would say.)

What was your role? Lending me an ear, pure and simple. In particular, the first letter in the collection "Noodles of Nothing"—wherein I tell you how much I love the writing style of William James and how I love to read letter collections—sets the tone for the whole book.

Bakerism 3: *Any thought to text books?*

That's a much more serious project, and yes I have thought of it and even started it. The title will be *Quantum Foundations in the Light of Quantum Information*. I've been courted by three publishers so far (Kluwer, Springer-Verlag, and World Scientific), and I've told them all "yes". But I haven't signed any papers. For this one, I'm going to work much harder to get the best deal I can. Don't hold your breath: It'll be two or three years in the making.

Thinking of William James, let me attach a little story I wrote up about Emma a couple months ago. [See 22-09-03 note "Cosmology" to C. H. Bennett.] Actually maybe I'll put the whole context, since we've never had too much parenting talk between ourselves. I bet you've had to go through similar things yourself. (Put yourself in the place of Abner and read from bottom to top.) [See 22-09-03 note "A Thought of You" to A. Shimony.]

23-12-03 *Back in the Fight* (to H. J. Kimble)

You may have heard that the Perimeter Institute—or should I say a faction within the PI—finally gave me the axe (and showed me just what a dangerous thing it is to act like one cares about quantum foundations). So I'm out there acting like an unemployed physicist again for all intents and purposes (working under the assumption that Bell Labs will eventually tank).

23-12-03 *Great Americans* (to C. M. Caves)

I was thinking of the above title, and I was thinking of you. And I decided to send you a small gift for the holidays. Amazon.com says the package will arrive in about a week. It contains two books that I have enjoyed very much: *Philosophy and Social Hope*, by Richard Rorty, and *The Metaphysical Club: A Story of Ideas in America*, by Louis Menand. The writers were my two favorites of this year's reading. You might know Menand from his writing in the New Yorker; the book is a Pulitzer prizewinner in history.

Anyway, I learned a lot about quantum mechanics from these books. And I learned a lot about true Americanism (i.e., the kind of Americanism we need to strive to get back to ... and actually mould into a more stable, long-lasting form). Done right, I think the two subjects are probably the same thing.

I hope you get as much out of the books as I did. When I read a book, I keep an index card in it for recording small notes and page numbers on the things that really interested me (usually things I deemed somehow connected to quantum mechanics). Below is the tabulation for these books.

Louis Menand, *The Metaphysical Club: A Story of Ideas in America*

Holmes on immortality – 60
Holmes's pragmatism – 62, 63
Darwinism discussion – 122, 123
Darwinism as nonscience – 127
James's speech on Shaw – 147, 148
Quetelet's view on crime – 188
Schweber reference – 473 #31, 474 #44
nature and open markets – 195
Bayesian probability – 197
higgledy-piggledy – 199

Richard Rorty, *Philosophy and Social Hope*

Zen and the Art of Motorcycle Maintenance – 8
anything goes (not!) – 18
Appleby's point – 37
some things beyond our control – 51
truth as a nonepistemic notion (Davidson) – 164 #5
What is evidence? – 149-151

23-12-03 *Little Comments* (to R. Schack)

I've read your paper now. I like it; it's fine. [. . .]

By the way, I'd definitely like to swipe some material from this for the Matteo Paris thing. Your discussion on subjectivity has me thinking about the proper definition again. I wonder if I would now go so far as your first sentence in your section 2? Mind you, I just don't know. An alternative I'm playing with at least might go something like this: "To say that a quantum state for a system is subjective means that the state is not determined by any objective facts about the system. The quantum state, instead, takes its ground in the agent, whose beliefs the state is a compact summary of." Or something like that. It is a little more careful in that it does not say that the state is determined by no facts, but rather if it is, those facts are not determined by the actual system itself.

Here is a lovely passage drawn from near the conclusion of *The Metaphysical Club*:

> Academic freedom and the freedom of speech are quintessentially modern principles. Since the defining characteristic of modern life is social change—not onward and upward, but forward, and toward a future always in the making—the problem of legitimacy continually arises. In a premodern society, legitimacy rests with hereditary authority and tradition; in a modernizing society . . . legitimacy tends to be transferred from leaders and customs to nature. Agassiz and the senior Holmes and Benjamin Peirce all assumed that social arrangements are justified if they correspond with the design of the natural world . . . But in societies bent on transforming the past, and on treating nature itself as a process of ceaseless transformation, how do we trust the claim that a particular state of affairs is legitimate?
>
> The solution has been to shift the totem of legitimacy from premises to procedures. We know an outcome is right not because it was derived from immutable principles, but because it was reached by following the correct procedures.

And our own point is that Dutch-book coherence—and more generally, though yet to be worked out fully, quantum 'coherence'—is the correct procedure.

07-01-04 *Probability Tables* (to P. G. L. Mana)

Sorry for the long absence. I think your paper is great; it'll make a good contribution. I also encourage you to post it on `quant-ph` once you have finalized it.

A couple of comments on the parts of your text related to me.

1) "It is unknown to me how the photons produced in this process tend to respond to various interventions." You call this sentence "unquestionably meaningful" even for the (de Finettian) Bayesian. I don't think it is nearly as meaningful as you think. For instance, how would you precisely define "tend" without a notion of ignorance or probability? On the other hand, if you try

to define it through a notion of objective chance or propensity, then you'll give the de Finettian heartburn again.

I think what you're driving at is that you really want to think of your "preparations" as "objective facts". So, if they're objective facts, why don't you just drop the fancy language of preparations and be done with it? But then, show me an independent definition or meaning or description of these objective facts outside of their use in your probability tables. For me, your s_1 ... s_M are just prior states of belief, and I think that is the only consistent account a Bayesian can have. The conceptual role of the $r_1 \ldots r_L$, however, are different. They represent data upon which we update; they represent sense impressions. (Better yet: They represent the feedback the world gives to our prior beliefs, consequent to our stimulations to or interventions upon it.)

Concerning the preparation-vectors s_j themselves it is just as bad. For you yourself say, "Note, in particular, that the numerical values of the vectors s_j and r_i ... depend upon the *whole* collection of probabilities p_{ij}: if one of these is changed, then ... all the vectors will change." For the de Finettian, all probabilities—no matter where and for what phenomenon they are used—are of the same subjective flavor. Thus, just as there can be no such thing as an unknown probability, there can be no such thing as an unknown preparation-vector.

2) "[I]n fact, the quantum state rather appears to be *part* of a state of knowledge (or belief) ... Perhaps the point is that CFS's notion of a 'quantum' state of belief implicitly assumes the *existence* and the *particular structure* of the whole set of quantum POVMs (i.e., the interventions)."

Agreed. The upshot of Gleason's theorem (and Gleason-like theorems with POVMs) is that GIVEN the structure of measurements, a quantum state is nothing more and nothing less than a compendium of probability distributions (and thus a state of belief for the de Finettian). However, the upshot of my analysis in Section 7 in `quant-ph/0205039` and much of my web book *Quantum States: What the Hell Are They?* (posted at my webpage) is that the assignment of a given POVM to an intervention is every bit as subjective as the quantum state itself. I.e., a POVM is a kind of belief itself. So, the GIVEN in Gleason's theorem is not to be taken lightly or ignored. And you, with your probability tables, have identified this issue from another track. That's excellent.

Anyway, as I say, I enjoyed your paper. I think you're doing excellent work. Send us the reformatted version as soon as you can. We can argue about the business above at our leisure.

07-01-04 *Deep Breath* (to G. Brassard)

Brassardisme 7: *My talk at QIP is next week and I still have not a clue how I could possibly fill 50 minutes with musings on*

> **Title:** Quantum Foundations in the Light of Quantum Information
>
> **Abstract:** The late Rolf Landauer has once claimed that "information is physical". The main point of this talk is to argue that "physics is informational"! At the moment, quantum information theory is replete with beautiful theorems on what is and is not possible according to quantum mechanics. For example, quantum key distribution is possible but quantum bit commitment is not. But the axioms of quantum mechanics are strange and ad hoc, reflecting at best the history that led to discovering this new world order. It's time to pause and reflect on what is really fundamental and what are merely consequences. Could information be the answer?

Panic is high and steadily rising.

Take a deep breath. There's a lot of good stuff on the theme out on the web now. You could take the opportunity to give a summary of the various issues out there and how they all converge in the same direction: Namely—as I see it—quantum states and operations are expressions of information, rather than expressions of a reality independent of information-using agents. If we can just swallow that idea whole and learn to savor it, rather than fight it—your conclusion could be—may be the best thing we can do to prepare ourselves for the best, most outlandish physics yet to come.

Here's some papers to look at that come to mind:

1. `quant-ph/0205039`, "Quantum Mechanics as Quantum Information (and only a little more)," Christopher A. Fuchs.
2. `quant-ph/0310017`, "Classical Teleportation of Classical States," Oliver Cohen.
3. `quant-ph/0203070`, "The Nature of Information in Quantum Mechanics," Rocco Duvenhage.
4. `quant-ph/0101012`, "Quantum Theory From Five Reasonable Axioms," Lucien Hardy.
5. `quant-ph/0211089`, "Characterizing quantum theory in terms of information-theoretic constraints," Rob Clifton, Jeffrey Bub, Hans Halvorson.
6. `quant-ph/0310101`, "A note on information theoretic characterizations of physical theories," Hans Halvorson.
7. `quant-ph/0310067`, "Can Quantum Cryptography Imply Quantum Mechanics?," John A. Smolin.
8. `quant-ph/0311065`, "Can quantum cryptography imply quantum mechanics? Reply to Smolin," Hans Halvorson, Jeffrey Bub.
9. `quant-ph/0306179`, "Gleason-Type Derivations of the Quantum Probability Rule for Generalized Measurements," Carlton M. Caves, Christopher A. Fuchs, Kiran Manne, Joseph M. Renes.
10. `quant-ph/0304159`, "Quantum information processing, operational quantum logic, convexity, and the foundations of physics," Howard Barnum.
11. `quant-ph/0208121`, "Betting on the Outcomes of Measurements: A Bayesian Theory of Quantum Probability," I. Pitowsky.
12. `quant-ph/0305088`, "Copenhagen Computation: How I Learned to Stop Worrying and Love Bohr," N. David Mermin.
13. `quant-ph/0305117`, "Why can states and measurement outcomes be represented as vectors?," Piero G. L. Mana.
14. `quant-ph/0307198`, "A de Finetti Representation Theorem for Quantum Process Tomography," Christopher A. Fuchs, Rüdiger Schack, Petra F. Scudo.
15. `quant-ph/0210017`, "Quantum theory from four of Hardy's axioms," Rüdiger Schack.
16. `quant-ph/0308114`, "The Bell-Kochen-Specker Theorem," D. M. Appleby.
17. `quant-ph/0311039`, "Multilinear Formulas and Skepticism of Quantum Computing," Scott Aaronson.

18. `quant-ph/0107151`, "Whose Knowledge?," N. David Mermin.

19. `quant-ph/0109041`, "How much state assignments can differ," Todd A. Brun, J. Finkelstein, N. David Mermin.

20. `quant-ph/0206110`, "Conditions for compatibility of quantum state assignments," Carlton M. Caves, Christopher A. Fuchs, Rüdiger Schack.

I'm sure I've missed a ton of things.
Skim and summarize!

07-01-04 *Four More* (to G. Brassard)

Let me give you four other papers that might be useful. None of these have appeared on the web yet.

1. Ari Duwell's "Quantum Information Does Not Exist" (appeared in the September 2003 issue of *Studies in History and Philosophy of Modern Physics*—a special issue edited by Bub and me on quantum information and foundations).

2. Jeff Bub's "Why the Quantum?". I think it will be his contribution for the Clifton memorial. Conceptually, it may do a better job than the earlier papers.

3. Piero Mana's "Probability Tables". This isn't quite the final version, but it must be quite close. It'll appear in our Växjö proceedings this year. If you want to mention it, you should get his permission. But, at the least, it may help you understand his previously posted paper.

4. Scott Aaronson's "Is Quantum Mechanics an Island in Theoryspace?" It'll appear in our Växjö proceedings this year. If you want to mention it, you should get his permission. It's a fun (and funny) paper.

OK, that exhausts my thoughts for the moment.

07-01-04 *A Really Important One* (to G. Brassard)

Forgot a really important one: Rob Spekkens's paper-in-the-writing, "In Defense of the Epistemic View of Quantum States: A Toy Theory." The last draft I saw of it was 65 pages. It's amazing the number of quantum-information style phenomena he nails with his toy theory.

You might ask him if he'd send you a copy of the draft to peruse, if you'd like to say a word or two about it.

Below is the abstract he used for his Caltech talk on the subject.

> Title: In defense of the view that quantum states are states of knowledge
>
> Abstract: I present a toy theory that is based on a simple information-theoretic principle, namely, that when one has maximal knowledge of a system, one's knowledge is quantitatively equal to one's ignorance. The object analogous to the quantum state in the theory is a uniform probability distribution over hidden states. Because the theory is, by construction, local and non-contextual, it does not reproduce quantum theory.

Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. Such phenomena include: the noncommutativity of measurements, interference, no information gain without disturbance, the multiplicity of pure state decompositions of a mixed state, the distinction between two-way and intrinsic three-way entanglement, the monogamy of pure entanglement, no cloning, teleportation, dense coding, mutually unbiased bases, locally immeasurable product bases, unextendible product bases, the possibility of key distribution, the impossibility of bit commitment, and many others. The diversity and quality of these analogies provide compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

08-01-04 *Calls to Copenhagen* (to M. Pérez-Suárez)

It's too bad your plans had to change to a Jan 20 arrival. It turns out I will be gone Jan 23–27, for my mother's 75th birthday party in Texas. Sorry I didn't warn you of this before, but I just found out myself yesterday. I had to make a mad dash to get the tickets.

Beyond that, 20 minutes ago, I just got a call to Copenhagen! The Niels Bohr Institute has asked come for a job interview February 2. Thus you can probably count on my being gone Feb 1–3.

I hope you'll understand that in neither of these cases could I say "no". I realize that this will leave you a little guidanceless at the very beginning of your stay, but I'll try to think of ways to make things go as smoothly as possible in my absence.

08-01-04 *Partial Exchangeability* (to D. Poulin)

I'm glad to hear there was enough material in the paper to make it worth three readings!

Poulinism 14: *Is it possible to find, for example, a 3-qubit state which (i) CAN be obtained by tracing out the 4th qubit of a 4-qubit symmetric state but (ii) CANNOT be obtained by tracing out the 4th and 5th qubits of a 5-qubit symmetric state?*

Yes it is. Jaynes gives some good examples in

- E. T. Jaynes, "Some Applications and Extensions of the de Finetti Representation Theorem," in *Bayesian Inference and Decision Techniques*, edited by P. Goel and A. Zellner (Elsevier, Amsterdam, 1986), pp. 31–42.

for the case of classical probability distributions (i.e., commuting density operators) and gives some good intuitive explanation for what goes wrong.

There are similar results for partially exchangeable (entangled) quantum states. Joe Renes is the expert on that, and can probably give you some good examples in the noncommutative case.

09-01-04 *Where Did You Come From, Where Did You Go?* (to S. J. van Enk)

Where did you come from Cotton-Eyed Joe?

Where have you been? I never got a reply from you after I answered the note you sent to Kiki's account.

Anyway, I have a question in case you're listening. Have you seen Oliver Cohen's paper "Classical Teleportation of Classical States", `quant-ph/0310017`? I remember one day (two or three years ago) you and I were talking at the chalkboard, and we were toying with trying to write down a classical coin-toss "teleportation" scheme. Do you remember if we succeeded? I remember we were playing with something quite similar to this. (I can certainly dig up a lot of places in my samizdats where I've taken it for granted that such a kind of scheme would work.)

Anyway, I've got to decide whether this paper should be a PRL. I'll attach below the considerations I sent Bob Garisto this morning to help you see what I'm thinking.

09-01-04 *Christmas with The Pogues* (to D. B. L. Baker)

I didn't write you the letter I had hoped to during the Christmas holidays.

Instead, let me send you a song I learned this Christmas—a happenstance by being in Ireland. With your better knowledge of music than me you've probably already known it for a long time. But I'll place the words below and attach an mp3 file with the song. The words, especially near the end, have such a powerful effect on me. I haven't been able to get the song out of my head all this morning, and consequently the present note.

> **Fairytale of New York**
>
> by The Pogues
>
> I could have been someone
> Well so could anyone
> You took my dreams from me
> When I first found you
> I kept them with me babe
> I put them with my own
> Can't make it all alone
> I've built my dreams around you
>
> The boys of the NYPD choir
> Still singing "Galway Bay"
> And the bells are ringing out
> For Christmas day

10-01-04 *Disappointment* (to M. Pérez-Suárez)

About books, I brought Nielsen and Chuang with me and you are free to use it (and any of my books). I don't think I brought Peres with me, however. I will try to remember to check in my office Monday. As far as linear algebra, matrix analysis books go, I probably have anything you could need. For library use we have the Dublin Institute for Advanced Studies, about a 20 minute walk from our offices. They have a decent sized math/physics library. Also, I have use of the Trinity College mathematics library (quite large). I have a library of about 290 philosophy books (mostly about pragmatism, Wittgenstein, postmodern stuff, etc.) here in my own house. So, basically, just tell me about any books you were thinking of bringing, and I will tell you whether there is overlap with what you can borrow from any of these sources.

10-01-04 *Cohen Again* (to S. J. van Enk)

van Enkism 5: *I saw the Cohen paper you mention, I even read some part of it and decided it was nothing I didn't know. Moreover, I thought he missed something, but now I don't remember what that was. I'll check more carefully. I also remember vaguely we talked about this, and I thought we did have some essential difference between classical and quantum teleportation. Was it secrecy??*

I think maybe in the version we came up with, Alice could learn something about Victor's state (i.e., probability distribution). But in his version—where the 'Bell basis' measurement automatically randomizes first—he doesn't have that problem. Of course there could be the issue of a third party correlated with the 'entangled pair'—like in the version Collins and Popescu consider.

So, I'm kind of torn. Maybe it is too late for this paper. If it had just been written five years ago. (It is the sort of thing you and I should have written; but who would have guessed it could be turned into a whole PRL?) On the other hand, I like the way the guy makes a clean simple point of the lesson: quantum teleportation is simply not exciting or remarkable if one takes the epistemic view of quantum states. It's a point I want more people to appreciate—even if a few of us realized it long ago—and his paper could be a good vehicle for that.

Also there's the issue that Spekkens will soon release his toy model to the world. Buried within that far more ambitious work (it's a 65 page manuscript and counting) is a classical teleportation protocol. Should Cohen lose all credit, or all attention, in the blinding light of Spekkens's paper? Alternatively might I help bring attention to Spekkens's paper by letting Cohen into PRL at the same time as forcing him to cite Spekkens?

Cohen is not a power hitter. He's only had six papers since 1999. So I don't think I'm creating undue competition for us by letting a cheap paper past PRL, and I can see that it might actually help him hold onto a career he might otherwise be forced to drop out of.

I tell you, I turn more social democrat every day . . .

Anyway, all of this (especially Spekkens' toy model) is clarifying and food for thought. A good lot of quantum information theory is simply regular probability/information theory applied in ways that had not been deemed interesting before. It helps us realize that what is unique in quantum mechanics is not the probabilities but what the probabilities are applied to. There, I think, lies the essence of quantum mechanics: It is localized in the Kochen-Specker theorem. Unperformed measurements have no outcomes. It's just a question of putting that in a more useful form before something can really come of it.

Thanks for calling me a father. It helped a lot.

10-01-04 `quant-ph/0106166` (to G. Brassard)

Brassardisme 8: *Is* `quant-ph/0106166` *obsolete in the face of* `quant-ph/0205039`*?*

I think it is, and I try to sell it that way. But strangely, a lot of people choose to read it first nevertheless—thinking of it as a 'lite' version of `0205039`. Certainly the intros of the two papers have different jokes and allusions; maybe they're worth looking at separately because of that.

Still, it would be nice if you did mention `quant-ph/0106166` along with `quant-ph/0205039`, after all ... since its title coincides with the title of your talk.

More a little later this evening.

I realized I told you in one of my other notes this morning to keep breathing slowly. Don't breathe too slowly, though. That wouldn't be healthy ...

10-01-04 *More Help Please* (to G. Brassard)

Brassardisme 9: *But seriously, there is something else you can do to help. Please send me as much information as you can gather easily on the 3 meetings that took place so far, i.e. both here in Montréal and the one in Växjö.*

Four meetings so far, and at least two coming up. Though the fourth was really just a component in the larger Växjö meeting: This time, it was titled "Quantum Logic meets Quantum Information." And the idea was to get people together who had an interest in making a bridge between the two communities. So, one of the pre-requisites was that the participants from each community were supposed to have a little knowledge and sympathy of the other at the outset. ('Quantum Logic', in case you don't know, is a certain community in quantum foundations that thinks that what is going on behind quantum mechanics is literally a change of the rules of formal logic ... i.e., from Boolean algebra to something else.)

I'll compile the information for you, but I can't do it until about 8 hours from now.

11-01-04 *The First Montréal Meeting* (to G. Brassard)

Workshop on Quantum Foundations in the Light of Quantum Information

Centre de Recherches Mathématiques Université de Montrál

16 – 19 May 2000

Gilles Brassard and Christopher A. Fuchs, organizers

<u>17 May</u>

Gilles Brassard, Université de Montréal
Quantum Foundations in the Light of Quantum Cryptography

Christopher A. Fuchs, Los Alamos National Laboratory
Quantum Foundations in the Light of Quantum Information

William K. Wootters, Williams College
Quantum Mechanics from Distinguishability?

Benjamin W. Schumacher, Kenyon College
Doubting Everett

<u>18 May</u>

N. David Mermin, Cornell University
Pre-Gleason, Post-Peierls & Compumentarity

Herbert J. Bernstein, Hampshire College
Why Quantum Mechanics?

Richard Jozsa, University of Bristol

Foundations of an Interpretation of Quantum Mechanics in the Light of Quantum Computing

Charles H. Bennett, IBM Research at Yorktown Heights
Entanglement-Assisted Remote State Preparation

19 May

Rüdiger Schack, University of London – Royal Holloway
Quantum Gambling and Bayesian Probability in Quantum Mechanics

Patrick M. Hayden, Oxford University
Two Lessons from the Heisenberg Representation, or How I Learned to Stop Worrying & Love Non-Commutativity

Jeffrey Bub, University of Maryland
Some Reflections on Quantum Logic

11-01-04 *The Second Montréal Meeting* (to G. Brassard)

Be warned: Some of these titles are my own constructions! [See 10-09-02 note titled, "Tentative Talk Schedule for Commune."]

11-01-04 *The First Sweden Meeting* (to G. Brassard)

Officially titled:

Quantum Theory: Reconsideration of Foundations
Växjö University, Växjö, Sweden
17-21 June 2001
Organizers: Christopher A. Fuchs, Pekka Lahti, and Andrei Khrennikov.

however, a significant part of the meeting was a session titled:

"Shannon Meets Bohr: Quantum Foundations in the Light of Quantum Information"

The participants and talks in this session were:

1. Herbert J. Bernstein, "Observer Participancy and Law without Law (or something like that)"
2. Doug Bilodeau, "Real Time: How Quantum Mechanics Describes the World"
3. Jeffrey Bub, "Information and Objectivity in quantum Mechanics"
4. Carlton M. Caves, "Quantum Dynamics and Maximal Information"
5. Henry J. Folse, "Bohr's Conception of the Quantum Mechanical State of a System and Its Role in the Framework of Complementarity"
6. Christopher A. Fuchs, "Quantum Foundations in the Light of Quantum Information"
7. Daniel Greenberger, "The Inconsistency of the Galilean Transformation in Quantum Theory"
8. Lucien Hardy, "Quantum theory from Five Reasonable Axioms"

9. Peter Harremoës, “Bell’s Inequalities and Graphical Models of Independence”
10. Richard Jozsa, “Quantum Computation and Foundational issues”
11. N. David Mermin, “Whose Knowledge?”
12. Asher Peres, “Indiscrete Quantum Information”
13. Itamar Pitowsky, “Range Theorems for Quantum Probability”
14. Arkady Plotnitsky, “Spectro-Cryptography: Bohr, Heisenberg and Quantum Mechanics as an Information Theory”
15. Rüdiger Schack, “Bayesian Probability in Quantum Mechanics”
16. Ben Schumacher, “Doubting Everett”
17. John Smolin, “Quantum Nonlocality without Entanglement”
18. Joseph Renes, “(Bayesian) Probability in Quantum Mechanics”
19. Daniel Terno, “Quantum Information in Relativity Theory”

11-01-04 *The Second Sweden Meeting* (to G. Brassard)

Officially titled:

> **Quantum Theory: Reconsideration of Foundations – 2**
> Växjö University, Växjö, Sweden
> 1-6 June 2003
> Organizers: Howard Barnum, Christopher A. Fuchs, Robin Hudson, and Andrei Khrennikov

however, a significant part of the meeting was a session titled:

> “Quantum Logic Meets Quantum Information”

organized by Barnum and Fuchs. The participants and talks in this session were:

1. Scott Aaronson, “Quantum Computing and Dynamical Quantum Models”
2. Guido Bacciagaluppi, “Classical Extensions, Classical Representations and Bayesian Updating in Quantum Mechanics”
3. “Howard Barnum, Using Probabilistic Equivalence to Construct Algebraic Structures from Operational Theories”
4. “Stephen Bartlett, Restrictions to Quantum Information Processing”
5. Paul Busch, “Less (Precision) Is More (Information): Quantum Information in Fuzzy Probability Theory”
6. Bob Coecke, “Probability from Logic”

7. Christopher Fuchs, "What is the Difference between a Quantum Observer and a Weatherman?"

8. Alexei Grinbaum, "C. Rovelli's 'Relational Quantum Mechanics' and the Problem of Information Theoretic Derivation of Quantum Theory"

9. Hans Halvorson, "Tomita's Theorem and its Role in the Information-Theoretic Characterization of Quantum Theory"

10. Lucien Hardy, "Quantum Theory as a Probability Theory"

11. Piero Mana, "Why Can States and Measurement Outcomes Be Represented as Vectors?"

12. Marcos Pérez-Suárez, "Quantum Mechanics and Information Theory: Some Further Musings on a Fuchsian Proposal"

13. Rüdiger Schack, "On Unknown Quantum Operations"

14. John Smolin, "Can Quantum Cryptography Imply Quantum Mechanics?"

15. Rob Spekkens, "In Defense of the Epistemic View of Quantum States"

16. Alexander Wilce, "Compactness and Symmetry in Quantum Logic"

11-01-04 *Hardy Facts* (to G. Brassard)

By the way, a little factoid about Hardy. You might recall that he was invited to our first Montréal meeting but had to withdraw at the last minute because of an illness in his family or something. Anyway, his paper "Quantum Theory from Five Simple Axioms" was motivated directly—he tells me—by his invitation to our meeting.

More still to come, but I've got to get the kids to bed at the moment.

11-01-04 *Special Issue of SHPMP* (to G. Brassard)

Continuing. The September 2003 issue of the journal *Studies in History and Philosophy of Modern Physics* was devoted to "Quantum Information and Computation". The editors of the special issue were Jeff Bub and me. Below is a tabulation of the papers, and further below that is the introduction we wrote for the issue. N.B. there was a horribly serious omission in that introduction; pathetically we forgot to mention David Mermin's paper. I am still atoning for that sin!

1. Howard Barnum: 'Quantum information processing, operational quantum logic, convexity, and the foundations of physics'

2. Charles Bennett: 'Notes on Landauer's principle, reversible computation and Maxwell's demon'

3. Armond Duwell: 'Quantum information does not exist'

4. Lucien Hardy: 'Probability theories in general and quantum theory in particular'

5. David Mermin: 'Copenhagen computation'

6. Itamar Pitowsky: 'Betting on the outcomes of measurements: a Bayesian theory of quantum probability'
7. Andrew Steane: 'A quantum computer only needs one universe'
8. Chris Timpson: 'On the supposed conceptual inadequacy of the Shannon information'
9. David Wallace: 'Quantum probability and decision theory revisited'

12-01-04 *Upcoming Conferences* (to G. Brassard)

I'm sure this is only a partial list, but it includes all the ones that I am confident will carry our theme.

- Probability in Quantum Mechanics, Centre for Philosophy of Natural and Social Science London School of Economics and Political Science, Monday, February 16, 2004, Organizers: Stephan Hartmann and Roman Frigg
- Seven Pines Symposium VIII: "Quantum Mechanics, Quantum Information, and Quantum Computation," University of Minnesota, May 5–9, 2004, Organizer: Roger H. Stuewer
- Philosophy of Science Association, annual meeting 2004 (this is the biggest philosophy of science meeting) Austin, Texas, November 18–21, 2004 within it there will be a workshop titled 'Quantum Information Theory and the Foundations of Quantum Mechanics: Is Information the Way Forward?,' Organizer: Christopher G. Timpson
- This meeting should also have a significant component on quantum foundations in the light of quantum information: New Directions in the Foundations of Physics, American Institute of Physics, College Park, April 30 – May 2, 2004, Organizer: Jeff Bub

12-01-04 *Miscellania* (to G. Brassard)

Brassardisme 10: *Your polite silence on what you think of my slides worries me!*

Silent only because there wasn't much to say yet, and believe it or not, I am a busy man too. I like Michelangelo. And though I can imagine that the burning bush may have laid out "I am that I am" as fundamental principle (in constructing the universe), I agree with you that it is hard to imagine it declaring C^* algebras in the same breath.

12-01-04 *Diode Lasers* (to P. G. L. Mana)

Thanks for the final draft; I've forwarded it to Andrei Khrennikov.

Manalogue 7: *But I realise now, that I have not understood completely your "philosophy": roughly asking, do you consider a diode laser a sense impression? Do you prefer to speak of sense impressions rather than "(macroscopic) reality"?*

I consider a "diode laser" as very much akin to (or maybe even precisely) a prior. (As in, the kind of prior a Bayesian would use as the starting point of his probabilistic calculations.)

Manalogue 8: *What do you mean by 'the world'? Your position seems almost Leibnizian.*

I don't know what Leibnizian means. What I mean by 'the world' is everything external to the agent: the source of all the stimulations upon him.

Manalogue 9: *Apart from some possible differences in basic "philosophical" points of view (which concern something broader than quantum mechanics), ... and the fact that you believe in the ultimate validity of the quantum-mechanical formalism [...]*

I wouldn't say I believe the latter at all.

In any case, I'll attach a note that expresses aspects of my latest position, such as it is.

13-01-04 *Decisions* (to S. J. van Enk)

van Enkism 6: *I just read Cohen's paper, and I don't know what I found wrong about it before: I now agree! The only difference with quantum teleportation is, which I mentioned before, you could use quantum teleportation for key distribution, and you cannot use classical teleportation for that purpose. Of course, QKD without teleportation is perfectly possible too, so that argument doesn't really discredit Cohen's protocol.*

If you don't disagree harshly then, I guess I'm going to recommend that it be published (with changes), despite the existence of

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quant-ph/9906105 [abs, ps, pdf, other] :
Title:  Classical Teleportation of a Quantum Bit
Authors:  N. J. Cerf (1), N. Gisin (2), S. Massar
Comments:  4 pages, RevTex
Journal-ref:  Phys.Rev.Lett.  84 (2000) 2521

quant-ph/0107082 [abs, ps, pdf, other] :
Title:  A classical analogue of entanglement
Authors:  Daniel Collins, Sandu Popescu
Comments:  13 pages, references updated
```

and Spekkens' new paper. My support, as you know, is chiefly motivated by the moral he draws, which neither of the first two papers above seem compelled to.

I suspect I'm the only referee, because it was referred to me by "a Divisional Associate Editor" (had to be Bennett?). So it seems to hinge on me (and by proxy, if there is a lack of protest, you).

14-01-04 *Hello Kitty* (to H. Mabuchi)

I've been meaning to write you for a couple of days. Emma turned five on January 12, and in her pile of presents was a Hello Kitty watch, wrapped with a tag listing "Uncle Hideo" as the source. Emma asked, "Who's that?" Later I showed her a picture of you, and she said "Wow." (She says wow a lot.) Anyway, Kiki and I thank you! Emma was proud to sport her new watch along with her new dress, bought especially for the occasion of her date with her dad later that evening.

On less happy matters, you've probably heard by now that the Perimeter Institute decided I was too risky a proposition for their world fame and dominance. It seems that hidden-variables theories (or even sillier things) will carry the day.

You know my secret dream (and you know I've got a thousand secret dreams): That someone of unique talent at Caltech would convince their physics and humanities departments to throw in half and half on me. The possibilities! Courses on Bayesian foundations for probability theory, courses fleshing out comparisons and contrasts between Bayesianism and pragmatism, courses on Wittgenstein and the foundations of quantum mechanics, courses on James and Dewey and the foundations of science, courses on why we should take some of the postmodern thinkers seriously, courses connecting the interpretational issues for quantum states to the philosophy of language, courses on beginning quantum mechanics for undergraduates, courses on standard quantum information theory, courses on matrix analysis methods in quantum information, ... What possibilities! And what better place for such possibilities? ... Pipe dreams.

Actually Wittgenstein has become my latest addiction. In preparation for writing my new paper with Schack—one that we've been tentatively calling "On Quantum Certainty"—I thought I should read Wittgenstein's book "On Certainty" to see if, on the off chance, there was some overlap of ideas. I had some indication that there might be from things I had read in Richard Rorty. Well, what a pleasant surprise! Wittgenstein is much deeper than I ever imagined. It was just silly of me to be put off for so long by the style in which his books are written. Within the first five pages of that 90 page book, he already said all that Rüdiger and I ever wanted to say and just kept going. Here is a man who, 52 years ago, could have tackled the Penrose question head on: How a pure quantum state can be epistemic and yet still give probability one predictions for some measurements? Anyway, reading Wittgenstein is certainly taking me down a more solid and careful path on the issue than I thought I was prepared for.

And speaking of the fancy term "epistemic", I hope you've noticed that Rob Spekkens finally put his toy-model paper on `quant-ph`. It is a masterwork. You want to teach your first-year students what quantum mechanics is all about? Make this paper part of the curriculum. It is that simple, and it is that convincing. (That is, at least it gets half the answer. We wouldn't want them to think there's nothing left to do.)

OK, now I'm just procrastinating. Too many more official things to write, and too little desire to do it; email and gossip are always easier.

I hope things are going well for you and your family of 17. And many thanks again—this time from Emma—for the watch.

15-01-04 *Idealism* (to R. W. Spekkens)

I'm planning to write you two notes, one titled "Pragmatism" and one titled "Idealism." The one titled "Pragmatism" has to do with what I think I need for self-preservation at the moment, and not to do with my philosophy. The one titled "Idealism" has to do with my philosophy, which is pragmatism, but has nothing to do with my career.

On page 2 you write, "There is one case wherein the distinction between an ontic state and an epistemic state breaks down ... " You know I still disagree with you: epistemic is always epistemic, period. If I could just get the discipline to finish my paper with Schack, "On Quantum Certainty," maybe we could communicate better. In the mean time—though I doubt it will help—let me try to entice your soul not with my words, but Wittgenstein's. Here's something I wrote Mabuchi yesterday:

> Actually Wittgenstein has become my latest addiction. In preparation for writing

> my new paper with Schack—one that we've been tentatively calling "On Quantum Certainty"—I thought I should read Wittgenstein's book *On Certainty* to see if, on the off chance, there was some overlap of ideas. I had some indication that there might be from things I had read in Richard Rorty. Well, what a pleasant surprise! Wittgenstein is much deeper than I ever imagined. It was just silly of me to be put off for so long by the style in which his books are written. Within the first five pages of that 90 page book, he already said all that Rüdiger and I ever wanted to say and just kept going. Here is a man who, 52 years ago, could have tackled the Penrose question head on: How a pure quantum state can be epistemic and yet still give probability one predictions for some measurements? Anyway, reading Wittgenstein is certainly taking me down a more solid and careful path on the issue than I thought I was prepared for.

Here are a couple of paragraphs from *On Certainty* that I had the word "Spekkens" written by:

> 21. Moore's view really comes down to this: the concept 'know' is analogous to the concepts 'believe', 'surmise', 'doubt', 'be convinced' in that the statement "I know ..." can't be a mistake. And if that *is* so, then there can be an inference from such an utterance to the truth of an assertion. And here the form "I thought I knew" is being overlooked.—But if this latter is inadmissible, then a mistake in the *assertion* must be logically impossible too. And anyone who is acquainted with the language-game must realize this—an assurance from a reliable man that he *knows* cannot contribute anything.
> 22. It would surely be remarkable if we had to believe the reliable person who says "I can't be wrong"; or who says "I am not wrong."

There were many more than that, but I'm too lazy to copy them in at the moment!

I'd say that a Wittgenstein reading is hardly a substitute for reading Fuchs and Schack, but who knows, maybe his mortars will soften the front.

16-01-04 *Also Holevo* (to R. W. Spekkens)

I forget to mention in my previous notes the business about Chapter 1 in Holevo's book. You didn't mention the connection between that and your toy model in your paper. It's certainly of the same flavor, but it is hard (for me at least) to tell from his general theorem (7.1) whether one would get all the sorts of quantum behavior from it that you do. Maybe it's obvious though ... (And I'm starting to think it is as I write this sentence to you.)

16-01-04 *More Diodes* (to P. G. L. Mana)

Manalogue 10 :

> [CAF wrote:] I consider a "diode laser" as very much akin to (or maybe even precisely) a prior. (As in, the kind of prior a Bayesian would use as the starting point of his probabilistic calculations.)

Now I must really say that I'm confused.

I mean that within the framework of quantum mechanics "diode laser" is a name for a particular quantum operation (trace-preserving completely positive map) on the electromagnetic field. And from my point of view quantum operations are as sinfully subjective as quantum states. Their giveness just happens to be the starting point of most quantum mechanical problems, but it's nothing more than that. So, in that role, quantum operations play the role that the prior (or "statistical model") plays in most textbook problems in a probability course.

Most of my samizdat *Quantum States: What the Hell Are They?* (posted on my webpage) is devoted to this issue, but maybe pick up at the 19 September 2001 note "Practical Art" and then go to the 20 September 2001 note "Pots, Kettles, and Frying Pans." And then some of the subsequent notes. Maybe you'll find some of your wanted examples there. The phraseology I used with you came up when Caves and Schack and I were discussing those passages later in Australia. Caves said, "I call a beamsplitter an objective fact." I said, "I call it a prior."

Manalogue 11: *But on the following do we perhaps agree again: this not-quite-identified-yet something that makes the restriction – <u>this</u> must be the physical point, the trace – and hopefully trail – left by something we may call 'reality'!*

Yes, I suspect I agree with that.

19-01-04 *Shipbuilding, Part III* (to G. Brassard)

Brassardisme 11: *Wow! Had you ever mentioned this wild idea to me? This is getting more exciting by the minute.*

Actually, no I hadn't. But here's a slogan to go with it: I told it to Howard Burton when I was visiting PI. (So if you ever hear it re-surface, you'll know where it came from.)

> About some piece of matter:
>
> Question: How sensitive do you thing we can make this thing to eavesdropping in a quantum crypto protocol? What is its ultimate limit?
>
> Answer: I don't know; let's weigh it.

(To be taken somewhat poetically, not completely accurately.)

19-01-04 *NBI* (to G. Brassard)

Brassardisme 12: *But (I repeat) had you told me about an interview at the Bohr Institute? When? What? Why? (And please don't tell me that the B.I. is in fact at Caltech!)*

The Niels Bohr Institute is in Copenhagen. It was literally the birthplace of quantum mechanics: Heisenberg and Pauli were Bohr's postdocs at the time.

And, yes I told you about the application long ago. But I hadn't told you that it got to the stage of an interview.

There. That exhausts all my known interview information!

20-01-04 *Caltech and Peirce* (to A. Shimony)

Maybe in partial payment I should tell you that in the last couple of evenings I've been reading a fun piece by Susan Haack called "'We Pragmatists . . . ': Peirce and Rorty in Conversation".

It's a fictitious conversation between the two men built out of quotes from their various works. It makes it quite clear how sly Rorty is being when he continually invokes the phrase 'We pragmatists ...'. It'd be nice if someone were to put together a similar dialogue between Rorty and Dewey. I sometimes wonder if the two of them are as close to each other as Rorty portrays.

21-01-04 *The Ambiguity of Language* (to R. W. Spekkens)

Spekkensism 24: *Here is my response* [to 15-01-04 note "Idealism"]. *Physical theories involve idealizations, like fields upon which there is no back-reaction, point particles, isolated systems, etc. Quantum states are idealizations in the same sense. Quantum theory is a model of a world wherein observers do have certainty, even though our world may not admit such an idealization. You must admit something like this occurs when it comes to quantum state assignments that would, ideally, be adopted by an agent, but in practice are not because they involve solving some computationally difficult problem.*

We seem to be reading two very different things. Let's hold off discussion until Rüdiger and I write something that'll wallop you over the head.

21-01-04 *WJ for T?* (to H. Mabuchi)

Mabuchism 2: *What's your home address these days? I wanna send you a book!*

What could be an encore to a Hello Kitty watch? Did you manage to get a copy of *William James for Toddlers*?!?

More on other matters before the day is out ...

22-01-04 *Generalized Measurements* (to many)

Dear friends in the quantum information and foundations communities,

Many of you may not know it, but the concept of a generalized quantum measurement or positive-operator-valued measure was introduced in

- E. B. Davies and J. T. Lewis, "An Operational Approach to Quantum Probability," Communications in Mathematical Physics **17**, 239–260 (1970).

Last night, John Lewis passed away here in Dublin, his home of 28 years, from complications due to a recent surgery. He was a good man, honest and upright, and left us a deep legacy. He will be missed.

22-01-04 *Bekenstein, Bohr, and Bohr* (to A. Peres)

Your meeting sounds like it is going to be a great one. I would be particularly keen to get a report on Jacob Bekenstein's talk.

Asherism 41: *I am slowly browsing throught your book "Paulian Idea" in the evening, when I am too tired to do anything productive, and I have until now two minor remarks. The first use of entanglement was not by Schrödinger in 1935, but by Heitler and London, Zeits. Phys.* **44** *(1927) 455.*

Thank you for letting me know that. I hadn't heard of that paper before.

Asherism 42: *Bohr's key idea was that "classical" and "quantum" labeled ways of description, not properties of systems. Is this a Bayesian idea?*

Not properly, no. But what it does have in common with Bayesianism is that the quantum state (much like a probability distribution) is not a property of a system.

Asherism 43: *BTW, don't forget "my" Niels Bohr Medal (it's a medal, not a prize).*

I certainly will not! I had already searched on the web for information about nominations, but was not able to find it. I will be in Copenhagen Feb 2 however for my job interview, and I was planning to ask Ole Ulfbeck and Eugene Polzik anything they knew then. Do you have any leads on what the official procedure is? (If I can figure that much out, I can organize the behind the scenes thing myself.)

23-01-04 *Entropy?* (to P. Grangier)

Sorry to take a while to reply to you. I'm on my way to Texas as I am writing you this note. Unfortunately (or maybe fortunately) I don't have their paper here to look at, so I have to focus on the parts you've given me.

Grangierisme 19: *I wanted to know your opinion about a recent preprint (*`quant-ph/0401021`*) where the authors write on p1:*

> For a pure quantum mechanical state Ψ the Von-Neumann definition gives $S_H[\Psi] = 0$. This seems to imply that a pure quantum mechanical state is lacking a statistical nature. This is of course not correct. For a general measurement we have uncertainty.

There are many ways to define the von Neumann entropy of a density operator, and not simply by the raw formula. My favorite comes from Wehrl (I believe): the von Neumann entropy is defined to be the minimum Shannon entropy for the measurement outcomes of a fine-grained von Neumann measurement, where the minimum is taken over all such measurements. (See pages 31 and 32, starting at Eq. (77), of my `quant-ph/0205039` to see a little discussion of this and to see it formalized.)

The von Neumann entropy of a pure state being zero does not mean that the pure state has no statistical character. It only means that for some measurement it will give an outcome with certainty. For the vast majority of other measurements it will give uncertainty. (For the uncertainty on average, see Eq. (80) in my paper; it was a number first calculated by Bill Wootters.)

Grangierisme 20: *Then they define an "uncertainty-related" entropy, which is non-zero for a pure state.*

What do you think about such an approach? Clearly it has the major disadvantage of being in plain contradiction with the so-called "third principle of thermodynamics", stipulating that (in most cases) the statistical entropy is zero at zero temperature. Even worse, I wonder whether it might be self-contradictory in some sense (that I was not able to pin down), or at least contradictory with various points in statistical physics. Your opinion would be appreciated.

They can make any measure they want, say something like the Eq. (80) I've already referred to. But then it won't correspond to thermodynamic entropy. You're quite right. That number controls the free energy of a system—i.e., the energy that can actually be extracted once it is stored there—and there are already plenty of good arguments that it is the von Neumann entropy (going all the way back to von Neumann's book itself, but also detailed in Lindblad's book, and some of Jaynes's papers, etc.). If you have stored energy in a system, and you have maximal knowledge of it—i.e., a pure state for it—then you can use that knowledge to re-extract the energy you've stored there. That's what the von Neumann entropy being zero is about.

Now if one were to make a restriction on what operations can be done on a system, or a restriction on what measurements can be done on a system, that would change the flavor of everything. But I doubt they've done anything like that. It sounds to me like they're just confused.

Suggest that they read Wehrl's paper.

23-01-04 *Flying to Texas* (to N. D. Mermin)

I'm flying to Texas as I write this to you. I'm popping in for the weekend for my Mom's 75th birthday. I start heading back Monday morning. The following Sunday I'm off for a visit to the Niels Bohr Institute.

Mostly I'm working on a proposal in the background, but I'll stop for a minute to have some conversation with you.

Merminition 136: *Somewhere in Jaynes's book (but I haven't been able to find it again) he says that to assign something probability zero is, by Bayes's theorem, to commit yourself to never updating the assignment of zero on the basis of any new information. This, of course, would be bad news for your position.*

I don't understand why you are thinking it would be 'bad news'. In fact for our understanding it is actually good news, for it tells you how opinions actually change: By Darwinism. As de Finetti points out pretty strongly in his Probabilismo, applications of Bayes' rule are not changes of opinion at all, but rather following one's initial opinion through to its consequences.

In terms of a Dutch-book scenario, if one has certainty for some event, then one is willing to bet one's life savings on it. If that event then does not occur, one has lost everything: The whims of Darwinian evolution have taken one out. And one's beliefs then do not propagate.

Merminition 137: *But I believe what he has in mind is this:*

$$p(a|bx) = \frac{p(ab|x)}{p(b|x)} \leq \frac{p(a|x)}{p(b|x)}.$$

So if $p(a|x) = 0$, then $p(a|bx) = 0$ too, for any b.

What he appears to overlook is that this is only so if $p(b|x) \neq 0$. The correct statement should to be that if you assign something probability zero, then only the occurrence of something else to which you assign probability zero can lead you to update the first probability to a non-zero value (assuming you are behaving rationally — i.e. following the laws of thought.)

I was reminded of this when I read in paragraph 69 of "On Certainty":

> *... If I am wrong about this, I have no guarantee that anything I say is true ...*

In the context of the other paragraphs surrounding Wittgenstein's 69, Wittgenstein's point granted. But I don't understand your 'passing thought'. You can try again, but I think my point above already negates it: two deaths don't make a life.

Merminition 138: [Regarding Spekkens.] *Doesn't the fact that* [his toy model] *has both epistemic AND ontic states make it uninteresting?*

I think you're missing something big here. It is interesting precisely because he has ontic states. What his work helps argue is that so many of these effects we find interesting in quantum information—like teleportation, superdense coding, 'nonlocality without entanglement', the no-broadcasting theorem, entanglement monogamy, etc., etc.—come about *solely* from the epistemic nature of quantum states (including the less than full use of the probability simplex, i.e., his epistemic restriction, or knowledge-balance principle). And in that light they are not nearly as interesting as we once thought they were.

It's a question of refocusing yet again. What his work shows us is that the interesting questions lie elsewhere. In particular, it teaches us that the crucial question must be, 'Information About What?' A quantum state is epistemic alright—all these examples are meant to help you believe that—but epistemic about what? That is the great unanswered question. Of course, I myself say, epistemic about the outcomes of other interventions. But what is left to do is to make that sentence precise enough that, from it, we can rederive the structure of quantum theory. It is about formalizing THE Paulian Idea—that is where he has taught us to refocus. (Though he would likely disavow much that I've said, being one who deeply longs for nonlocal hidden variables or some such.)

Here's another way to put it. Does THE Paulian Idea (i.e., that the agent cannot be detached from the phenomena he helps bring about) find any crucial expression in the phenomena of teleportation, superdense coding, 'nonlocality without entanglement', the no-broadcasting theorem, entanglement monogamy, etc., etc.? Probably not. All those phenomena seem to have more to do with the epistemic nature of quantum states.

Instead, the Paulian Idea may well be localized solely in Kochen-Specker phenomena. (Which is something itself longing to be reexpressed in Bayesian terms, but I need to write you a separate note about that one day.)

I'll paste some related thoughts from a letter to Oliver Cohen below. [See 15-01-03 note "Memory Lane" to O. Cohen.]

Does any of this help you see why I am excited by Rob's work?

23-01-04 *The Wittgenstein Bug* (to R. Schack)

What a lovely Wittgensteinian reply to my little (and of course exaggerated) email! If you had only numbered the paragraphs …

I'm stuck in a snowy Chicago airport at the moment, trying to make my way to Texas. And I'm not thinking particularly clearly any more. I'll try to say something sensible of your email next week.

Rüdiger's Preply

> I own Wittgenstein's complete works (in German) and I had read a lot of it in my undergraduate days. Your email made me reread quite a sample of "On Certainty". Is your edition split in 676 paragraphs as mine is?
>
> My summary of the book: There are many intelligible ways of using the phrase '*I know x*', but Moore's is not one of them.
>
> The book may be relevant for our program by helping us clarify the differences between the following statements:

I know that $2 + 2 = 4$.
I know that $12 \times 12 = 144$.
I know that this is a chair.
I know that the earth existed 100 years ago.
I know that the ground state of the hydrogen atom is $|\psi\rangle$.
I know that the state of this hydrogen atom is $|\psi\rangle$.
I know that the outcome of this measurement will be 0.
I know that this is a beam splitter.

But then, it might not be relevant for our program. See par. 8, which states that in most contexts there is no difference between '*I know that x*' and '*I am certain that x*'. Here is another one-sentence summary of his book: '*I know that x*' is either meaningless or synonymous with '*I am certain that x*'. For our own program, we have the following:

> '*I know that the state is* $|\mathrm{up}\rangle$ *and the* Z *component of spin is measured*' is a reformulation of '*My state of belief is* $|\mathrm{up}\rangle$ *and* Z'. This reformulation is philosophically fraught and meaningless, exactly as the sentence '*I know this is a tree*' in Wittgenstein's text.
>
> '*I know the outcome will be up*' can be viewed as a careless way of saying '*I am certain that the outcome will be up*' which in turn is equivalent to saying '*I am willing to bet a lot of money on the outcome up*'.

I am rather puzzled by your sentence

> Within the first five pages of that 90 page book, he already said all that Rüdiger and I ever wanted to say and just kept going.

and the follow-up

> Here is a man who, 52 years ago, could have tackled the Penrose question head on: How a pure quantum state can be epistemic and yet still give probability one predictions for some measurements?

I am not sure he said all we ever wanted to say about certainty. He might even have doubted that the Penrose question as stated by you has any meaning. Wittgenstein says

(i) You can be certain about something ('*the earth was here 200 years ago*') without being able to prove it. To doubt the thing would mean leaving the boundaries of the Sprachspiel (language game?).

(ii) The sentence '*I know that this is a tree*' is usually meaningless. It is only meaningful if there could exist reasonable doubt about this being a tree (for instance, one biology student could use the sentence to try to convince another student that the plant in question is a tree and not a shrub).

(iii) The sentence '*I know that this is a tree*' does not imply that this is a tree. It is not a sentence about the tree, but about me.

Is (i) relevant to the Penrose question? Not directly, I think.

How about (ii)? Maybe (ii) is relevant in the sense that the distinction epistemic versus ontic is meaningless. You know (unobjectionable usage!) that I want to replace that distinction by the pair subjective versus nonsubjective.

Is (iii) relevant? No. Nobody claims that '*Mabuchi is certain that the outcome is 0*' implies that the outcome is 0. Mabuchi could be wrong.

What I think is that Wittgenstein is trying to tackle a different question.

Here is another attempt:

(A) I know the outcome is 0.

(B) I know the outcome will be 0.

Wittgenstein would have no objections at all to (B), I believe. But (A) is tricky. It could be that all I want to say is that the outcome is 0. In that case I should just say that. On the other hand, if the outcome is in plain view and everybody can see that it is 0, then uttering (A) should make people doubt my sanity.

For us, (B) is the tricky one.

Reading Wittgenstein certainly stimulates thought. Here is yet another version of our quantum question: How can we assign a pure state and at the same time grant that somebody else may legitimately assign another pure state? Or the extreme case: How can we assign a pure state and at the same time grant that somebody else may legitimately assign an orthogonal state? Translated into Wittgenstein's terms: How can I know x and at the same time grant that somebody else may know the negation of x? Wouldn't that mean that we play in different Sprachspiele? Which brings us back to my old and yet unanswered challenge to you: Give a satisfactory account of a crisis in a quantum measurement scenario.

I hope that some of all this makes sense. In any case, I KNOW that we are on the right track!

23-01-04 *I Married a Teenage de Finetti?* (to G. Bacciagaluppi)

Attack of the Giant de Finetti
The Bride of de Finetti
The Fearless de Finetti Killers (or Excuse Me but Your Dutch Book Is In My Neck) — produced and directed by Roman Polanski, introducing Sharon Tate
Plan de Finetti from Outer Space
Abbott and Costello Meet de Finetti
The de Finetti Rises
Attack of the Killer de Finettis
The Bride of de Finetti Returns!
The de Finetti of London
de de de Finetti: de Finetti 3D! (Just when you thought it was safe to read David Lewis again.)

28-01-04 *Merminizing* (to N. D. Mermin)

Just wondering how you're doing? Did you get my note in reply to your 'passing thoughts'? I seem to have had some email problems, with people not getting my messages again lately. I hope you weren't one of them.

My own favorite paragraph from *On Certainty* comes from #30:

> Certainty is as it were a tone of voice in which one declares how things are, but one does not infer from the tone of voice that one is justified.

31-01-04 *Just the Opposite* (to N. D. Mermin)

Merminition 139: *I read in today's NY Times:*

> *Mr. Bush no longer declares, as he once did, that he is certain that sooner or later unconventional weapons will be found in Iraq.*

Can Ludwig get him honorably off the hook?

It's just the opposite! That was the part of the point of my last long note to you. For the Bayesian, the only way to back away from certainty is death. I.e., it's Darwin to the rescue!

01-02-04 *Wilczek, Einstein, and Bohr* (to G. Brassard)

Brassardisme 13: *And do you feel that Copenhagen and Bohr's ghost are receptive?*

I was once at a conference where someone asked Frank Wilczek what it was like to live in Einstein's old house in Princeton. The fellow asked, "Have you ever felt Einstein's ghost?" Wilczek answered, "No, but every morning I wake up a little more dubious of quantum mechanics."

I'm going to bed now. I'll tell you if Bohr enters my dreams.

02-02-04 *Many Thanks* (to A. Shimony)

At the moment, I'm in Copenhagen, just finishing a visit to the Niels Bohr Institute. I think the talk only went so-so. But I did get to see Niels Bohr's office and desk! That was quite nice.

For my leisure reading, I brought Donald Davidson's book *Subjective, Intersubjective, Objective*. I'm not finding him nearly as hard to read as everyone has been warning me: It makes me worry that I'm missing something.

03-02-04 *The Land of Bohr* (to G. L. Comer)

I'm writing to you from Kastrup airport, just as I'm leaving the land of Bohr. Yesterday I walked into Bohr's old office and had a look at his desk. Thinking back on the moment, how I wish I had sat at it, even if only briefly. I could have; there was nothing to stop me. But it didn't dawn on me at the moment that there was an experience passing me by. It reminds me of a couple of lines in The Pogues' song, "Fairy Tale of New York." The man laments "I could have been someone," to which the woman cries out in reply, "Well so could anyone!"

When I visited you in Meudon that year [1992], I went into the Cathedral of Notre Dame and tried to imagine Napoleon taking the crown away from the Pope at the last minute of his coronation. "I crown myself Emperor," he said.

I hope I won't forget the lesson so quickly this time around.

03-02-04 *Napoleonic History* (to D. B. L. Baker)

Hey historian,

Can you point me toward a book where there's a good, vivid description of Napoleon crowning himself emperor?

Thanks (if you can),

05-02-04 *Facts, Values and Quanta* (to D. M. Appleby)

Wow! Thank you for the color-full and value-laden paper. I'm just printing it out now. I'll try to respond to you as soon as possible.

Will you be at the LSE meeting Monday February 16?

Most of the stuff in my talk you will have heard before (over and over and over), but there is one point that is new (and I think noteworthy). Plus there is a line-up of several other good-looking talks. It'd be great to see you there.

BTW, regardless of content, I love your title!

05-02-04 *Explain* (to H. Mabuchi)

Home from Copenhagen, and found a package waiting for me: It's the book by Richard Powers you sent me. Thanks! Now explain. Why did you send it? Because it's a good novel, and you know that I haven't read any fiction in a while? Or did you have something more specific in mind?

Hideo's Reply

> As for the book: it's a novel — read it! I suspect you'll sympathize with one of the characters. I declare it to be the best book I've ever read.

06-02-04 *Replies to Your Comments* (to N. D. Mermin)

Here goes something of a reply.

Merminition 140:

> [CAF wrote:] This is where quantum information (including the collateral fields of quantum cryptography, computing, and communication theory) has a unique role to play. Its tasks and protocols naturally isolate the parts of quantum theory that should be given the most foundational scrutiny. "Is such and such effect due simply to a quantum state being a state of information rather than a state of nature, or is it due to the deeper issue of what the information is about?"

Is superconductivity (a famous quantum effect) a state of nature or a state of information or is it due to .... I'm not trying to be nasty. Just suggesting thoughts that may occur to some readers.

That is precisely the sort of thing that I am asking that we examine—though I don't quite know where superconductivity fits into the quantum-informational classification scheme yet. How can I be faulted for asking the same question that your straw man is?

Merminition 141:

> [CAF wrote:] Recent investigations by several workers are starting to show that a plethora ...

Have even you succumbed to the use of "plethora" (which means excess, too much, etc.) simply to mean lots and lots without any negative connotations? Then the battle has been lost.

You forget that I did not know English before I started writing email in 1991, and even then I did not get a real boost in my grammar until I started corresponding with you in 1996. It is an ongoing process with me, and you have taught me the meaning of plethora. It's not a lost battle; thanks.

Merminition 142 :

> [CAF wrote:] On the other hand, other phenomena, such as the potential computational speed-up of quantum computing, seem to come from a more physical source: In particular, the answer to the question, "Information about what?"

Quantum computational speedup comes from the answer to the question, "Information about what?" Do you really mean that?

Yep, I mean that. What is the answer to "Information about what?"? I like to say, "the consequences of our interventions into nature." But that needs to be tightened up into a more formal statement. It has its base in Kochen-Specker, but I don't know how to carry the thought any further than that at the moment. Let me reiterate the point I made to you about Spekkens the other day: [See note titled "Flying to Texas," dated 23 January 2004.]

Merminition 143 :

> [CAF wrote:] When we finally delineate an answer to this physics will reach a profound juncture. We will for the first time see the exact nature of 'quantum reality' ...

By putting the term in quotes I assume you mean we will finally understand what the term means. But it can also be read as saying that there is a quantum reality that we will understand. Not sure what you want the reader to take from this.

Maybe both ideas.

Merminition 144 :

> [CAF wrote:] Trickle-down effects could be the solution to the black-hole information paradox and even the meshing together of quantum theory and gravitational physics—some of this can already be seen in broad outline.

Really!? Would you care to expand on this?

I was a little more careful in my formulation this time around. The 'broad outline' part really only attaches to the black-hole information paradox business. Who would have thought that it is a QM-foundational paradox on the order of all the usual ones, but I think that's all it is. In non-Bayesian approaches to quantum mechanics, when one has a mixed state, one usually gets all fuddled up either, a) trying to identify the REAL pure state underneath the mixed state, or b) trying to seek a purification of the state that is the REAL state of some bipartite system. From the Bayesian point of view that is a fruitless exercise: There is no demand that a mixed state be derivable from any REAL pure state. All those ancillas, environments, and purifications are generally factitious.

Similarly for the Bayesian point of view of quantum operations, à la Fuchs and Schack (but maybe not yet Caves). There is no demand that a proper trace-preserving quantum operation be derivable from some REAL unitary operation on a larger system. And that is all I think is going on with the black-hole information paradox. They can't find any natural bipartite system to pin a unitary dynamics on. But so what? It, like the EPR paradox, is a pseudo-problem. It can be good for clarifying concepts, but it is not a feature of nature.

Merminition 145 :

> [CAF wrote:] Since the beginning of quantum theory, much of what the enthusiasts have called 'foundational work' has been pseudoscience pure and simple. But the field can be made as respectable as quantum theory itself, if done right. Quantum information is the technique for the task.

Why get on the defensive? You're the one who disparaged foundational work. Why should these guys share this negative view?

Just pounding on the idea that my program is quantum foundations with that little something extra (i.e., legitimacy).

Anyway, why draw attention to it? Look at one question I got in Copenhagen the other day. A fellow said, "This is all very nice for 'Sunday physics,' but what do you do for the rest of the week?" I looked him straight in the eye and told him that this was the same kind of physics that went into my calculations for Kimble's teleportation experiment. Apparently he needed to be told that.

People have to get it in their heads that this is serious physics, as serious as anything else in the now vast edifice of quantum information and computing. They're drawing an imaginary line.

Merminition 146 :

> [CAF wrote:] With regard to quantum mechanics then, the Bayesian view of probability combined with Gleason's theorem on Hilbert-space measures leads ineluctably to the idea that a quantum state is a collection of gambling commitments and nothing more.

Ouch! You've been trying for 5 years to bring me around to this point of view and I'm still not there. Try to tell a solid state physicist that the BCS ground state of a superconductor is a set of gambling commitments. She'll kick you out of her office. Try to tell a chemist that a chemical bond is a gambling commitment. Who is your audience?

Position statement. But your point is well taken. Unfortunately, I can't help it that a probability distribution is not a solid object (as we've all been led to believe from our early educations), but I can't lie about it either. That point is the core of my research program. What one sentence could I write that would either a) soften the blow, or b) make it all seem more reasonable? One sentence alone?

Merminition 147 :

> [CAF wrote:] What is already clear enough, nonetheless, is that from a Bayesian approach the formal structure of quantum theory represents not so much physical reality itself, but rather a behavior change from standard Bayesianism for gambling agents *immersed* within a quantum world. The trace of a 'quantum reality' (which we would so dearly love to formalize) must be found in the difference.

I've missed the punch line. The difference between what and what? Between behavior as described by standard Bayesianism and behavior of gambling agents immersed in a quantum world? It sounds like you're saying that there is no reality for standard Bayesians but a little bit emerges when quantum phenomena enter the story. Is this really what you mean?

Something like that. Standard Bayesianism makes no reference to anything about reality. Quantum mechanics seems to: It is a layer on top of Bayesianism that has to do with setting priors, and modifying the update rule when there is physical contact between the agent and the system he is stimulating (in old language, measuring).

Merminition 148 :

> [CAF wrote:] Quantum mechanics holds the promise of drastically changing our world view.

That happened a long time ago. Before even I was born, if you can believe it.

I don't think so. It changed physical practice long ago, but it has yet to change our world view in a widespread way. That's why there is still so much effort to make Everett, decoherence (einselection), consistent histories, Bohmian mechanics, modal interpretations, etc., etc., work.

None of those wimps have had the nerve to embrace the Paulian idea (whatever it is).

OK, that's my response to all your comments.

Now, all that said, I am a little disappointed in what I've been able to muster for this proposal. I tried to be inspiring and sober at the same time; I suspect I failed.

Writing is not an easy profession, is it?

06-02-04 *For Your Amusement* (to H. J. Kimble)

I just wrote a note to David Mermin regarding some quantum foundational stuff we've been talking about, and the passage below came out. I thought you might find some amusement in it. When I said, "looked him straight in the eye," I was imagining what you would do in a similar situation! Probably make my actions look like cheesecake in comparison.

> Look at one question I got in Copenhagen the other day. A fellow said, "This is all very nice for 'Sunday physics,' but what do you do for the rest of the week?" I looked him straight in the eye and told him that this was the same kind of physics that went into my calculations for Kimble's teleportation experiment. Apparently he needed to be told that.
>
> People have to get it in their heads that this is serious physics, as serious as anything else in the now vast edifice of quantum information and computing. They're drawing an imaginary line.

12-02-04 *The House Philosopher* (to J. Preskill & H. Mabuchi)

This morning I finally completed all the requirements for the IST faculty-search application. As you'll see, I followed John's suggestion full stop:

Preskillism 9 : *I also think you are right that to have a chance of success you need to emphasize what makes you unique: your passion for and productive contributions to the foundations of quantum theory.*

It felt good.

Attached for the heck of it are my teaching and research proposals. It may not come through in the documents, but I gave quite a bit of thought to how I might actually pull off these course suggestions.

Thanks for giving me the incentive to go down this path, whatever the outcome: It was a very good exercise.

The Structure of Quantum Mechanical Information

Quantum Foundations in the Light of Quantum Information. Nothing has done more to revitalize the idea that quantum mechanics can be understood at a deep intuitive level—with the concomitant benefits to physics this could bring—than the development of quantum information. There is a reason for this. Quantum mechanics is predominantly about information—plain, ordinary Shannon information or uncertainty. Embracing this idea is the starting point of my research program.

But it is not the end point: No physicist would be doing his job if he did not strive to map reality itself—that is, reality as it is independently of any information processing agents. The issue is one of separating the wheat from the chaff: Quantum mechanics may be predominantly about information, but it cannot *only* be about information. Which part is which? The usual way of formulating the theory is a thoroughly mixed soup of physical and informational ingredients.

This is where quantum information (including the collateral fields of quantum cryptography, computing, and communication theory) has a unique role to play. Its tasks and protocols naturally isolate the parts of quantum theory that should be given the most foundational scrutiny. "Is such and such effect due simply to a quantum state being a state of information rather than a state of nature, or is it due to the deeper issue of what the information is about?" Recent investigations by several workers are starting to show that many of the previously-thought 'fantastic' phenomena of quantum information—like quantum teleportation, the no-cloning theorem, superdense coding, and nonlocality without entanglement—come about simply because of the epistemic nature of the quantum state. On the other hand, other phenomena, such as the potential computational speed-up of quantum computing, seem to come from a more physical source: In particular, the answer to the question, "Information about what?"

When we finally delineate a satisfying answer to this, physics will reach a profound juncture. We will for the first time see the exact nature of 'quantum reality' and know what to do with it to achieve the next great stage of physics. Trickle-down effects could be the solution to the black-hole information paradox—perhaps already seen in broad outline—and even the meshing together of quantum theory and gravitational physics. In the meantime the approach proposed here is a conservative and careful one; the work to be done is large. The effort aims not to say first what 'quantum reality' is, but what it is not and gather insights all along the way.

Since the beginning of quantum theory, much of what the enthusiasts have called 'foundational work' has been pseudoscience pure and simple. But the field can be made as respectable as quantum theory itself, if done right. Quantum information is the technique for the task.

Being Bayesian in a Quantum World. But what is information? The Bayesian approach to the idea is that it has to do with our individual expectations, however they might come about. In particular, a probability distribution is a property of one's head, not of what is outside it: Its existence is manifested only in the betting behavior of the agent who uses it. With regard to quantum mechanics then, the Bayesian view of probability combined with Gleason's theorem on Hilbert-space measures leads ineluctably to the idea that a quantum state is a collection of gambling commitments and nothing more.

There are, however, technical differences between quantum theory and straight up Bayesianism. For one thing, when one updates a probability after gathering data, one uses Bayes' rule for the transition. But, when one updates a quantum state, one uses the formal apparatus of quantum collapse. The two transition rules, though similar, are not identical. What this (as only one example) means is that the foundations of the Bayesian approach to probability need to be carefully rethought when probabilities are brought to bear on quantum phenomena. If Bayes' rule is not to be used directly in the quantum regime, something about the usual arguments for it must fail to apply there. And so too, with all of Bayesian decision theory. The work to be done will fill a book. In fact, the finished project should compare in scope and magnitude to the content of Bernardo and Smith's magisterial *Bayesian Theory* and have the same impact on practice, *e.g.* quantum feedback and control.

What is already clear enough, nonetheless, is that from a Bayesian approach the formal structure of quantum theory represents not so much physical reality itself, but rather a behavior change (from standard Bayesianism) for gambling agents *immersed* within a quantum world. The trace of this 'quantum reality' we are striving to formalize must be found in the difference.

Quantum Mechanics as a Powerful Hint. In my opinion, the most profound statement yet to come out of quantum theory is the Kochen-Specker theorem. For it licenses the slogan, "Unperformed measurements have no outcomes." This is just a beginning. If one canvasses the philosophic traditions for one that has significantly developed this slogan, one will find the now mostly-forgotten tradition of pragmatism fathered by William James and John Dewey. As a source of ideas for what quantum mechanics can more rigorously justify, no block of literature is more relevant: The connections between the two fields cry out for systematic study. Quantum mechanics holds the promise of drastically changing our worldview on the wide scale. It is time to let that happen.

Teaching Proposal for Caltech

A good summary of my last 11 years would be that I have lived and breathed quantum mechanics. I tried to let it and the worldview it suggests infuse every aspect of my life. What I find most attractive about Caltech is that it is an institution with enough resource, foresight, and intellectual talent to play the role of a seed for something like that on the grand scale.

The course suggestions proposed here are based on such an idea. Proposed are five courses I would like to develop *over the next few years*. They all mutually interlock, and the insights from the students' reactions to each are meant to play a significant role in the development of the others.

Quantum Foundations in the Light of Quantum Information. This course, for graduate students and high-level undergraduates, would be the torch bearer for the curriculum. There are deep rumbles in the foundations of quantum mechanics, and enough technical material has amassed to make a full survey course viable and certainly desirable. The lectures would highlight papers by Scott Aaronson, Marcus Appleby, Howard Barnum, Hans Briegel, Jeffrey Bub, Paul Busch, Adán Cabello, Ignacio Cirac, Rob Clifton, Carlton Caves, Giacomo D'Ariano, myself, Nicolas Gisin, Hans Halvorson, Lucien Hardy, Alexander Holevo, Piero Mana, David Mermin, David Meyer, Itamar Pitowsky, Sandu Popescu, Robert Raussendorf, Rüdiger Schack, Robert Spekkens, Guifre Vidal, David Wallace, William Wootters, and a few others. The continuous thread running through the lectures would be how each paper implicates the others. The goals are 1) to leave the students with a palpable sense that real progress is finally being made on this seemingly timeless problem, and 2) to promote a clear vision of how they might use the techniques and ideas explored in the course to good effect in their everyday research—be it quantum information and computing, condensed matter theory, or theoretical astrophysics.

The lecture notes in this course will be a crucial component in its construction. The plan is to put together a seamless-enough and thorough-enough document to avail of a recent proposition for a book of the same name by Springer-Verlag. The point is to leave a lasting and developable mark in the practice and applications of quantum mechanics. Quantum foundations work is worth little if it cannot leave that.

Innovative Undergraduate Quantum Mechanics. I once wrote these words as a battle cry in one of my quantum foundational papers,

> The task is not to make sense of the quantum axioms by heaping more structure, more definitions, more science-fiction imagery on top of them, but to throw them away wholesale and start afresh. We should be relentless in asking ourselves: From what deep *physical* principles might we *derive* this exquisite mathematical structure? Those principles should be crisp; they should be compelling. They should stir the soul. When I was in junior high school, I sat down with Martin Gardner's book *Relativity for the Million* and came away with an understanding of the subject that sustains me today: The concepts were strange, but they were clear enough that I could get a grasp on them knowing little more mathematics than simple arithmetic. One should expect no less for a proper foundation to quantum theory. Until we can explain quantum theory's *essence* to a junior-high-school or high-school student and have them walk away with a deep, lasting memory, we will have not understood a thing about the quantum foundations.

We are certainly far from that dreamy ideal today, but a soberer version of the lesson is the same: The only meter for progress in quantum foundations is the ease of quantum practice and the ease with which students absorb the subject.

To that end, quantum information has already taught us several lessons at the methodological level that can be made use of today. What I envision is developing a course that takes its base in Hideo Mabuchi's previously successful Ph125 and trying to tweak it to perfection. The great lesson of quantum information is how much of the meaning and mystery of quantum mechanics can be isolated in two and three-level systems and their interactions. Therefore it is useful to spend a solid amount of time sussing out that regime as thoroughly as possible. Simple quantum information effects like the no-cloning theorem, quantum cryptography, Bell inequalities, entanglement monogamy, quantum teleportation, and the Kochen-Specker theorem are not 'Sunday physics', but the very heart of the matter. More than anything, they give an intuition for the necessity of the Hilbert-space formalism.

The trick is in pushing that intuition deep into the students' psyche, while at the same time not losing sight of the need to get through a semi-standard curriculum of harmonic oscillators, perturbation theory, hydrogen atoms, the Zeeman effect, etc., as efficiently as possible. How to do it? Some of physics is just roll-your-sleeves-up hard work, but it is a good gamble that as the course develops much of the method will lead its own way. Inconsistencies in thought and method have a way of declaring themselves—it is that system of checks and balances which has tugged research in 'quantum mechanics as quantum information' in a uniform direction ever since the beginning anyway. In all seriousness, this is a case where basic quantum theory stands to gain as much from the reaction of the undergraduate as any amount of cloistered introspection.

Bayesian Theory. Bayesian Theory is an approach to probability theory and statistical decision-making that is quickly gaining traction in all aspects of science. José Bernardo and Adrian Smith characterize it this way in their masterwork on the subject:

> The theory and practice of Statistics span a range of diverse activities .... What is the nature and scope of Bayesian Statistics within this spectrum of activity? Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterizing how an individual should act in order to avoid certain kinds of undesirable behavioral inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behavior. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."

My own interest in Bayesianism sprouts from the sensibleness it helps bring to quantum mechanics: For, one might consider much of the formal structure of quantum theory as a prescriptive set of rules for uncertainty accounting in the light of some (yet to be fleshed out) more basic features of the world. In other words, quantum theory is not so much about physics itself, but rather decision theory in the *presence* of physics. This is the research program of 'Being Bayesian in a Quantum World.'

It is crucial for this research program, however, that the physics community gain a proficiency and comfort with the subject long previous to that. This motivates me to try to introduce and maintain a top-notch, Bayesian theory course for graduate students and high-level undergraduates. The course, based initially on Bernardo and Smith's text *Bayesian Theory*, would have a university-wide appeal, being applicable to all the sciences and some fields in the humanities. Covering both foundational topics and rigorous technicalities in statistical modelling, it should be particularly relevant to those students associated with the IST Center for the Mathematics of Information.

Quantum Mechanics and Anti-representationalist Philosophies. There are various threads connecting the quantum research program proposed here to a wider philosophical tradition, which to my knowledge has never been greatly examined in this context. The tradition comes under the rubric of what Richard Rorty calls 'anti-representationalist philosophies.' This tradition, spearheaded by the pragmatism of William James and John Dewey, also includes thoughts of (the later) Ludwig Wittgenstein, Martin Heidegger, Donald Davidson, Hilary Putnam, Rorty himself, and several others. How else can one understand the implications of the Kochen-Specker theorem than by realizing it hints at something like James' analysis of the concept of 'truth'? How else can one make sense of a Bayesian take on pure quantum states than to explore the same paths as Wittgenstein in his book *On Certainty*?

Since becoming immersed in the subject, I have found nothing more exciting than these trains of thought. For they indicate the extent to which quantum foundations research may be the tip of an iceberg—indeed, something with the potential to drastically change our worldview, even outside the realm of physical practice. William James once said this of the method of pragmatism,

> [Within it] theories ... become instruments, not answers to enigmas, in which we can rest. We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

Two thousand years hence, this may be the greatest, most lasting legacy of quantum theory.

Within the humanities division, or possibly shared between physics and the humanities, I would like to develop a course for graduates and undergraduates that would meditate on these threads and tie them together as much as possible. The lectures and discussions would focus on a selection of materials drawn from my personal library of nearly 300 books on the subject. The goal with respect to interested philosophy of science graduate students would be to introduce them to enough material and provide them with enough guidance for a solid thesis topic in the subject.

Writing Physics. The greatest physics laboratory for me has always been my keyboard. With the practice of almost 17,000 emails since my first notebook computer in 1997 (a plethora of which are long and detailed), I believe I have taught myself a skill worth passing on to physics students in general: It is in line with what David Mermin has called *writing physics*,

> Physicists traditionally replace talk about physics by a mathematical formalism that gets it right by producing a state of compact nonverbal comprehension. The most fascinating part of writing physics is searching for ways to go directly to the necessary modifications of ordinary language, without passing through the intermediate nonverbal mathematical structure. This is essential if you want to have any hope of explaining physics to nonspecialists. And my own view is that it's essential if you want to understand the subject yourself.

My trick has been to never write for my own mind, but always for someone else's. That is why email has been essential to me. (Some of my best papers, technical and nontechnical, have been cut and pastes from old emails.) Indeed, email provides a kind of immediate feedback that no other kind of writing can.

Based on these ideas, and Mermin's own from his paper of the same name, I would like to create an innovative technical writing course for undergraduates within IST. The goal is to have better papers, and better scientists because of better papers. The goal is to have IST writing physics.

13-02-04 *Anniversary Cheer* (to J. Preskill)

NOTE: With hindsight, this letter seems to represent the first time I had incorporated William James's "will to believe" doctrine into my palette of thinking.

What welcome news to wake up to this morning, and what a great day to receive it on! Ten years ago today, I worked up the nerve to trot across a neighborhood park and asked Kiki to dinner. It was our first date. (You can read about the weeks leading up to it in the story of our dog Wizzy, in "Quantum States: W.H.A.T.?" on my webpage, pages 142–143.) [See 16-02-02 note "Some Things Should Not Pass."] Lesson learned: Without nerve first, there isn't hope second.

15-02-04 *Step Forward* (to G. Brassard)

Let me attach the proposals I finally sent off to Caltech last week. [See 12-02-04 note "The House Philosopher" to J. Preskill and H. Mabuchi.] I hope you enjoy them.

Today I fly out for the LSE meeting on Probability and Quantum Mechanics, and to give the "Popper Colloquium" the next day. I think I need to do a particularly good job here, as we stand to gain a lot of interest from the philosophers.

I've gotta, gotta, gotta get to work on the Asher paper for us. Let's hope that starts up in about a week: In any case, I'm sure the editor of the special issue will wait for us. I know him personally!

17-02-04 *From London* (to A. Fine)

Sorry to take so long to reply: I'm at the LSE now and going to give my first colloquium ever to a philosophy department. (Look in the obituary column tomorrow.)

Fine-ism 2: *I think we have some serious disagreements about how to think about QM, the role of pragmatic considerations, etc. But, then, choose any two folks who work in this area and you'll find the same sort of thing!*

Interesting. I hope you'll explain as you get a chance. In particular, it would be useful to me if you can pinpoint where you already disagree with my `quant-ph/0205039` "QM as QI (and only a little more)". Following that, I'd like to hear about the disagreements with my later thoughts (for instance, the ones in the proposals) after pragmatism sunk more deeply into my soul.

21-02-04 *Philosophical Training* (to A. Shimony)

I am just back in Dublin from a (mentally) long week in London at the LSE and then at All Souls College in Oxford. It was my first extended engagement with full-fledged philosophy departments. I tell you, I felt like I was in the frying pan the whole time! It was defense, defense, defense; they made me earn my travel expenses. But—no doubt—I got some good training in the process.

21-02-04 *My Visit* (to S. Savitt)

I am just back in Dublin from an exhaustingly long week at the LSE in London and All Souls College in Oxford. It was my first real engagement with philosophy departments. I tell you, I felt like I was in the frying pan the whole time! It was defense, defense, defense. What does not kill you makes you stronger? ... Actually, except for the constant effort I had to put into keeping from being bushwhacked, I think I fared pretty well. Even picked up a convert or two ...

Looking forward to seeing you.

22-02-04 *Fidelities* (to S. L. Braunstein)

Braunsteinism 3: *I have a question, you may know the answer to. What is the fidelity one needs to surpass to demonstrate teleportation of a qubit from an alphabet $|0\rangle$, $|1\rangle$, $|0\rangle \pm |1\rangle$, $|0\rangle \pm i|1\rangle$? Presumably it's higher than 2/3. Do you know of any reference which has calculated this?*

No, it's actually precisely 2/3. In fact, in d dimensions, if one uses the $d(d+1)$ vectors drawn from a complete set of mutually unbiased bases, the breaking point fidelity one needs is $2/(d+1)$.

I'll attach the draft of a paper that I sorely need to finish which contains these calculations. (It's a paper for the Holevo festschrift, and now the editors are really bothering me for it.) The bit about MUBs in particular can be found around equations 43 through 46.

I hope things are going well for you. For myself, I've been traveling far, far, far too much to get any work done. I doubt the viewers of `quant-ph` even remember me anymore.

24-02-04 *Weird Forms of Realism* (to K. Brading)

Thanks for the note; I very much appreciate it. I wish I had met you at Oxford.

I think the best answer I can give to your question at the moment is recorded in Sections 4 and 5 of my paper, "The Anti-Växjö Interpretation of Quantum Mechanics." (You can find the paper at my website; there is a link below.) Since then, I have developed those thoughts somewhat, but I think the gist of what I'm striving for is already expressed in a fairly easy-to-read form in that older paper (or, I should say two emails). Reading over the sections again, I think they answer you quite directly.

I did a Google search on your name a little while ago to try to figure out who I'm talking to. It didn't help much. But I did learn that you've just obtained a faculty position at U. Notre Dame. ... Wait! I did meet you: It was at the tea before the talk, wasn't it? Right. Well anyway, congratulations on your new position! Carry out some great work there.

Katherine's Preply

> I was at your talk last Thurs at the Phil of Physics seminar in Oxford, and wanted to drop you a line to say how much I enjoyed it. I don't know whether you knew that you were coming into a group dominated by committed realists — of various stripes, but realists nonetheless — and I thought you did a great job. Personally, I found your approach very congenial indeed, but I wondered whether you wanted to put your position concerning scientific realism as strongly as it appeared. You seemed to resist any temptation to talk about 'The World', and yet one motivation you were giving for your approach is that we may hope that it will lead us to a new route 'back to the world'.
>
> Would you find the following way of expressing things acceptable, or is there a reason why you feel you need to go further:
>
>> The world is such that if I update my beliefs according to these rules (QM...) then I will be successful in getting around. But there is no inference from this to realist claims about 'how the world is in itself', and without relation to my activity of going around in it. So when I make some claim, such as 'The photons in this beam are vertically polarized', that's shorthand for 'The world is such that if I believe that the photons in this beam are vertically polarized (and if I act accordingly) then I am likely to be successful in my further related activities'. Usually this shorthand doesn't get us into trouble ('There's a tree over there'), but sometimes it does.
>
> How I understood the message of your talk was that, rightly understood, QM forces us to move to the epistemically modest longhand, rather than the naively realist shorthand, BUT that there are clues as to how we might then go forwards into approaching 'the

world in itself' again. I very much liked the suggestive discussion of disturbance at the end of your talk.

Anyway, whatever you think of the above — if anything — all I really wanted to do was to write a brief line saying that there were at least two not-so-realist bodies in the audience — myself and Peter Morgan — and that I for one like very much what you're doing.

25-02-04 *Promised Message* (to N. D. Mermin)

Merminition 149: *The following true story will appeal to nobody but you and maybe not to you either, but here it is.*

Do you remember my funny way of solving Bernstein-Vazirani (that I spoke on at the Bennett symposium)? You want to determine an n-bit number a, and are given a U that acts on a n-bit input register containing x to shift the one bit output register by the bit-wise modulo-2 inner product of a and x. My solution starts by replacing U by a bunch of cNOT gates — one for each non-zero bit of a controlled by the corresponding bit of x, all of them targeted on the output register.

Anyway I showed this to my class of physicists and computer scientists, and somebody remarked that it was pretty clumsy having to reconfigure the hardware every time you wanted to do it for a different value of a.

Instantly one of the CS students said no, that wasn't necessary. All you needed was an additional n-bit register into which you put a. The hardware was then fixed, consisting of n doubly controlled NOTS (i.e. Toffoli gates) all targeted on the output register and controlled by pairs of corresponding bits of x and a.

Thinking back on this a few days later it struck me that the way I presented it the choice of Hamiltonian (in the form of U) associated with the different possible a's was objective — different arrangements of the classical hardware. But the way the sharp CS student suggested doing it changed that choice of Hamiltonian to a specification of the state of the additional register. The selection of the Hamiltonian from among all possible Hamiltonians in his scheme was on exactly the same footing as (and in fact was identical to) the specification of a state vector.

That's all. Sounds less entertaining now that I've written it out.

Nope, a very deep point I would say ... Sounds worthy of a slogan. How about, "A quantum operation is just a quantum state in disguise." ??

26-02-04 *Thanks* (to A. Peres)

Thanks for writing me back; I really had started to worry.

Asherism 44: *Please remind me where you have written "there are no quantum states (the ghost of Bruno de Finetti)". My question is related to Netanel's PhD work on quantum gravity. In some spacetimes, there are obviously no quantum states.*

The place where I made the most to-do of the phrase "quantum states do not exist" was in `quant-ph/0205039`, "Quantum Mechanics as Quantum Information (and only a little more)". However the phrase made its first public appearance in `quant-ph/0105039`, *Notes on a Paulian Idea*. In fact, it comes from a letter I wrote to you dated 1 December 1998:

Letter to Asher Peres, 1 December 1998, "Here Comes the Judge"

Asherism 45: *The measuring process is an external intervention in the dynamics of a quantum system.*

I know that I've already expressed what I'm about to say several times, but let me just set things off to a good start again: I really like this turn of phrase! For years now, I have believed exactly what you say, namely,

Asherism 46: *[I]t is preferable not to use the word "measurement" which suggests that there exists in the real world some unknown property that we are measuring.*

but I had never invented such a clever phrase to express it. For a long time instead I made use of the word "creation" in my tract-like emails on the subject. In some ways, I still think—taking my cue from John Wheeler—that that word captures a certain central truth of quantum theory, but your word is more encompassing. I think it captures better the idea that quantum measurement is a double-edged sword, learning *and* creating.

But I wouldn't be doing my scholarly duty, if I didn't remind you of some passages of Niels Bohr, whose words you seem to have so much respect for. In Bohr's 1949 article "Discussion with Einstein on Epistemological Problems in Atomic Physics," he wrote:

> [At the Solvay meeting in 1927] an interesting discussion arose about how to speak of the appearance of phenomena for which only predictions of statistical character can be made. The question was whether, as to the occurrence of individual effects, we should adopt a terminology proposed by Dirac, that we were concerned with a choice on the part of 'nature,' or, as suggested by Heisenberg, we should say that we have to do with a choice on the part of the 'observer' constructing the measuring instruments and reading their recording. Any such terminology would, however, appear dubious since, on the one hand, it is hardly reasonable to endow nature with volition in the ordinary sense, while, on the other hand, it is certainly not possible for the observer to influence the events which may appear under the conditions he has arranged. To my mind, there is no other alternative than to admit that, in this field of experience, we are dealing with individual phenomena and that our possibilities of handling the measuring instruments allow us only to make a choice between the different complementary types of phenomena we want to study.

And, more to the point, in his 1958 article "Quantum Physics and Philosophy: Causality and Complementarity," he wrote:

> In the treatment of atomic problems, actual calculations are most conveniently carried out with the help of a Schrödinger state function, from which the statistical laws governing observations obtainable under specified conditions can be deduced by definite mathematical operations. It must be recognized, however, that we are here dealing with a purely symbolic procedure, the unambiguous physical interpretation of which in the last resort requires a reference to a complete experimental arrangement. Disregard of this point has sometimes led to confusion, and in particular the use of phrases like "disturbance of phenomena by observation" or "creation of physical attributes of objects by measurements" is hardly compatible with common language and practical definition.

> In this connection, the question has even been raised whether recourse to multivalued logics is needed for a more appropriate representation of the situation. From the preceding argumentation it will appear, however, that all departures from common language and ordinary logic are entirely avoided by reserving the word 'phenomenon' solely for reference to unambiguously communicable information, in the account of which the word 'measurement' is used in its plain meaning of standardized comparison.

Do you see these passages as meshing well with your new terminology? I don't really, but then again I don't feel that Bohr was all that right when he came to these points. I think it might be useful for your ultimate reader to add some words about your opinion on this.

Asherism 47: *As a concrete example, consider the quantum teleportation scenario. The first intervention is performed by Alice: she has two spin-$\frac{1}{2}$ particles and she tests in which Bell state these particles are.*

In which state they *are*?!?! There's got to be a better way of putting this ... especially one more consistent with your whole view of the measurement process.

Asherism 48: *I do not want to use the word "histories," which has acquired a different meaning in the writings of some quantum theorists.*

What happened to your nice word "chronicle"? I liked it a lot. Can't you find some way to reinstate it? I guess I liked it because it always reminded me of a passage that I like to quote from Pauli. In a letter to Markus Fierz in 1947, he wrote:

> Something only really happens when an observation is being made .... Between the observations nothing at all happens, only time has, 'in the interval,' irreversibly progressed on the mathematical papers!

Asherism 49: *Quantum mechanics is fundamentally statistical, and any experiment has to be repeated many times (in a theoretical discussion, we shall imagine many replicas of our gedankenexperiment).*

Asherism 50: *Each one of these records has a definite probability, which is experimentally observed as its relative frequency among all the records that were obtained.*

Asherism 51: *Note that the "detector clicks" are the only real thing we have to consider. Their probabilities are objective numbers and are Lorentz invariant.*

As a disciple of the Reverend Bayes, you should know that I strongly dislike all these expressions. A good Bayesian would say that probability quantifies a state of knowledge. It is that and that alone, meeting an operational verification *only* through a subject's betting behavior. In particular, probability has no *a priori* connection to the frequency of outcomes in a repeated experiment. It matters not whether that repetition is real or, instead, imaginary and virtual.

Let me just try to drive this home with two of the simplest possible examples.

(1) Consider a coin for which you have no reason to believe that a head will occur over a tail in a toss. Now imagine that you flip that coin an infinite number of times,

tabulating the number of heads and tails. Do you really believe that a frequency of precisely 1/2 *must* occur in the infinite limit? Answer this question to yourself very honestly. What is to bar you from getting a head as the outcome in literally every single toss? What is there in the coin to favor a random-looking sequence to a nonrandom one? Nothing. To say something like, "Well the nonrandom-looking sequences have probability zero" is just to beg the question. For one thing, you have to invoke the concept of probability again to even say it. For another, even if you allow yourself that, it still carries no force: just take the set of nonrandom-looking sequences and add to it any sequence that you would consider a valid random one (i.e., one that you believe could be generated by a repeated coin toss). That set still has probability-measure zero, but now you would have to say that that means the random-looking sequence could not be generated by your imaginary coin. The point: probability has no direct connection to frequency.

(2) Consider now the case where I toss a coin repeatedly for you. I assure you strongly that the coin is weighted 80–20 heads vs. tails *or* 20–80, but I steadfastly refuse to tell you which way it goes. What probability would you ascribe to the outcome of a head upon my first toss? Fifty-percent of course. The point is, my probability ascription is not your probability ascription. As I toss the coin ever more, if you are rational, your probability ascription will *very likely* approach mine, but there are no absolute assurances. The point: again probability has no direct connection to frequency. But also, two perfectly rational people can make different probability assignments to the same experiment without being inconsistent with each other. There is nothing objective about a probability assignment per se.

If you'd like to understand better this point of view, then I'd strongly suggest the wonderful collection of essays, "Studies in Subjective Probability," edited by Henry E. Kyburg, Jr., and Howard E. Smokler (Wiley, NY, 1964). Also there is a later edition of the book with a few other essays. For a much more in-depth study I couldn't recommend anything more than Ed Jaynes' book, "Probability Theory: The Logic of Science." It unfortunately may never be published properly, but preprints of the parts that were written can be found at the "Probability Theory as Extended Logic" web page, `http://bayes.wustl.edu/`.

Asherism 52: *[P]robabilities are objective numbers .... On the other hand, wave functions and operators are nothing more than abstract symbols. They are convenient mathematical concepts, useful for computing quantum probabilities, but they have no real existence in Nature.*

I think it is really cute the way you cite Stapp here!

But again I want to come back to the last point above. A good Bayesian already knows that probabilities cannot be taken to be objective numbers, existent "out there" in nature. And a good quantum physicist similarly knows that the wave function is *not* something "out there" existent in nature. The similarity of these two points of view is not an accident: it had to be the case! For, from one point of view, the wave function is nothing more than a compendium of probabilities. It may be the case that "unperformed measurements have no outcomes," but it is not the case that unperformed measurements have no probabilities! If we specify the probabilities of the outcomes for every *potential* measurement, then we have specified the wave function. There cannot be a difference in the objectivity levels of these two mathematical objects: they are both abstract symbols that summarize our states of knowledge. Their empirical meaning comes about precisely in how a rational being would behave in the light of that knowledge.

There is a famous quote in Bayesian probability theory due to Bruno de Finetti. It starts out the two volumes of his book.

> My thesis, paradoxically, and a little provocatively, but nonetheless genuinely, is simply this:
>
> PROBABILITY DOES NOT EXIST.
>
> The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time, ..., or Fairies and Witches, was an essential step along the road to scientific thinking. Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading conception, an illusory attempt to exteriorize or materialize our true probabilistic beliefs.

In contrast, my paper "Bayesian Probability in Quantum Mechanics" with Caves and Schack, opens up with the following lines:

> My thesis, paradoxically, and a little provocatively, but nonetheless genuinely, is simply this:
>
> QUANTUM STATES DO NOT EXIST.
>
> The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time, ..., or Fairies and Witches, was an essential step along the road to scientific thinking. The quantum state, too, if regarded as something endowed with some kind of objective existence, is no less a misleading conception, an illusory attempt to exteriorize or materialize the information we possess.
>
> — *the ghost of Bruno de de Finetti*

I don't know of any better way to express it than this.

Asherism 53: *The physical evolution that leads to Eq. (1) is the following. The intervener receives a quantum system in state ρ and adjoins to it an auxiliary system (an ancilla) in a known state ρ_a. The composite system, in state $\rho \otimes \rho_a$, is subjected to a unitary transformation ...*

In my present way of speaking, I like to make this sound much less absolute. I usually say that this is only one *representation* of a superoperator. It is the superoperator that is the only thing that need be given; anything else is just one way or another of thinking about it. I guess the point is I see no reason to give unitary evolutions and von Neumann measurements a special status in the axiomatics of the theory. POVMs and superoperators are perfectly good (and much less specific) starting points for the theory. You can certainly do as you please, but it seems to me that there is a lot to be gained from this point of view. For instance, it seems to me to de-emphasize the point of view that Charlie Bennett labels "the Church of the Larger Hilbert Space" (which is, as he admits, a euphemism for the many-worlds interpretation).

Asherism 54: *If we wish to consider only the states just before and after the intervention, without entering into the detailed dynamics of the intervention, the result appears as a discontinuous jump of the wave function (often called a "collapse"). Clearly, this jump is not a dynamical process. It results from the introduction of an ancilla, followed* *by its deletion, or that of another subsystem. The jump is solely due to abrupt changes in our way of delimiting what we consider as the quantum system under study.*

I agree with you to some extent here, but not the whole way. The abrupt change that comes about when we conceptually delete a subsystem from a larger composite system corresponds to a *partial trace*. Where does the *random* jump come from that corresponds to the measurement outcome label k? I've heard you say things like this many times, i.e., that the statistics in quantum mechanics comes about because of a mismatch in two languages, the quantum and the classical—your talk at QCM was just one example—but how does one see that you're not just talking about a partial trace here? I am perfectly willing to take measurement/intervention as a primitive of the theory—it's the very thing that gives the theory meaning—but you seem to want to go further, to have it fall out of something more primitive, namely the act of drawing a conceptual line in an overall unitary dynamics. I guess I still don't see it.

Asherism 55: *Summing over them is like saying that peas and peanuts contain on the average 42% of water, instead of saying that peas have 78% and peanuts 6% [25].*

I loved the citation here! You don't think there might be a connection between Stapp and the USDA, do you?

Asherism 56: *Between these two events, there is a "free" (that is, deterministic) evolution of the state of the quantum system. What distinguishes such a free evolution from an intervention is that the latter has unpredictable output parameters, for example which one of the detectors "clicks," thereby starting a new chapter in the history of the quantum system. As long as there is not such a branching, the evolution will be called free, even though it may depend on external classical fields, that are specified by the classical parameters of the preceding interventions.*

I like this distinction, but I cannot completely agree with it. Take any trace-preserving completely positive map that is not unitary, i.e., most any quantum channel like the amplitude damping channel or the depolarizing channel will do. It gives rise to no "branching" changes in the quantum state, but I think one would be hard pressed to call it a "free" evolution.

I overheard Herb Bernstein once say something like, "Of course there are two kinds of evolution in quantum mechanics; there are the times when we learn something and then there are the times when we learn nothing." The line struck me. I know that this is something like what you're trying to get at, but the cut isn't between unitary and nonunitary then.

Asherism 57: *Quantum mechanics asserts that during a free evolution the quantum state undergoes a unitary transformation.*

This needs cleaning up exactly because of the point above. By the way, note that there is a typo in the sentence following Eq. (7).

Asherism 58: *Note that* $\mathrm{Tr}(\rho_f) = \mathrm{Tr}(\rho'_f)$ *is the joint probability of occurrence of the records k and μ. This is the only observable quantity in this experiment. ... Einstein's principle of relativity asserts that both descriptions given above are equally valid. Formally, the states ρ_f (at time t_f) and the state ρ'_f (at time t'_f) have to be Lorentz transforms of each other.*

I am afraid that this might be construed to mean that the only way the equality of the traces can be satisfied is if ρ_f and ρ'_f are "Lorentz transforms" of each other. Surely that's not true and that's not what you mean. Maybe you could clarify this a bit.

Asherism 59 : *As a further simplification, let all the U and V operators be unit matrices, so that the two particles are really free, except at the intervention events.*

What do you mean by the phrase "really free" here? Is it a joke?

Asherism 60 : *We thus have to accept that unit matrices of any order may also be legitimate Lorentz transforms of each other.*

So I guess what you're meaning is that you want this problem to partially define what is meant by the very term "Lorentz transform?" I'm looking forward to understanding the business about Green's functions much better. After that I'll come back for some more substantial comments on everything in this section following Eq. (10).

Asherism 61 : *Quantum nonlocality has led some authors to suggest the possibility of superluminal communication.*

Notice the correction to *your* spelling mistake ... ahem. Grin. Anyway, in this connection let me point you to a very nice article by a philosopher: J. B. Kennedy, "On the Empirical Foundations of the Quantum No-Signalling Proofs," Phil. Sci. **62**, 543–560 (1995). The point he makes—and I think validly so—is that any so-called "proof" that quantum mechanics cannot support superluminal signalling by using entanglement is essentially circular. It's not that the "proofs" are invalid, but essentially that there was no use in doing them in the first place: no-signalling is much more of an *axiom* than a result of the standard structure of quantum mechanics. Kennedy argued by way of digging into the historical references, finding for instance von Neumann's original motivation for introducing the tensor product rule for combining Hilbert spaces—it was essentially to block the possibility of superluminal signalling! You might enjoy reading the paper.

Asherism 62 : *The classical-quantum analogy becomes complete if we use statistical mechanics for treating the classical case. The distribution of the bomb fragments is given by a Liouville function in phase space. When Alice measures $\mathbf{J}_1$, the Liouville function for $\mathbf{J}_2$ is instantly altered, however far Bob is from Alice. No one would find this surprising, since it is universally agreed that a Liouville function is only a mathematical tool representing our statistical knowledge. Likewise, the wave function ψ, or the Wigner function, which is the quantum analogue of a Liouville function, are only mathematical tools for computing probabilities. It is only when they are considered as physical objects that superluminal paradoxes arise.*

Indeed you have captured the point here. Part of this, i.e., that the proper comparison is between state vectors and Liouville distributions, is what Carl and I were striving to convey in our paper for Nathan Rosen's festschrift—so I'm already very sympathetic to this. (Of course I know that you already made this point long ago in your paper "What is a State Vector?") More particularly, though, this is one thing that Steven van Enk and I had planned to write in a silly little paper—titled "Entanglement is Super

... but not Superluminal!"—for our contribution to a book on superluminal signaling! I hope you won't mind the overlap.

The thing that holds so many up from immediately grasping this point of view is the *difference* between the quantum and classical states of knowledge (i.e., those things that are changing instantaneously as you say). Classically, the probability is attached to an existent property. "The particle has a position; I just don't know it. I capture what little bit I do know with a probability assignment." Quantumly, the probability is attached to *potential* measurement outcomes and nothing more. How is it that my probability assignment for some potential measurement outcome can change from 50-50 to complete certainty when there is no sense in which that measurement outcome can already be said to be "out there" without the performance of a measurement? That's the thing that has people perplexed and, I think, is the source of their confusion about using entanglement for communication.

Asherism 63: *In our approach, there is no 'delayed choice' paradox. It is indeed quite surprising that JAW, who is a hard core positivist, introduced this 'delayed choice' idea.*

Sometimes it's pretty hard to figure out what precisely it is that John is trying to say. Perhaps you shouldn't be overly harsh on him. In some of his discussions of "delayed choice" he makes it very clear that he is not talking about affecting the "past" itself (whatever that might mean) but instead "what we have the right to say about it." Combine that with one of the phrases he is fond of saying, i.e., "The past exists only insofar as it is recorded in the present," and I think you will have to position him closer than not to the positivist you used to think of him as.

01-03-03 *Wither Entanglement!* (to T. Siegfried)

Thanks for the letter. I'm glad you're enjoying parts of *Notes on a Paulian Idea* (even if some other parts rankle you). Kluwer has picked it up to make for a more official-looking (even if ridiculously expensive) second edition this summer. But that means I've got to get off my duff and get the thing re-edited by June. I couldn't be a journalist for sure: I'm so bad with deadlines. I envy you.

Siegfriedism 1: *At the moment, I'm trying to work out a way of doing something on quantum entanglement as a foundation for 21st century technologies (based on a symposium at the recent AAAS meeting). Any suggestions of non-obvious angles to pursue, or particularly significant papers you've seen lately?*

It'd be hard for me to suggest an angle to pursue, as I don't really think entanglement is ultimately at the base of what is interesting in quantum information and computing. I think entanglement is an auxiliary effect that is sometimes useful, but there is something deeper going on. What the deeper thing is, now that's harder to say, but my feeling is that it has something to do with the particular structure of quantum measurements (which in turn capture something about the zing, zip, verve, pizzazz of the world).

But seriously, there are plenty of things in quantum information that don't derive their power from entanglement. Simple BB84 and B92 quantum cryptography are two examples. Or the stuff we called "quantum nonlocality without entanglement" (an old paper by Bennett and several of us, `quant-ph/9804053`) which makes use of the tensor product structure for combining systems, but not quantum entanglement in particular.

Also, in fact, one can see how the tensor product structure (and with it entanglement) arises in a simple way as a byproduct of the structure of measurements and a simple locality condition. The argument is in the "Wither Entanglement?" section of my `quant-ph/0205039`. So, there is some substance to my speculation above.

Finally, let me just mention my favorite paper of the year: Rob Spekkens', "In defense of the epistemic view of quantum states: a toy theory," `quant-ph/0401052`. In that paper, Rob shows how a load of (what were once thought to be) 'fantastic' quantum information effects can come about—qualitatively at least—even in a local hidden variable theory, as long as one is talking about the epistemic states of the theory. Among these are things like teleportation, superdense coding, an analogue of entanglement monogamy, secure key distribution, and the list is quite a bit larger than that. The point is, entanglement in the proper sense powers none of those effects—for his toy theory can have no entanglement; it is a local hidden variable theory. The paper is quite easy to understand; in fact it contains almost no physics at all, just elementary combinatorics. You ought to have a look at it. It's the sort of thing that leaves you with a warm, fuzzy feeling that you've understood something quite deep.

Siegfriedism 2: *Also, any comments on Bub's recent paper?:* `quant-ph/0402149`

It's a pretty good paper until he flubs it up by still thinking that decoherence is of any importance for quantum foundational issues. In general the work by Clifton, Bub, and Halvorson is some nice stuff and a suggestive hint of the kinds of efforts we should keep striving at. I think taking C^* algebras as a starting point is far too advanced a starting point to get at the real nub of the matter, but what we're all doing is chipping away at an iceberg ... and I think their work contains a legitimate insight.

01-03-04 *Midnight Murmurs* (to R. W. Spekkens)

Good to hear from you. I enjoyed your story about the Bielefeld meeting. It sounds like the same meeting where I first concocted the image of a Holy City. It was on the island of Ischia, near Naples, and I'm pretty sure a lot of the same people were there. (The story is recorded in the letter spanning pages 450 and 451 of *Notes on a Paulian Idea*, VUP edition.) You didn't describe how your talk went though. Did it make some impact?

Spekkensism 25: *Thanks for promoting my paper.*

It's hard for me to keep my mouth shut about the paper. [...] For instance, I just wrote this to Tom Siegfried (who writes the science column for the *Dallas Morning News*):

> Finally, let me just mention my favorite paper of the year: Rob Spekkens's, "In defense of the epistemic view of quantum states: a toy theory," `quant-ph/0401052`. In that paper, Rob shows how a load of (what were once thought to be) 'fantastic' quantum information effects can come about—qualitatively at least—even in a local hidden variable theory, as long as one is talking about the epistemic states of the theory. Among these are things like teleportation, superdense coding, an analogue of entanglement monogamy, secure key distribution, and the list is quite a bit larger than that. The point is, entanglement in the proper sense powers none of those effects—for his toy theory can have no entanglement; it is a local hidden variable theory. The paper is quite easy to understand; in fact it contains almost no physics at all, just elementary combinatorics. You ought to have a look at it. It's the sort of thing that leaves you with a warm, fuzzy feeling that you've understood something quite deep.

It is hard for me to keep my mouth shut.

Spekkensism 26: *Most recently, I needed to prepare a talk for the quantum gravity meets quantum information workshop at PI. (It's too bad you were unable to make it to the workshop. We needed a talk on how mass is nothing but Hilbert space dimension. How is that coming along anyway?)*

Basically, I'm not thinking at all. And I don't foresee that I will again until I have the job issue settled and have a place to plant the family. Mentally I'm empty, outside of those basic urges. (Before you ask: Yes, I could go back to Bell Labs. But I think it would signal the end of all that I really love.)

Spekkensism 27: *How was the response to your talk at Oxford? I would expect there to be animosity towards your views there. Did you manage to shake the convictions of any of the Everettians?*

It felt like it was quite successful in some ways actually: At the very least I did some of the best sparring I've done in a while, and I felt like it made an effect, though it was quite exhausting. I got, I think, four emails, maybe five, afterward from people I didn't know expressing various kinds of interest. For instance, I got this from Katherine Brading (never met her before):

> I was at your talk last Thurs at the Phil of Physics seminar in Oxford, and wanted to drop you a line to say how much I enjoyed it. I don't know whether you knew that you were coming into a group dominated by committed realists – of various stripes, but realists nonetheless – and I thought you did a great job. Personally, I found your approach very congenial indeed ...

That's what it's all about: Chipping away at an iceberg, getting people to rethink.

01-03-04 *Thinking Out Loud, As Usual* (to H. Halvorson)

Thanks for the note. I guess we *have* never met; I've certainly seen you in the distance.

Halvorsonism 1: *In some of my current work, I'm picking up a question that has been close to your heart over the past few years — viz., how much of QM is just "laws of thought"? I'm going to have occasion to talk to some general philosophical audiences about the topic, and I'd like to give them an accurate representation of your position. So, I was wondering if I could ask you a favor: Could you point me to what you take to be the two or three (or more, if you have time!) most significant passages or results in your corpus that discuss, or bear on, this topic? In other words, where should one look first if one wants to quickly learn the correct answer to the question?*

It's a phrase I stole from Boole, you know. (See pages 527–529 and 351 in *Notes on a Paulian Idea*, Växjö U Press edition. I did send a copy of that to you, didn't I?)

It would be hard to tell you the "correct" answer because all of these thoughts are in constant transition. It's just been something I've been groping for, for the last eight or nine years. (I think I invented the phrase at a bar in Albuquerque, Jack's Liquor and Lounge, in the Fall of 1995.)

Be warned that by the phrase I don't mean something like a Kantian a priori category, i.e., a position like von Weizsäcker's in his book *The Unity of Nature*. I don't mean something like, "an understanding using the terms of quantum mechanics is the precondition for possible experience." Rather I have started to toy rather strongly with a Darwinian kind of idea: Using the rules of quantum mechanics for manipulating and updating our expectations (i.e., as a "law of thought") is the presently best known means for survival, given that we are immersed in the particular world we are. That is, I want to view quantum theory as a branch of decision theory that is contingent upon properties of the world we live in ... and it is something we locked into only in our most recent turn in evolutionary development.

Also, I should try to make it clear that, in this light, I view quantum mechanics as a normative theory in a sense akin to the one Bernardo and Smith use to describe Bayesian probability theory:

> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."

That's the short of it.

The best reference for the long story at the moment is my samizdat *Quantum States: What the Hell Are They?*,—unfortunately the whole of it—posted at my webpage in pdf format. But that's too much material (and too loosely organized) for you. So within that, let me point you more specifically to: pages 49–50 "Note on Terminology," pages 83–85 "Replies on Practical Art," pages 144–147 "Psychology 101," and pages 150–155 "A Wonderful Life." Maybe also pages 35–38, "Identity Crisis." That might do for first pass.

Ultimately, I'd like to synthesize (and "consistify") these 235 pages of email into a single paper of 20 or 30 pages, but unfortunately that hasn't happened yet. If anything looks like sheer nonsense, or the writing is detractingly ambiguous, let me know, and I'll try to clarify for you.

Good luck. If the stuff provokes any thoughts in you, I'd love to hear them. Also, I'd love to know the sorts of things you're already thinking (that your note above alluded to).

03-03-04 *Title and Abstract* (to UBC)

Below is the title and abstract for my talk next week. If there's anything else I've neglected to send you, let me know.

> Title: Two Strangely Similar Problems in Quantum Information
>
> Abstract: The deepest question one can ask in quantum information and computing is what makes it tick, what gives it its power? The field is still new enough that no one really knows the answer—speculations range across the board, from quantum coherence to quantum entanglement to parallel worlds, and to stranger things still. My own hunch is that the source of the power comes from the particular structure of quantum mechanical measurements, with entanglement being a secondary effect that derives from this source. In this talk, I will demonstrate a few tools for exploring that hunch. One is a representation of quantum measurements from the standpoint of probability theory. Another one is connected to a question that first arose in the context of developing criteria for certifying the success and failure of quantum teleportation experiments.

> Strangely enough, two open questions with regard to these tools seem to have the same answer.

04-03-04 *Curl Activator* (to N. D. Mermin)

Merminition 150: *Don't know if you wanted comments on the opening serenade of your paper with Rüdiger, but here are a few:*

- (a) The Bayesian view of quantum states is that it is not: The quantum state is not something the system itself possesses.
- (b) What distinguishes this view from a more traditional "Copenhagen-interpretation style" view is that there is no pretense that a quantum state represents a physical fact.

"this view" in (b) seems to refer to (a). But it's a non-trivial jump from not being something the system itself possesses, to not representing a physical fact. E.g. the state of a quantum computer represents the initialize procedure (measure all the qubits and apply NOT to those that register 1) and the sequence of gates that have been applied. While this history is not "possessed by" the qubits (since it can't be recovered from them) it's a big step to say that the history (and the quantum state it gives rise to) is not a physical fact. (Namely your denial that gates [hamiltonians] are objective, or that what has been measured [as opposed to the measurement outcome] is objective.)

> It is the outcomes of quantum measurements that represent physical facts within quantum theory, not the quantum states.

Repeating myself, shouldn't you acknowledge here that although the outcomes of measurements represent physical facts, what it is that has been measured is not, in your view, a physical fact.

> In particular, there is no fact of nature to prohibit two different agents from using distinct pure states for a single system. [Footnote: Contrast this to the treatment of Refs. Mermin2001 and Brun2001b.]

Please cite Mermin2002 (J. Math. Phys., **43***, 4560-66) where the argument is (in response to you guys) at least explicitly made contingent on the assumption that probability 0 means objective impossibility (and, I believe, no other assumption).*

So, my words did curl your hair! Makes me proud.

Thanks for the comments. Rüdiger and I had a nice time on the phone discussing them this evening. The main thing it helped us realize is that we've gotta, gotta, gotta get that "On Quantum Certainty" paper written for you.

Anyway, the first main comment didn't cause us to make a change in the draft: The introduction wasn't the place to defend the view; we just wanted to state the view.

About Mermin2002, I've included it in the citations. I've never actually read the paper though (the dangers of not putting something on `quant-ph`). Could you send me the file?

Concerning this one:

Merminition 151:

> In any case, this does not imply that a single agent can believe willy-nilly in anything he wishes. To quote D. M. Appleby, "You know, it is *really* hard to believe something you don't actually believe." Difficult though this may be to accept for someone trained in the traditional presentation of quantum mechanics, the only thing it demonstrates is a careful distinction between the terms *belief* and *fact*.

It's not clear what "this" refers to. From the syntax alone it would appear to be Appleby's remarks. This is reinforced by the "believe" (Appleby) and "belief" (you). But I assume you have in mind some or all of what you have to say before Appleby appeared on the scene.

I think you got confused by not noticing that the "Difficult though this ..." sentence, was outside of the footnote. If you ignore the footnote it works fine. Still though I did adjust the words slightly, just in case the reader loses track after a diversion to the footnote.

Just in case you're curious, in the next email, I'll send you the completed paper. I'll have to send it as a pdf file though, so that you get the figures. As I told you before, it's pretty much a throw-away paper, using as it does nothing but old technical material.

Still, you might enjoy the Introduction, the Concluding Remarks, and Section VI on "Subjectivity of Quantum Operations." They package the story in a way you may not have seen before.

Of use to me would be to know how the "stimulus-response" imagery at the end strikes you.

> Is there something in nature even when there are no observers or agents about? At the practical level, it would seem hard to deny this, and neither of the authors wish to be viewed as doing so. The world persists without the observer—there is no doubt in either of our minds about that. But then, does that require that two of the most celebrated elements (namely, quantum states and operations) in quantum theory—our best, most all-encompassing scientific theory to date—must be viewed as objective, agent-independent constructs? There is no reason to do so, we say. In fact, we think there is everything to be gained from carefully delineating which part of the structure of quantum theory is about the world and which part is about the agent's interface with the world.
>
> From this perspective, much—*but not all*—of quantum mechanics is about disciplined uncertainty accounting, just as Bayesian probability theory in general. Bernardo and Smith write this of Bayesian theory,
>
>> What is the nature and scope of Bayesian Statistics ... ?
>>
>> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."
>
> In fact, one might go further and say of quantum theory, that in those cases where it is not just Bayesian probability theory full stop, it is a theory of stimulation and response. The agent, through the process of quantum measurement stimulates the world external to himself. The world, in return, stimulates a response in the agent that is quantified by a change in his beliefs—i.e., by a change from a prior to a posterior quantum state. Somewhere in the structure of those belief changes lies quantum theory's most direct statement about what we believe of the world as it is without agents.

I think it's a particularly crisp way of expressing the program. Anyway, it's all perfectly tame as written down, but the phrases arose out of a discussion during my recent visit to the Oxford philosophy department, where I was extolling the virtues of the "sexual interpretation of quantum mechanics." Caught up in the moment, I said something like: "Mermin's got this thing about 'correlation without correlata', but what I'm looking for in my quantum foundations program is 'stimulation without stimulata'!" It was nonsense and didn't fit, but we had a good laugh … and the word 'stimulate' stuck with me, waiting to rise again on a more legitimate occasion.

04-03-04 *Catalog of Probabilities* (to J. N. Butterfield)

I can't quite remember if it was you that I was talking to after Michael Redhead's talk at LSE about this. … But in any case, I'll get these quotes into my computer this way.

In Michael's talk, he implied (or rather said outright) that he introduced the term "catalog of probabilities" for quantum states. I thought it was a much older term, probably going back to Pauli, but I haven't been able to confirm that. Anyway, I just ran across these quotes of Schrödinger in a Brukner/Zeilinger paper; they were taken from S's 1935 papers on entanglement. They quote the German and offer these as translations. I'm not sure if these are their translations or, rather, they are the translations in the Wheeler/Zurek book.

> For each measurement one is required to ascribe to the ψ-function (= the prediction catalog) a characteristic, quite sudden change, which depends on the measurement result obtained, and so cannot be foreseen; from which alone it is already quite clear that this second kind of change of the ψ-function has nothing whatever in common with its orderly development between two measurements. The abrupt change by measurement … is the most interesting point of the entire theory. It is precisely the point that demands the break with naive realism. For this reason one cannot put the ψ-function directly in place of the model or of the physical thing. And indeed not because one might never dare impute abrupt unforeseen changes to a physical thing or to a model, but because in the realism point of view observation is a natural process like any other and cannot per se bring about an interruption of the orderly flow of natural events.

and

> Whenever one has a complete expectation-catalog — a maximum total knowledge — a psi-function — for two completely separated bodies …

These don't explicitly contradict Michael, as they don't use the word "probability," but they certainly convey the idea that a quantum state is catalog of something.

Anyway, words, just words — not something particularly important. But I like to get the history of QM straight when I can.

06-03-04 *Receipt* (to G. Bacciagaluppi)

Thanks for the long detailed note! Now that's the kind of note I like to see!!

I hope to get back to you with something of substance before my physical arrival in Freiburg.

Since you brought up de Finetti, let me attach our latest concoction on the man. Rüdiger and I just sent it off yesterday for a review book on quantum-state estimation. It's mostly a throw-away paper with no new technical work, but I did use some new forms of expression in the Introduction and Conclusion sections, and also Section VI on the subjectivity of quantum operations. In particular, I managed to clean up "stimulation without stimulata" enough, as to make it usable in the public forum. Hope you enjoy.

06-03-04 *Lost Cousins of de Finetti* (to G. Bacciagaluppi)

Actually, I need to cite your paper "Classical Extensions, Classical Representations and Bayesian Updating in Quantum Mechanics" in the paper I just sent you. Can you send me the citation information on that? I didn't find it posted anywhere on `quant-ph`. Did you post it somewhere else? Anyway, I'll get a chance to fix things up when the proofs come back to me next week.

You really should post it on `quant-ph`! (If you can post it fast enough, I'll get a chance to cite it in a way that people can find it.)

06-03-04 *Cutting the Umbilical* (to A. Plotnitsky)

Please allow me to attach a paper that Rüdiger and I have just finished. [See C. A. Fuchs and R. Schack, "Unknown Quantum States and Operations, a Bayesian View," `quant-ph/0404156`.] It's mostly a regurgitation of old material—it was put together for a review volume on quantum-state estimation—but there are some new means of expression that may strike your sensibilities, or at least put you in a better position to contrast us with Bohr, Heisenberg, and even Pauli. I'm thinking in particular here about the Introduction and Conclusion sections, and also Section VI on the subjectivity of quantum operations. You'll understand why I titled this note "Cutting the Umbilical" when you read the first sentence of the second paragraph in the paper. I don't know whether Ref. 23 still expresses Asher's point of view today—he has probably backed off on that slightly—but given your interest in the term "quantum state", and how it should be best used, I thought it might be worthwhile to point you in the direction.

06-03-04 *Darwinism Down* (to M. Pérez-Suárez)

Attached is the samizdat "Darwinism All the Way Down" to the extent that it's been put together—a lot of material is still missing.

Anyway, the great mistake is recorded on page 9. The especially sad thing was how it contradicted what I was saying as early as the note "Identity Crisis" of 22 August 2001 (in "Phase Transition"), where I had already pretty much sorted everything out. It was as if I had forgotten everything of the previous year. Anyway, it was the solution to this problem which effectively forced me to a Wittgensteinian notion of certainty. (Though I only started reading Wittgenstein this January.)

07-03-04 *All Kinds of Veils* (to A. Peres)

Asherism 64: *I am absolutely elated by "my" discovery that there are no quantum states (and therefore no "problem of time" in quantum gravity, etc.). Heisenberg et al. used only algebras of operators to compute observable quantities. Then Schrödinger came and "stole the show"—and completely messed it up. He did that after Einstein called his attention to de Broglie's thesis, and wrote "he has partly lifted the great veil" or something like that. You are an inexhaustible source of references. Where is that "great veil" mentioned?*

I will look for the "great veil" reference tomorrow at the office, where I have a copy of Jammer's *The Philosophy of Quantum Mechanics*. It is a very thorough historical reference. Another place where you are likely to find it, and other interesting things, is a little book titled *Letters on Wave Mechanics*, edited by Karl Przibram, with letters by Einstein, Schrödinger, Lorentz, and maybe a couple of others. Your library might have it. I remember finding one letter particularly amazing within it. It was a letter written by Einstein to Schrödinger, in great excitement, after Einstein's first quick reading of Schrödinger's paper on the time independent Schrödinger equation. Einstein says something like "what a great insight!" but then he quotes Schrödinger's equation *incorrectly* and complains something like, "You will note that this equation has this and this and this undesirable property. On the other hand, if you had considered this equation [where Einstein now writes the correct Schrödinger equation as if he had never seen it], all of these problems will be fixed." But then the really lovely thing that Einstein says next is something like, "However, for the life of me, I can't think of a physical interpretation for the wave function that appears in this equation."

Unfortunately, my copy of the book was burned up in the fire, and I haven't replaced it since. It's a nice little book.

08-03-04 *The Great Veil* (to A. Peres)

Asherism 65: *Then Schrödinger came and "stole the show"—and completely messed it up. He did that after Einstein called his attention to de Broglie's thesis, and wrote "he has partly lifted the great veil" or something like that. You are an inexhaustible source of references. Where is that "great veil" mentioned?*

Apparently the quote is "lifted a corner of the great veil." However, I haven't been able to pin down the exact origin of it yet. You can at least read it here:

`http://www.aip.org/history/einstein/quantum1.htm`.

Jammer's book wasn't as much help as I thought it would be. For instance, I'm discovering how bad the index is. My method of looking was to cross-reference a) Einstein and de Broglie, and b) Einstein and Schrödinger. There are quite a few places where I found the names within the text, but no cataloguing within the index.

Actually, I just found this story:[76]

> Working on his doctorate in 1909, a young aristocrat, Prince Louis-Victor de Broglie, discovered a mathematical relationship between Planck's Constant and a yet to be observed wavelike property of moving masses. His examiners were of a mind to reject the paper, and wanting an outside opinion, sent a copy to Einstein who replied: "He has lifted the corner of a great veil." The dissertation was accepted—fifteen years later it earned de Broglie a Nobel Prize, the first ever awarded for an academic thesis.

08-03-04 *IC-POVM Entropies* (to A. H. Jaffe)

Thanks for the letter.

Jaffe-ism 1: *I hope you'll remember me — we met briefly in London and then Oxford — I asked you about the Shannon entropy of an informationally complete POVM but then had to go.*

I do remember talking to you in London right after my talk; you were to the left of the seat I was sitting in and wanted to know something about one of my transparencies. Unfortunately, though, I can't visualize your face. And I don't remember talking to you again in Oxford: Everything about that trip is a big blur!

[76] See H. A. Medicus, "Fifty Years of Matter Waves," Phys. Tod. **27**(2), 38 (1974) for more details.

Jaffe-ism 2: *My "Bayesian bona fides" can be indicated (I hope) by the fact that I reviewed Jaynes' book for* Science *last year.*

I'd very much like to read that. Could you send me an electronic copy of the article?

Jaffe-ism 3: *Anyway, on to something more specific. I've been thinking about the entropy of the probabilities related to a set of informationally complete POVMs. Obviously, it depends on the detailed set of POVMs chosen. But it seems that there are 'better' and 'worse' choices here — related, I wouldn't be surprised, but have not yet worked out — to the shape/volume of the accessible area of the probability simplex.*

There are probably a load of interesting questions one can ask here. For instance, given a density operator ρ, what is the minimum Shannon entropy a measurement can generate, subject only to the condition that the measurement be an IC-POVM. I suspect the answer is $S(\rho) + \log D$, where D is the Hilbert-space dimension, but I don't have a proof of this. In other words, the optimal POVM is just an infinitesimal variation of the basis that diagonalizes ρ, but having D^2 linearly independent outcome operators, rather than D.

Maybe a more interesting question is: What is the *largest* Shannon entropy, subject only to the constraint that the POVM be a minimal IC-POVM (i.e., it have precisely D^2 outcomes)? Again I don't know the answer, but I would bet it has something to with using *symmetric* IC-POVMs (at least if ρ is a pure state). (Actually it's probably obvious in the pure state case that the thing to do is align one of the outcomes of the SIC-POVM with the pure state.)

In general, the SIC-POVMs are the most interesting ones of the lot I think. You can read about them here:

> `quant-ph/0310075`:
> Title: Symmetric Informationally Complete Quantum Measurements
> Authors: Joseph M. Renes, Robin Blume-Kohout, A. J. Scott, Carlton M. Caves

It'd be an interesting project to say everything that can be said about the entropies such things can generate.

Jaffe-ism 4: *Of course, you can also use the entropy to assign distributions in the face of less-minimal information — i.e., known averages, as usual. But in this case we would expect that the results should depend on POVM, since that's what determines what the averages correspond to. Thinking about it a bit further, I imagine there's a further subtlety: there must be something subjective about the POVM itself. So if two different agents use maxent to assign a density matrix, under what circumstances will they get 'compatible' assignments (to use the language of your work).*

Over the last couple or three years, Caves, Schack and I have been steadily moving further away from our Jaynesian roots to a more Bernardo and Smith kind of Bayesianism. One of the upshots of this is a recognition that POVMs, too, have a subjective component (if they are not wholly subjective in the same way as any probability assignment – which is what I personally believe).

For your fun, let me attach the draft of a new paper by Schack and me. [See "Unknown Quantum States and Operations, a Bayesian View," `http://arxiv.org/abs/quant-ph/0404156`.] (It's effectively complete, except that we'll add a couple of footnotes and references at proof time.) Mostly it's a throw-away paper in that it has almost no new technical results; it was put together for a review volume on quantum-state estimation. However, the Introduction, Concluding Remarks, and Section VI on the subjectivity of quantum operations, may give you some insight on where this program is going.

Good luck in your work, and I'm very happy to meet you. There are so many things that need to be done to complete this Bayesian view of QM.

09-03-04 *Alchemy Quote* (to M. Pérez-Suárez)

Thanks for the quote. See you later today.

Marcos's Preply

I should be writing equations, but I find myself writing only numbers. I'll take this exhaustingly boring exercise as a excuse for making a remark on my not well articulated answer to your question about the alchemists' psycho-physical parallelism. One of the leading ideas (I'd say, that THE core of the subject) in alchemy, leading back to the (mythological?) Hermes Trismegistus (three times magister), is precisely this (translated from the supposedly original):

> It is true without lie, certain and most veritable, that what is below is like what is above and that what is above is like what is below, [...]

I have fortunately included this as a quote in one of the chapters for those Lecture Notes I have been writing. I'm afraid that most alchemical texts are so full of symbolism and esoteric that only an "initiate" (that is, someone already acquainted with this imagery, like Jung, for instance) can make any sense of them. But this is "the point" here: The Macrocosm (the Universe, if you want) is "in a likeness" (well, more than that, I'd say) to the Microcosm (the psyche), and they are intimately related to each other so that processes in one of the two domains have their correspondences in the other.

Hope that this provides further insight in Pauli's involvement with alchemical notions.

09-03-04 *Reality of Wives, Part 2* (to J. Preskill)

I remember how much you seemed to enjoy my story a couple of years ago about the reality of wives (in a note to you and Landahl). [See 21-07-01 note "The Reality of Wives" to A. J. Landahl and J. Preskill.] Attached is a throw-away paper that Rüdiger and I just sent off for a collection of review papers on quantum-state estimation—they wanted one from a Bayesian too. I say it's throw-away because there are no new technical results, outside of a little foundational argument; predominantly it's a cut and paste job. (We probably won't post it on `quant-ph`.) Nevertheless I send it because you might still get something out of reading the Introduction, Conclusions, and Section VI on the subjectivity of quantum operations. They indicate a little of what I've been on about when I previously mentioned the issue in connection to the black-hole information problem. The main point is that, to us, a quantum operation is to a conditional probability distribution what a quantum state is to an unconditioned distribution. To the extent that a Bayesian does not demand a mechanical or dynamical explanation for a quantum state assignment, he need not (should not) demand one for a quantum operation either.

09-03-04 *Bitbol* (to A. Plotnitsky)

Plotnitskyism 13: *I know Bitbol's work—he has a book on Schrödinger. I doubt I'd have a chance to see him this time (too tight and too late to arrange), but I am back to France in May/June for a longer time, and perhaps I should write him, mentioning that you suggested this (with your permission, of course).*

Sure, no problem. Bitbol's later work has turned, I would say, toward more Copenhagenian styles of interpreting quantum mechanics. Here's his web page:

`http://perso.wanadoo.fr/michel.bitbol/page.garde.liste.html`.

Most of his papers are in French, but a few are in English. I particularly enjoyed his "Non-representationalist theories of knowledge and quantum mechanics."

12-03-04 *Your Post-Partum* (to G. Brassard)

I had a chance to read your report on our meetings. It was great and to the point.

Let me attach my own latest concoction: It is a contribution (quickly) pasted together for a review volume on quantum state estimation. They wanted one article from a Bayesian point of view. So, there's not a lot of new technical material there, but I think you might enjoy reading the Introduction, Conclusions, and Section VI on the subjectivity of quantum operations. Particularly, I hope you enjoy the concluding remarks. It's the closest I've come yet to declaring the "sexual interpretation of quantum mechanics" in the public forum. The choice of the word "stimulation" in that section shouldn't be lost on you! (Key point to notice: In opposition to Mermin's desire for 'correlation without correlata', there's no use to talk about the 'stimulation' unless you're also going to talk about the 'stimulata'!)

So the queue to the construction of our joint paper is getting shorter. Upon my return to Dublin from Vancouver, I've got five days to put something together for the Holevo festschrift (I'm 3/4 way there already). And then I'll get to work on our project. As I say, the editor seems to be particularly tolerant on us ...

12-03-04 *Putting It Sharply* (to W. G. Unruh)

I'm afraid I wasn't very clear yesterday about why I consider measurement the crucial component of quantum theory. Let me attach an article Rüdiger Schack and I just put together for a review volume on quantum state estimation. I think the first three or four paragraphs in the introduction, plus the 'concluding remarks' section, make our crispest statement yet of the interpretation of quantum mechanics we're shooting for (and distinguishes it from a certain thread in all the various 'Copenhagen interpretations' I know).

Stimulation and response, and maybe a little creation in the process. Let me know if the paragraphs clarify anything. (And one day soon, I'll tell you more about how Section VI on the subjectivity of quantum operations impinges on the black-hole information problem.)

14-03-04 *The House Philosopher, 2* (to S. Savitt)

Thanks again for dinner, the nice visit to your home, and the pleasant conversation about Goodman. (I'll definitely plunge more carefully into Goodman.)

For the heck of it, let me go ahead and attach the proposals I wrote for Caltech. (I'll just forward an old letter.) [See 12-02-04 note "The House Philosopher" to J. Preskill and H. Mabuchi.] The proposals had to be short (one and two pages), but maybe they still give a glimpse of the kind of program I'd put together if I lived in the best of all possible worlds. But I'm not fooling myself: The dice have only a very, very slim chance of falling in that direction.

I'll keep in touch with you about things. Have a great trip to merry old England.

15-03-04 *Via Alaska Airlines* (to D. M. Appleby)

I apologize for the long delay, but I've finally read your paper on probability. It happened on my flight from Vancouver to Los Angeles the last few hours. Now I'm in the Aer Lingus lounge in LAX, waiting to make the rest of my way home to Dublin.

It's a good paper! What parts were you afraid I would disagree with? I think you did the community a great service with the paper, and I'll try to advertise it as best I can. I particularly enjoyed your argumentation in Section 6 on retrodictive inferences.

A few very minor remarks.

1. Of the paragraph in Section 1 starting, "Hume famously argued that one ... ", I wrote, "Good way of saying it."

2. Of the sentence in the same section, "On the face of it, taking an epistemic view of the state vector amounts to giving up on the idea of physical reality altogether," I wrote, "Yuck." Rather than "on the face of it" you might have written "ostensibly" or "at first pass". I guess "on the face of it" conveys the idea somewhat, but I fear the reader may not immediately see that you're being rhetorical. You do, in fact, recover with:

3. "However, I feel it may be consistent with a much more subtle and interesting kind of realism ... " to which I wrote "Better."

4. In Section 4, where you say, "For instance half-lives are typically tabulated next to masses, as if they were just one more physical property," I wrote "Interesting!" That is a good point. However,

5. from that point onward, all the way to the end of the section, I could not understand what you were getting at.

6. At section 9, I wrote "Utilities!!" I think you could do a lot better in general in that discussion.

7. Finally Section 12 was far too abrupt! You had me licking my lips and then left me hanging. (It looks a little like you were exhausted by the time you got to the last section and, so, wrapped it up pretty quickly.) I was tantalized by this comparison of probabilities to qualia—I had never seen that before—and I wanted to see how you developed the point. Also, despite the mention of Bohr in the last two paragraphs, you never did come back to the point of a "much more subtle and interesting kind of realism."

All very minor points.

In an earlier note, you wrote me,

Applebyism 7: *It is all about classical probability—coins, and the like. I do appreciate that quantum probabilities are fascinatingly and intriguingly different. And that is what I am going to start thinking about now.*

I don't think you'll find any consideration in your paper that is changed in the quantum setting. ("Where did you make use of the difference between classical and quantum?," I ask rhetorically.) At least as long as you are talking about actually performed trials—rather than counterfactually performed trials (as in my bureau of standards)—I think everything will be OK. In particular if we look at things in the right way, I suspect the only difference between classical and quantum will be in their notions of event, not in their notions of probability. In the quantum case, the events are direct consequences of the agent himself (via his conjugal relations with the external world)—stimulandum, stimulation, stimulata. (Correlation without correlata? No! Rather, no stimulation without stimulata! No stimulation in the agent without a corresponding stimulation to the external world.) Somehow, taking that into account in the agent's reasoning leads to an apparent modification of the probability calculus with respect to counterfactual measurements (i.e., how he updates his beliefs about the bureau of standards).

By the way, concerning,

Applebyism 8: *Matthew thinks that he cannot pick up a glass of wine. Only an American pragmatist could be so simple-minded—so horribly unsophisticated—as to imagine that it is possible to genuinely do anything.*

thank you! It's been a long time since I've been proud to be an American!

All the best, and it really is a fine paper. Thanks for writing it.

PS. Let me attach the latest concoction Schack and I brewed; it's something we haven't posted yet. There's not much new in it—it was a quickly pasted together article for a review volume on quantum state estimation—however you might still enjoy reading the introduction, conclusion, and Section 6 on the subjectivity of quantum operations. In particular, I'm kind of proud of the language choices I made in the first three paragraphs or so, and also in the concluding section. I really want to rearrange how we think of quantum theory—from a big theory of everything to a little theory from the inside. A theory of stimulation and response, and maybe a little creation in the process. It's the latter part of that sentence that gives me the will to keep working at this.

15-03-04 *Via Aer Lingus* (to D. M. Appleby)

I also reread your note "Poetry, subjectivity, mirroring and algorithms" of 11/19/2002, which has been sitting in the "must reply to" box in my email program since then. It's a great note, and I loved its construction, but I still don't know how to reply. Hence, I think I'm going to file it into the regular "Appleby, Marcus" box at this point.

In your "Facts, Values and Quanta" you say "a probability statement cannot be identified with a fact about the world, as it exists independently of us." I think that is all I have ever meant by "subjective." But, then again, maybe that's just hindsight playing a trick on me.

16-03-04 *Bayes or Bust* (to A. H. Jaffe)

Thanks for sending your review. It went down well with my coffee this morning. It's quite nice.

Jaffe-ism 5: *Conversely I suppose I share some of Jaynes's suspicion of the fundamental status of Dutch book-type arguments, although I do think they tell you something — but perhaps not everything — about the meaning of probability assignments once you've derived the rules by, for example, the Cox derivation.*

I hope I haven't given the impression in my writings (or the writings with Caves and Schack) that I think the Dutch-book argument is the end-all and be-all of probability theory. I'm quite a bit more liberal than that. The better go at it, I think, is given by a decision theoretic derivation, say, like the one in Bernardo and Smith. Cox, on the other hand, is nice, but I personally have never found it nearly as convincing as decision theoretic ideas. . . . Well, that's not true: I did once, but somewhere in 1994 I got suspicious of it and started to think its assumptions aren't nearly as compelling as Jaynes finds them. (If I can recall my particular arguments, eventually I'll write them up and send them to you.) Maybe my opinion is more like Earman's in his book *Bayes or Bust*: No single Bayesian style argument for the probability calculus is completely compelling, but the whole package taken together lends quite some evidence that everything is on the right track.

16-03-04 *The IQSA Meeting* (to F. E. Schroeck)

Thanks for the invitation to the IQSA meeting. I wish I could be there, but I think now I'm so over-committed that I ought to make a decision not to get myself in any deeper. I'll have just gotten home from a meeting in Waterloo at the time your meeting starts up: My semi-constant absence is pretty tough on the family.

But good luck to you; it sounds like a great meeting. Beside Barnum, another few people you might try out of the quantum information community are Jeff Bub (`jbub@carnap.umd.edu`), Lucien Hardy (`lhardy@perimeterinstitute.ca`), and Rob Spekkens (`rspekkens@perimeterinstitute.ca`). Spekkens in particular has done some really exciting work lately, which you can find on `quant-ph`.

18-03-04 *Hubert Space* (to R. Schack)

Damned editors. They are so very worthless. I'll look at the proofs in the next couple of days. Did you notice they changed one of our Hilbert spaces to a Hubert space!

18-03-04 *ABL, Appleby, and Grue* (to S. Savitt)

Savittism 7: *Second, it raises a question in my mind. Bill (Unruh) insists that the right way to think of QM, to say what the theory is, is the time-symmetric ABL formalism. Might it not be interesting to figure out whether Paul Humphrey's criticism of the propensity view would or would not apply to the probabilities needed for ABL if you tried to think of them as propensities?*

Good question. Off the cuff, though, I'd bet the same criticism applies: the main issue, I think, is about the difference (or whether there should be no difference) between predictive probabilities and inferential probabilities. One might think of the ABL rule as giving propensities, but then one has to account for what one might mean by an "unknown ABL probability." At that point, one invokes inferential probabilities, which are clearly epistemic. So, one either ends up with a mixed theory (propensities and epistemic probabilities, which by a miracle obey the same formalism), or one says, "this is good evidence once again for a purely epistemic view of probabilities."

But that's off the cuff, and I'll certainly think about the issue further.

By the way, interesting coincidence. On my flight home from Vancouver, I fulfilled an obligation to read Marcus Appleby's paper "Facts, Values and Quanta," which is really mostly devoted to the interpretation of probability (outside of quantum issues). Here's the link: `http://www.arxiv.org/abs/quant-ph/0402015`. If you can get past the sickly sweet introduction (praising some guy named Fuchs), it's not a bad paper. There are some novel arguments in there that I liked. In particular—and the reason I say "interesting coincidence"—at one point he uses a modification of Goodman's grue to discredit frequency interpretations. Given a couple of your remarks in our conversation the other night about "predicted frequencies being seen," you might find reading the paper useful, at least as some food for thought.

24-03-04 *Give Me Fungible* (to C. H. Bennett)

Can you give me a citation to the first time where you called quantum information fungible? Better yet, can you *also* send me the text of the paper?

25-03-04 *Reining in My Life* (to G. Brassard)

I'm finally beginning to rein my life back in. I finally finished the Holevo festschrift and sent it off. [See "On the Quantumness of a Hilbert Space," `http://arxiv.org/abs/quant-ph/0404122`.] If you can, let me know what you think about the Introduction, Conclusions, and Eqs. (13) and (14). More importantly, can you think of anything in quantum computing that would bolster the vision (or the hope) expressed in Paragraph 5 of Section 1.

Now, I finally start the process of writing our joint paper. It is brewing. I'll send you the thoughts as they come next week. Part of it will certainly have to do with the content of the Holevo paper: It is quantum crypto that powers quantum computation! (I like pipe dreams.)

By the way, when are you going to take me to that Greek seafood place in Montréal again?

25-03-04 *Hot Off the Presses* (to P. C. E. Stamp & W. G. Unruh)

Attached is a paper I just sent off to the editors of the Holevo festschrift. [See "On the Quantumness of a Hilbert Space," `http://arxiv.org/abs/quant-ph/0404122`.] It contains the material I was hoping to get to for the second part of my talk in Vancouver. The fifth paragraph of the introduction, in particular, tries to say (in a sentence) what I think all of this has to do with the source of power of quantum computing—something I was hoping to play up even further with you guys, but I ran out of time.

Anyway, I hope you enjoy the verbiage, if not the equations.

26-03-04 *So Slow Chris* (to A. Peres)

I apologize for being so slow to reply. I have been hurriedly trying to finish my paper for the Holevo festschrift, and I pretty much dropped all else to get it done. I will attach the LaTeX file to the present note: Perhaps the introduction and conclusion sections will amuse you. (I haven't posted it yet on `quant-ph`; I'll probably do so Monday before departing for Germany myself.)

Asherism 66: *I have read again your* `quant-ph/0205039`*, and figures 1 and 2 reminded me of my paper "Convex probability domain ..." with Danny Terno, JPA* **31** *(1998) L671 [*`quant-ph/9806024`*]. Is there any relation?*

Yes, there absolutely is. It was quite an oversight on my part not to cite your paper there. (In fact, I thought I had until I received this from you.) It was because of your paper with Danny that I decided to make the "bureau of standards" measurement have precisely D^2 outcomes. If the measurement had any more than D^2 outcomes, say N, then the allowed region would have zero volume in the N-simplex, as you and Danny show. I have been meaning to repost that paper with various fix-ups. This will surely be one of them. (There, I just fixed it actually.)

What I think is the next important step is to say more about the geometric properties of these convex sets. For instance, with regard to qubits, the convex region is actually always an ellipsoid (no matter what the bureau-of-standards measurement). The ellipsoid varies from measurement to measurement, but it is always an ellipsoid. One question is, what is the proper characterization of the region in higher dimensions? What are the invariant geometrical features? And I can think of several questions beyond that. Would you be interested in collaborating on this?

I am glad to hear you survived your voyage back to Germany and did not come back with a renewed bitter taste. I very much enjoyed reading the (partial) autobiography you sent me. You have had a stirring and very impressive life.

26-03-04 *Nonuniqueness from Nonexistence* (to A. Peres)

By the way, I've been meaning to tell you how pleased I am of your embracing the point that "quantum states do not exist" in your quantum gravitational research.

To that end, let me attach another paper of mine—this one with Schack. There is not a lot new in it by way of technicality. It was effectively pasted together from old publications for the purpose of a review volume on "quantum state estimation," however, we reworked the language in the old articles significantly to take into account the more consistent view that we now have. The main parts that will be relevant to you are the introduction, the conclusions, and Section 6 on "The Subjectivity of Quantum Operations."

If quantum states do not exist (even pure states), then they surely cannot be unique (even pure states).

You will note how I used your 1984 paper "What Is a State Vector?" as a point of contrast for our present view. I hope you will not think that I was picking on you. The reason I singled out your paper is because it is the very clearest statement on the subject. If you think, however, that I should change any of the language before posting it, please let me know. For instance, I did not attempt to contrast your 2004 view with your 1984 view—they may be significantly different!! (And I would guess they are.) Unfortunately, it may be too late for the version appearing in the volume: I was in such a rush sending off the draft—and returning the proofs—that the possible sensitivity of my statement didn't dawn on me at the time. But it will not be too late for `quant-ph`; please let me know.

26-03-04 *Hot Off the Press; Closing My File* (to J. Preskill)

Attached is a paper I just sent off to the editor of the Holevo festschrift. [See `http://arxiv.org/abs/quant-ph/0404122`.] I thought I might send it to you too. It's the last remaining thing on my CV that's not publicly available. With it you can close my ITS file . . .

I remember your asking me during my visit to Caltech in October, "Remind me why this symmetric POVM is interesting?" The present paper is an attempt to answer that. I.e., it's because I think these structures lie closer to the heart of what Hilbert space is than do orthogonal sets. In particular, I'm banking that "No" will be the answer to the open question in Section 6, and that the SIC ensembles will be the only minimal sets giving Q_d. But I could be wrong.

If you want to throw in on the open question, I'd be grateful for any ideas (or even the flat-out solution). Chris King and I spent a solid couple of weeks on it last summer, but didn't make any great progress.

26-03-04 *Thanks* (to M. Pérez-Suárez)

I just wanted to say quickly (before I start to enjoy my Friday night beer) that I enjoyed our session together today. It was stimulating. Great to see you asking questions like that that are on the edge: I.e., what can we glean from this Bayesian program? How does this piece link with that piece? So forth. Keep it up! That's what a researcher is: A big bundle of questions.

05-04-04 *Elevator Stories* (to W. K. Wootters)

I'm glad you are following through with a contribution to Asher's festschrift. I'm still waiting on three other people too, so you're not the last one. ... But the faster you can get it to me, the better! (There are always further uncertainties with referees, etc., etc.)

I'm in Munich at the moment, taking a little holiday with Kiki and the kids at Kiki's parents' place. A couple of days before that, however, I was in Freiburg visiting Guido Bacciagaluppi and Harald Atmanspacher and was lucky enough to run across a recent issue of Foundations of Physics at their institute: It had your paper "Why Things Fall" in it, which I had not heard of before! Very nice. I got a chance to read it this morning.

In that regard, maybe let me attach two of my own papers that haven't made their way onto `quant-ph` yet. The first, "Unknown States and Operations, a Bayesian View," is a kind of throw-away in that it doesn't contain really any new technical results—it was pasted together for a review volume on quantum tomography—but you might get something out of the introduction, conclusions, and Section 6 on the "subjectivity of operations" (which are new) that I haven't yet been able to adequately express to you. In particular, I hope the choice of words there will better help you see how I am starting to view quantum mechanics as a whole—i.e., as predominantly a theory "from the inside." As I see it, it is a theory that helps us gamble on our little parts in the act of creation. But I also hope the argument in Section 6 will help you appreciate why I am starting to take such an austere position about what is "real of a quantum system"—little more (and maybe no more) than its dimensionality.

In the tradeoff though, I hope this austerity provokes an image of another austerity in physics for you: The equivalence principle. And that's where the elevator stories come in, and my pleasure this morning in having read your article. With "elevator stories," I'm referring to my second attached article, "On the Quantumness of a Hilbert Space." You'll see what I'm talking about if you open it up.

What I didn't go so far as to mention there is that I am starting to wonder if Hilbert-space dimension is not so unlike gravitational mass as to actually BE gravitational mass. It's a wacky thought—almost surely wrong in detail, but maybe evocative of a fruitful direction of thought. At least I hope so. Q: "What's the ultimate limit to this system's sensitivity to quantum eavesdropping?" A: "I don't know, let's weigh it." Wouldn't something like that be cool?

Anyway, part of this thinking is motivated by Bekenstein-entropy-bound kinds of things. From a Bayesian point of view of QM, I can't see what the entropy bound could possibly mean but that: If one posits an E and an R for a system, at the same time as positing a Hilbert-space dimension above that allowed by the bound (assuming a completely mixed density operator), one is living in a state of sin—i.e., one is being incoherent in one way or the other. But part of the thinking is just motivated by a desire for even more austerity, full stop.

If you have any comments on the quantumness paper, let me know: It's still at a stage where it can be modified before posting.

05-04-04 *Gambling on Creation* (to W. K. Wootters)

Looking back over the note I just wrote you, I doubt you could understand what I meant by, "As I see it, it is a theory that helps us gamble on our little parts in the act of creation." Let me try to make some sense of that by attaching a couple of old emails. Both are written in a polemical style, but if you can ignore that they might help draw a picture.

06-04-04 *Participatory Universe* (to Z. D. Walton & T. Toffoli)

I'm just starting to understand your paper. I hope you'll forgive me if it takes a while. I am certainly sympathetic to your goal: "Derive quantum mechanics from the premise that we live in a participatory universe." What a great thing it would be!

In the meantime, maybe you guys would enjoy (as a little "bathroom reading") something I put together a while ago: It's titled *Notes on a Paulian Idea*, `quant-ph/0105039`. The thing I call "the Paulian idea" is the idea that the (quantum) observer cannot be detached from the phenomena he *helps* bring about. If you'd like the file bound as a (paperback) book, let me know, and I'll have a copy (or copies) of the Växjö University Press edition sent to you.

I also have a semi-historical document I'm putting together: "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature". Presently, it stands at 124 pages of quotes drawn from (some fraction of) the 459 references it cites. I'm hoping to post it next spring, but I've still got quite a lot of work to do to get it into shape before then. (It'll probably double, if not triple in size before then.) In any case, though, if you're interested in seeing it as it stands, I'll send you a PDF file of the draft. (As payment, I only request that you compile and point out to me any typos you find in it.)

15-04-04 *Slowly Coming Back* (to S. Hartmann)

Slowly I'm coming back to speed on my backlog of work. I decided to stay in Munich until the 14th (yesterday) and after Freiburg, I almost didn't do a thing work-wise. In an impromptu way, I decided to take a real vacation (not something I do very often)—I neither read nor wrote almost any email!

Tomorrow I'll write Caves and Schack about your proposal of pitching in on the "Being Bayesian in a Quantum World" conference. I'm quite thrilled about the idea. In the meantime, let me paste in the list I had drawn up of potential participants to give you a firmer idea of the sort of thing I had been having in mind.

By the way, I looked at Hagar's paper to the level of being able to find the part you told me about: "Motivated in this way Fuchs then goes on to 'trash' (Fuchs' own description of his attempt to understand QM) about as much of QM as he can . . . " I thought he could only be talking about my lines in `quant-ph/0205039` where I say:

> The task is not to make sense of the quantum axioms by heaping more structure, more definitions, more science-fiction imagery on top of them, but to throw them away wholesale and start afresh. We should be relentless in asking ourselves: From what deep *physical* principles might we *derive* this exquisite mathematical structure? Those principles should be crisp; they should be compelling. They should stir the soul. When I was in junior high school, I sat down with Martin Gardner's book *Relativity for the Million* and came away with an understanding of the subject that sustains me today: The concepts were strange, but they were clear enough that I could get a grasp on them knowing little more mathematics than simple arithmetic. One should expect no less for a proper foundation to quantum theory. Until we can explain quantum theory's *essence* to a junior-high-school or high-school student and have them walk away with a deep, lasting memory, we will have not understood a thing about the quantum foundations.

> So, throw the existing axioms of quantum mechanics away and start afresh! But how to proceed? I myself see no alternative but to contemplate deep and hard the tasks, the techniques, and the implications of quantum information theory. The reason is simple, and I think inescapable. Quantum mechanics has always been about information. It is just that the physics community has somehow forgotten this.

But indeed he was right. At the beginning of the "Intermission" section, I write:

> Let us take a deep breath. Up until now I have tried to trash about as much quantum mechanics as I could, and I know that takes a toll—it has taken one on me. Section 3 argued that quantum states—whatever they are—cannot be objective entities. Section 4 argued that there is nothing sacred about the quantum probability rule and that the best way to think of a quantum state is as a state of belief about what *would* happen if one were to ever approach a standard measurement device locked away in a vault in Paris. Section 5 argued that even our hallowed quantum entanglement is a secondary and subjective effect. ...
>
> Subjective. Subjective! Subjective!! It is a word that will not go away. But subjectivity is not something to be worshipped for its own sake. There are limits: The last thing we need is a bloodbath of deconstruction. At the end of the day, there had better be some term, some element in quantum theory that stands for the objective, or we might as well melt away and call this all a dream.
>
> I turn now to a more constructive phase.

It's good to have people out there watching you: they set you straight from time to time when you forget what you've written.

16-04-04 *What is the Difference between a Quantum Observer and a Weatherman?* (to A. J. R. Parker)

Thanks for your interest. You can find most of what I've written on quantum foundations posted at my website (link below). Further materials can be found at the `quant-ph` archive.

With regard to your query about a link between theories of consciousness and quantum foundations, I don't have any answers. The only tangential link I see between my own quantum foundational program and such things is that I see some affinity between the direction I'm turning toward and the American pragmatist tradition (particularly James, Dewey, and Schiller). In that connection, I bring up something the quantum computer scientist Scott Aaronson wrote me a while ago:

> PS. I remember you talked about William James in the samizdat ... Have you read the *Principles of Psychology*? I'm working through it now. I've decided to recommend it to people as "the most up-to-date, state-of-the-art book about consciousness" (without telling them the publication date).

Scott is a serious scientist (much more serious than I am). Thus it might be worth your while.

Good luck.

Andrew's Preply

> I note that you recently gave the above paper at All Soul's College Oxford.

I am a psychiatrist, but also studying for an MA in philosophy of mind. My interest is the nature of consciousness and whether quantum mechanics has serious implications for current neuroscientific and philosophical theories of consciousness. My current position is that it does … that is, it seems, if my understanding of what it is to be an "observer" is broadly correct.

The latter issue seems to be addressed head on by very few people in the field — although I have to admit, I have to skip over the equations given my non-physics/maths background. Perhaps there is an understanding growing which I am not aware of?

I wonder if your paper above may help, and whether it is available to send by e-mail.

19-04-04 *Part 2.1* (to D. M. Appleby)

I'm sorry; you've caught me at one of those times when I am in "bad correspondent" mode. (At the moment it looks like this is going to continue for a while—at least until I get my post-sabbatical plans settled.)

Presently, I guess I don't have much to say about your note except that I still don't like propensity, even as a view from somewhere. For it would mean—as far as I understand the term—that from my (or your or his or her, etc.) point of view nature has a certain tendency to this or that (quantifiable by the probability distribution each of us happens to use). I just think that terminology is misleading, attempting as it does to once again materialize probabilities, rather than let them stand for sheer ignorance or opinion. The only direction I see forward is for all of us to recognize starkly that the world owes us nothing. Regardless of our probability assignments, the world owes us nothing.

Beyond that, I also don't think I'm happy with the conception of physics as a *view* from somewhere. The term I protest is "view." Presently, I think it's all about survival, period, with the last bit of representationalism banished. I agree with the "somewhere-centeredness" you are striving for—though in my terms, for each of us, it is about MY survival (remember the note I sent you titled "Me, Me, Me"?)—but I would be hesitant to call that a "view." Do have a read of Rorty's "philosophical papers," Volume 1—I think he does a pretty good job of what I'm shooting for (what I'm shooting for in QM particularly, where I think the evidence is greater than what he's got to work with). But that's about all I can say at the moment.

Or maybe I can swipe a few (relevant) words from a recent note that Michel Bitbol wrote me. I'll place them below. Maybe they add a little gloss to what I'd like to say myself (if it were a better world with fewer time constraints).

Anyway, really, thanks for your long note. I hope I'm not offending with this short reply, but you're not alone: I've only been replying to small fraction (the most urgent) of my emails lately.

21-04-04 *From Glub to Snowflakes, Creation, and Construction* (to M. Bitbol)

Thanks for your thoughtful letter. I have very much enjoyed reading it (maybe five times now).

Bitbolism 1: *At this stage, I have to state my major point of disagreement with you: I suspect the remark you made about the "many-worlds interpretation" applies quite well to some of what you say. Let me explain this.*

You write: "What I find egocentric about the Everett point of view is the way it purports to be a means for us little finite beings to get outside the universe and imagine what it is doing as a whole." I deeply agree with you.

But, then, you begin to do essentially the same as the naive Everettian. You try to describe our situation in the world from a sort of vantage point. Here is the sentence in your paper that made me suspicious: "I think the solution is in nothing other than holding firmly—absolutely firmly—-to the belief that we, the scientific agents, are physical systems in essence and composition no different than much of the rest of the world. But if we do hold firmly to that—in a way that I do not see the Everettist as holding to it—we have to recognize that what we're doing in the game of science is swimming in the thick middle of things."

Of course, once again, I am delighted by the Pascalian metaphor of "swimming in the thick middle of things". But you seem to take it as more than a metaphor: a definite belief about (not to say a faithful description of) the world and our position in it. You write seriously that "we, the scientific agents, are *physical systems in essence and composition no different than much of the rest of the world". Isn't this a way of extrapolating one of our pragmatic-adaptative concepts, one of the concepts we need to swim with some success in the midst of the "glub" (here, the concept of a "physical system"), in order to describe everything including us as if it were seen from outside? I hear you saying something like "the world as I see it from my cosmic exile is made of physical systems and each one of us is one of these physical systems". But if we are "swimming" in the deep ocean of whatever we call "reality", we have absolutely no context-independent concept at our disposal, not even the very general meta-concept of "physical system". We must say that we ignore* everything *of the "thing in itself", including whether it is organized or not in a plurality of "physical systems". And we must therefore content ourselves with stating the formal conditions of our cognitive aptitudes (within it). This latter attitude is typical of the Kantian and neo-Kantian lineage of philosophy (when one gets rid of the foundational aspect of Kant himself and holds a pragmatic variety of Kantianism, as I do). I hope you'll recognize it as a radical variety of your view... I wonder whether you'll become a member of our radical club or rather decide to stick to your position as it stands.*

I don't know that I have an answer for you at the moment. (I hope you understand that all my efforts, all my writings, are of the groping variety—I have no final answers to anything. Nor do I hold any pretense of being consistent from one email or paper to the next. The only thing I can promise is that I do strive for consistency, and I welcome exercises like the one you've presented me.)

To make a start of an answer though, I think my usage of the term "physical system" is considerably more nuanced than you probably guess. (Though, it may not have been so nuanced at the time of my writing the anti-Växjö paper. Alternatively, I may have simply lapsed while I was writing those notes.) I say that it is nuanced, because I often toy with the idea that "physical systems" are agent-defined. You will find this idea probably first appearing in my writings in the chart on page 292 of *Notes on a Paulian Idea*. There I ask, "What is a quantum system?" And I reply to myself, "A line drawn in the sand." (It goes back to a 1995 letter to Greg Comer.)

Maybe I can do a better job of what I am thinking, though, by attaching a couple of other emails that I've been promoting recently. They're attached to this letter in a file titled `ForMarcus.pdf`. In the letter "Me, Me, Me" to Mermin and Schack within that collection, I give a definition of what I mean by "system." I hope that definition will make you think twice about the characterization of my views you gave above.

In general lately, I'm not even sure what I can make of the idea of "thing in itself"—it now sounds too static for what I'm trying to get at. To that end, let me attach another little compilation—this one titled `ForSlusher.pdf`. In particular, I hope the note within that titled "The World Is Under Construction" will help you better see what I am talking about.

Finally, let me paste below still another note to help muddy the waters. It is a string of earlier notes culminating in a few remarks to Jeff Bub; so in reading it linearly you will be traveling backward in time. Its relevance here is that I think you think when I utter the words "physical system" I am doing it in the sense that William James describes below as a case of "nothing but." But I sincerely hope that is not what I am doing.

Anyway, all of this is probably not the sort of thing you were expecting as a reply: It is quite roundabout. But I am trying to put three lines of thought (as expressed in the three collections mentioned above) into a consistent whole. If I can do that, I think it will count as something of a direct answer to your query.

Does any of this make sense to you? Or does it all look more like the ramblings of a crazy man?

21-04-04 *Essential Incompleteness* (to W. G. Demopoulos)

Thanks for sending me your paper "Some Remarks on Elementary Propositions and Partial Boolean Algebras." I've taken a shot at understanding it—i.e., I've read it by some measure—but I probably didn't fare as well as I should have.

As you argued in your earlier letter to me (one from last year sometime), our views—or maybe just our languages—may not be so incompatible as one might think. However, I am left with the feeling that this is only a contingent feature of the particular stages of the game we happen to be at, at the moment. In particular, from my own view, I think it is quite important that we strive to stop thinking of quantum states as states of knowledge about the TRUTH VALUE of this or that proposition (even if truth value is not invariant with respect to 'experimental arrangement'—the idea you are toying with). My feeling is that the imagery of measurement outcomes mapping to truth values (in this context anyway) will only cloud our vision for how to take the next big step.

What is the next big step? I think it is a deeper understanding of how—very literally—the world "is in the making" (to use a Jamesian phrase). To try to make that idea at least graspable (if not either clear or consistent yet), and to try to show you quantum theory's role in all this, let me attach four letters I've written recently. They're contained within the attached files `ForMarcus.pdf` and `ForSlusher.pdf`. I think they are my best statements to date of what I am shooting for; and I think that goal fundamentally conflicts with the idea of "measurement" propositions having truth values in the conventional sense.

That is not to say, however, that I am yet ready to give up on the idea of physical systems having autonomous properties. The question is, what can still be pinned down as a property in the conventional sense?

In the letter you wrote me way back you said,

Demopoulosism 1: *What I've tried to address in my paper is the question whether there is anything in the quantum theory's conceptual framework that plays a role analogous to that played by a classical state. My suggestion is that the closest analog of this is given by the elementary physical propositions that are true of the system. They, rather than the quantum state, expresses the underlying reality that the theory purports to describe.*

As I said above, that's where I don't want to go. Instead, at least at the moment, the only thing I am willing to think of in quantum theory that "plays a role analogous to that played by a classical state" is a system's dimensionality. [Question: What is this system's ontic state? Answer: D, just D.] Here's the way I put it in the paper "On the Quantumness of a Hilbert Space" which will be appearing on `quant-ph` tomorrow:

> In this paper, I present some results that take their *motivation* (though not necessarily their interpretation) in a different point of view about the meaning of a system's dimensionality. From this view, dimensionality may be the raw, irreducible concept—the single *property* of a quantum system—from which other consequences are derived (for instance, the maximum number of distinguishable preparations which can be imparted to a system in a communication setting). The best I can put my finger on it is that dimensionality should have something to do with a quantum system's "sensitivity to the touch," its ability to be modified with respect to the external world due to the interventions of that world upon its natural course. Thus, for instance, in quantum computing each little push or computational step has the chance of counting for more than in the classical world.

This language is definitely not completely consistent with the vision I outline in the attached letters but it is the best I can do at the moment. ("Modification without modificata?!?," I hear you asking. "What could it mean?")

In any case, I have looked at your new paper, and I have re-looked at the paper you gave me last year, and I will be bringing both of them with me to Maryland. It would be a pleasure to talk further about all this—how we differ, or even how I missed something and the gaps may be erasable after all. (The latter would be very nice if it is the case.)

21-04-04 *Can Your Toy Model Do This?* (to R. W. Spekkens)

Attached is an old set of notes that I finally glossed up into a paper and sent off to quant-ph today. [See `arXiv:quant-ph/0404122`.] It dawned on me that one ought to ask a similar question of your toy model: I'm thinking of the part of my paper surrounding Eqs. (13) and (14). There one sees that quantum systems' sensitivity to eavesdropping scales supermultiplicatively in the number of systems making up a composite system. Does a similar effect arise for your toy model? I don't have a strong feeling one way or the other which way the answer will go.

22-04-04 *Language Games* (to N. D. Mermin)

Correlation without Correlata … Modification without Modificata … No Stimulation without Stimulata!

I actually have found a way to make sense of the last one, "no stimulation without stimulata," within my budding point of view about quantum mechanics. Have I told you? No stimulation to the agent (in old language "the observer"), without simultaneously recognizing that the world must be stimulated in return. No stimulation to the agent without the external world being stimulata—i.e., that which is stimulated—in its own right.

Language games.

23-04-04 *Demonizing Bayesians* (to J. D. Norton & J. Earman)

Nortonism 1: *As you may know, I've been commissioned to speak on Maxwell's demon at this year's offering of New Directions and give my response to Charles Bennett's reply. Last year I had only a visceral sense that something was wrong. Since I've got to stand and speak, I tried to make these concerns more precise and now I think they are very precise. The result can be downloaded (in draft form) at: …*

Of course with any paper like this there is always a nagging worry that I have missed something obvious or not so obvious. So, if you do look at the paper, I'd be grateful for any reactions.

I've started to absorb a bit of your paper now and am enjoying it. I'm looking forward to overhearing the discussions in Maryland. (My only advice at this stage is that you might find a way to make the title a little less belligerent—but that's your business. I've got no room to talk given some of the introductions to my own papers. See for instance, `quant-ph/0106166` where I demonized the Zurekians.)

In the meantime, could one or the other of you send me electronic versions of your 1998 and 1999 exorcist papers? I don't have that journal easily available where I am.

It is good for me to think about these demon problems again, as I haven't really thought about them since long before my transformation to (some flavor of) personalistic Bayesianism. The justification for Landauer's principle that I had used in my own mind previously always involved considering the erasure from two points of view: 1) what happens from the demon's view, and 2) what happens from a "view from nowhere" that includes the system the demon is acting upon and the demon himself. Matching inside-view to outside-view probability distributions obtains (or, at least, so I had thought at the time) the result. But a personalistic Bayesian has no *a priori* reason to match those distributions—for they must be attached to some agents if they exist at all, and there can be nothing that automatically requires the agents to have compatible assignments.

Anyway, it's a good opportunity for me to rethink all these things. Now I am inclined to wonder whether something like a Dutch-book argument might not banish the demon forever. Namely, could one show that an agent (i.e., a demon) who claims 1) to be ignorant of the molecule's position after removing the partition in a Szilárd piston, and 2) to be able to extract work ad infinitum, is simply incoherent (in something like a Dutch-book sense)? Or something like that ...

Looking forward to seeing John N. again and meeting John E. for the first time.

24-04-04 *Trees for the Woods* (to R. Schack)

I read `t.tex` with breakfast this morning, and I don't quite know what to tell you at this stage. When we last talked about it, I thought you were going to make a clean simple point in the paper: That when one generates a 50/50 distribution via a quantum measurement on a pure state, one has much more than simply the 50/50 state of belief about the outcome. One also has the belief that no further part of the world can have any insider information about the outcome. And it is that that makes a quantum random number generator interesting. But at least in the present draft I don't yet see that idea cropping up (or even presented in the bold way that it ought to be). I.e., at this stage it's hard to see the forest for the trees.

26-04-04 *Batting Eyes and Smiling Faces* (to L. Henderson)

Hendersonism 3: *There is no option to photocopy it, except if they do it for you, and in this case that would cost $113.75, since the thesis is 455 pages long (it is a Long thesis, ho ho). If you still want it I can order it for you, but it will take about 5–7 days so I wouldn't be able to bring it to the conference.*

To show you how weird I am (read devoted), I'll take it! (They'll regret it though: I'll just post the damned thing to the internet once I get it and then they'll never get their $113.75 again.) Can I pay you back in Maryland this weekend? (Cash.) Also, maybe if you'll bat your eyes and give a big smile, they'll be compelled to give it to you before Thursday. I'll pay for your extra trouble there too: beer, dinner, fantastic ideas, ... some combination of all three.

See you soon! And thanks again ...

27-04-04 *Jeffrey Knew Certainty* (to R. Schack)

- R. C. Jeffrey, "De Finetti's Radical Probabilism," *Probabilità e Induzione—Induction and Probability*, edited by P. Monari and D. Cocchi, (Biblioteca di Statistica, CLUEB, Bologna, 1993), pp. 263–275.

27-04-04 *At It Again: Delirium Quantum* (to H. M. Wiseman)

I'm at it again with my 'samizdatery': I put together a little collection of emails that may be relevant for a meeting on the philosophy and history of science I'm attending next week. And I am flirting with the idea of posting it on `quant-ph` (though I haven't made a decision yet). The document is attached: `DeliriumQuantum.pdf`. [See "Delirium Quantum" `arXiv:0906.1968v1`.]

The reason I'm writing is to make sure the document doesn't offend you. The first two letters contain direct quotes of yours, and Section 4 mentions you both at the beginning and at the end. Let me know if I have your approval to post as is.

There is one little bit of new stuff in the paper that you may not have seen before (or at least a combination of words that you may not have seen before): Footnote #2 on page 7. I dream that I might have finally hit the sweet spot of clarity with those words, but I probably haven't.

27-04-04 *At It Again: Delirium Quantum* (to A. Sudbery)

I'm at it again with my 'samizdatery': I put together a little collection of emails that may be relevant for a meeting on the philosophy and history of science I'm attending next week. And I am flirting with the idea of posting it on `quant-ph` (though I haven't made a decision yet). The document is attached: `DeliriumQuantum.pdf`. [See "Delirium Quantum" `arXiv:0906.1968v1`.]

The reason I'm writing is to make sure the document doesn't offend you at a personal level. Section 5 in particular quotes you directly. Let me know if I have your approval to post the document as is, or whether you would suggest some changes.

There is one little bit of new stuff in the paper that you may not have seen before (or at least a combination of words that you may not have seen before): Footnote #2 on page 7. I dream that I might have finally hit the sweet spot of clarity with those words, but I probably haven't.

28-04-04 *Darwin, Qubit, Butterfield* (to C. G. Timpson)

Timpsonism 1: *Here's a question for you: Do you have any idea of when the phrase 'quantum information' might have begun to appear in print? It's in Peres and Wootters' 1991 PRL, anything earlier do you think?*

Good question! I wish I knew the answer. Looking through my own database, the earliest paper I could find with "Quantum Information" in the title was:

- Masanori Ohya, "On Compound State and Mutual Information in Quantum Information Theory," *IEEE Transactions on Information Theory*, **IT-29**(5), 770–774 (1983).

But I also seemed to recall an early paper by Roman Ingarden titled "Quantum Information Theory". So I Googled it and found a nice paper by Barbara Terhal that references it. It was a 1976 paper. (Barbara's paper is on `quant-ph`, titled "Is Entanglement Monogamous?") As Barbara stresses though, what those people (as well as Holevo) were concerned with in the early days was using quantum states and quantum channels for transmitting classical information.

The idea of transmitting quantum states for the sheer beauty of sending intact quantum states was something that only arose with the Bennett, Schumacher, Wootters club. I wonder if maybe the phrase appeared in Ben's PhD thesis:

- Benjamin Wade Schumacher, *Communication, Correlation and Complementarity*, Ph.D. thesis, The University of Texas at Austin, 1990.

The word "qubit," by the way, was invented by Schumacher. And, as opposed to what Artur Ekert's webpage says, I believe it was in a private conversation between Schumacher and Wootters somewhere in New England, not at the Broadway meeting in the UK.

Maybe I'll check with Schumacher and Wootters on these things.

Timpsonism 2: *Enjoy the Seven Pines, I believe Jeremy B and Olly Pooley are going.*

I didn't know Jeremy would be there! That's great. Even more fun now. (Don't know the other fellow though.)

28-04-04 *Quantum History* (to B. W. Schumacher & W. K. Wootters)

Since I'm turning into the amateur historian of the field, could I get either or both of you to record your earliest recollections of the words "qubit" and "quantum information"?

I know that Ben produced "qubit," but where and when? (Ekert has a webpage that says the word was coined at the Broadway meeting in 1993, but I thought it came earlier than that.)

Concerning the idea of calling a quantum state "quantum information," where did that come from? Do either of you know?

Ben's Reply

OK, here is the whole story as I remember it. I'm not sure that I've ever written it down.

In the spring of 1990 at the Santa Fe Institute, Bill (Wootters) and I discussed the idea that, by using suitable code words and decoding observables, you could reach the Holevo bound for classical information transfer over a quantum channel. I don't think I knew that Holevo had previously conjectured this; Bill will have to speak for himself. We worked on this for a couple of years. The problem proved to be horribly difficult.

In 1991–92 I had a student named Eric Nielsen who was doing a senior honors project on Maxwell's demon. I arranged to have Bill come to Kenyon for a couple of days in May of 1992 as an "outside examiner" for Eric. He did, and we also spent a day or two trying everything we could think of to find our way into the Holevo capacity problem. We didn't get anywhere.

After a couple of years of struggling with the problem, I was pretty discouraged. I drove Bill back to the airport in Columbus, which takes about an hour. As often happens when nothing is working, the talk turned a little crazy. Maybe, we said, we are asking the wrong questions. Maybe in quantum mechanics, the old ideas of information are just not appropriate. Maybe we need a new idea of "quantum information". And we could measure it in "qubits"! The idea of measuring something in "qubits" (like Noah's ark) struck us as immensely funny, and we laughed a good deal.

(Who made the joke? Honestly, I do not remember for sure. I think that I was the one who made the "quantum information" remark, but the joke sounds like Bill. Don't know, though.)

Then I dropped Bill off at the airport and drove back. During the drive back, I started thinking about our joke. It occurred to me that it was not a bad idea, actually. I understood several things immediately – such as the fact that "quantum coding" could not be a copying process because of the no-cloning theorem, and the fact that the qubit would be a generic two-level system. When I got back to Gambier, I spent the summer cooking up the data compression theorem.

Interestingly, I later ran across one of my notebooks from graduate school, circa 1985. I had proved the theorem about typical subspaces then, so this was undoubtedly in the back of my mind somewhere, though I did not remember it and actually proved it a second time. (I think it is also implicit in Ohya and Petz, but their book did not come out till 1993, I think, and I did not read that till much later.) What was new in 1992 was the physical picture.

This was first presented at the IEEE meeting on the Physics of Computation in Dallas in October of 1992. This was about a 12-minute talk, as I recall, so it went by pretty quick. But Charlie Bennett was in the audience and thought that "qubits" were cool. (I knew Charlie from previous meetings in various locales. He had explained superdense coding to me in the fall of 1991 in Spain.)

At the time, I definitely talked about quantum information as a distinct concept – in fact, the paragraph at the end of my 1995 data compression paper (which was written in 1993) is pretty much a verbatim quotation of end of my 1992 talk – a paragraph I had carefully composed in advance to put my idea across in the brief time I had in Dallas. I was quite consciously suggesting that there was a new field to be explored.

The Broadway meeting in 1993 was undoubtedly when lots of people who cared heard about this, and when Richard Jozsa made his suggestion for improving the proof. I think that my talk in Broadway tracked the Dallas talk closely, except for being longer.

BTW, I never considered another spelling for "qubit". So what Mermin says is "orthographically preposterous" seemed obvious to me.

Later, of course, we returned to the Holevo capacity problem and were more successful, thanks to Bill's persistence and a few tidbits from the data compression paper. So the story has a very happy ending.

So the whole thing started for me more or less as a really funny joke. There's a moral there somewhere. I will be interested to hear if Bill's recollection matches mine.

Bill's Reply

Ben's recollections sound entirely right to me. In the course of our discussion in the car, Ben suggested that maybe we needed to think about a new kind of information that is fundamentally quantum mechanical. And I remember very clearly that he is the one who came up with "qubit" as a unit of this information. "And we could measure it in qubits!" were very likely his exact words, including the exclamation point!

29-04-04 *More Reading* (to G. Musser)

I'm at the Dublin airport waiting for my departure, but I've got a moment to write.

Musserism 1: *I've been reading your "only a little more" paper, actually. I do not pretend to understand all, or much, of it, but it is fascinating, and engagingly written. It almost seems that the role of information theory is not to explain the quantum per se, but to peel away the subjective aspects of the theory and see whether some ontological nugget remains.*

Thanks, I'm flattered. And, yep, that's it. Here's the way I put it in a recent proposal:

> Quantum Foundations in the Light of Quantum Information.
>
> Nothing has done more to revitalize the idea that quantum mechanics can be understood at a deep intuitive level—with the concomitant benefits to physics this could bring—than the development of quantum information. There is a reason for this. Quantum mechanics is predominantly about information—plain, ordinary Shannon information or uncertainty. Embracing this idea is the starting point of my research program.
>
> But it is not the end point: No physicist would be doing his job if he did not strive to map reality itself—that is, reality as it is independently of any information processing agents. The issue is one of separating the wheat from the chaff: Quantum mechanics may be predominantly about information, but it cannot ONLY be about information. Which part is which? The usual way of formulating the theory is a thoroughly mixed soup of physical and informational ingredients.
>
> This is where quantum information (including the collateral fields of quantum cryptography, computing, and communication theory) has a unique role to play. Its tasks and protocols naturally isolate the parts of quantum theory that should be given the most foundational scrutiny. "Is such and such effect due simply to a quantum state being a state of information rather than a state of nature, or is it due to the deeper issue of what the information is about?" Recent investigations by several workers are starting to show that many of the previously-thought 'fantastic' phenomena of quantum information—like quantum teleportation, the no-cloning theorem, superdense coding, and nonlocality without entanglement—come about simply because of the epistemic nature of the quantum state. On the other hand, other phenomena, such as the potential computational speed-up of quantum computing, seem to come from a more physical source: In particular, the answer to the question, "Information about what?"
>
> When we finally delineate a satisfying answer to this, physics will reach a profound juncture. We will for the first time see the exact nature of 'quantum reality' and know what to do with it to achieve the next great stage of physics. Trickle-down effects could be the solution to the black-hole information paradox—perhaps already seen in broad outline—and even the meshing together of quantum theory and gravitational physics. In the meantime the approach proposed here is a conservative and careful one; the work to be done is large. The effort aims not to say first what 'quantum reality' is, but what it is not and gather insights all along the way.

Anyway, you know what you're talking about: You got the central idea.

Maybe I could recommend another thing you might enjoy reading (at least parts of): a paper of mine titled, "The Anti-Växjö Interpretation of Quantum Mechanics," `quant-ph/0204146`. Particularly I'm thinking of the two ending sections (built from letters to Preskill and to Wootters); ignore the other parts. They try to build a picture of quantum theory as a "theory from the inside."

In addition to that, let me give what I think is my present best compact description of the idea. It comes from a paper Schack and I posted on `quant-ph` a couple of days ago:

> Is there something in nature even when there are no observers or agents about? At the practical level, it would seem hard to deny this, and neither of the authors wish to be viewed as doing so. The world persists without the observer—there is no doubt in either of our minds about that. But then, does that require that two of the most celebrated elements (namely, quantum states and operations) in quantum theory—our best, most all-encompassing scientific theory to date—must be viewed as objective, agent-independent constructs? There is no reason to do so, we say. In fact, we think there is everything to be gained from carefully delineating which part of the structure of quantum theory is about the world and which part is about the agent's interface with the world.
>
> From this perspective, much—*but not all*—of quantum mechanics is about disciplined uncertainty accounting, just as is Bayesian probability in general. Bernardo and Smith write this of Bayesian theory,
>
> > What is the nature and scope of Bayesian Statistics … ?
> >
> > Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way.
>
> In fact, one might go further and say of quantum theory, that in those cases where it is not just Bayesian probability theory full stop, it is a theory of stimulation and response. The agent, through the process of quantum measurement stimulates the world external to himself. The world, in return, stimulates a response in the agent that is quantified by a change in his beliefs—i.e., by a change from a prior to a posterior quantum state. Somewhere in the structure of those belief changes lies quantum theory's most direct statement about what we believe of the world as it is without agents.

Finally, for more on the "golden nuggets," have a look at another new paper of mine (just the intro and conclusions): "On the Quantmness of a Hilbert Space," `quant-ph/0404122`.

I'll write more about arranging a discussion once I'm in the States. Gotta run now.

Musserism 2: *We should get you to write for us someday!*

Never thought of that. But now that you mention it, if *Scientific American* would ever like an article on "progress in information-based interpretations of quantum mechanics" or an article on "Being Bayesian in a Quantum World," let me know and I'll drop everything else (well, almost) to get it done.

I'll be back in less than 48 hours.

29-04-04 *Turgidity* (to H. M. Wiseman)

> **turgid** (tûr'jîd) adjective
> 1. Excessively ornate or complex in style or language; grandiloquent.
> 2. Swollen or distended, as from a fluid; bloated.

Which definition?

Wisemanism 33 : *I read the footnote and it seemed clear enough. However the last sentence was a bit turgid: Somewhere in the structure of those belief changes lies quantum theory's most direct statement about what we believe of the world as it is without agents.*

Just to pick this sentence apart, you have a theory of our beliefs about the world (quantum mechanics) making a statement about our beliefs about the world. Is this circular, or are we talking about different sorts of beliefs? Or maybe in the "what we believe of the world as it is", "we" is the authors of this paper. In this case I suggest it would be much punchier if you replaced the above phrase by "the world".

Lovely that you say that! After I wrote the said passage for our paper, I wrote to Rüdiger:

> I'm pretty pliable with a lot of the writing. But I've got to admit I'm pretty proud of the first two and the last two pages (before references, that is). So I'll pray that you spare the knife on those, especially on the concluding remarks—my pride-and-joys. In the words of John Wheeler at his 83rd birthday conference, "Give the old man his firecrackers!"

To which he replied:

> I am surprisingly happy with your intro and conclusion. Just one thing. The paradox in the penultimate sentence in the Concluding Remarks, starting "Somewhere. . . ", is clearly intended. I am not too sure it makes sense, though. I am not sure at the moment if I really believe this sentence.

Yes, I was intentionally slightly mystical with that sentence: I like to end my papers that way—probably a bad habit, but I have this secret dream that it'll help draw people into the program if the whole thing doesn't look too dry. Have a look at the introductory and concluding sections of my paper "On the Quantumness of a Hilbert Space" (just posted a few days ago). They're maybe my most mystical passages yet . . . or at least I hope so!! (If they're not, I've really been fooling myself!)

However, you do point out an ambiguity that I had not intended. By "we" I meant "we physicists," or maybe more particularly the community of those physicists who wish to squeeze some statement about (our beliefs about) reality out of quantum mechanics—not "we authors." Being something of a budding pragmatist (in the philosophical sense), I have to add for myself the reminder that *at best* a physical theory is always a statement of belief about reality. In its own way it is a gambling commitment. That's why I wrote "about what we believe of the world as it is without agents" rather than simply "about the world as it is without agents" (i.e., more along the lines of your suggestion).

Beyond that, however, you were right that I was talking of two different sorts of belief in the sentence. One can talk about accepting a quantum state ρ and one can talk about accepting the very structure of quantum theory. In both cases, I would say, one is accepting a state of belief. However the objects of those beliefs are very different. In the first case, I would say one is not at all accepting a belief about reality as it exists independently of the agent. The quantum state, for me, is only my state of belief about how nature will react back upon me consequent to one of my potential stimulations upon it. On the other hand, accepting the very structure of quantum theory is—I am banking—a hidden statement of what we believe of nature as it is without agents. There is a hierarchy here: At one level we cannot get rid of the agent in any convincing way, at the other level we might be able to. That's the idea. Finally, the question is how to unlock the hidden statement. I am banking that the best place to look is in the structure of how the former types of belief (i.e., quantum states) are changed consequent to the acquisition of a measurement outcome. (I.e., in the deviation from Bayes' rule encoded by the Kraus collapse rules.)

Hope that was worth reading. (Now I return back down to 35,000 feet where the airplane is situated.)

29-04-04 *Ontic Elements for Quantum Systems* (to H. Atmanspacher)

Thanks once again for your hospitality during my visit to IGPP. And more than that, thanks for sending me your paper with Primas, "Epistemic and Ontic Quantum Realities." I finally got a chance to give it a go today. (I'm flying to the States as I write this. It's a 10 day visit, three separate meetings, so I plan to have a few good days to let your ideas rattle around in my head before I have to return to my more usual world.)

You know of course how committed I am to making the ontic/epistemic split absolutely clear within quantum theory. And I am very happy that it looks like (at least in some instances) we would be compelled to make the split at the same place. For instance, for me, as for you, a density operator should be classified as an epistemic element. (A radical Bayesian about probability must go further—as Schack and I argue in our latest paper on `quant-ph`—and also classify quantum operations (i.e., quantum channels) as epistemic, but that is not so important to me at the moment.)

What I am having difficulty understanding on this first reading, however, is what you are taking to be the ontic elements within quantum theory? What is a quantum system's "mode of being at a given instant" (as you say)? In my own case, the only thing I am willing to relegate to the ontic realm (at this stage in my thought) is the quantum system's Hilbert-space dimension (in the finite-dimensional case)—it signifies something about the system's "sensitivity to the touch". (See my new paper, "On the Quantumness of a Hilbert Space," `quant-ph/0404122`, to make that remark maybe epsilon clearer.) Maybe I would be willing to relegate more than stark dimensionality to the ontic realm upon further reflection (and maybe that's where your paper will lead me), but that's where I stand at the moment.

In the hope of making some intellectual progress on these issues during this week of freedom, could I ask you to drop the C^* language for the moment and consider a very simple case so that I might see how your view fares with regard to my pre-established prejudices. Consider a simple qubit or qutrit, i.e., a 2-state or 3-state quantum system. What would you identify as the potential ontic states for those systems. Can you identify them in Hilbert-space language for me? What are such a system's properties? I should be in email contact almost every day despite my travels; if you could answer these questions in a language that I can hope to understand quickly, that would be most helpful for getting the ball rolling.

After that, I can see there may be some disagreements between the two of us that may not be so easily erasable. But I think that is at a further step down the line and we shouldn't quite go there yet. (For instance, I am thinking of your invoking systems with infinite degrees of freedom for a "solution" to the measurement problem. In my own mind, recognizing that quantum states are epistemic already DISsolves the measurement problem enough. That was part of the picture I tried to present when I spoke at your institute.)

On the other hand, I was very much intrigued by your idea of relative onticity. Something feels very right about that, and I can see myself plunging into the idea and trying to adapt it to my own purposes.

Thus I hope you can see that my respect for your work is very strong. There are points to be hammered out, but I am definitely adding your thoughts to my toolbox.

02-05-04 *Precision* (to A. Wilce)

Do you still remember the precise quote of what David Albert said at the end of my talk? If you do, I'd like to get it recorded into my records.

Alex's Reply

> "I think we can pretty exactly quantify the interpretational progress made here – and it's zero."

06-05-04 *SIC-POVMs* (to F. E. Schroeck)

Schroeckism 1: *I was reading "On the Quantumness of a Hilbert Space" (given to me by Stan Gudder) and I wanted to look up Carlton Caves' paper "Symmetric Informationally Complete POVMs". It wasn't online. In particular, I wanted to know why the factor* $1/(d+1)$ *occurs in your eqn 15. Can you give me either a hint, or Cave's e-mail address (or both)?*

By the way, the first half of the paper is great, and the other half is to be read.

Wow, thanks!

It's a simple consequence of the definition of a SIC-POVM. The definition is that the POVM consists of d^2 rank-1 elements, all of equal trace, and such that the pairwise inner products of nonindentical elements are constant.

Using the fact that the d^2 elements form a resolution of the identity, and taking the trace of the identity, you get that the trace of each element must be $1/d$.

Now to get the values of the pairwise inner products, you just square the identity (and consequently square the resolution of the identity) and pull the same trick: Namely taking the trace of the consequent.

Hope that makes it clear enough.

I'm kind of surprised you couldn't find Carl's notes at his webpage, but maybe he removed them.

06-05-04 *Section 4.1* (to W. G. Demopoulos)

I'm on the plane now. I guess, at this stage, I don't have much to add to what I wrote you earlier after all. The "snowflakes" bit captures most of what I had wanted to say. The only other thing I can recommend to have a look at is Section 4.1 (trying to justify noncontextuality for our probability assignments in QM) in `quant-ph/0205039` (i.e., "QM as QI (and only a little more)"). I'll be curious to know how that argument meshes with your own.

What I would like to go into further eventually is the similarity and differences between 1) your point of view of the underlying events in QM as being preexistent (but maybe unique) realities, and 2) my point of view that they are best supposed as (unique) creations. I also get the feeling that your view collides with another one of my favorite doctrines: Namely, that we should take POVMs as just as basic as standard quantum measurements (because they so prettily match Bayes' rule). But I'll have to think about that.

06-05-04 *Middle of the Meeting* (to A. Peres)

Asherism 67: *I finished to read (many parts of)* No time to be brief*, Pauli's biography by Enz. The book is overcomplete, with plenty of irrelevant gossip on other physicists. Yet, there are many interesting things in it. I never imagined that Pauli had such a tortured soul and needed help from Jung and similar crooks. Why did you call your future book* Notes on a Paulian idea*?*

Because most of the notes in there were deeply influenced by the collection of Pauli's more-philosophical papers *Writings on Physics and Philosophy* that I had read in 1995. Also the letters from Pauli to Fierz (that I had found in one of Laurikainen's books) made a similar effect.

Anyway, in particular, the phrase "Paulian idea" is meant to capture the two explicit quotes of Pauli's that I placed at the very beginning of the book.

I agree with your remark about Jung. I had a fun time recently reading the Jung/Pauli correspondence volume. There was a lot of trash from both sides, but on the other hand, at least there were several letters on Pauli's side that were quite interesting—and said, I think, some deep physics. But with regard to Jung's side of the conversation, I really could not make heads from tails—I think it was very likely total nonsense.

Now I should probably return to listening in on the present talk.

11-05-04 *The Late, Late, Late Show* (to A. Sudbery)

I'm at it again with my 'samizdatery': I put together a little collection of emails that may be relevant for a meeting on the philosophy and history of science I'm attending next week.

There is one little bit of new stuff in the paper that you may not have seen before (or at least a combination of words that you may not have seen before): Footnote #2 on page 7. I dream that I might have finally hit the sweet spot of clarity with those words, but I probably haven't.

Sudberyism 12: *The footnote on p. 7 is very clear, though I personally have problems with the notion that quantum mechanics is all about your (my, our) interventions in the world and its response. I know that's what the textbooks say – you only get a prediction for the result of a measurement – but I don't think that's an accurate description of the theory (as it actually exists in the practice of working scientists).*

You mean after all this technical work (of mine and so many others), and all my attempts to find the best English yet to tell the story (of "measurement" ... better yet "intervention" ... or still better "stimulation"), I still haven't gotten any further than what the textbooks say?

One more round, Delia's gone, one more round. ☺

Sudberyism 13: *I was sorry not to get to the LSE meeting on probability. Was it good? Lots of elevated blood pressure? You know about our Foundations meeting in York in September, don't you? No invited speakers, just a free-for-all. I'd like to incorporate a round table or two, maybe one on Everettism and one on Bayesianism, or even just one on both, face to face.*

Yeah, the LSE meeting was pretty good. I enjoyed it. And even more than that I enjoyed sparring with Butterfield, Saunders, Brown, and a few others in Oxford afterward.

I didn't know about the September meeting. Is there a website? I ought to try to come to that if I can.

12-05-04 *Pragmatism, Quantum, Bayes, and pragmatism (no capital)* (to J. Woodward)

I had a look at your profile on your Caltech webpage this morning, and then I worked my way to your Stanford Encyclopedia article. It was a nice experience reading parts of it—I'll give it a more serious consideration over the next couple of days—for it looks like there is some real overlap in our concerns. In particular, one might almost describe what I'm shooting for in rewriting the notion of quantum measurement is something like a manipulationist account of it. In place of "measurement", I think it is firmer ground to think of a quantum measurement click as a system's *response* to an external intervention ... that is, rather than think of it as the *revelation* of some pre-existent value. My latest ways of putting these things are detailed in "Delirium Quantum", particularly in the section "Me, Me, Me". Also, there's plenty to read about it scattered throughout my book *Notes on a Paulian Idea* (I gave Jed a copy that you can borrow, or I can have one sent to you), for instance on pages 394 or 509–510. What I have written is surely cloudy thinking in comparison to your corpus, but maybe it's enough to show that my heart is in the right place and that our interactions could be mutually beneficial.

Anyway, that's just a taste of things, and an attempt to put all the issues on the table.

12-05-04 *Does Hilbert-Space Dimension Gravitate?* (to J. Preskill)

Preskillism 10: *In my view, you were the most cogent of the "philosophers" I talked to in Stillwater.*

That's because I'm a physicist in disguise.

"Philosophy is too important to leave to the philosophers." – J.A.W.

13-05-04 *Epistemic Pure States* (to H. Atmanspacher)

Thanks for the note. I apologize for not getting back to you sooner, but the conferences I was at knocked me for more of a loop than I had expected. I'm still recovering, and now my backlog of tardy work is piled even higher!

Let me just say a very short word at the moment. With your note and the attached paper, you confirmed what I feared: That you take a pure quantum state to be an ontic state. This contrasts with our Bayesian approach where all quantum states (pure or mixed) are epistemic.

Let me attach two papers and one pseudo-paper that say something about this approach. One paper is one of my own with Schack which talks about a Bayesian view of quantum states in general. The other is due to Rob Spekkens. In it, he gives the most compelling case I've ever seen that pure quantum states should be viewed epistemically. He does this by getting to the core of the matter in a toy model (that is not quantum mechanics). His model lacks a lot—for instance, his observers are still detached in that it is a hidden variable model—but I still think it is a strong argument for the epistemicity of the quantum state. Finally the pseudo-paper is something I threw together for the

Seven Pines meeting in Minnesota. The relevant sections are the last two, "Me, Me, Me" and "The Big IF". What they try to give a hint of is a view in which quantum theory is not taken so much as a "theory of the world," but rather as a theory of decision-making for agents *immersed* in and *interactive* with the quantum world. Making that change in point of view is a crucial component in what we are after.

Atmanspacherism 1: *It could be that this provides a bridge between your and our approaches. In my view and Primas', there is no absolutely ontic description anyway (one cannot even speak without a context). In this sense, our ontic states are already relative to a context which, however, is unspecified.*

I hope so. I will think about it.

13-05-04 *10 Lines and MaxEnt* (to R. Schack)

One of the meetings I'm back from is the Seven Pines meeting in Minnesota. A few philosophers were there including John Earman and a guy named Geoffrey Hellman (a student of Hilary Putnam's). In Hellman's (short) talk, he briefly mentioned that the interpretation of probability was important for quantum foundational issues. He mentioned frequency interpretations, propensity interpretations, and our approach. He very nicely complimented us by calling it a "radical move" (though of course, in private, he said that he didn't believe that it could be the "right move"). Earman said in one of our discussion sessions: "You mean $\psi^*\psi$ as a Bayesian degree of belief? Could not be."

So it goes: Even the Bayesians won't believe us!

13-05-04 *LNP Volume* (to M. G. A. Paris)

Just use the abstract we already wrote. It's only six sentences:

> The classical de Finetti theorem provides an operational definition of the concept of an unknown probability in Bayesian probability theory, where probabilities are taken to be degrees of belief instead of objective states of nature. In this paper, we motivate and review two results that generalize de Finetti's theorem to the quantum mechanical setting: Namely a de Finetti theorem for quantum states and a de Finetti theorem for quantum operations. The quantum-state theorem, in a closely analogous fashion to the original de Finetti theorem, deals with exchangeable density-operator assignments and provides an operational definition of the concept of an "unknown quantum state" in quantum-state tomography. Similarly, the quantum-operation theorem gives an operational definition of an "unknown quantum operation" in quantum-process tomography. These results are especially important for a Bayesian interpretation of quantum mechanics, where quantum states and (at least some) quantum operations are taken to be states of belief rather than states of nature.

17-05-04 *Big Bang 1910* (to G. L. Comer)

The following passage comes from William James's posthumous book, *Some Problems of Philosophy*—started in March 1909 and worked upon until his death in August 1910.

> It is a common belief that all particular beings have one origin and source, either in God, or in atoms all equally old. There is no real novelty, it is believed, in the universe, the new things that appear having either been eternally prefigured in the absolute, or being results of the same *primordia rerum*, atoms, or monads, getting into new mixtures. But the question of being is so obscure anyhow, that whether realities have burst into existence all at once, by a single 'bang,' as it were; or whether they came piecemeal, and have different ages (so that real novelties may be leaking into our universe all the time), may here be left an open question, though it is undoubtedly intellectually economical to suppose that all things are equally old, and that no novelties leak in.

17-05-04 *Jamesian Exchangeability* (to R. Schack)

[[The quote below comes from William James's book *Some Problems of Philosophy*. There was no further text in this note beyond the quote.]]

> A concept, it was said above, means always the same thing: Change means always change, white always white, a circle always a circle. On this self-sameness of conceptual objects the static and 'eternal' character of our systems of ideal truth is based; for a relation, once perceived to obtain, must obtain always, between terms that do not alter. But many persons find difficulty in admitting that a concept used in different contexts can be intrinsically the same. When we call both snow and paper 'white' it is supposed by these thinkers that there must be two predicates in the field. As James Mill says: 'Every colour is an individual colour, every size is an individual size, every shape is an individual shape. But things have no individual colour in common, no individual shape in common; no individual size in common; that is to say, they have neither shape, colour, nor size in common. What, then, is it which they have in common which the mind can take into view? Those who affirmed that it was something, could by no means tell. They substituted words for things; using vague and mystical phrases, which, when examined, meant nothing.' The truth, according to this nominalist author, is that the only thing that can be possessed in common by two objects is the same *name*. Black in the coat and black in the shoe are the same in so far forth as both shoe and coat are called black—the fact that on this view the name can never twice be the 'same' being quite overlooked. What now does the concept 'same' signify? Applying, as usual, the pragmatic rule, we find that when we call two objects the same we mean either (a) that no difference can be found between them when compared, or (b) that we can substitute the one for the other in certain operations without changing the result. If we are to discuss sameness profitably we must bear these pragmatic meanings in mind.
>
> Do then the snow and the paper show no difference in color? And can we use them indifferently in operations? They may certainly replace each other for reflecting light, or be used indifferently as backgrounds to set off anything dark, or serve as equally good samples of what the word 'white' signifies. But the snow may be dirty, and the paper pinkish or yellowish without ceasing to be called 'white'; or both snow and paper in one light may differ from their own selves in another and still be 'white,'—so the no-difference criterion seems to be at fault. This physical difficulty (which all house painters know) of matching two tints so exactly as to show no difference seems to be the sort of fact that nominalists have in mind when they say that our ideal meanings are never twice the same. Must we therefore admit that such a concept as 'white' can never keep exactly the same meaning?

It would be absurd to say so, for we know that under all the modifications wrought by changing light, dirt, impurity in pigment, etc., there is an element of color-quality, different from other color-qualities, which we mean that our word *shall* inalterably signify. The impossibility of isolating and fixing this quality physically is irrelevant, so long as we can isolate and fix it mentally, and decide that whenever we say 'white,' that identical quality, whether applied rightly or wrongly, is what we shall be held to mean. Our meanings can be the same as often as we intend to have them so, quite irrespective of whether what is meant be a physical possibility or not. Half the ideas we make use of are of impossible or problematic things,—zeros, infinites, fourth dimensions, limits of ideal perfection, forces, relations sundered from their terms, or terms defined only conceptually, by their relations to other terms which may be equally fictitious. 'White' means a color quality of which the mind appoints the standard, and which it can decree to be there under all physical disguises. *That* white is always the same white. What sense can there be in insisting that although we ourselves have fixed it as the same, it cannot be the same twice over? It works perfectly for us on the supposition that it is there self-identically; so the nominalist doctrine is false of things of that conceptual sort, and true only of things in the perceptual flux.

What I am affirming here is the platonic doctrine that concepts are singulars, that concept-stuff is inalterable, and that physical realities are constituted by the various concept-stuffs of which they 'partake.' It is known as 'logical realism' in the history of philosophy; and has usually been more favored by rationalistic than by empiricist minds. For rationalism, concept-stuff is primordial and perceptual things are secondary in nature. The present book, which treats concrete percepts as primordial and concepts as of secondary origin, may be regarded as somewhat eccentric in its attempt to combine logical realism with an otherwise empiricist mode of thought.

18-05-04 *Agents, Interventions, and Surgical Removal* (to J. Woodward)

(I'll drop the "Professor Woodward" bit and act like I know you, now that this is a second contact.)

I had time over the weekend to better digest your Stanford Encyclopedia article, "Causation and Manipulability." Per my first guess, we do indeed use a lot of words in common—intervention, manipulation, agent—though your concern is causation and my concern is the elicitation of "quantum measurement outcomes." As evidence of this, see for instance Section 4 of my paper "Quantum Mechanics as Quantum Information (and only a little more)" posted at my website (link below).

But there are also more than words in common. There is the common concern of exorcising the (anthropomorphic) agent if possible from each of our domains. Can it be done? My suspicion about quantum mechanics is that there is a level at which it can be done and a level of which it can't. Both levels are needed, however, for an explication of what the theory is about.

What I liked in learning from your paper is the similarity of our strategies. (Rüdiger Schack tells me I'm a cherry-picker when it comes to extracting content from papers; sorry if I'm doing that here.) Anyway, the way I see it is we both start from rather firm starting points—though they necessarily involve the notion of an agent—and only then, make a step toward de-anthropomorphizing the picture. In your case, it's to start with a manipulationist account of causation and then to move to an interventionist one. The ultimate goal is to talk of causes in a way that makes no reference to human action.

In my case, I try first to make sense of a quantum measurement outcome as a birthy sort of event that happens when two pieces of the world metaphorically bump into each other *in the case that* one of the pieces is an active agent (who is equipped with subjective probabilities, etc., etc.). Of course, after that I'd like to go the next step—to imbed these anthropocentric "births" in a larger account of a world in continuous creation, in analogy to your strategy of imbedding humanly manipulations within a larger (de-anthropocentrized) interventionist account. At present, I only have a glimpse of how to do that. About the only thing I feel confident of is that, if it's going to be done, the project is going to have to be carried out in a relatively oblique manner—namely, one that stops talking about the births themselves and transfers attention to systems' capacities to take part in these kinds of mini-creations. (This is the stuff I call "Zing!" in footnote 10 of the paper mentioned above.)

Why must the strategy be oblique? Your paper gave me a nice metaphor for that:

> Human beings cannot at present alter the attractive force exerted by the moon on the tides (e.g., by altering its orbit). More interestingly, it may well be that there is no physically possible process that will meet the conditions for an intervention on the moon's position with respect to the tides – all possible processes that would alter the gravitational force exerted by the moon may be insufficiently "surgical". For example, it may very well be that any possible process that alters the position of the moon by altering the position of some other massive object will have an independent impact on the tides in violation of condition (M2) for an intervention.

You see, as opposed to classical gravitational phenomena, I believe quantum mechanics puts us in a new situation. The world appears to be wired together so intricately that every attempt to extricate or "surgically remove" the agent from the quantum measurement process leaves a conceptually empty structure (I'm thinking of Everettian and Bohmian quantum mechanics, etc., here). What quantum mechanics is mostly about is making-do in light of that.

That's on the negative side of things. On the positive side of things, knowing that the world is made of a "stuff" that causes us to "make-do" in this way is something I find tremendously exciting. I.e., it's not the making-do that's exciting, but the stuff that gives rise to it that is. This is because I think it leads to a far more interesting and creative world than we might have imagined otherwise. In the next note, I'll attach a historical/philosophical document I've been working on that tries to convey some of this. The title is, "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature," and it's fairly large (922K) ... so don't get frightened that something has gone wrong with your email. The thing is still far from complete, but I hope it conveys a flavor of the kinds of thoughts I'm up to in my most philosophical moments.

How do I live with such a crucial piece of quantum theory (and the next stage of physics it hints at) being so anthropocentric? Here's the way I put it in that document: "Observers, a necessary part of reality? No. But do they tend to change things once they are on the scene? Yes. If quantum mechanics can tell us something deep about nature, I think it is that." And it is that idea that I'm working my tail off to formalize.

Anyway, a long letter just to say I got a lot out of reading your paper and to reiterate that I think we have plenty of potential for fruitful interaction. [For instance—on a different subject—my radically Bayesian nose smells the agent remaining, despite your efforts, in your account of nondeterministic causation (right after equation CD). That is because, for such a Bayesian, the probabilities in the difference you consider still make crucial reference to an agent for their very existence.]

18-05-04 *Striving for Consistency* (to A. Grinbaum)

Grinbaumism 1: *I am still thinking what to respond to your* quant-ph/0404122*. Frankly, I was slightly unhappy to read that "sensitivity to the touch" means "ability to be modified with respect to the external world due to the interventions of that world upon its natural course". This sounds so realist, and the sentence is so different from the rest of your article. I know that you had acknowledged that the dimension of the Hilbert space is for you the only remaining element of physical reality, but I certainly see no need to come to realism from "sensitivity to the touch". Why put accent on touch rather than sensitivity?*

You are right about that, and the only thing I can do for the moment is plead guilty. I strive for consistency, but I don't always achieve it—worse yet, sometimes my repertoire for proper expression runs limp. (You'll see in the first letter below how I made fun of myself on just this issue.) [See 21-04-04 note "Essential Incompleteness" to W. G. Demopoulos.]

Anyway, let me try to make up for it—or make the matters worse!, I don't know which—by sending you some further expressions of what I'm trying to get at. For one thing, see the attached pseudo-paper (which I haven't yet decided whether I will submit it to the archive or not, your vote would count). [See "Delirium Quantum" arXiv:0906.1968v1.] For another, see the further letter below that I wrote to Jim Woodward this morning. [See 18-05-04 note "Agents, Interventions, and Surgical Removal" to J. Woodward.]

To answer your question, it's very unlikely that I'll be in Europe in the Autumn. I'm now scheduled to return to Bell Labs in mid-August, and the Fall is likely to be a quite hectic time with us getting settled into our new house, etc. Sorry; I would have liked to have been at your defense. But I know you'll do well.

Glad you enjoyed Dublin.

19-05-04 *The Barbecue Quest* (to G. Musser)

Sorry for the slow reply.

Musserism 3: *To follow up our conversation regarding entanglement, I looked at the "Wither Entanglement" section of* quant-ph/0205039*. If I follow, the idea that entanglement is a statement about our knowledge, rather than about a system, is demonstrated by the derivation of the tensor-product rule. What is the physical meaning of that rule?*

I'm not sure what to say beyond what I said in the paper. The point is simply that the tensor-product rule for generating a Hilbert space to describe composite systems is a consequence of a) the structure of localized quantum measurements, augmented solely with b) classical, subluminal communication between observers at each site.

Maybe I should put the point this way: If one is looking for a mystery in quantum mechanics, one need look no further the structure of (local) measurements on single systems. That's where the real mystery is. Why is the structure of quantum measurements such that it can power a Kochen-Specker no-coloring theorem? If we had a stick-to-your-ribs answer for that, I think we'd finally be at the end of the quest.

Musserism 4: *I was also told to think of QM as nondeterminstic. More recently I was told (cf. John Earman), no, it is completely deterministic, because the Schrödinger equation uniquely maps a wave function at t_0 to another at t_1. Now Griffiths tells me to think of it as nondeterministic* *again: "The successive events of a history are, in general, not related to one another through the Schrödinger equation." Help.*

I'm sure all three of us have very different reasons for saying what we say. Here's the way I would put it. You cannot forget the distinction between a) states of knowledge/expectation/belief, on the one hand, and b) what *is* or what *is happening* in the world, or more particularly what an agent can provoke of it, on the other. Neither has an implication on the other. Quantum time evolution—which is smooth and deterministic—is about how an agent's state of knowledge changes over time. The singular, unpredictable, event-like "measurement outcomes" are about the response an agent's stimulation to a system external to himself will provoke. Those events or responses seem to come out of the blue. It is they that are (among) the real things of the world. The quantum state is just a fancy way of organizing the agent's expectations—thus it is not among the real things of the world. The only time there is a nonsmooth (indeterministic) change in a quantum state is when the agent holding it incorporates a new piece of *data* into his expectations.

I thought I had said this much better in *Notes on a Paulian Idea* somewhere, but if I did, I couldn't find the place with a quick perusal. The closest thing I could find was in the notes surrounding "The Evolution of Thought", pages 163-168 (in my website version). Still, you might get something useful out of reading the note titled "A Non-Randy Non-Bugger" on page 163 nonetheless.

By the way, I had the secretary in Sweden send you a copy of the Växjö University Press cut of *Notes on a Paulian Idea*. (Kluwer is publishing it later this year too.) I hope it doesn't take too long to arrive. The way to treat the book is as bathroom reading (i.e., not systematically)—think of it as a kind of snippet book on information-based interpretations of quantum mechanics. It's main purpose is to inspire. If you want to find my insults on various people, use the name index in the back.

Musserism 5: *If after returning NJ you ever teach a senior-level or first-year graduate-level course in QM, please let me know. I took such a course in grad school but the knowledge evidently has a short exponential decay timescale. I need to work through the math again.*

Flattery. (Thanks.) Better flattery would be to tell me that *Scientific American* wants a full article on this stuff.

19-05-04 *Bathroom Reading* (to P. C. E. Stamp)

Per your request, I just had the secretary in Sweden send you a copy of the Växjö University Press cut of *Notes on a Paulian Idea*. I'm sure I don't need to say it, but the way to treat the book is as bathroom reading (i.e., not systematically, to put it mildly)—think of it as a kind of snippet book on information-based interpretations of quantum mechanics. Its main purpose is to inspire.

Particularly, something relevant to our last conversation (in the wrap-up session) can be found in two notes in the Barnum chapter, titled "It's All About Schmoz" and "New Schmoz Cola". The best part is after Barnumism 5.

The main point I was trying to make to you—when I thought I could do it better than Aspelmeyer—is that in an information-based interpretation of quantum mechanics (like the one Caves, Schack, Mermin, Peres, Hardy, and few others of us are trying to put together) is that: Though we take the wavefunction to be information, we never take it to be information about "what is existent" in the quantum system, or "what is happening in the quantum system." That it cannot be information about something like that is what I take to be the great lesson of the

Bell-Kochen-Specker theorem. Rather it is always information about how a system will *respond* to an external stimulation (i.e., in usual language, a measurement ... but I want to get away from the usual language because it carries the wrong imagery). This is the way to think of it when the wave function is about a single system and the way to think of it when the wave function is about a composite system—it doesn't matter.

So, what I am asking—among other things—is that the notion of "measurement" be replaced with the idea of "stimulation and response." We only use wave functions to calculate our expectations with regard to how a system will respond to one or another of our possible stimulations upon it. In this light, there is no contradiction between an "inside view" of a measurement where an unpredictable click really happens and an "outside view" (encompassing both the original system and the first observer), where a wave function for the composite system undergoes a smooth and deterministic transition. This is because the "outside view" wave function refers again to *nothing more than* the outside observer's expectations for how that composite system will respond to external stimulations upon it.

Hope that helps clarify at least what I was trying to say.

24-05-04 *Admissions* (to H. Mabuchi)

Mabuchism 3: *Did you find time to read the Richard Powers novel I sent you?*

I've got to admit that I haven't read it yet. But because of my upcoming visit, it's hittin' high priority. You should be doubly honored, it'll be the first fiction book I've read in over 10 years. (Unless you think I ought to count David Deutsch's book *The Fabric of Reality* ...)

25-05-04 *Bathroom Reading* (to G. Hellman)

It was good meeting you at the Seven Pines meeting. I was particularly impressed with your fairness, balance, and patience in taking in a wide range of viewpoints. If you could send me some of your "relation without relata" philosophy-of-mathematics papers, I would enjoy reading them. (The best format is to send them electronically if you can, but snail-mail will do as well.)

I just had the secretary in Sweden send you a copy of the Växjö University Press cut of *Notes on a Paulian Idea*. I'm sure I don't need to say it, but the way to treat the book is as bathroom reading (i.e., not systematically, to put it mildly)—think of it as a kind of snippet book on information-based interpretations of quantum mechanics. Its main purpose is to inspire; I hope it fits the bill at least a little. The name index at the back is hopefully a handy way to look up one or another point of view. You might be particularly interested in the early discussions with Jeff Bub.

I intend to write a lengthy note to John Earman to reply to his take, "Psi-star-Psi, a Bayesian degree of belief? Cannot be!", from the wrap-up session. If you're interested, I'll forward it to you too once it's constructed.

25-05-04 *Bathroom Reading* (to A. Duwell)

It was good seeing you again a couple of weeks ago. (Get that paper on `quant-ph`, young man!)

I just had the secretary in Sweden send you a copy of the Växjö University Press cut of *Notes on a Paulian Idea*. I'm sure I don't need to say it, but the way to treat the book is as bathroom reading (i.e., not systematically, to put it mildly)—think of it as a kind of snippet book on information-based interpretations of quantum mechanics. Its main purpose is to inspire. I hope it fits the bill at least a little.

Concerning the little discussion we had on noncontextuality for quantum probability assignments, the section I was talking about was 4.1 in `quant-ph/0205039`. If you have any thoughts on the argument (except for the sentence where I used a little circularity for rhetorical purposes), I'd like to hear them.

Finally, the paper with a discussion on the classical analogue(s) to Schumacher compression that I was telling you about is: `quant-ph/0008024`. Don't know if it's as clear as I remember it to be, but maybe it's a start.

25-05-04 *Bathroom Reading* (to A. J. Leggett)

It was good seeing you again at the Seven Pines workshop. I particularly enjoyed the question you asked me at the end of my talk because I thought it got at the heart of the matter. It is the laboratory procedures that refine our predictability that come first in defining the very notion of the (bad word) "measurement." It is only post facto that a set of operators from the quantum mechanical formalism is associated with such a procedure. Without the idea that knowledge is being refined, somewhere, somehow, the word measurement should never be used.

Anyway, I know you didn't request it, but I decided to have the secretary in Sweden send you a copy of the Växjö University Press cut of my *Notes on a Paulian Idea*, since I was already sending it to several others anyway. I guess if anything, all I really want out of this is to present a convincing case to you that the stance I am taking on the wave-function is not giving up on realism (which is roughly the way you put it). It's the opposite, as I see it—it's just recognizing a spade for a spade and trying to divert our attention to the parts of quantum theory that stand a chance of being realistically construed. I try to make the case in several places in the book, but the parts you might be particularly interested in are the early discussions with David Mermin, Jeff Bub, and Rolf Landauer. Maybe some of the stuff to John Preskill too. (Mermin, by the way, wrote the foreword, if that'll help you get past the first page.)

Regarding the book as a whole, I'm sure I don't need to say it, but the way to treat it is as bathroom reading (i.e., not systematically, to put it mildly)—think of it as a kind of snippet book on information-based interpretations of quantum mechanics. Its main purpose is to inspire if it can. The name index at the back is the best bet for looking up one or another point of view; unfortunately I didn't have a subject index made for the initial publication.

If—in the end—you're interested in any of the more technical results needed to prop up these goals (and there is a lot of technical work to be done), most of my papers can be found at `arXiv.org` in the `quant-ph` listings (`quant-ph/0205039` being the most comprehensive). In the one titled, "Quantum States and Operations, a Bayesian View," you'll find extensive references to everyone else's work along the same lines as well.

25-05-04 *Bathroom Reading* (to M. Janssen)

It was good meeting you at the Seven Pines workshop. I particularly enjoyed the little discussion we had near the end. I learned a lot from what you saw as a difference between Jeff Bub's and my approaches/goals.

I know you didn't request it, but thinking about this, I decided to have the secretary in Sweden send you a copy of the Växjö University Press cut of *Notes on a Paulian Idea*. Anyway, I'm sure I don't need to say it, but the way to treat the book is as bathroom reading (i.e., not systematically, to put it mildly)—think of it as a kind of snippet book on information-based interpretations of quantum mechanics. Its main purpose is to inspire; I hope it fits the bill at least a little. The name index at the back is maybe the best bet to look up one or another point of view; sorry I didn't have a subject index prepared at the time. Anyway, you might be particularly interested in the early discussions with Jeff Bub (which in turn was mostly based on Jeff's reading of the notes to Mermin).

If—in the end—you're interested in any of my more technical results toward these goals, most of my papers can be found at `arXiv.org` in the `quant-ph` listings.

25-05-04 *Cathy and Erwin, the Movie* (to C. G. Timpson)

You're not being dim-witted at all. Instead, thanks for giving me a new opportunity to clarify things still further. I'll take the task seriously if you can wait a day or two for my reply: I want to do it right (certainly righter than before).

The central issue is to never forget that a quantum state is a state of belief *about* how a system will **respond** to an external stimulation. Or better yet—and maybe necessarily so—as I put it in `quant-ph/0404156`, "The agent, through the process of quantum measurement, stimulates the world external to himself. The world, in return, stimulates a response in the agent that is quantified by a change in his beliefs—i.e., by a change from a prior to a posterior quantum state." In particular, the state should never be construed as being about "what is existent" or "what is happening" in the quantum system.

Aside: From this point of view, the quantum system is a "philosopher's stone" that brings about a transformation in the agent. (I just wanted to record that while I was thinking it.)

But anyway, I'll try to say all this more clearly and at length—and particularize it to the context of your two questions—as I get a chance to compose something.

26-05-04 *Rorty, Delirium* (to A. Fine)

Hey, did you know that Richard Rorty calls you his favorite philosopher of science? I read that a while ago, and I've been meaning to tell you. What kind of reaction does it elicit from you? I'm a great fan of some of Rorty's (more tempered) writings. (But I'm a cherry-picker when it comes to papers: I absorb the parts I think I can use, and try to hold no ill will for the parts I simply ignore.)

Anyway, I was thinking of that when I was sending the attached cheerleading paper to a friend yesterday. [See "Delirium Quantum" `arXiv:0906.1968v1`.] (The first and second sections lean heavily on Rorty.) So while you're on my mind, I'll send it to you too. I haven't quite decided whether I'm going to post it yet, or whether it's a little too delirious for the moment.

26-05-04 *Bathroom Reading and Subliminal Suggestions* (to D. Howard)

It was good meeting you at the Seven Pines workshop.

Per your request, I had the secretary in Sweden send you a copy of the Växjö University Press cut of my *Notes on a Paulian Idea* a couple of days ago. It shouldn't take too long to get there. I'm sure I don't need to say it, but the way to treat the book is as bathroom reading (i.e., not systematically, to put it mildly)—think of it more as a kind of snippet book on information-based interpretations of quantum mechanics. Its main purpose is to inspire if it can. The name index at the back is the best thing presently for looking up one or another point of view; unfortunately I didn't have a subject index made for the initial publication. Thinking of our discussions in Minnesota, you might particularly enjoy reading the correspondence with Mermin, Peres, Bub, and Landauer; they're maybe the most relevant things to start off with.

The quote I read aloud after your talk, showing that Pauli clearly understood the concept of entanglement as early as 1927, can be found on page 334 in a note titled "Pauli Understood Entanglement." You'll find the Rosenfeld quote that you're fond of on pages 299–301 in a note titled "How Do I Sleep?"

Concerning the other quotes I promised to send you (from the Bohr-Pauli correspondence), they'll come in the next email in a big attachment—a little over 1MB—titled `PauliProject.pdf`. Let me know if you have any trouble opening the file; if necessary I'll seek another means to send it to you.

Sorry for the delay, but I was tweaking the document and resisted sending it until it found a new plateau of semi-stability. In general the thing is a mess and very, very far from complete. But I haven't given up hope that I might get it posted on `quant-ph` by December. It's mainly a question of getting a load of (pre-marked) material out of my file cabinet and into the computer, and then getting a useful indexing system made. The introduction has a long way to go too, but that pales in comparison to the former task.

Definitely, if you have any suggestions for further materials that I should include in the thing (from your own work or anyone else's), or you catch any typos, please let me know.

26-05-04 *The Pauli Program* (to D. Howard)

I forgot to mention in my last long note a couple of references that might interest you. These concern what you called "The Pauli Program" (I think)—namely, the idea of taking how subsystems combine into composite systems as one of the basic pillars of quantum mechanics.

Two things to read:

1) Lucien Hardy's axiom-system paper. You can find it here: `http://www.arxiv.org/abs/quant-ph/0101012`. Hardy's paper is one of the few—maybe the only one—I can think of where an axiom for combining systems plays such a prominent role. (I would say his system is too positivistic/instrumentalistic for my taste, but nevertheless he makes wonderful mathematical progress.)

2) Section 5, "Wither Entanglement?," of my paper `http://www.arxiv.org/abs/quant-ph/0205039`. There I show how the tensor-product structure can be gotten out of a Gleason-type theorem. It's not clear to me how such a result would fit into a Pauli program—it may be antithetical to it—but it might provide some food for thought nevertheless. I myself certainly remain sympathetic to some aspects of the Pauli program. (You can see by looking at pages 548–554 of the book I'm sending you that I've long had an interest in the idea.)

By the way, don't forget to send me the papers you promised.

27-05-04 *Conflict* (to K. R. Duffy)

The unrelenting external world smites me again: Kiki just told me that Emma's school performance is next Tuesday night. So the Pub is out.

01-06-04 *Bathroom Reading, 2* (to M. Janssen)

Thanks for the great Slater quote. I love collecting things like that.

Thanks also for the encouraging words about our research program. After writing you, I used the same letter as a template and extended it a bit more in writing to Tony Leggett. Let me paste that in below, as it might give you a little more orientation for where to seek out further information (for instance papers by Bub, Hardy, Mermin, Spekkens, etc., that I think are great steps forward in the effort).

Michel's Preply

> For your amusement I quote from a letter of July 27, 1924 from John C. Slater to Minnesota's own John H. Van Vleck with Slater's impression of Copenhagen from which he is just returning (the letter is written as he sails along the coast of New England):
>
>> Don't remember just how much I told you about my stay in Copenhagen. The paper with Bohr and Kramers was got out of the way the first six weeks or so—written entirely by Bohr and Kramers. That was very nearly the only paper that came from the institute at all the time I was there; there seemed to be very little doing. Bohr does very little and is chronically overworked by it. The paper that was all written last Fall still hasn't been revised and printed, and I don't know when it will be. Bohr had to go on several vacations in the spring, and came back worse from each one. Kramers hasn't got much done, either. You perhaps noticed his letter to *Nature* on dispersion; the formulas that he had before I came, although he didn't see the exact application; and except for that he hasn't done anything, so far as I know. They seem to have too much administrative work to do. Even at that, I don't see what they do all the time. Bohr hasn't been teaching at all, Kramers has been giving one or two courses.
>>
>> There were visits from both Pauli and Heisenberg in the course of the spring. I don't know whether you have met either one. Heisenberg is a very nice red haired unassuming young chap, talks a little English, and everybody likes him. Pauli is as different as he could be, a big fat Jew, with a very good opinion of himself and a great liking to hear himself talk. Still, he is a good natured and accommodating person, and well liked.

01-06-04 *Dreams* (to A. Sudbery)

Thanks for keeping me apprised of the Foundations meeting in September. I really feel like I should be there—I want to be there—but I don't think I can get back so soon after my move back to the States. Also there's the little issue that Bell Labs has dried up as far as travel funds are concerned now, and I don't have any other grants.

On a different matter, strangely I had a dream about you the other night. We were, of course, debating quantum foundations. (To set the scene, you had had some more dental work done, so your mouth was bleeding again; I think we were in an empty conference room.) And for some reason in the dream, I thought it was really important for you to read some passages I had scanned into my computer the night before—that is, passages that I had really scanned in, and not just in the dream. I'm not making this up. Anyway, upon waking, I couldn't figure out why it was so all-fired important that you read the passages, but still I'll send them to you. They're attached [from William James's essay, "The Sentiment of Rationality"].

> A completed theoretic philosophy can thus never be anything more than a completed classification of the world's ingredients; and its results must always be abstract, since the basis of every classification is the abstract essence embedded in the living fact—the rest of the living fact being for the time ignored by the classifier. This means that none of our explanations are complete. They subsume things under heads wider or more familiar; but the last heads, whether of things or of their connections, are mere abstract genera, data which we just find in things and write down.
>
> When, for example, we think that we have rationally explained the connection of the facts A and B by classing both under their common attribute x, it is obvious that we have really explained only so much of these items as *is* x. To explain the connection of choke-damp and suffocation by the lack of oxygen is to leave untouched all the other peculiarities both of choke-damp and of suffocation—such as convulsions and agony on the one hand, density and explosibility on the other. In a word, so far as A and B contain l, m, n, and o, p, q, respectively, in addition to x, they are not explained by x. Each additional particularity makes its distinct appeal. A single explanation of a fact only explains it from a single point of view. The entire fact is not accounted for until each and all of its characters have been classed with their likes elsewhere. To apply this now to the case of the universe, we see that the explanation of the world by molecular movements explains it only so far as it actually *is* such movements. To invoke the "Unknowable" explains only so much as is unknowable, "Thought" only so much as is thought, "God" only so much as is God. *Which* thought? *Which* God?—are questions that have to be answered by bringing in again the residual data from which the general term was abstracted. All those data that cannot be analytically identified with the attribute invoked as universal principle, remain as independent kinds or natures, associated empirically with the said attribute but devoid of rational kinship with it.
>
> Hence the unsatisfactoriness of all our speculations. On the one hand, so far as they retain any multiplicity in their terms, they fail to get us out of the empirical sand-heap world; on the other, so far as they eliminate multiplicity, the practical man despises their empty barrenness. The most they can say is that the elements of the world are such and such, and that each is identical with itself wherever found; but the question Where is it found? the practical man is left to answer by his own wit. Which, of all the essences, shall here and now be held the essence of this concrete thing, the fundamental philosophy never attempts to decide. We are thus led to the conclusion that the simple classification of things is, on the one hand, the best possible theoretic philosophy, but is, on the other, a most miserable and inadequate substitute for the fulness of the truth. It is a monstrous abridgment of life, which, like all abridgments, is got by the absolute loss and casting out of real matter. This is why so few human beings truly care for philosophy. The particular determinations which she ignores are the real matter exciting needs, quite as potent and authoritative as hers. What does the moral enthusiast care for philosophical ethics? Why does the *Æsthetik* of every German philosopher appear to the artist an abomination of desolation?
>
> Grau, teurer Freund, ist alle Theorie
> Und grün des Lebens goldner Baum.
>
> The entire man, who feels all needs by turns, will take nothing as an equivalent for life but the fulness of living itself. Since the essences of things are as a matter of fact

disseminated through the whole extent of time and space, it is in their spread-outness and alternation that he will enjoy them. When weary of the concrete clash and dust and pettiness, he will refresh himself by a bath in the eternal springs, or fortify himself by a look at the immutable natures. But he will only be a visitor, not a dweller, in the region; he will never carry the philosophic yoke upon his shoulders, and when tired of the gray monotony of her problems and insipid spaciousness of her results, will always escape gleefully into the teeming and dramatic richness of the concrete world.

So our study turns back here to its beginning. Every way of classifying a thing is but a way of handling it for some particular purpose. Conceptions, "kinds," are teleological instruments. No abstract concept can be a valid substitute for a concrete reality except with reference to a particular interest in the conceiver. The interest of theoretic rationality, the relief of identification, is but one of a thousand human purposes. When others rear their heads, it must pack up its little bundle and retire till its turn recurs. The exaggerated dignity and value that philosophers have claimed for their solutions is thus greatly reduced. The only virtue their theoretic conception need have is simplicity, and a simple conception is an equivalent for the world only so far as the world is simple—the world meanwhile, whatever simplicity it may harbor, being also a mightily complex affair. Enough simplicity remains, however, and enough urgency in our craving to reach it, to make the theoretic function one of the most invincible of human impulses. The quest of the fewest elements of things is an ideal that some will follow, as long as there are men to think at all.

But suppose the goal attained. Suppose that at last we have a system unified in the sense that has been explained. Our world can now be conceived simply, and our mind enjoys the relief. Our universal concept has made the concrete chaos rational. But now I ask, Can that which is the ground of rationality in all else be itself properly called rational? It would seem at first sight that it might. One is tempted at any rate to say that, since the craving for rationality is appeased by the identification of one thing with another, a datum which left nothing else outstanding might quench that craving definitively, or be rational *in se.* No otherness being left to annoy us, we should sit down at peace. In other words, as the theoretic tranquillity of the boor results from his spinning no further considerations about his chaotic universe, so any datum whatever (provided it were simple, clear, and ultimate) ought to banish puzzle from the universe of the philosopher and confer peace, inasmuch as there would then be for him absolutely no further considerations to spin.

This in fact is what some persons think. Professor Bain says —

> A difficulty is solved, a mystery unriddled, when it can be shown to resemble something else; to be an example of a fact already known. Mystery is isolation, exception, or it may be apparent contradiction: the resolution of the mystery is found in assimilation, identity, fraternity. When all things are assimilated, so far as assimilation can go, so far as likeness holds, there is an end to explanation; there is an end to what the mind can do, or can intelligently desire .... The path of science as exhibited in modern ages is toward generality, wider and wider, until we reach the highest, the widest laws of every department of things; there explanation is finished, mystery ends, perfect vision is gained.

But, unfortunately, this first answer will not hold. Our mind is so wedded to the process of seeing an *other* beside every item of its experience, that when the notion of an absolute datum is presented to it, it goes through, its usual procedure and remains pointing at the void beyond, as if in that lay further matter for contemplation. In short, it spins for itself the further positive consideration of a nonentity enveloping the being of its datum; and as that leads nowhere, back recoils the thought toward its datum again. But there is no natural bridge between nonentity and this particular datum, and the thought stands oscillating to and fro, wondering "Why was there anything but nonentity; why just this universal datum and not another?" and finds no end, in wandering mazes lost. Indeed, Bain's words are so untrue that in reflecting men it is just when the attempt to fuse the manifold into a single totality has been most successful, when the conception of the universe as a unique fact is nearest its perfection, that the craving for further explanation, the ontological wonder-sickness, arises in its extremest form. As Schopenhauer says, "The uneasiness which keeps the never-resting clock of metaphysics in motion, is the consciousness that the non-existence of this world is just as possible as its existence."

The notion of nonentity may thus be called the parent of the philosophic craving in its subtilest and profoundest sense. Absolute existence is absolute mystery, for its relations with the nothing remain unmediated to our understanding. One philosopher only has pretended to throw a logical bridge over this chasm. Hegel, by trying to show that nonentity and concrete being are linked together by a series of identities of a synthetic kind, binds everything conceivable into a unity, with no outlying notion to disturb the free rotary circulation of the mind within its bounds. Since such unchecked movement gives the feeling of rationality, he must be held, if he has succeeded, to have eternally and absolutely quenched all rational demands.

But for those who deem Hegel's heroic effort to have failed, nought remains but to confess that when all things have been unified to the supreme degree, the notion of a possible other than the actual may still haunt our imagination and prey upon our system. The bottom of being is left logically opaque to us, as something which we simply come upon and find, and about which (if we wish to act) we should pause and wonder as little as possible. The philosopher's logical tranquillity is thus in essence no other than the boor's. They differ only as to the point at which each refuses to let further considerations upset the absoluteness of the data he assumes. The boor does so immediately, and is liable at any moment to the ravages of many kinds of doubt. The philosopher does not do so till unity has been reached, and is warranted against the inroads of those considerations, but only practically, not essentially, secure from the blighting breath of the ultimate Why? If he cannot exorcize this question, he must ignore or blink it, and, assuming the data of his system as something given, and the gift as ultimate, simply proceed to a life of contemplation or of action based on it.

01-06-04 *Shielding the Mathematics* (to I. Pitowsky)

I thought of you the other day as I was reading a passage in one of Quine's books. In particular I thought of the conversation we had on the plane to New Jersey, where you told me your thoughts on how a frequentist account of probability might be salvaged. It struck me that the move you're toying with is in no longer "shielding the mathematics" in Quine's sense.

Anyway, I'll place the passage below for your enjoyment.

- W. V. Quine, *Pursuit of Truth*, revised edition (Harvard U. Press, Cambridge, MA, 1992).

Let us recall that the hypothesis regarding the chemical composition of litholite did not imply its observation categorical single-handed. It implied it with the help of a backlog of accepted scientific theory. In order to deduce an observation categorical from a given hypothesis, we may have to enlist the aid of other theoretical sentences and of many common-sense platitudes that go without saying, and perhaps the aid even of arithmetic and other parts of mathematics.

In that situation, the falsity of the observation categorical does not conclusively refute the hypothesis. What it refutes is the conjunction of sentences that was needed to imply the observation categorical. In order to retract that conjunction we do not have to retract the hypothesis in question; we could retract some other sentence of the conjunction instead. This is the important insight called *holism*. Pierre Duhem made much of it early in this century, but not too much.

The scientist thinks of his experiment as a test specifically of his new hypothesis, but only because this was the sentence he was wondering about and is prepared to reject. Moreover, there are also the situations where he has no preconceived hypothesis, but just happens upon an anomalous phenomenon. It is a case of his happening upon a counter-instance of an observation categorical which, according to his current theory as a whole, ought to have been true. So he looks to his theory with a critical eye.

Over-logicizing, we may picture the accommodation of a failed observation categorical as follows. We have before us some set S of purported truths that was found jointly to imply the false categorical. Implication may be taken here simply as deducibility by the logic of truth functions, quantification, and identity. (We can always provide for more substantial consequences by incorporating appropriate premises explicitly into S.) Now some one or more of the sentences in S are going to have to be rescinded. We exempt some members of S from this threat on determining that the fateful implication still holds without their help. Any purely logical truth is thus exempted, since it adds nothing to what S would logically imply anyway; and sundry irrelevant sentences in S will be exempted as well. Of the remaining members of S, we rescind one that seems most suspect, or least crucial to our overall theory. We heed a maxim of minimum mutilation. If the remaining members of S still conspire to imply the false categorical, we try rescinding another and restoring the first. If the false categorical is still implied, we try rescinding both. We continue thus until the implication is defused.

But this is only the beginning. We must also track down sets of sentences elsewhere, in our overall theory, that imply these newly rescinded beliefs; for those must be defused too. We continue thus until consistency seems to be restored. Such is the mutilation that the maxim of minimum mutilation is meant to minimize.

In particular the maxim constrains us, in our choice of what sentences of S to rescind, to safeguard any purely mathematical truth; for mathematics infiltrates all branches of our system of the world, and its disruption would reverberate intolerably. If asked why he spares mathematics, the scientist will perhaps say that its laws are necessarily true; but I think we have here an explanation, rather, of mathematical necessity itself. It resides in our unstated policy of shielding mathematics by exercising our freedom to reject other beliefs instead.

01-06-04 *ReAktivation* (to M. Pérez-Suárez)

Pérez-Suárezism 6: *Great, my copy of 'The Activating Observer' includes 113 pages, 455 references.*

It's now at 187 pages, 513 references. Mainly I've significantly beefed up the pragmatism references—James is quoted extensively now. Also I've added a fair amount from the Jung-Pauli correspondence (Pauli's side). If you want it at this stage, I can send you the pdf; I don't think I'll be working on it again for a while.

Pérez-Suárezism 7: *Btw, I have found a very interesting letter (at the very least, its opening paragraph) from Pauli to Bohm, which is item 1313 (a nice number) in the PWB collection, vol. 1950–1952. I think that is one of his clearest statements on the "attached observer". I'll take the book and copy it for you (the paragraph I referred to, not the whole thing) later on.*

I should peruse those volumes. Are many of the letters in English? Thanks for the offer. Please try to include as much of the letter as possible.

01-06-04 *Quantum System as Philosopher's Stone* (to M. Pérez-Suárez)

First a joke: Notice the similarity between the symbol drawn near the top of this page

`http://www.starchamber.com/1997/09/the-rewards-of-the-infinite.html`

and a modern quantum circuit. Archetypes everywhere!

(I'll cc this note to David Mermin; he might enjoy the joke too.)

More seriously, the phrase in the title of this note is something I've toyed with as a title for a paper I'd like to write one day. It's been on my mind ever since I wrote these words for `quant-ph/0404156`:

> In fact, one might go further and say of quantum theory, that in those cases where it is not just Bayesian probability theory full stop, it is a theory of stimulation and response. The agent, through the process of quantum measurement stimulates the world external to himself. The world, in return, stimulates a response in the agent that is quantified by a change in his beliefs—i.e., by a change from a prior to a posterior quantum state.

With the aid of a quantum system—and only with its aid—a transmutation takes place in the agent. Of course, these ideas flow directly from Pauli; I'm just trying to give them more rigorous substance. (See for instance, the Heisenberg entry "Wolfgang Pauli's Philosophical Outlook" in my compendium. Or more generally, any number of entries under Pauli himself.)

Pérez-Suárezism 8: *As I promised, here you have the paragraph I told you from Pauli's letter to Bohm, dated December 3, 1951:*

> "Since Descartes, it was the ideal of natural philosophy to conceive a system of laws in which an entirely loose and untied observer is looking from outside at a part of the world completely determined by these laws. For me, however, it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to *conceive* the disturbances in the observer's own body and own brain connected with his own observations."

Well, it didn't turn out to be either the opening paragraph of the letter or his clearest statement on the rejection of the ideal of the "detached observer". Actually, I am not sure as to what he means by his last sentence (I have my own guess, though).

I think he means "quantum system as philosopher's stone."

02-06-04 *Dreams, 2* (to A. Sudbery)

Sudberyism 14: *We Everettians must be getting to you.* ☺

Keep in mind that I (like most people) also have dreams about snakes and rats too.

02-06-04 *The Chemical Wedding* (to M. Pérez-Suárez)

I promise to write more tomorrow. Thanks for the references. I have to admit that I'm getting more interested in the alchemical aspects of the whole issue again. Maybe it's your dangerous influence upon me!

Marcos's Preply 1

Thanks for your note. As soon as I read your words under the heading "Subject", I found myself thinking that it would make for a great title!

Yes, the similarity is really amazing, much more than I was expecting it to be while downloading the webpage. But let me suggest to you two books, namely,

- A. Roob: *Alchemy and Mysticism* (Taschen) (unbelievably INexpensive given its contents),
- S. Klossowski de Rola: *Alchemy. The Secret Art* (Thames and Hudson) (A tougher nut than the previous one. Sometimes it is not even that easy to discern whether what you are reading is the main text or paragraphs quoted from alchemical writings.)

and a website: `http://www.levity.com/alchemy/`.

The three of them are quite interesting if only for the huge amount of (enrapturing, breathtaking, amazing ...) images from many ages (although Middle Ages and Renaissance take most of them, for sure) that they include. But to me they are interesting in more than that.

I'll think harder on Pauli's sentence in that letter, but he writes:

> [...] much more satisfactory if the laws of nature themselves exclude in principle the possibility even to *conceive* the disturbances in the observer's own body and own brain connected with his own observations.

I mean, he says "excludes". This is where I get lost.

Marcos's Preply 2

Take a look at this (included in one of the books I told you about in a previous note). This is my own translation from the Spanish (the edition I own):

> At the microphysical level there is a tight ontological [imbrication?] between the subject and the object under observation. It has been necessary to admit that subjectivity is an active agent in the development of natural processes, processes that some alchemists define as the permanent inversion of the inner and the outer.

And later on, he quotes Whitehead after making (the author) some reference to the "mercurial" character of physics: "Exactness is a fake".

I insist that this is a wonderful book, if only for its pictorial reproductions. If only it were larger ...

02-06-04 *Leggett* (to G. Brassard)

Thanks for the note. I'm just about to go out for a "good-bye" pub meeting with my friend Ken Duffy. (When John Lewis died, he got reassigned to Maynooth; today was his last day here.)

Your note is cherished. I'll send you that longer reply tomorrow.

In the meantime:

Brassardisme 14: *You may be interested to know that I gave our talk on QFLQI (with your name explicitly as coauthor on the title slide) a few days ago in front of 2003 Physics Nobel Prize laureate Tony Leggett. He showed interest with several questions, but in the end I don't think he bought the project.*

Pasted below is my own recent letter to Leggett. [See 25-05-04 note "Bathroom Reading" to A. J. Leggett.] Like with you, I saw him have little flashes of understanding, but ultimately he didn't buy it. But that's OK, with concerted efforts, we'll eventually get there.

I did get this recently from a Minnesota professor (Michel Janssen) who was at the same meeting as Leggett:

Janssenism 1: *Anyways, may this serve as a surrogate for a substantial response to your work. I too am glad that we had that chat. Without it, I'm afraid, I would have just dismissed your stuff as crackpot physics. Now I see it as the one serious contender to my favorite scheme—Everett + decoherence.*

These kinds of reactions keep rolling in ... just no job market for me.

03-06-04 *Your Address and Six or So Books Coming* (to H. Mabuchi)

If you don't mind, I might have some books from Amazon or Powell's shipped to your house to await my arrival. That'll save me quite a bit in shipping. ...

I think in total I ordered six books and had them shipped to your home address. Thanks for letting me do this. The books are all on Schiller, Boutroux, and pragmatism. Feel free to dig into them if you want.

04-06-04 *Earwax* (to P. C. E. Stamp)

Stampede 1: *Thanks for sending the copy of your book – and I will put it in the bathroom, where it will be a useful antidote to the fashion magazines that my wife leaves there.*

Quoting you:

Stampede 2: *Regarding the information theory programme – I have indeed got the message that you are trying to do something analogous to, e.g., Skinner and Watson, with your black box (ie., stimulus-response) approach. However I have several remarks to make on this*

(i) Like many people I find the black box approach scientifically sterile – and I didn't need, e.g., Chomsky to tell me why, it has been understood to be sterile for a long time now (from this perspective Skinner-Watson was apparently just an ephemeral aberration). The problem with a black box approach is that it ignores what may be going on inside or outside the box, which exists without the stimuli and may not be completely characterised by the responses. Of course hidden variable theories have been toasted so far, most generally by Bell-Kochen-Specker, but that is no reason to cease looking for something more than a "measurement calculus" (which is essentially what the information approach is doing as far as I can see). This is what physicists do, and they have ended up with quite a dazzling display of objects ranging from non-Abelian gauge fields to macroscopic order parameters and 'wave-functions' to topological quantum fluids to complex spin nets, etc. The attempt to strip this down to nothing but stimulus and response, in a measurement or information theoretic calculus, is very much like the attempt by Skinner and Watson to take all the colour out of psychology – at least in my opinion. This is something of a gut reaction, but I suspect it is shared by many physicists.

If you can say this, you haven't listened to me very carefully. It seems to me that you're just rather offhandedly drawing associations with things you have a distaste for because of little more than a similarity of words.

For instance, how can you make your characterization above mesh with these words that I wrote in `quant-ph/0205039`? (I hope you'll read the passage all the way to the end; the average listener doesn't seem to hear anything beyond the first paragraph.):

> So, throw the existing axioms of quantum mechanics away and start afresh! But how to proceed? I myself see no alternative but to contemplate deep and hard the tasks, the techniques, and the implications of quantum information theory. The reason is simple, and I think inescapable. Quantum mechanics has always been about information. It is just that the physics community has somehow forgotten this.
>
> This, I see as the line of attack we should pursue with relentless consistency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

> Let me try to give a better way to think about this by making use of Einstein again. What might have been his greatest achievement in building general relativity? I would say it was in his recognizing that the "gravitational field" one feels in an accelerating elevator is a coordinate effect. That is, the "field" in that case is something induced purely with respect to the description of an observer. In this light, the program of trying to develop general relativity boiled down to recognizing all the things within gravitational and motional phenomena that should be viewed as consequences of our coordinate choices. It was in identifying all the things that are "numerically additional" to the observer-free situation—i.e., those things that come about purely by bringing the observer (scientific agent, coordinate system, etc.) back into the picture.
>
> This was a true breakthrough. For in weeding out all the things that can be interpreted as coordinate effects, the fruit left behind finally becomes clear to sight: It is the Riemannian manifold we call spacetime—a mathematical object, the study of which one can hope will tell us something about nature itself, not merely about the observer in nature.
>
> The dream I see for quantum mechanics is just this. Weed out all the terms that have to do with gambling commitments, information, knowledge, and belief, and what is left behind will play the role of Einstein's manifold. That is our goal. When we find it, it may be little more than a minuscule part of quantum *theory*. But being a clear window into nature, we may start to see sights through it we could hardly imagine before.[77]

The very point of the whole program is to see quantum systems as MORE than just black boxes. But to do that, a lot of crud to do with observers or agents has to first be excised from the theory—or, at least, that's what the program I'm working on is about: Getting to the physical kernel of quantum mechanics. Here's the way I put it in a recent proposal:

> No physicist would be doing his job if he did not strive to map reality itself—that is, reality as it is independently of any information processing agents. The issue is one of separating the wheat from the chaff: Quantum mechanics may be predominantly about information, but it cannot *only* be about information. Which part is which? The usual way of formulating the theory is a thoroughly mixed soup of physical and informational ingredients.
>
> This is where quantum information (including the collateral fields of quantum cryptography, computing, and communication theory) has a unique role to play. Its tasks and protocols naturally isolate the parts of quantum theory that should be given the most foundational scrutiny. "Is such and such effect due simply to a quantum state being a state of information rather than a state of nature, or is it due to the deeper issue of what the information is about?" Recent investigations by several workers are starting to show that many of the previously-thought 'fantastic' phenomena of quantum information—like quantum teleportation, the no-cloning theorem, superdense coding,

[77]I should point out to the reader that in opposition to the picture of general relativity, where reintroducing the coordinate system—i.e., reintroducing the observer—changes nothing about the manifold (it only tells us what kind of sensations the observer will pick up), I do not suspect the same for the quantum world. Here I suspect that reintroducing the observer will be more like introducing matter into pure spacetime, rather than simply gridding it off with a coordinate system. "Matter tells spacetime how to curve (when matter is there), and spacetime tells matter how to move (when matter is there)." Observers, scientific agents, a necessary part of reality? No. But do they tend to change things once they are on the scene? Yes. If quantum mechanics can tell us something truly deep about nature, I think it is this.

and nonlocality without entanglement—come about simply because of the epistemic nature of the quantum state. On the other hand, other phenomena, such as the potential computational speed-up of quantum computing, seem to come from a more physical source: In particular, the answer to the question, "Information about what?"

When we finally delineate a satisfying answer to this, physics will reach a profound juncture. We will for the first time see the exact nature of 'quantum reality' and know what to do with it to achieve the next great stage of physics. Trickle-down effects could be the solution to the black-hole information paradox—perhaps already seen in broad outline—and even the meshing together of quantum theory and gravitational physics. In the meantime the approach proposed here is a conservative and careful one; the work to be done is large. The effort aims not to say first what 'quantum reality' is, but what it is not and gather insights all along the way.

The last thing I want to do is view quantum theory as a species of a rather sterile positivism, or as you put it, as no more than a "measurement calculus". It is clearly more than that.

Where you misread me is in thinking that taking an epistemic view of the quantum state—epistemic, in particular, about the potential results of measurements and nothing more—is necessarily throwing out the baby with the bathwater. It is not. It is instead a strategy for getting around all the nasty foundational problems of a realistically construed "wave-function collapse" while not going so far as the emptiness of Everett or Bohm (or the unchecked and little-motivated speculation of GRW). The point is to salvage realism by looking for it at a *higher level* than the level of quantum states and their equations of motion.

For one can ask, and one should ask: Why this "measurement calculus" rather than another? There is some *property* of the stuff of the world, or of physical systems, that forces the particular calculus we use upon us—it is not arbitrary, and to that extent, it tells us something about the world as it is without the gambling agent.

Here's yet another way I've put the program recently, in a letter to Jim Woodward: [See note dated 18-05-04, and titled "Agents, Interventions, and Surgical Removal."]

There, I hope that's enough to defend myself from your accusation of being to quantum mechanics what Skinner and Watson were to psychology. Or, at least, to show you that my heart is not where you think it is.

You want color? Then let me tell you about the actual origin of my use of the phrase "stimulation and response"—it was not Skinner or Watson or the like. Instead, the words comes from the palette of imagery I've been building up ever since I started calling my view "the sexual interpretation of quantum mechanics". (See *Quantum States: What the Hell Are They?* posted on my webpage, last paragraph page 49. See also the note to Mermin below.) Put yourself in a lascivious mindframe, and then once again approach the word "stimulation." I hope it leaves an indelible mark on you.

14-06-04 *RMTTR,BOWEAL* (to H. Mabuchi)

The title to the note would have been "Reality Modifiable To the Roots, But Only with Effort and Luck," but it looked too long, and so I shortened it.

Our conversation this morning emboldened me; I rather enjoyed the part about the Upanishads, etc. In that regard, let me attach two of my pseudo-papers, "Delirium Quantum" and "The Activating Observer". You might enjoy perusing parts of both. Discussions like the one with you this morning make me feel like maybe I'm not going too far after all. Indeed, maybe I'm not going far enough!

OK, the last sentence was a little of a joke . . . but maybe ultimately not. Still, if I use quantum mechanics as the firmest place I can start, I don't think it yet countenances a move that far, i.e., that that which is without is actually within (Brahman = Atman). But I think QM does move somewhat in the direction, by making that which is without to be somewhat malleable. And to that extent, that which is without looks to be somewhat within. (It's kind of fun writing like this.) Anyway, true enough, the effort does remain with the distinction without/within—which I am presently comfortable with. It doesn't completely erase it.

If you enjoy anything in these writings, give me feedback.

14-06-04 *The Toy Intro* (to H. Mabuchi)

Finally, I'll share with you the toy intro I wrote for the basic quantum mechanics talk. Since I almost surely won't say it out loud (or at least in such detail), I might as well share it with somebody. [See 17-06-04 note, "Preamble," to H. Mabuchi.]

14-06-04 *Concrescence?* (to W. K. Wootters)

BTW, on my flight yesterday, I sketched out the following little introduction for my summer school lectures here at Caltech. I don't know if I'll actually say those things. But does any of it sound like the Whiteheadian ideas—I think you called it concrescence—you told us about in Montréal two Falls ago? [See 17-06-04 note, "Preamble," to H. Mabuchi.]

Bill's Reply

> Yes, it seemed to me when I was reading Whitehead that the emergence of a definite fact, out of quantum potentialities, should count as an example (maybe the only example) of concrescence. It is the process of becoming concrete.
>
> I don't have my Whitehead with me here, but from a couple of webpages I got the following definitions:
>
> concrescence – a dipolar process involving an interplay between physical feelings and mental valuations (PR 108) in which prehensions of early phases are contrasted in later phases. In short, it is the production of novel togetherness (PR 21.)
>
> Whitehead defines 'concrescence' as a process in which prehensions are integrated into a fully determinate feeling or satisfaction. A 'satisfaction' is a unity of physical or mental operation attained by an actual entity.

17-06-04 *Preamble* (to H. Mabuchi)

I think I would like you to also post the little text file below along with my other suggested readings for my "Intro to QM" lecture. You can give it the title "Preamble". It was something I sketched out on my flight over here, and reading over it again, I kind of like it.

> A lecturer faces a dilemma when teaching a course at a farsighted summer school like this one. This is because, when it comes to research, there is often a fine line between what one thinks and what is demonstrable fact. More than that, conveying to the students what one thinks—in other words, one's hopes, one's desires, the potentest of one's premises—can be just as empowering to the students' research lives (even if the

ideas are not quite right) as the bare tabulation of any amount of demonstrable fact. So I want to use one percent of this lecture to tell you what I think—the potentest of all my premises—and use the remaining ninety-nine to tell you about the mathematical structure from which that premise arises.

I think the greatest lesson quantum theory holds for us is that when two pieces of the world come together, they give birth. [Bring two fists together and then open them to imply an explosion.] They give birth to FACTS in a way not so unlike the romantic notion of parenthood: that a child is more than the sum total of her parents, an entity unto herself with untold potential for reshaping the world. Add a new piece to a puzzle—not to its beginning or end or edges, but somewhere deep in its middle—and all the extant pieces must be rejiggled or recut to make a new, but different, whole. That is the great lesson.

But quantum mechanics is only a glimpse into this profound feature of nature; it is only a part of the story. For its focus is exclusively upon a very special case of this phenomenon: The case where one piece of the world is a highly-developed decision-making agent—an experimentalist—and the other piece is some fraction of the world that captures his attention or interest.

When an experimentalist reaches out and touches a quantum system—the process usually called quantum 'measurement'—that process gives rise to a birth. It gives rise to a little act of creation. And it is how those births or acts of creation impact the agent's *expectations* for other such births that is the subject matter of quantum theory. That is to say, quantum theory is a calculus for aiding us in our decisions and adjusting our expectations in a QUANTUM WORLD. Ultimately, as physicists, it is the quantum world for which we would like to say as much as we can, but that is not our starting point. Quantum theory rests at a level higher than that.

To put it starkly, quantum theory is just the start of our adventure. The quantum world is still ahead of us. So let us learn about quantum theory.

Hideo's Reply

From *Three Farmers on Their Way to a Dance*, Richard Powers' first novel:

> "Sander," a German photographer of the early 1900's, "at the same time as those working in physics, psychology, political science, and other disciplines, blundered against and inadvertently helped uncover the principle truth of this century: viewer and viewed are fused into an indivisible whole. To see an object from a distance is already to act on it, to change it, to be changed."

18-06-04 *Retracing Thoughts* (to J. Woodward)

Retracing yesterday's conversation in my head (I've got nothing better to do; my lectures being over), I realized I didn't quite adequately respond to one of your questions near the end. It was just before we arose from the table. You asked something like, "Then what are the random variables in these probability distributions?" I replied, "Measurement outcomes." But I don't want to leave you with the impression that such an empiricist or positivist take is the end of the story. Indeed the bit below, which I wrote for the research proposal John Preskill gave you and others, addresses the very issue with the phrase, "Information about what?"

No physicist would be doing his job if he did not strive to map reality itself—that is, reality as it is independently of any information processing agents. The issue is one of separating the wheat from the chaff: Quantum mechanics may be predominantly about information, but it cannot *only* be about information. Which part is which? The usual way of formulating the theory is a thoroughly mixed soup of physical and informational ingredients.

This is where quantum information (including the collateral fields of quantum cryptography, computing, and communication theory) has a unique role to play. Its tasks and protocols naturally isolate the parts of quantum theory that should be given the most foundational scrutiny. "Is such and such effect due simply to a quantum state being a state of information rather than a state of nature, or is it due to the deeper issue of what the information is about?" Recent investigations by several workers are starting to show that many of the previously-thought 'fantastic' phenomena of quantum information—like quantum teleportation, the no-cloning theorem, superdense coding, and nonlocality without entanglement—come about simply because of the epistemic nature of the quantum state. On the other hand, other phenomena, such as the potential computational speed-up of quantum computing, seem to come from a more physical source: In particular, the answer to the question, "Information about what?"

When we finally delineate a satisfying answer to this, physics will reach a profound juncture. We will for the first time see the exact nature of 'quantum reality' and know what to do with it to achieve the next great stage of physics. Trickle-down effects could be the solution to the black-hole information paradox—perhaps already seen in broad outline—and even the meshing together of quantum theory and gravitational physics. In the meantime the approach proposed here is a conservative and careful one; the work to be done is large. The effort aims not to say first what 'quantum reality' is, but what it is not and gather insights all along the way.

What these paragraphs are not detailed enough convey is that the issue boils down to the need for a much better understanding of the implications of the Kochen-Specker theorem: That is, the theorem that blocks measurement outcomes from being preexistent realities in the usual quantum description (i.e., without the supplementation of nonlocal hidden variables, etc.). My own feeling is that it is this part of quantum theory which gives rise to the computational speed-up in quantum computation. So, these things are intimately related I believe.

Thus, as much as I endorse taking "measurement" (or "intervention" as Asher Peres and I have called it in our articles; see pages 99, 146, 336, 384, 394, etc., and most notably page 417) as the very foundation of what the theory is about—those little acts of creation I always talk about—there is still so much work to be done to get this clarified. Part of it hinges on clarifying the particular form of "noncontextuality" for the quantum probability assignment, but also more philosophically I think it is an opportunity to rethink variations on James and Dewey's notion of truth. (Maybe I wouldn't want to apply that notion to everything in the world, as a true pragmatist (say Richard Rorty) would, but it does strike me as an appropriate concept for these particular quantum mechanical issues.)

Anyway, I hope that tells you a little more.

I've also been thinking a little more about "Making Things Happen" in the context of quantum computing. I think there's a lot of thought one could give that. I haven't yet decided if I would count a quantum measurement as an example of "making things happen". However, one most certainly should count the enacting of a quantum computational algorithm—let's say, factoring a large composite number—as an example: The number goes in the quantum computer, the factors come out. What is interesting is that there are some new models of quantum computation that involve solely the action of quantum measurement, and make no use of time evolution at all. How do these ideas mesh together? Don't know at the moment.

I'm sitting in Steel, Rm. 305, till the end of the stay, by the way. The phone number on my desk is 4493.

For the fun of it, I'll paste below the intro to my first lecture for the CBSSS summer school that I told you about. I didn't do it quite so William-Jamesy when I did it in real life, but it conveys the idea. (These were the notes I wrote myself on the flight the other day.) The second lecture—the one I gave after we parted—went fabulously. You must've put me in the right mood. The students here are like eager sponges!

21-06-04 *Teleportation* (to G. Musser)

Musserism 6: *P.S. How do you interpret teleportation?*

Below is an outtake from a letter I wrote to Oliver Cohen a while ago; I think it answers your question. [See 15-01-03 note "Memory Lane" to O. Cohen.] I decided to be lazy and not change the page numbers for *Notes On a Paulian Idea* listed below: They refer to the web edition—which can be downloaded at my webpage—rather than the VUP edition which I had sent to you. (Did you get it?) If you have trouble downloading it, let me know and if worse comes to worst, I'll fix the page numbers up for you.

28-06-04 *Decisions, Decisions* (to J. Woodward & C. R. Hitchcock)

There's no doubt I think the time is ripe to meld philosophy of science and honest-to-god quantum information physics—both physics and philosophy will greatly benefit from the marriage—but finding a place that'll share this belief with me is mostly beyond my control. Of course I'll try to turn the tide by writing more papers (and hopefully ever more convincing papers), but that will take some time in the coming. In any case, I hope I have sufficiently piqued your interest in the subject that you might follow it a little and use your accumulated body of work on causality to make a contribution to it. Vice versa, I hope the field will give you something to think about from a fresh perspective and maybe lead to something new in your own work.

In case you're interested, you can read about the quantum de Finetti theorems I told you about in my paper with Schack, "Unknown Quantum States and Operations, a Bayesian View," posted here: `http://www.arxiv.org/abs/quant-ph/0404156`. In the time-evolution version of the theorem (as it stands), we only imagine a dynamics imposed upon our quantum systems from the outside—say, by some "device." If we were to try to prove a version of the theorem sufficient for an account of an internal dynamics—i.e., like the stuff I was telling you about at the Red Door—then I am sure we would have to take into account something like the conditions for your "interventions" to make it fly. Anyway, food for thought and some work for the future.

29-06-04 *Late, Sorry* (to G. Musser)

I'm sorry I'm late. The travel and flu took a big toll on me, and I forgot about email for a while . . . despite my promises. Bad behavior.

Anyway, let me zoom in on the paragraphs where you mention me.

Musserism 7: *... Many of those struggling to grok quantum mechanics say Einstein's mistrust [of QM] was well-founded. "This guy saw more deeply and more quickly into the problems that plague us today," says Christopher Fuchs of Bell Labs ....*

Did I really say that? It's kind of a strange construction. But maybe it's only a question of intonation. Does something like this more capture the moment: "This guy saw more deeply—and actually more quickly—into ..." (emphasizing quickly maybe)? Also, I am a bit reluctant to say that he saw into the "problems that plague us today," because I'd rather not think of them as such horrible problems. But if I said it that way, then I said it, and it's your property now. But, given the chance, I think I would say it this way if I could: "This guy saw more deeply—and actually more quickly—into the central issues of quantum mechanics than many give him credit for." I say this because he so quickly gave such a clean argument for the idea that the quantum mechanical wave function represents a "state of knowledge" rather than a "state of nature." And I think that realization quashes over half of the mysteries already. See, for instance, Section 3 of my "only a little more" paper.

The only thing that differentiates Einstein from our little effort at this juncture is that he thought physics should ultimately be about the "states" of nature, whereas the guys in my camp think that the great lesson of quantum mechanics is that physics must be about something else.

Musserism 8: *If hidden variables have a fatal flaw, it is that any attempt to reconstruct quantum mechanics from something deeper is limited by physicists' imagination.*

I'm not sure I understand this. What do you mean?

Musserism 9: *Rather than try to build the theory from the bottom up, the most respected efforts to explain the quantum work from the top down. Fuchs, a leader in this area, says that so much of quantum mechanics pertains to our information about a system rather than the system itself. As Einstein argued, it is a theory of hearsay rather than of direct knowledge. Fuchs is trying to strip away these subjective aspects so that the objective properties of nature can stand out.*

Our information about a system. More accurately, our (statistical) information about how the system will evoke a response in us—i.e., the thing usually called a "result" of a measurement. (Asher Peres likes to say, "Unperformed measurements have no results.")

Musserism 10: *Physicists had long assumed, for example, that measuring a quantum system causes it to "collapse" from a range of possibilities into a single actuality. Fuchs argues it is just our uncertainty about the system that collapses. Uncertainty about a quantum system is very different from uncertainty about a classical one, because the process of measurement cannot help but disturb a quantum system. And this sensitivity to disturbance is a clue to what a quantum system really is. Quantum mechanics may prove to be more complex than either its inventors or detractors ever envisioned ....*

This is the paragraph that particularly troubles me. For it makes this position of mine look like that of a closet hidden-variablist (i.e., one that hasn't had the intellectual honesty to see what he really stands for).

There are two issues to try to separate, and I'm not sure what's the best way to do it so that the paragraph is both accurate and pithy at the same time. In a quantum measurement, I would say two things happen: 1) something is created that was not there before, the measurement "click"—this idea bars hidden variables—and 2) something changes, this is the observer's wave function for the system, i.e., his knowledge. Physicists often get in the bad habit of muddling the two things, the creation ("the measurement result coming out of nowhere") and the change due to the creation. The "collapse" properly only refers to the change of the wave function and, thus, to knowledge. But what is the knowledge about? It is not really about the system alone, or what is there, i.e., what properties it has—if it were, then we would be talking about hidden variables ultimately—but rather about what will come about the next time the observer touches it.

Think of a quantum system as a kind of philosopher's stone. Its power is in transmuting an observer's state of knowledge. And presumably it is transmuted too in the process. Interestingly, quantum theory is predominantly about the observer's side of this state of affairs. Think about how much two parents are transformed by the process of giving birth. One parent is the quantum system, one parent is the observer; the child is the click. Quantum theory effectively only interrogates one of the parents—the observer—and, to the extent that we get some characterization of the other parent from this interrogation, it is only second-hand.

I doubt these words are helpful to you, but I'm trying.

On another point, I know that I've used the word "disturb" before, but I have been trying to get away from it because of all the incorrect imagery it evokes—it just calls out for one to think of hidden variables being disturbed. Thus, I would like to keep the idea of "sensitivity," but I hope I never say "sensitivity to disturbance" anymore (if I ever did). In the past, rather than the parenting imagery I used above, I have used the imagery of an oyster, a grain of sand, and a pearl to try to capture the idea of system, observer, and click respectively. And it was in that context that I started to speak of a "quantum system's sensitivity to the touch." No oyster, no pearl. No sand grain, no pearl. The pearl is a manifestation of the oyster's sensitivity to the touch in the presence of a sand grain.

"And the shape of this sensitivity is a clue to what a quantum system really is." — For some reason I like that sentence, but I don't know how you would fit it into what you write.

If you need more help, I should be pretty steady on my email now that I'm back in Dublin. (Though I'm flying out to house-hunt in NJ as of July 6, and you can expect an almost total shutdown until that's over with July 16 or 17.)

Sorry if I'm causing you more trouble than I'm worth.

30-06-04 *Fancy Words First* (to M. Janssen)

Janssenism 2: *[L]et me just ask you what I should read to get over the next hurdle I'll have to take before this information stuff has a prayer of dislodging Everett. The hurdle is simply this: how is your information approach different from the old ignorance interpretation of QM. And I'm sure you won't mind me spelling this out a bit to make sure you know what I mean. I take the ignorance interpretation to be saying things like this: of course, an electron has a definite position (as well as definite whatever) at all times, it's just that typically we don't know what that is. That's why we can only describe the electron in terms of a superposition of it being in all places it could possibly be. When we perform a measurement we find out what its real position is. So QM is just like classical statistical mechanics basically: it gives us probability distributions over values of variables we don't know the exact value of. The only difference is that QM tells us that we can't ever know all values of all variables at the same time, because typically the measurement of the value of one variable disturbs the value of others. I buy (what I take to be) the standard line against this view: the probability distributions generated by state vectors in Hilbert space cannot be reduced to classical probability distributions.*

From talking to Christoph Lehner, who witnessed your exchange with David Albert at this year's

Ryno-Bub fest, I understand that Albert's criticism was along these same lines (but maybe Christoph was just reading that into the exchange: it's certainly Christoph's first line of defense when I told him my Everettian faith had been tested). I'll be seeing Christoph in a week and a half and I'd appreciate it if you provide me with some target-specific ammunition.

Thanks for the note, and thanks for the alert to the *Science* article. I hope I can retrieve it.

Thanks also for the question. I'll do my best to clarify things for you; give me a couple of days to work something up specific to your question. The short story is that I am about as far away from the ignorance interpretation you speak of as one can imagine: Quantum measurement is—for me—not a case of revealing, but a case of making (making something that was explicitly not there before). That I take to be the significance of the Bell and Kochen-Specker theorems.

Particularly, you've got to understand that I draw a distinction between the quantum world (i.e., the world presumably as it really is) and the quantum formalism. The latter is not a reflection of the former in any exact-correspondence sense. That is to say, quantum theory is not a story of the world as it is completely independent of decision-making agents. Rather it is—from this point of view—a normative theory for the optimal survival of agents immersed in this kind of world. To the extent that we are using *this* normative theory of survival (i.e., this theory of decision-making and gambling) rather than *that* normative theory, we are saying something about the world as it is independent of the agent. But the strategy for saying something about the world is necessarily more oblique than one thought one could obtain in classical times.

All of this is much too subtle of stuff for David Albert to have absorbed in my talk . . . predominantly because he does not listen. His actions (at my talk and every other one I have ever seen him attend) make it clear that his purpose for attending talks is not to listen, but to have a good spit.

Anyway, I will try to write you (or direct you to) something of detail within a couple of days. In the meantime, let me attach a little poetic piece I wrote for my summer-school lectures at Caltech last week. It says a little more about the point of view above. Feel free to share any of these notes with your friend. [See 17-06-04 note, "Preamble," to H. Mabuchi.]

30-06-04 *Quail and Pheasant?* (to J. B. Lentz & S. J. Lentz)

> *Leggett, Preskill, and 22 other physicists, philosophers, and historians have gathered here for the Seven Pines Symposium. Packed into a slightly ramshackle lodge in a wooded state park, the scholars—all of them men—will share their insights, suites of rooms without telephones, and meals of roast quail and pheasant at a long communal table. Perhaps not since the famous Solvay Conferences of the early 20th century, at which Niels Bohr and Albert Einstein debated the meaning of quantum theory in their free time, has physics seemed so genteel.*

I thought you might enjoy this article from *Science* [A. Cho, "Elite Retreat Takes the Measure of a Weirdly Ordinary World," Science **304**, 1896 (2004)], which one of my acquaintances from the same meeting just sent me. I learned from it that I was eating quail and pheasant! I myself never imagined I was eating anything but chicken.

Anyway, I was particularly pleased to see the napkin pictured in the article: It means someone was taking notes at my lecture or explaining it to someone else. The fellow in the blue shirt looking down at me is the Mercedes man.

The atmosphere the author builds in the story is basically the sort of thing Kiki envisions when we dream of opening an institute in the New Hampshire countryside. Scientists eating good food because someone has put it in front of them, and happy for it ... even if they don't quite know what it is.

30-06-04 *Memory Lane* (to D. B. L. Baker)

Just ran across this on the web and it reminded me of the time we both happened to see Vampire Circus and reported it to each other one Monday morning in about 1979/80.

> Things weren't as complex in 1972 when the Spanish film *La Noche de Walpurgis* was released in the grindhouses and drive-ins of America as *The Werewolf vs. The Vampire Woman*. A big hit in Europe, cementing Paul Naschy's position as Spain's preeminent horror film star, it came to America with all the fanfare and bluster of a dusty, ratty, moth-eaten circus. I saw it as a young teenager one gummy-eyed late night in the '70's on San Antonio's KENS-TV's *Project Terror* program ("Where the scientific and the mystifying merge ..."). *Project Terror*'s programming consisted mainly of European horror at the time, and although some were heavily edited, one could see a glimpse of a bare breast once in a while and some cool but cheap gross-out gore. Ah, adolescence! I don't remember either actively liking or disliking it, but I do remember thinking that the werewolf makeup looked cheap and that the vampire woman (played by Patty Shepard) looked pretty hot, so much that I rooted for her in the climactic battle with the good guy wolfman Waldemar Daninsky played by Paul Naschy. A silly diversion, nothing more, nothing less. I hadn't thought about the movie since I saw it over 25 years ago. I've seen some other Naschy horror titles on video since then (*House of Psychotic Women* is one that immediately comes to mind), and they're certainly fun to watch if you don't mind the fast, cheap, and out of control nature of the movies, but they contain none of the cruel poetry and transgressive power of the best of '70's Euro-horror (de Ossorio's *Blind Dead* series, Rollin's vampire films, Argento's Suspiria, even a couple of Franco's films). Naschy's work is a throwback to the Universal horrors of the '30's and '40's, albeit with nudity and gore, but unfortunately adds nothing new to the mix.

30-06-04 *Writing Physics* (to G. Musser)

Musserism 11: *Another issue that I'm struggling with is this business of separating states of knowledge from states of nature. Isn't a state of knowledge a state of nature where "nature" includes the observer? The wavefunction is a statement about me, but I'm part of nature, right?*

Honestly, I am quite intrigued by your question. It's perceptive and a good one—you articulated it well. I hope to use this as an opportunity to "write physics" in Mermin's sense. If you haven't looked at it yet, read David's article "Writing Physics":

`http://www.lassp.cornell.edu/mermin/KnightLecture.html`.

01-07-04 *Friends and Enemies* (to S. Hartmann)

Wow! Thanks for the letter. After reading your letter, I wanted to hire the guy you were talking about; I hope I get a chance to meet him some day.

Anyway, in reading it, it dawned on me that I ought to contrast my emotions with the old phrase, "With a friend like that, who needs enemies?" After reading your letter, I thought, "Wow, with a friend like that, maybe I can afford even a few more enemies!"

Which brings me to Amit. I'd be more than happy to look at your student's paper! . . . Ahh, don't get me wrong, Amit and I actually have a very friendly relationship—he is truly a nice guy, and we've had a lot of personal conversations. It's just that he didn't read me nearly as thoroughly or carefully as he thought he had. I found his paper pretty annoying. For instance, all this talk about "decoherence" and "for all practical purposes" in one of the sections had nothing to do with me: I've even gone out of my way to disavow that decoherence has anything whatsoever to do with quantum foundational issues. (See for instance the introduction to my `quant-ph/0106166`. That's probably the strongest statement, but there are many places beyond that, and in more technical detail.) Also, in his paper it keeps coming through that he had never grasped what I take the quantum state to be knowledge (now belief) about. Else, when talking about me, he would have never talked about the wave function being "knowledge of the state." That's a phrase I would never use. Here's the way I explained it most recently to Philip Stamp.

> The main point I was trying to make to you . . . is that in an information-based interpretation of quantum mechanics (like the one Caves, Schack, Mermin, Peres, Hardy, and few others of us are trying to put together) is that: Though we take the wavefunction to be information, we never take it to be information about "what is existent" in the quantum system, or "what is happening in the quantum system." That it cannot be information about something like that is what I take to be the great lesson of the Bell-Kochen-Specker theorem. Rather it is always information about how a system will *respond* to an external stimulation (i.e., in usual language, a measurement . . . but I want to get away from the usual language because it carries the wrong imagery). This is the way to think of it when the wave function is about a single system and the way to think of it when the wave function is about a composite system—it doesn't matter.
>
> So, what I am asking—among other things—is that the notion of "measurement" be replaced with the idea of "stimulation and response." We only use wave functions to calculate our expectations with regard to how a system will respond to one or another of our possible stimulations upon it. In this light, there is no contradiction between an "inside view" of a measurement where an unpredictable click really happens and an "outside view" (encompassing both the original system and the first observer), where a wave function for the composite system undergoes a smooth and deterministic transition. This is because the "outside view" wave function refers again to NOTHING MORE THAN the outside observer's expectations for how that composite system will respond to external stimulations upon it.

If Amit had understood that properly, he would not have misapplied Hemmo's argument to this context.

But they say in the Hollywood circles, no advertisement is bad advertisement. (And Caltech is close to Hollywood.) So, I'm keen to hear about your student's paper. Certainly, feel free to share any of the technical parts of my emails with him (like some of the stuff in here) that you wish. And I can certainly give pointers to other relevant materials after reading the draft if that would be useful to you guys.

I suppose I'm hard on the philosopher, being such a moving target to attack or analyze—i.e., the fact that my position is evolving rather than being settled yet (maybe it'll never be settled). But all I'm shooting for is airtight clarity, and that can be a tough process. Reading other's perceptions of the program is quite useful in that regard.

01-07-04 *Quail and Pheasant?, 2* (to S. Hartmann)

You might enjoy this and the story below too. [See 30-06-04 note "Quail and Pheasant?" to J. B. Lentz & S. J. Lentz.] It was nice to see (*what I hope will eventually be*) a quantum-Bayesian trademark—i.e., a probability simplex with a restricted area—making an appearance in such a prominent place.

05-07-04 *Your Friend Philip* (to M. Janssen)

Let me come back to your question now:

Janssenism 3 : *[L]et me just ask you what I should read to get over the next hurdle I'll have to take before this information stuff has a prayer of dislodging Everett. The hurdle is simply this: how is your information approach different from the old ignorance interpretation of QM. And I'm sure you won't mind me spelling this out a bit to make sure you know what I mean. I take the ignorance interpretation to be saying things like this: of course, an electron has a definite position (as well as definite whatever) at all times, it's just that typically we don't know what that is. That's why we can only describe the electron in terms of a superposition of it being in all places it could possibly be. When we perform a measurement we find out what its real position is. So QM is just like classical statistical mechanics basically: it gives us probability distributions over values of variables we don't know the exact value of. The only difference is that QM tells us that we can't ever know all values of all variables at the same time, because typically the measurement of the value of one variable disturbs the value of others. I buy (what I take to be) the standard line against this view: the probability distributions generated by state vectors in Hilbert space cannot be reduced to classical probability distributions.*

As I've already told you, as I see it, my view is pretty diametrically opposed to the "ignorance interpretation" you describe. You can find some early and very direct discussion of this in my paper with Kurt Jacobs, "Information Tradeoff Relations for Finite-Strength Quantum Measurements." Here's a link to the version sitting on the archive: `quant-ph/0009101`.

The part relevant for you is the introduction. Reading over this again for the first time in almost four years, it doesn't strike me as half bad (even though I've become more metaphysical since then). Anyway, read that, and see how it moves you. Don't forget footnote 27.

It's hard to tell you where the very "best" thing to read is, as my view has been evolving (and I think becoming much more consistent) over the last few years. The view will never be completely consistent until we get a full-fledged derivation of quantum mechanics from it (and I hope you'll contribute to it eventually). But I'm still relatively proud of my paper "Quantum Mechanics as Quantum Information (and only a little more)." I think I gave you a copy of that, but in case you threw it out, get the version on my web page.

(The version on `quant-ph` has more typos.) The relevant parts for a first, philosophical reading are: Section 3, "Why Information?", and Section 4, "Information about What?". I think they answer your question pretty directly. You might want to also look at Section 10, "The Oyster and the Quantum." I'm not completely sure how that section fits in with the stuff I've been thinking most recently, but I think there's still a significant overlap.

Finally, let me supplement all that with some recent words that I recorded for Philip Stamp. They're placed below. I think that's about as complete a picture I can give you at the moment without getting into the project of writing a new paper. [See 19-05-04 and 04-06-04 notes, "Bathroom Reading" and "Earwax," to P. C. E. Stamp.]

05-07-04 *Flavors of Realism* (to M. Janssen)

Let me finally send you two more supplements to what I've already sent you before you face your friend. These things address the ways in which I see this foundational program for quantum mechanics as a flavor of realism (and a result of Darwinism).

The most important among these is Sections 4 and 5 in my paper "The Anti-Växjö Interpretation of Quantum Mechanics" posted at `quant-ph/0204146`.

Second to that, though, let me also attach the handout I put together for the Seven Pines meeting, "Delirium Quantum." (There was no particular reason that it needed putting together for Seven Pines per se; I just needed some event as a catalyst, and Seven Pines was it.) The most relevant sections for your particular question are Sections 4 and 5 (again).

06-07-04 *World and Observer Intertwined* (to G. Musser)

I'm planted in Munich now for the evening, and it's two and a half hours until beer-thirty with my in-laws. So, let me see what I can work up for you between now and then. As I've already expressed, your main question is a good one, and I had hopes of writing you a little essay in reply. But as usual, I wasn't being realistic with myself about my duties and my distractions. Thus, the stuff below will have to do for the moment.

First off:

Musserism 12: *... most physicists still regard hidden variables as a long shot. Quantum mechanics is such a rainforest of a theory, filled with indescribably weird animals and endlessly explorable byways, that seeking to reduce it to classical physics seems like trying to grow the Amazon from a rock garden. Instead of attempting to reconstruct the theory from scratch, why not take it apart and find out what makes it tick? That is the approach of Fuchs and others in the mainstream of studying the foundations of quantum mechanics.*

They have discovered that much of the theory is subjective. It does not describe the objective properties of a physical system, but rather the state of knowledge of the observer who probes it. Einstein argued as much when he critiqued the concept of quantum entanglement–the "spooky" connection between two far-flung particles. [The following is presumably the problematic sentence.] Fuchs argues that what looks like a physical connection is actually an intertwining of the observer's knowledge about the two particles. After all, if there really were a connection, physicists should be able to use it to send faster-than-light signals, and they can't.

I like all of that quite a bit. The only thing I would change is your sentence, "They have discovered that much of the theory is subjective." I'm not sure a reader would quite get what you mean. If you came upon this sentence cold, not preconditioned by my idiosyncrasies, would you know what it means for a theory to be subjective (or even have a guess at it)? Your sentence immediately following it wards off quite a bit of the trouble, but you might be a little more preemptive. Maybe I'd say something like this:

> They have discovered that, though conditioned by the peculiarities of nature, much of the theory concerns not nature itself but the observer's interface with nature. In other words, the theory does not primarily describe the objective properties of physical systems, but rather the possible states of knowledge of the observers who probe them.

That's probably a bit long for you, but I'm sure it's shortenable with a little work (and can made to sound more in your own style), but most importantly I think it's more on the right track.

Now to the next paragraph. Even up to here, I'm OK with it:

Musserism 13 : *Physicists had long assumed that measuring a quantum system causes it to "collapse" from a range of possibilities into a single actuality. Fuchs argues it is just our uncertainty about the system that collapses.*

The only way I might amend the first sentence if I were writing it myself would be to make it something like this:

> Physicists had long assumed that measuring a quantum system causes it to literally 'collapse' from a range of possibilities to a single actuality. Fuchs argues it is just our uncertainty about the system that collapses.

The trouble only really starts with the next sentences:

Musserism 14 : *Uncertainty about a quantum system is very different from uncertainty about a classical one, because the process of measurement inevitably disturbs a quantum system. And this sensitivity to disturbance is a clue to what a quantum system really is.*

So, let's focus on them. Maybe I would change them to something like this:

> It is only that the uncertainty is about a very different beast in the quantum case than in the classical one. Classically when one speaks of uncertainty, one assumes that it is always uncertainty about a some preexisting property that has nothing to do with the act of observation. In the quantum case the uncertainty is about something at the interface between the system in the observer—the result that comes about through the very act of probing the system. Like Einstein's entangled particles, the world and the observers within it are wired together so tightly that the theory cannot be about the one without also being about the other.

I know it probably doesn't sound like something you yourself would compose—I get truly pissed off any time someone tries to change my own writing—but maybe it gives you something to work with that starts off a little closer to what's inside my head.

Here's the whole lot pasted together:

> . . . most physicists still regard hidden variables as a long shot. Quantum mechanics is such a rainforest of a theory, filled with indescribably weird animals and endlessly explorable byways, that seeking to reduce it to classical physics seems like trying to grow the Amazon from a rock garden. Instead of attempting to reconstruct the theory from scratch, why not take it apart and find out what makes it tick? That is the approach of Fuchs and others in the mainstream of studying the foundations of quantum mechanics.
>
> They have discovered that, though conditioned by the peculiarities of nature, much of the theory concerns not nature itself but the observer's interface with nature. In other words, the theory does not primarily describe the objective properties of physical systems, but rather the possible states of knowledge of the observers who probe them. Einstein argued as much when he critiqued the concept of quantum entanglement—the "spooky" connection between two far-flung particles. Fuchs argues that what looks like a physical connection is actually an intertwining of the observer's knowledge about the two particles. After all, if there really were a connection, physicists should be able to use it to send faster-than-light signals, and they can't.
>
> Physicists had long assumed that measuring a quantum system causes it to literally 'collapse' from a range of possibilities to a single actuality. Fuchs argues it is just our uncertainty about the system that collapses. It is only that the uncertainty is about a

> very different beast in the quantum case than in the classical one. Classically when one speaks of uncertainty, one assumes that it is always uncertainty about a some preexisting property that has nothing to do with the act of observation. In the quantum case the uncertainty is about something at the interface between the system and the observer—the result that comes about through the very act of probing the system. Like Einstein's entangled particles, the world and the observers within it are wired together so tightly that the theory cannot be about the one without also being about the other.

I hope that's helpful and not just annoying. I might (repeat *might*) get a chance to write a little more, but let's start with this.

My wife and I are just in Munich for the night to drop off the kids with the grandparents while we're house hunting. We fly out for NJ at 7:00 tomorrow morning.

06-07-04 *A Little More* (to G. Musser)

Back again. It's beer-thirty now, but the family's still shopping. Still no proper essay, but let me come back to that set of questions of yours.

Musserism 15 : [CAF wrote:]

> The only thing that differentiates Einstein from our little effort at this juncture is that he thought physics should ultimately be about the "states" of nature, whereas the guys in my camp think that the great lesson of quantum mechanics is that physics must be about something else.

What is it about, then? Also, are you agreeing with Bohr that physics isn't about nature but what we can say about nature?

I hope the reworking of your old paragraphs that I just sent you, along with the opening of my Caltech lectures, already answers this question. I don't think Aage Petersen's rendition of Bohr—"There is no quantum world. There is only an abstract quantum physical description. It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns what we can say about nature."—gets it quite right. Instead, the model of QM is predominantly about something arising between the world external to the observer and the observer himself. Don't you see the difference of tone? It is not as if we are talking about something from afar (as Petersen makes it sound like), but something we are deep within the middle of and intimately involved with. A quantum mechanical measurement result has its source as much in the system as it does with the observer—that is an ontological statement and not just an epistemological one. After all these years, there's probably still no better picture of the process than in John Wheeler's story of the game of twenty questions (surprise version):

> The Universe can't be Laplacean. It may be higgledy-piggledy. But have hope. Surely someday we will see the necessity of the quantum in its construction. Would you like a little story along this line?
>
> Of course! About what?
>
> About the game of twenty questions. You recall how it goes—one of the after-dinner party sent out of the living room, the others agreeing on a word, the one fated to be a questioner returning and starting his questions. "Is it a living object?" "No." "Is it here on earth?" "Yes." So the questions go from respondent to respondent around

the room until at length the word emerges: victory if in twenty tries or less; otherwise, defeat.

Then comes the moment when we are fourth to be sent from the room. We are locked out unbelievably long. On finally being readmitted, we find a smile on everyone's face, sign of a joke or a plot. We innocently start our questions. At first the answers come quickly. Then each question begins to take longer in the answering—strange, when the answer itself is only a simple "yes" or "no." At length, feeling hot on the trail, we ask, "Is the word 'cloud'?" "Yes," comes the reply, and everyone bursts out laughing. When we were out of the room, they explain, they had agreed not to agree in advance on any word at all. Each one around the circle could respond "yes" or "no" as he pleased to whatever question we put to him. But however he replied he had to have a word in mind compatible with his own reply—and with all the replies that went before. No wonder some of those decisions between "yes" and "no" proved so hard!

And the point of your story?

Compare the game in its two versions with physics in its two formulations, classical and quantum. First, we thought the word already existed "out there" as physics once thought that the position and momentum of the electron existed "out there," independent of any act of observation. Second, in actuality the information about the word was brought into being step by step through the questions we raised, as the information about the electron is brought into being, step by step, by the experiments that the observer chooses to make. Third, if we had chosen to ask different questions we would have ended up with a different word—as the experimenter would have ended up with a different story for the doings of the electron if he had measured different quantities or the same quantities in a different order. Fourth, whatever power we had in bringing the particular word "cloud" into being was partial only. A major part of the selection—unknowing selection—lay in the "yes" or "no" replies of the colleagues around the room. Similarly, the experimenter has some substantial influence on what will happen to the electron by the choice of experiments he will do on it; but he knows there is much impredictability about what any given one of his measurements will disclose. Fifth, there was a "rule of the game" that required of every participator that his choice of yes or no should be compatible with *some* word. Similarly, there is a consistency about the observations made in physics. One person must be able to tell another in plain language what he finds and the second person must be able to verify the observation.

Musserism 16: *But ultimately don't you suggest that there is more to nature than is captured in the quantum formalism?*

Yes I do. Here's how I put it in the "Delirium Quantum" thing I sent you:

The whole subject matter of my *Notes on a Paulian Idea* is in toying with the idea that the cleanest expression of quantum mechanics will come about once one realizes that its overwhelming message is that the observer cannot be detached from the phenomena he *helps* bring about. I italicize the word *helps* because I want you to take it seriously; the world is not solely a social construction, or at least I cannot imagine it so. For my own part, I imagine the world as a seething orgy of creation. It was in that orgy before there were any agents to practice quantum mechanics and will be in the same orgy long after the Bush administration wipes the planet clean. Both of you have probably heard me joke of my view as the "sexual interpretation of quantum mechanics." There is no one way the world is because the world is still in creation,

> still being hammered out. It is still in birth and always will be—that's the idea. What quantum mechanics is about—I toy with—is each agent's little part in the creation (as gambled upon from his own perspective). It is a theory about a very small part of the world. In fact, I see it as a theory that is trying to tell us that there is much, much more to the world than it can say. I hear it pleading, "Please don't try to view me as a theory of everything; you take away my creative power, my very promise, when you do that! I am only a little theory of how to gamble in the light of a far more interesting world! Don't shut your eyes to it."
>
> The question is, how to get one's head around this idea and make it precise? And then, once it is precise, what new, wonderful, wild conclusions can we draw from it? That is the research program I am trying to define.

And in another part:

> But, you should remember that these quantum states we speak of are not perspectives. They are personal possessions. To modify Tilgher's quote at the beginning of de Finetti's "Probabilismo" for our own purposes,
>
>> A quantum state is not a mirror in which a reality external to us is faithfully reflected; it is simply a biological function, a means of orientation in life, of preserving and enriching it, of enabling and facilitating action, of taking account of reality and dominating it.

Musserism 17: *That there is a there, there?*

Are you a fan of Gertrude Stein? Carl Caves and I wrote this in a 1996 paper:

> The information gathered from repeated measurements on quantum systems is indeed drawn from an inexhaustible well, but it is a well of potentialities, not actualities. Asked where all this information resides, we reply, with apologies to Gertrude Stein "There is no where there."

I wouldn't use that language anymore (i.e., talking about measurement results as information unqualified), but I still like the idea of using Stein when possible.

Musserism 18: *Or at least, you seem to suggest that there are "buried" variables – they're not hidden, because they do appear in the formalism, but they're so entangled with subjective elements that they are hard to tease out.*

I still don't know quite how to respond to this one. The language of "variables" is on the wrong track. But this is why I wanted to write a little essay for you. (These things are never for free by the way: You're the muse and the laboratory. If the explanation turned out good enough, I'd find a way to incorporate it into a paper.)

Similarly I'd love to give you an extended explanation of this:

Musserism 19: *Another issue that I'm struggling with is this business of separating states of knowledge from states of nature. Isn't a state of knowledge a state of nature where "nature" includes the observer? The wavefunction is a statement about me, but I'm part of nature, right?*

The root of this one goes even deeper, all the way back to Bayesian probability. Quantum mechanics just adds an extra layer (or reinforcement) to it. The best I can do is give you a hint of an answer that comes from a little piece of an exchange with Marcus Appleby after his paper `http://www.arxiv.org/abs/quant-ph/0402015` (definitely recommend reading it if you want to go a little more deeply into this). You are right: The observer is certainly part of nature. Here's the exchange:

> In your "Facts, Values and Quanta" you say "a probability statement cannot be identified with a fact about the world, as it exists independently of us." I think that is all I have ever meant by "subjective." But, then again, maybe that's just hindsight playing a trick on me.

But I need to write so much more on the subject.

06-07-04 *Preliminary Yes* (to G. Musser)

I might use "pertains to" rather than "describes", but otherwise I think the paragraph is accurate. The only other thing is whether "sexual interpretation" is too risqué for *Scientific American*: If it's not, I guess I'm OK with it.

Gotta run for the flight. I'll think about the rest of your note over the Atlantic, and may possibly reply.

George's Preply

> This is all very helpful. I've tinkered with the first two paragraphs of that passage, but as you said, the real issues concern the third paragraph. Here is my latest attempt:
>
> > Physicists had long assumed that measuring a quantum system causes it to "collapse" from a range of possibilities into a single actuality. Fuchs argues that it is just our uncertainty about the system that collapses. Uncertainty about a quantum system is very different from uncertainty about a classical one, and this difference is a clue to the objective world. In the classical case, a particle has some velocity; uncertainty means that observers do not know this velocity until they measure it. In the quantum case, the velocity does not even exist until observers go to look for it. The concept of velocity describes the interface between system and observer. Fuchs calls this idea the "sexual interpretation of quantum mechanics." He has written: "There is no one way the world is, because the world is still in creation, still being hammered out." The same thing can be said of our understanding of quantum reality.
>
> Please let me know asap if this works or not.

07-07-04 *A* (to G. Musser)

Stuck on the runway at Heathrow with the threat of an equipment change! Arrggh!

Musserism 20: *How literally should I take Wheeler's game of 20 Questions? There is a rhyme and a reason behind the answers I get to my 20 questions – namely the (pre-existing!) agreement among the participants. There is an objective reality.*

You should read that passage much more carefully! (Second Arrggh!!) It is not about the "game of 20 questions" but the "game of 20 questions (surprise version)". The whole point of the story is exactly the opposite of what you took from it. [But, see 12-08-04 note "Background Noise" to G. Musser.]

07-07-04 *A.5* (to G. Musser)

B and C still coming, but now I'm stuck in Heathrow for five hours (at least) and not in a particularly good mood. I'll probably return to complete them in NJ somewhere in the middle of some night.

07-07-04 *B* (to G. Musser)

Musserism 21 : [CAF wrote:] *"It is a theory about a very small part of the world ... a theory that is trying to tell us that there is much, much more to the world than it can say." How is this not hidden variables?*

Sure, they may not be hidden variables in the pre-existing sense – i.e. in the sense that a properly designed experiment can come asymptotically close to ascertaining their pre-experiment value. But does not "more to the world" imply something hidden?

Take a break from me for a moment and ask yourself how the Everett interpretation is not a hidden-variable theory? (It almost seems you would have asked the Everettian the same thing you asked me.) A hidden-variable theory is a very specific thing: If one were to know the value (even if only hypothetically and not operationally) of all the variables (including possibly the ones on the inside of the observer), then one can predict the outcome of all measurements with certainty. It is a fancy way of saying measurement outcomes pre-exist, even if nothing one would ever call a measurement is actually performed.

The determination or setting of specific measurement outcomes (in any quantum mechanical experiment) has always been outside of the quantum mechanical formalism. There is nothing in the formalism that determines whether one will get this click or whether one will get that click in some measurement device. But that does not make it a hidden-variable theory. What is hidden?

Here is the way Pauli put it:

> Like an ultimate fact without any cause, the individual outcome of a measurement is ... in general not comprehended by laws. This must necessarily be the case ...
>
> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.

(The conjunction of these thoughts is what I call "the Paulian idea"—hence the name of my book.) "Like an ultimate fact without any cause, the individual outcome of a measurement is not comprehended by laws."

The way I see it, quantum measurement outcomes are ultimate facts without specific call for further explanation. And indeed the quantum formalism supplies none. Thus there is more to the world than the quantum formalism can supply. Nothing to do with hidden variables.

But more specifically, regarding your point:

Musserism 22: *"It is a theory about a very small part of the world... a theory that is trying to tell us that there is much, much more to the world than it can say." How is this not hidden variables?*

How does the theory tell us that there is much more to the world than it can say? It tells us that *facts* can be made to come into existence, and not just at some time in the remote past called the "big bang" but here and now, all the time, whenever an observer sets out to perform (in antiquated language) a quantum measurement. I find that fantastic! And it hints that facts are being created all the time all around us. But that now steps out of the domain of what the quantum formalism is about, and so is the subject of future research. At the present—as a first step—I want rather to make the interpretation of the quantum formalism along these lines absolutely airtight. And then from there we'll better know how to go further.

Doesn't that just make you tingle? That (metaphorically, or maybe not so metaphorically) the big bang is, in part, right here all around us? And that the actions we take are *part* of that creation! At least for me, it makes my life count in a way that I didn't dare dream before I stumbled upon Wheeler, Pauli, and Bell-Kochen-Specker.

But let me get away from this speculation and rope myself back in on your particular question: How is this not some hidden variables account? Simple: If there are any extra facts being created around us, they nevertheless do not impinge on the individual quantum measurement outcome.

When I say that QM is a theory about a very small part of the world, you should literally think of a map of the United States in relation to the rest of the globe. The map of the US is certainly incomplete in the sense that it is obviously not a map of the whole globe. But on the other hand it is as complete as it can be (by definition) as a representation of the US. There are no hidden variables that one can add to the US map that will magically turn into a map of the whole globe after all. The US map is what it is and need be nothing more.

Does that help any?

I think a good bit of the problem comes from something that was beat into most of us at an early age. It is this idea: Whatever else it is, quantum theory should be construed as a theory of the world. The formalism and the terms within the formalism somehow reflect what is out there in the world. Thus, if there is more to the world than quantum theory holds out for, the theory must be incomplete. And we should seek to find what will complete it.

But my tack has been to say that that is a false image or a false expectation. Quantum theory from my view is not so much a law of nature (as the usual view takes), but rather a law of thought. In a slogan: Quantum mechanics is a law of thought. It is a way of plagiarizing George Boole who called probability theory a law of thought. (Look at the first couple of entries in the Rüdiger Schack chapter of *Notes on a Paulian Idea*.) Try to think of it in these terms, and let's see if this helps.

Let us take a simple term from probability theory, namely a probability distribution over some hypothesis $P(h)$. This function represents a gambling agent's expectations about which value of h will obtain in an observation or experiment. Suppose now the agent gathers a separate piece of data d from some other observation or experiment and uses it to conditionalize his expectations for h; i.e., he readjusts his expectations for h to some new function $P(h|d)$ by using Bayes' rule. Now here's a question for you. Is there anything within abstract probability theory that will allow the agent to predict precisely which value of d he will find when he gathers his data? Of course not. It's almost silly to pose the question. Abstract probability theory has nothing to do with the actual facts of the world. But then, doesn't that mean that probability theory is an incomplete theory? It can't, for instance, explain its own transitions $P(h) \longrightarrow P(h|d)$ since probability theory alone can't tell us why this d rather than that d. Moreover if probability is incomplete in this way, shouldn't we be striving to complete it? Both silly questions, and I hope for obvious reasons.

So:

1. There is no particular mystery in the transition $P(h) \longrightarrow P(h|d)$.
2. We would never expect probability theory to provide a mechanism to determine which value of d is found or produced in the experiment. The value d represents a fact of the world, and probability theory is *only* a theory about how to manipulate expectations once facts are given.
3. But also no one would be compelled to call probability theory incomplete because of this.
4. In particular, admitting this does not amount to having a hidden-variable explanation of probability theory.

So I say with quantum mechanics. The story is almost one-to-one the same: You just replace probability distributions with quantum states. ... But then you reply, "But there's a difference; quantum theory is a theory of physics, it is not simply a calculus of thought." And I say, "That's where you err." Quantum theory retains a trace of something about the real, physical world but predominantly it is a law of thought that agents should use when navigating in the (real, physical) world. In particular, just like with probability theory, we should not think of quantum theory as incomplete in the usual sense. If it is incomplete in any way, it is only incomplete in the way that the US map is incomplete with respect to the globe: There's a lot more land and ocean out there.

Teasing out (your words) the trace of the physical world in the formalism—i.e., the part of the theory that compels the rest of it as a useful law of thought—is the only way I see to get a solid handle on what quantum mechanics is trying to tell us about nature itself.

With this let me now go back to the US map for one final analogy. I said that there is a sense in which the US map is as complete as it can be. However there is also a sense in which it tells us something about the wider world: If we tabulate the distances between cities, we can't help but notice that the map is probably best drawn on the surface of a globe. I.e., the US already reveals a good guess on the curvature of the world as a whole—it hints that the world is not flat. And that's a great addition to our knowledge! For it tells a would-be Columbus that he can safely go out and explore new territories. Exploring those new territories won't make the US map any more complete, but it still means that there is a great adventure in front of him.

OK, with that, I'm going to call this discussion to an end. If you still think of me as a hidden variableist, I've either failed in my explanatory powers or my view is simply inconsistent. (I would bank on the former rather than the latter.) Either way I'm going to call the discussion to an end.

Good luck finalizing your article (if you haven't already finalized it).

07-07-04 C (to G. Musser)

Musserism 23: *I hate to say it, but if you write for* Scientific American*, as I hope and expect you will, it will be very similar to the process we are now undergoing. You will come up with something, I or one of my colleagues will critique and perhaps rewrite it, and around and around we will go.*

The trick is to ensure that the writing converges before the pissedoffedness diverges.

I tend to be gentler when someone gains my trust ...

28-07-04 *Three, Two, One* (to G. Brassard)

I'm sorry for the absence: I apologize to everyone now, and constantly. What I really need is three months off to clean the palette and get a fresh start. . . . But I don't have that privilege.

Brassardisme 15: *I spent 4 out of 5 consecutive nights with 3 to 4 hour sleep per night last week+ for the purpose of finishing my survey of pseudo-telepathy, since I wanted to have something in the Asher special issue (having given up on you), and I was more than late with the submission. I would appreciate* at least *a word of acknowledgement-of-receipt! By the way, I'll be away (mostly vacation) all of August, and therefore unable to react quickly if you ask me for an urgent and/or a version typeset according to* Foundations of Physics *nonsense (such as double-spaced).*

Great paper, the part I've read. Please make sure to add the authors' affiliations, fix any typos, and email me the original TEX/LATEX/whatever. Also, yes, please do make it double-spaced.

Little point of history: Podolsky actually wrote the EPR paper. Einstein was not particularly pleased with it, but reluctantly agreed to publish it as it was. He thought it obscured the cleaner argument he had—the one he reports in several letters (to Schrödinger, Born, Besso, and others). A good/fun place to read about all this is in the first couple of chapters of Arthur Fine's book *The Shaky Game*.

Brassardisme 16: *My talk on QFLQI in Toronto last Friday went extremely well. Well over one hundred people in attendance (something never seen in summer time) and by far the liveliest crowd I've ever seen! They kept me with questions for 90 MINUTES after the end of the talk. Wonderful, except that I almost missed my flight back! Of particular interest among the attendance was Marlan Scully from Texas A & M University. Do you know him?*

That is great news! I wish I could have been there to hear it. It's nice to see this program getting so much enthusiasm. Yes, I know Marlan and have some good stories to tell about him. What has his reaction? Did he say anything interesting?

02-08-04 *My Own Apology* (to C. G. Timpson)

I enjoyed your envoi and am thankful you wrote it: Your willingness to take the program seriously is already a great compliment—in fact, it's its only toehold in the Oxford community.

I liked the way you made it clear that even if one adopts a kind of instrumentalism for the quantum state, one can still ask, "Why this instrumentalism, rather than that instrumentalism?" And to the extent that one can provide reasons for the choice, one is going beyond instrumentalism. While I'm here, let me paste in something I wrote up for a reporter at *Scientific American* that addresses the same issue. I used a metaphor I hadn't used before that maybe better captures the idea: It's how an accurate map of an isolated country can already reveal that there's a larger world out there (through the curvature required to make sense of the distances between cities). Yet it's no sin of the map that it doesn't cover the whole world. Likewise, I think of quantum theory.

12-08-04 *Background Noise* (to G. Musser)

I'm coming back intermittently. My wife, kids and I are presently in Texas, visiting my mom, and soon to start driving our mini-van back to New Jersey. (We had it stored here, because we had to drive down our ailing old dog before taking off for Ireland.)

You had a point about the pre-agreement in Wheeler's game of twenty questions. That is one part of the image that I'd like to not take too seriously. I'd rather have Darwinism all the way down, if the world will support it.

If you haven't read it yet, read Louis Menand's *The Metaphysical Club*. (Menand also writes for the *New Yorker*, I'm told.) It's an easy in into the idea that the world is Darwinism all the way down. Very enjoyable book; I loved it.

Yeah, there's a lot about your note below that I like. But I've got too much to do to parse it at the moment.

George's Preply

> You write: "If you still think of me as a hidden variableist, I've either failed in my explanatory powers or my view is simply inconsistent." I'm not seeking to label your ideas as an "ism". I realize that the term "hidden variables" is a loaded one within the foundations-of-QM community, but I have been using it simply as a foil to try to understand your work. I'm not an antagonist but a persistent student.
>
> Let me see whether I understand your vision of a world in continuous creation. Ernst has bequeathed me his cat; it arrives in a box. I open it to find the cat is ... alive. Before I do, the cat is neither alive nor dead; it is not even in a purgatorial superposition. In fact, the categories of cat alive and cat dead don't even exist yet. When I open the box, I create the fact of a live cat. That fact had to come into existence *sometime*. In the classical deterministic universe, it came into existence when the universe did. All the arbitrariness was in the ICs. The quantum universe makes up facts as it goes along. The arbitrariness is spread out over time. It's the distinction between my wife, who makes plans and carries them out, and me, who decides step by step on the fly.
>
> If we're talking about, say, particle spin, I play an even more active role in the creation, by virtue of how I conduct my experiment. I tell the particle: You are spinning up toward the ceiling of my lab or down toward the floor; now choose.
>
> Is that the general idea? There are lots of issues in there, but I want to get this down.

12-08-04 *The Fruit* (to G. Musser)

By the way, let me show you the fruit of my extended absence—it hasn't been an easy few weeks. We got a nice house in Cranford, the second town closer to NYC on the Raritan Valley Line than Westfield. (I don't know if you're familiar with the towns around there.) Now I'm 8 miles from Bell Labs, 10 miles from Newark Airport, and 19 from the WTC. Anyway, an 1890 house in great shape, three levels, three fire places, 0.4 acre yard, a nice patio, etc. I think it's going to be a good place for thought. Drop by some time.

03-09-04 *The Fruit of Someone Else's Imagination* (to D. Poulin)

Poulinism 15: *You seem to be comfortable with this situation, but I'm not. In particular, I'm not very happy being the fruit of someone else's imagination, specially knowing that this person is the fruit of my imagination. What's your response to this? Did I take a wrong turn or do you agree with my conclusion? Is there any way out of this?*

Oh student! Yes, you took a wrong turn: You're not the fruit of anyone's imagination. (You've always been beyond my imagination anyway.)

But there's no time for email this week. Maybe next week. (In any case, what could I say differently this time than I've said before?) Instead, let me let another French speaker—Michel Bitbol—talk to you for the time being. He captures much of my position in the letter below. [See 10-12-03 note "First Meeting" to M. Bitbol.] Does it help any?

PS. You should come visit us sometime: We now have the best house we've ever had: about 400 square meters (three levels plus basement) on a .43 acre park-like yard, quiet street, walking distance to train station (.5 mile), and only 45 from New York thereafter. The guest room is the biggest in the house.

David's Preply

> It appears to me that there is no way around the fact that quantum states are not real, they represent our knowledge about systems. At first, I thought that one could get away with a "mild flavored" notion of subjectivity: the type discussed by Rovelli in his "Relational Quantum Mechanics". But I have been re-reading this lately, and I think that deep inside he is a many-world guy, but doesn't want to admit it. He talks about certain consistency conditions and other imposed relations between distinct observers. He never says so clearly, but in the back of his head there is a God's eye point of view from which all observers' "subjective" state assignments only reflect the bits and pieces of the "real" wave function of which they have only partial access (they are "stuck in a branch"). (Note that he rejects this in his paper ... but there is no way around it.)
>
> But once you recognize that states represent your knowledge, you get caught in a spiral that leads you to the unavoidable conclusion that there is nothing real. I guess that this was the point of your Phys. Today letter: if you believe that quantum theory is right (and believe in causality), then there is no room in it for an objective reality. Of course, there is room for inter-subjective agreement among several independent observers (emergent objectivity), but this is not fundamental. A more profound theory might eventually admit elements of reality, but not quantum theory the way it is today.
>
> You seem to be comfortable with this situation, but I'm not. In particular, I'm not very happy being the fruit of someone else's imagination, specially knowing that this person is the fruit of my imagination. What's your response to this? Did I take a wrong turn or do you agree with my conclusion? Is there any way out of this?

03-09-04 *Newer, Newish Thoughts* (to D. Poulin)

I'll also paste in one of my more recent smears on "reality". It's a PDF file.
Have a good weekend.

03-09-04 *Reach of the Black Cow* (to C. H. Bennett)

I thought fondly of you as I drove to work this morning: The radio announcer on JAZZ 88.3 said that the music set he just played was sponsored by the Black Cow in Croton.

Anyway, I'm back at Bell Labs now. My new addresses and phone numbers are listed below. We ought to have a good work weekend in Wendel some time. Or alternatively, you and Theo should come visit us: We've got a great house now in a great area. (If you've got a broadband connection, I'll send you some pictures.)

07-09-04 *Targeted Answer* (to D. Poulin)

When I wrote you the other day, I had forgotten about the little piece of literature below. It's much more of a direct answer to what you were asking:

Poulinism 16: *But once you recognize that states represent your knowledge, you get caught in a spiral that leads you to the unavoidable conclusion that there is nothing real. I guess that this was the point of your Phys. Today letter: if you believe that quantum theory is right (and believe in causality), then there is no room in it for an objective reality. Of course, there is room for an inter-subjective agreement among several independent observers (emergent objectivity), but this is not fundamental. A more profound theory might eventually admit elements of reality, but not quantum theory the way it is today.*

You seem to be comfortable with this situation, but I'm not.

The outcome of a quantum measurement is not the fruit of the measurer's imagination, for he cannot predict it. It is first-class evidence of a reality (at least partly) independent of himself. The "identity of the outcome" is something in the world that quantum theory cannot capture. But I don't see that as a blemish on the theory that is waiting to be *fixed* by a more "profound theory." The way I explain this is below; it is something I wrote for a *Scientific American* reporter. [See 07-07-04 note titled "B" to G. Musser.]

07-09-04 *Summer-School Words* (to D. Poulin)

Poulinism 17: *Now, I should probably read the papers ten times before asking these dumb questions, (I don't have much time to do this these days) but what gets to provoke reality? You say many times that it is our interventions into the world (measurements) that forces reality into existence. Does this require consciousness?*

I'll only comment with the words I wrote for the intro to my summer-school lectures at Caltech this summer. They're below. [See 17-06-04 note "Preamble" to H. Mabuchi.]

07-09-04 *Realism* (to H. Barnum)

Barnumism 9: *I would like to be realistic about, say, electrons, if not about the wavefunction. Can you think of a way to do that? The problem is, one is led to say things like "Yeah, there really are electrons, and there are some right here in this box". The latter statement seems to be about their wavefunction, however. Maybe that's not a problem.*

For the heck of it, while we're on the subject of realism, let me place below three recent letters/compositions that plead again that I'm really a realist. I think I used some new modes of expression that might be useful. I particularly like the analogy that makes use of a U.S. map and a globe that I explain near the end.

Barnumism 10: *I don't think it's just the dimension of their Hilbert space, though. Maybe it's a little bit more: a Hilbert space with a particular group action on it...? Wavefunctions aren't* *real, but maybe the place where they live, equipped with its observables and their group-theoretic structure, is. . . ?)*

Oh well. . .

Yeah, it is something more like that—I think in my more democratic moments. Bill Unruh spent a good amount of time at the Minnesota meeting this summer trying to suggest something like that to me too.

08-09-04 *Fuchsian Pick List* (to S. Hartmann, C. M. Caves & R. Schack)

I'm trying to make up for lost time before Rüdiger arrives in New Mexico. Attached is my reworking of Stephan's last list of potential invitees.

You'll see I blithely rearranged according to my tastes. I hope I won't cause any offense. I did try to annotate pretty thoroughly. And definitely, I'm not above bargaining on any of these positions: Anyone I moved into the "waiting list" can certainly be moved back out of it and vice versa. The main thing I am shooting for is a happy, productive, mutually-respectful crew. With the exception of Price, Fitelson, and Ismael, I know everyone in the present invitee list personally, and I think they will work well together . . . providing a little friction, but not too much friction. You'll see, I was pretty negative on Bohmians and other exotic sorts. The main thing I personally want to see at this meeting is a sense of progress, rather than a continuing sense of standstill (which is what I see every time I see a confirmed Bohmian in the room).

If we could open the floodgates, I would love to see everyone in the present list plus the waiting lists all together hashing it out. It would be a grand get-together. But there is no doubt that it'd be much too large for the present meeting.

10-09-04 *Soul's Return* (to S. Hartmann, C. M. Caves & R. Schack)

After 37 days of crossing the ocean and (mostly) sitting in US customs, my household goods finally made their way to Cranford today. That includes my 331 books on James, pragmatism, Wittgenstein, and Rorty: It's almost as if my soul was returned to me!

Perhaps I'll be a more creative writer next week.

13-09-04 *Blurb* (to Princeton University Press)

Here's the blurb.[78] Two points: 1) If you decide to use it, you have to keep the bad with the good, i.e., use the whole thing. I.e., you can't just cut the positive part from the review. My integrity is at stake. And 2) my affiliation "Bell Labs, Lucent Technologies" must be used along with my name. Blurb below.

> Roland Omnès has a knack for writing in at least three of the world's major languages: English, French, and Science. I have at times found a lifetime's worth of insight from a single paragraph of his. The present work is no exception. For though I disagreed

[78] For R. Omnès, *Converging Realities: Toward a Common Philosophy of Physics and Mathematics*, (Princeton University Press, Princeton, 2005).

with whole swaths of the book, I learned much from it and had to fight every inch of the way to shore up my position against its onslaught. No discussion of reality should ignore this powerful and novel exposition.

13-09-04 *Silence from the Podium Is Sometimes Golden* (to S. Hartmann, C. M. Caves & R. Schack)

Hartmannism 1: *I think that it is important that we come up with a structure of the conference very soon. We need this before we write the speakers to give them instructions.*

I actually think it preferable if not all invitees are allotted a formal speaking spot. I'm thinking particularly of the format of the Seven Pines meeting in Minnesota this spring. Maybe only a little over half the participants actually spoke. The ones that did would speak in pairs (with somewhat related subjects), then we would take a coffee break, and come back for discussion. The two speakers would be placed in chairs in front of the audience and the discussion would go for about as long as the talks. The discussions were often more riveting than the talks.

Also, myself, I'm all for longer talks generally. It is just too hard to convey one's point and the flavor of one's thinking in 30 minutes ... especially when a subject is not mature. Thus, I would suggest strict 45 minute talks (two at a time), then break, and readjourn for one hour of discussion/debate. Or something like that kind of scheduling.

Like I say, I think it worked quite well at Seven Pines.

How to weed out speakers from pure discussants? As a first round, I would suggest something like the call that was made for the Banff meeting next week. Here are the exact words:

> It is time to begin the process of scheduling of talks for the workshop. If you would like to speak, please reply to this message with a short description of what you would like to present.
>
> Also feel free to suggest a topic that someone else from the list of participants might speak about.
>
> We hope to follow a relaxed model for the workshop that will allow us to have ample time to talk and work with one another ...
>
> For this reason, please do not volunteer to give a talk out of any sense of duty or obligation, or if you would give a talk that many people have already heard. Ideally less than half of the participants should give scheduled talks.

After we see what kind of response we get from that, then we could cajole as many people as we need to fill the gaps.

In the end, at Seven Pines everyone participated in just about the right way.

Any thoughts or counterpoints?

I'll write that blurb before you wake up in the morning. I guess Rüdiger should be in Albuquerque by now.

14-09-04 *Assessment Letter* (to whom it may concern)

To whom it may concern:

I have been asked to assess the International Center for Mathematical Modelling in Physics, Engineering, and Cognitive Sciences at the University of Växjö, Sweden. I gladly take on this task, as I have nothing but positive things to say about the Center. It has been a driving force in world-wide quantum mechanical research, bringing researchers together from every corner of that specialty for a kind of symbiosis I have seen nowhere else in the world (even in my dealings with the Newton Mathematical Institute at Cambridge University, the Perimeter Institute for Theoretical Physics in Waterloo, Canada, or the Aspen Center for Theoretical Physics in the United States).

I first became involved with the ICMM in December 2000 when Andrei Khrennikov invited me to organize a conference with him for the following year (apparently at the suggestion of Jeffrey Bub of the University of Maryland). The meeting was titled *Quantum Theory: Reconsideration of Foundations*. What an experience and what a meeting that was! Andrei gave me a pleasant budget of "seed money," and with it—along with significant matching funds from the various researcher's own grants—I was able to attract 15 world-class physicists from the field of quantum information and computing, a few of whom were actually founding fathers of the field, for a special session on "Shannon Meets Bohr: Quantum Foundations in the Light of Quantum Information." That meeting was a watershed moment for the field: For the first time it became internationally recognized that quantum information and computing have very deep things to say about quantum foundations, and more importantly, vice versa. Progress was seen all around, and a real sense of unity came of it. The conference proceedings emanating from the meeting, published by Växjö University Press, have come to be a standard reference on the subject.

Since then I have had several encounters with Professor Khrennikov, organized another conference with him in Växjö (a conference just as successful as the first one), joined the advisory board of the ICMM, and published a book in Växjö University Press's series on Mathematical Modelling in Physics, Engineering, and Cognitive Sciences—all wonderful experiences. In particular, the VUP publication of my book *Notes on a Paulian Idea* has led to an outpouring of research in these directions and gained enough recognition that Kluwer Academic Publishers has picked up the book for a second-edition printing in their series Fundamental Theories of Physics. In all, I have had more email traffic and praise about this book than all of my other publications combined (and that is not a skimpy record, with over 1500 citations recorded in the ISI Web of Science, Science Citation Index). All of this goes to show the important role that the ICMM has come to take in our community, through the meetings it has funded, the conference proceedings and books it has disseminated, and the collaborations it has spawned.

And this is just my personal experience with the ICMM. Since its founding, literally hundreds of researchers have been affected by or participated in ICMM events. Moreover, with the conference proceedings being widely read, the prestige of the ICMM conferences is taking on a life of its own. For instance, I recently received this request from a respected Italian professor in quantum optics and quantum information: "I am very interested in the Växjö conferences. I presume there will be one next year in June, right? Can you put me in the mailing list? I'm currently working extensively on the convex structure of quantum mechanics (states, POVM's, channels, and their relative connections), on which with my group, we are writing a set of 4–5 papers ...." Evidence like this cannot be ignored. I can attest from my interactions with so many in the community that you would hear a similar stories all around.

The ICMM is a gem in the world of fundamental quantum mechanical research. I hope similar levels of activity will be funded for years to come.

Sincerely,

Christopher A. Fuchs

14-09-04 *Barbecue Blurb, First Draft* (to S. Hartmann, C. M. Caves & R. Schack)

Being Bayesian in a Quantum World
1-6 August 2005, Konstanz, Germany

To be a Bayesian about probability theory is to accept that probabilities, whenever used, represent degrees of belief. This is in distinction to the idea that probabilities represent long-term frequencies or intrinsic, chancy propensities "out there" in nature itself. But, how well does this mesh with the existence of quantum mechanics? To accept quantum mechanics as a physical theory is to accept the calculational apparatus of quantum states and the Born rule for determining probabilities in measurement situations: "These probabilities are given by fundamental physical law!" one might eject. Does this dilemma spell the death of Bayesianism? Or does it rather give us an opportunity to rethink what quantum mechanics is actually about?

There is no doubt that we live in a quantum world. From transistors to lasers to nuclear warheads, the evidence is all around us. One might take from this that the quantum formalism ideally represents a mirror image of the world: That is, the wave function is so successful as a calculational tool precisely because it represents an element of reality. A more Bayesian (or, at least, Bayesian-like) perspective is that, if a wave function generates probabilities, then they too must be Bayesian degrees of belief, with all that that entails. In particular, quantum probabilities have no firmer hold on reality than the word "belief" in "degrees of belief" already indicates. From this perspective, the only sense in which the quantum formalism mirrors nature is through the normative constraints it places on gambling agents doing their best to navigate within such a quantum world. To the extent that an agent should use *this* structure (i.e., quantum mechanics) for his uncertainty accounting rather than *that* structure (i.e., some foil theory) tells us something about the world itself—i.e., the world independent of the agent and his particular beliefs at any moment. The task of the quantum Bayesian is to make this argument explicit and rigorous and to reap any benefit this can give to philosophy and physical practice.

Hogwash or deep idea? The time is ripe for a debate. At the present meeting, in the beautiful surroundings of Lake Konstanz, we envision a fifty-fifty mix of philosophers (who have thought long and hard about probability and quantum foundations) and quantum-information physicists (who have developed an impressive box of mathematical tools for prying apart the probabilistic structure of quantum mechanics) to set the tone. The goal is make real progress on these issues through a unique complementation of talents. All proposed participants have been hand picked for their registered interest in the debate and their known ability to "talk to the other side."

The format for the meeting will be . . .

At present we can commit to the following funding . . .

We look forward to seeing you in Konstanz!

— — — — — — —

> This day is called the feast of Being Bayesian in a Quantum World:
> He that outlives this day, and comes safe home,
> Will stand a tip-toe when the day is named,
> And rouse him at the name of BBQW.
> He that shall live this day, and see old age,
> Will yearly on the vigil feast his neighbours,
> And say 'To-morrow is Saint de Finetti:'

Then will he strip his sleeve and show his scars.
And say 'These wounds I had on de Finetti's day.'
Old men forget: yet all shall be forgot,
But he'll remember with advantages
What feats he did that day: then shall our names.
Familiar in his mouth as household words
Rüdiger the king, Hartmann and Fuchs,
Caves and Poulin, Barnum and Timpson,
Be in their flowing cups freshly remember'd.
This story shall the good man teach his son;
And BBQ BBQWian shall ne'er go by,
From this day to the ending of the world,
But we in it shall be remember'd;
We few, we happy few, we band of brothers;
For he to-day that sheds his blood with me
Shall be my brother; be he ne'er so vile,
This day shall gentle his condition:
And gentlemen in Germany now a-bed
Shall think themselves accursed they were not here,
And hold their manhoods cheap whiles any speaks
That fought with us upon Saint de Finetti's day.

15-09-04 *Quanta of Rain* (to H. J. Folse)

I'm quite concerned about you, your family, and your house. I will keep my fingers crossed that things don't go too badly for you—at this point it's fairly clear that you're going to get at least some troubles from the storm. In any case, you're in my thoughts.

... for that and another reason actually. Caves, Schack, Stephan Hartmann, and I are organizing a meeting to be held in Konstanz next August, titled "Being Bayesian in a Quantum World" — we're hoping for roughly 17 (quantum-information) physicists and 17 philosophers. The theme is about hashing out how well Bayesian probability fits with quantum mechanics. And we figure we ought to have one resident expert on Bohr and/or "the" Copenhagen interpretation around for part of the discussion. As it stands, you're top on the list if we do indeed go that route. We'll let you know something soon.

15-09-04 *Certain Epiphanies* (to M. Pérez-Suárez)

Pérez-Suárezism 9: *On certainty: Now I am certain (and not* just *in the sense of assigning probability one) that an assignment of probability one to an event does not make of it a certain event and that, in much the same way, probability zero for an event does not make of it an impossible event. At least, this is definitely what follows from the measure-theoretic approach to probability, and both Dutch-book coherence and probability theory in the more general framework of decision theory (as in Bernardo and Smith's book) may be seen as "just" providing the former (axiomatic) approach with an operational meaning, thus keeping intact those conclusions.*

I'm glad for this epiphany. When I had it, it may have been the most important transition in my life.

18-09-04 *Caves and Schack* (to M. Pérez-Suárez)

I'm writing you from a wireless connection for my laptop in the American Airlines Admirals Club in Chicago, as I'm making my way to the Banff meeting. It is kind of exciting; I've never done this before.

I think either Caves or Schack would be fine for you. I don't think you can go wrong with either of them. Carl has the better track record under his belt of nurturing many students, but Rüdiger has his own strengths and might have more personal time for you (though beware). Caves group would help fill in the gaps though, and probably prove far more stimulating.

Only one comment about something you wrote:

Pérez-Suárezism 10: *The only problem as for me is that I'm not sure how much I could get involved in the work he and his group are doing, mostly due to his not being a "full fledged" subjectivist, which might cause me some trouble I use the verb "might" for a good reason: I simply don't know if it would). On the other hand, his work on issues related in one way or another to QKD is of course pertinent to me. But ...*

You shouldn't underestimate Carl's breadth and versatility. And he is often willing to travel where his students lead him researchwise. He will be a full fledged subjectivist one day, if he's not already (and he might be already) ... simply because logic will ultimately lead him there.

I will write both Caves and Schack now preparing the way for your proposal. Thereafter it's up to you.

18-09-04 *Quantum Foundations in Sci Am* (to G. Musser)

Musserism 24: *Now that I've shaken the monkey of our Einstein special issue off my back, perhaps we can return to the idea of your writing an article for us. The first step would be to prepare a proposal that I could circulate among my colleagues for their formal approval. The ulterior motive of such a proposal is to get you (and me and our high-energy editor, to whom I would probably hand the baton) thinking about what the article would say, how it might be structured, and how the topic might be motivated for the nonphysicist.*

I think the article would be a service not just to the lay person but also to the foundations-of-QM community. The preparation of a nontechnical account is often an excellent excuse to think through issues. Then again, I don't need to convince you of that, since you're the one who referred me to Mermin's essay.

Let me now apologize for my latest extended absence: You wrote that to me over five weeks ago! This relocation and getting resettled in Bell Labs has just been a bear. Anyway, don't think that I've lost interest!

I've put a marker in my calendar to come back to the matter Nov 15. Unfortunately, I've got a big presentation to give to the president of Bell Labs Nov 11 on our efforts in quantum info, and between that and the conferences I'm attending in the meantime, I know that I won't honestly be able to do anything for you before then. But I'm hoping to hit the project with a vengeance soon after that.

BTW, I was prompted to finally reply to you by seeing your article at a newsstand in Chicago. (I'm on the way to Banff.) And I would have bought the issue too! But there was a tear in the cover and it was the only one they had left. Anyway, I'll get it soon, as I have a chance. I look forward to reading what you've written.

04-10-04 *Bush and Reality* (to me)

From Bob Herbert, "Bush and Reality," *New York Times*, 4 October 2004:

> For 90 minutes, at least, democracy seemed to be working. The two men in dark suits took their places at the lecterns. The analysts, the handlers, the spinmeisters and the hangers-on had been cleared out of the way. With no commercial interruptions, more than 60 million Americans got a rare, unedited, close-up look at the candidates in one of the most important presidential elections in the nation's history.
>
> John Kerry got the better of President Bush in last Thursday's debate in Coral Gables, Fla. The president seemed listless, defensive and not particularly well prepared. His facial expressions and body language at times were odd. Some of his strongest supporters were dismayed by his performance, and polls are showing they had reason to be concerned.
>
> There undoubtedly were many reasons for Mr. Bush's lackluster effort. But I think there was one factor, above all, that undermined the president in last week's debate, and will continue to plague him throughout the campaign. And that was his problematic relationship with reality.
>
> Mr. Bush is a man who will frequently tell you – and may even believe – that up is down, or square is round, when logic and all the available evidence say otherwise. During the debate, this was most clearly displayed when, in response to a question about the war in Iraq, Mr. Bush told the moderator, Jim Lehrer, "The enemy attacked us, Jim, and I have a solemn duty to protect the American people, to do everything I can to protect us."
>
> Moments later Senator Kerry clarified, for the audience and the president, just who had attacked the United States. "Saddam Hussein didn't attack us," said Mr. Kerry. "Osama bin Laden attacked us. Al Qaeda attacked us."
>
> Given a chance to respond, Mr. Bush flashed an unappreciative look at Senator Kerry and said, "Of course I know Osama bin Laden attacked us – I know that."

06-10-04 *Incompleteness* (to H. Price)

By the way,

Pricey Quote 1: *I'm fascinated to hear that you've been reading stuff written under what I usually think of as my other hat, such as "Truth as convenient friction". Somewhere I've got a rough piece I wrote about 20 years ago, trying to argue that there were two attractive avenues to explore in QM (in the light of the difficulties with either answer to the question whether the wave function is a complete description): first, backward causation HV approaches, and second, a view which rejects the question, on the grounds that the very notion of complete description is suspect. I was reminded of it when I read the attachments you sent before.*

could you send me that draft as you get a chance (if it is in electronic form, that is). I would enjoy reading it.

Regarding on my own take on the "incompleteness" of wave functions, let me paste in a note I wrote a little while ago to a reporter at *Scientific American*. He kept hammering on how I could say that the wave function is not a complete description of a quantum system (actually, I would say it's not even a description … at least in a correspondence sense) and yet not be a (closet) supporter of hidden variables extensions. In my frustration, the note below resulted. In writing it, I became quite keen on the metaphor of the map (especially the bit about curvature at the end). [See 07-07-04 note to Musser, titled "B".]

08-10-04 *Krugman, Reality* (to me)

From Paul Krugman, "Ignorance Isn't Strength," *New York Times*, 8 October 2004:

> I first used the word "Orwellian" to describe the Bush team in October 2000. Even then it was obvious that George W. Bush surrounds himself with people who insist that up is down, and ignorance is strength. But the full costs of his denial of reality are only now becoming clear.
>
> President Bush and Vice President Dick Cheney have an unparalleled ability to insulate themselves from inconvenient facts. They lead a party that controls all three branches of government, and face news media that in some cases are partisan supporters, and in other cases are reluctant to state plainly that officials aren't telling the truth. They also still enjoy the residue of the faith placed in them after 9/11.
>
> This has allowed them to engage in what Orwell called "reality control." In the world according to the Bush administration, our leaders are infallible, and their policies always succeed. If the facts don't fit that assumption, they just deny the facts.
>
> As a political strategy, reality control has worked very well. But as a strategy for governing, it has led to predictable disaster. When leaders live in an invented reality, they do a bad job of dealing with real reality.

08-10-04 *Emerson* (to M. Pérez-Suárez)

Emerson. I don't have much to tell you, because I don't know that much about Emerson yet. But I do know that Cornell West has argued that pragmatism has its roots in Emerson. You can read about that in his book, *The American Evasion of Philosophy: A Genealogy of Pragmatism*. I read a couple of chapters of this book—the parts on Emerson in fact—but then stopped reading because his writing style drove me to distraction. Unfortunately, I don't remember much of what West said.

Here's another source that you might look at: David Jacobson's book, *Emerson's Pragmatic Vision: The Dance of the Eye*. I've got it in my collection, but a) I haven't read it, and b) it's still packed up in a box somewhere (until I get some new bookshelves), so I can't look at it. However, here's a blurb on the book: `http://www.psupress.org/books/titles/0-271-00896-2.html`. Also, I can tell you that William James knew Emerson personally; Emerson was a friend of the James family, in fact, when William was a child. Beyond that, though, you're left to your own research.

Don't ask me questions about LSE and Kluwer. Some things I'm too ashamed to talk about . . .

08-10-04 *Aage Bohr* (to G. Musser)

Musserism 25: *What do you think of Aage Bohr et al.'s letter in the October* Physics Today*?*

I haven't been able to see that article, because a) unfortunately the Bell Labs electronic library does not carry the most recent three months of the magazine (that would cost the library more), and b) my own subscription seems to have ended in August (or at least the September and October issues were not in my mail pile). Maybe I'm behind in my APS membership.

Anyway, if you've got the article in electronic form, please send it to me. If you don't, can you tell me what it said?

12-10-04 *Reality, Cohen, Christopher Reeve* (to me)

From Richard Cohen, "The Dreams and Realities of a Superman," *New York Times*, 12 October 2004:

> As she aged, Marlene Dietrich became a recluse. She was seen by a few people, but never by the public nor in public. She was determined to stay as she had been – a striking beauty. Her decline and death would remain her own business. The myth would endure.
>
> Christopher Reeve took a different tack. After considering and rejecting suicide, he thrust himself back into public life. In the words of the writer Roger Rosenblatt, who got to know Reeve when he worked on the actor's biography, Reeve had to choose between a "horizontal and a vertical life. He aggressively chose the vertical life."
>
> This does not mean that Reeve ever stood again after being thrown from a horse in 1995. It did mean that he avoided becoming a recluse. It did mean that he made public appearances, was interviewed several times by Barbara Walters, appeared before Congress and at the 1996 Oscar awards ceremony, returned to film and even became a director. He refused to become invisible.
>
> Death often rebukes, and this is one of those occasions. Reeve's insistence that he would someday walk again was proved sadly false – as some always said it would be. He was criticized by knowledgeable people for what they characterized as his ignorant or opportunistic optimism – he appeared in a TV ad promoting spinal cord research – that gave false hope to others in his condition. What these people needed above all, Reeve's critics said, was the determination to face reality – not the bogus dream that the past could somehow become the future. [. . .]
>
> There always was a kind of lie to the life of Christopher Reeve. His faith that he would walk again, his optimism in the face of insurmountable bleakness – all of this was belied by what we all sensed was a dismal truth. But he kept at it – kept us at it – and made us think about what had become of him and others like him. "It was his way of standing up," Rosenblatt said.
>
> R.I.P., Christopher Reeve. You always stood tall.

12-10-04 *Three Children* (to D. B. L. Baker)

Thinking of Child #3, there is something else I had wanted to send you in addition to the little note I wrote commemorating Elizabeth's birth ... but until now had forgotten. It's the introduction to my first lecture at a summer school at Caltech a couple months back. I'll paste it in below. It is along the lines of something I wrote for Madelynn's birth: Some themes die hard with me. Please keep it for all three of your girls. I had them all in mind along with my two as I wrote it. [See 17-06-04 note "Preamble" to H. Mabuchi.]

12-10-04 *Being Bayesian in a Quantum World* (to W. T. Grandy, Jr.)

Thanks for the nice note; I'm flattered. And most importantly, I'm glad you're enjoying my paper. There's a lot of work to be done, but more and more people are taking note of the sensibility of (at least the broad outline of) the program, and that leaves me hope that we'll see some real progress in quantum foundations in the next few years.

In case you're not aware of some of the other papers melding elements of Bayesianism with quantum mechanics, let me put a list below for you.

First, some of my own (with colleagues):

- Quantum information: How much information in a state vector?
  `http://www.arxiv.org/abs/quant-ph/9601025`
- Quantum Probability from Decision Theory?
  `http://www.arxiv.org/abs/quant-ph/9907024`
- Information Tradeoff Relations for Finite-Strength Quantum Measurements
  `http://www.arxiv.org/abs/quant-ph/0009101`
- Unknown Quantum States: The Quantum de Finetti Representation
  `http://www.arxiv.org/abs/quant-ph/0104088`
- Notes on a Paulian Idea: Foundational, Historical, Anecdotal and Forward-Looking Thoughts on the Quantum
  `http://www.arxiv.org/abs/quant-ph/0105039`
- Quantum Probabilities as Bayesian probabilities
  `http://www.arxiv.org/abs/quant-ph/0106133`
- Quantum Foundations in the Light of Quantum Information
  `http://www.arxiv.org/abs/quant-ph/0106166`
- The Anti-Växjö Interpretation of Quantum Mechanics
  `http://www.arxiv.org/abs/quant-ph/0204146`
- Quantum Mechanics as Quantum Information (and only a little more)
  `http://www.arxiv.org/abs/quant-ph/0205039`
- Conditions for compatibility of quantum state assignments
  `http://www.arxiv.org/abs/quant-ph/0206110`
- Gleason-Type Derivations of the Quantum Probability Rule for Generalized Measurements
  `http://www.arxiv.org/abs/quant-ph/0306179`
- A de Finetti Representation Theorem for Quantum Process Tomography
  `http://www.arxiv.org/abs/quant-ph/0307198`
- On the Quantumness of a Hilbert Space
  `http://www.arxiv.org/abs/quant-ph/0404122`
- Unknown Quantum States and Operations, a Bayesian View
  `http://www.arxiv.org/abs/quant-ph/0404156`

Secondly, some from my colleagues:

- C. M. Caves and R. Schack, "Properties of the frequency operator do not imply the quantum probability postulate"
  `http://www.arxiv.org/abs/quant-ph/0409144`
- R. Schack, "Quantum theory from four of Hardy's axioms"
  `http://www.arxiv.org/abs/quant-ph/0210017`
- R. Schack, T. A. Brun, and C. M. Caves, "Quantum Bayes Rule"
  `http://www.arxiv.org/abs/quant-ph/0008113`
- R. W. Spekkens, "In defense of the epistemic view of quantum states: a toy theory." (This paper is not explicitly Bayesian in bent, but it is big a step in the right direction. I think this is the most important paper in quantum foundations in quite some time.)
  `http://www.arxiv.org/abs/quant-ph/0401052`
- R. W. Spekkens, "Contextuality for preparations, transformations, and unsharp measurements." (This paper is not explicitly Bayesian in bent, but it is big a step in the right direction.)
  `http://www.arxiv.org/abs/quant-ph/0406166`
- D. M. Appleby, "Facts, Values and Quanta"
  `http://www.arxiv.org/abs/quant-ph/0402015`
- D. M. Appleby, "Probabilities are single-case, or nothing"
  `http://www.arxiv.org/abs/quant-ph/0408058`
- D. M. Appleby, "The Bell-Kochen-Specker Theorem"
  `http://www.arxiv.org/abs/quant-ph/0308114`

There are several more that I could recommend, but since I suspect your main interest is in the Bayesian aspects of the program, I'll leave it at that for the moment.

Also, by the way, if you would like a copy of the Växjö University Press edition of my 700 page samizdat *Notes on a Paulian Idea* (which you can preview online in a 500 page format at the link above), let me know and I'll have a copy sent to you. All I'll need is your mailing address. You can find my eulogy to Ed Jaynes on page 49 of the VUP edition under the heading "Probability Does Not Exist," and general references to him throughout the book (by looking in the name index). Your name, in fact, is listed in the index five times.

Thanks again for writing. It makes me very happy to learn of people out there sharing in the spirit!

12-10-04 *Blowing in the Wind* (to G. L. Comer)

I thought about both of us as I was reading the article below yesterday. I particularly loved the line, "Half the people you knew believed that if only they could figure out what Bob Dylan was saying, the secrets of the universe would be revealed." I've certainly done that: With John Lennon, then Paul Simon, and then Bob Dylan, and before and after and in between with John Wheeler and John Coltrane, and now mostly with William James and Richard Rorty. I keep waiting for the secrets of the universe to be revealed, and it's usually in poetry and music and stirring prose. Or at least I used to. Now mostly I worry about getting a new roof on the house and getting the place rigged with central air and heat . . . and how much that's going to cost my pocket book.

12-10-04 *Endophysics Meeting* (to H. Atmanspacher)

Will you be going to this meeting:

> Endophysics, Time, Quantum and the Subjective
> January 17–22, 2005, Bielefeld, Germany

I saw your name listed in the invited speakers list (and that the meeting is invitation only). I think I would be interested in attending, and I think I could speak on aspects of quantum information that are probably little known to the audience. Do you think there is any room left? Do you know the organizers?

12-10-04 *Equiangular Lines* (to C. H. Bennett)

Bennettism 21: *Today I just noticed a dissertation* `quant-ph/0410071` *on your favorite subject.*

You mean on maximal sets of equiangular lines in Hilbert spaces of general finite dimension D?

Good to hear from you. When you get back, I'm hoping to drop in on you guys and get you fired up on this pesky question. I've told John we can call it the "SIC Project." (SIC is really for Symmetric Informationally Complete, but the overtones are clear.) I want to know everything I can about these sets of quantum states, abstract and applied. (Renes, for instance, now has a nice QKD protocol in terms of them.)

A much more important thing for you to look at (if, for instance, you've only got the stomach to look at one such thing this year) is Rob Spekkens' paper `quant-ph/0401052`. I like to think that's really a very important paper.

Tell Theo hello for me and send my congratulations on her retirement.

What was quantum about your cobwebs?

13-10-04 *The Incentive Program* (to G. Bacciagaluppi)

Thanks for the note, and it was great to hear your good news. I had such a wonderful time in Paris when I last visited (Grinbaum and Bitbol), that I can well see you might be in paradise. I stayed at some little hotel in the Latin Quarter and found, to my surprise, that my brain could still produce fresh thoughts: There is just something about even a simple walk in the streets there that is so promotional of thought. And Bitbol, particularly, was a great find: I like his style of philosophy and certainly have much in common with him when it comes to quantum foundations. I think there is something very deep in his phrase "the blinding closeness of reality."

But I write this note for another purpose. Thinking of our tripartite interests last night (i.e., yours, mine, and Jenni's), I decided it was time to finally unpack the box of single malts and get the bottles organized [...]

13-10-04 *The Free City* (to D. B. L. Baker)

I know that over the years you've heard me talk about the Copenhagen interpretation of quantum mechanics, the Bohmian interpretation (in Ischia in particular), the many-worlds interpretation, and so forth. There's quite a long list of such "interpretations" now; they come a dime a dozen. Well, to the list I've been trying to add my own—I call it the "sexual interpretation of quantum mechanics." I call it this because the name really captures the main idea I want to get across—that quantum mechanics hints of a world in constant creation. Moreover, that such creation comes about in an almost sexual way whenever two conceptual parts of the world "touch" each other. (The first written mention of this that I can find in my records is in a letter dated 3 September 2001 to Michael Nielsen, now posted in my *Quantum States: What the Hell Are They?* on the web; I'm not sure how long I had been talking about the idea before then. On the other hand, the phrase made its national debut in a little description in *Scientific American* last month, page 91.)

OK, that's the introduction; now for the story I wanted to record. A couple of years ago, a friend (who shall remain nameless) and I were in Copenhagen for a few days. As it goes, we found out that there is a little section in the city, called Christiania or "the Free City," that's something like a big hippie commune. There's over 900 people who live there, and marijuana and hash are sold freely in the streets. As long as you stay within Christiania's boundaries, the police won't bother you about possession and such. It's great: In one of the beer gardens, there was a huge banner proclaiming "JUST SAY NO to hard drugs." My friend, giddy like a school kid, wanted to go to the Free City for the purpose of buying a joint—something he hadn't done in years—and I thought, "Why not?" So, I accompanied him.

We went to the beer garden I just mentioned, and he lit it up. But here and there throughout the day we had been talking about the "sexual interpretation," and it was really getting under his skin. He would say, "You're only doing this for shock value, to bring attention to yourself. The name adds nothing substantial to our understanding of quantum mechanics." And I would say, "No, no, no; it really does capture an important idea. And it is one that needs to be made just as plain as it can be." We went round and round in argument. He took his first hit. Exhaled, and at the same time finally said in exasperation, "What are you really hoping to get out of this?!" In equal exasperation, I said, "I guess I just want to believe that with every quantum measurement I perform, I *please* nature just a little." He took his second hit. "So what? What good would that do!?" I couldn't help myself! "If I do it right, maybe she'd eventually gasp my name!"

You should have seen the guy's eyes! They lit up! He looked at me with complete conviction and said, "I get it. I get it." And for a moment you could tell he really did. ... And for a moment, I really got it too!

15-10-04 *Bohr, Mottelson, and Ulfbeck* (to G. Musser)

If you ever sent me the lost email again that I had requested, I never got it.

Anyway, I've seen the letter in *Physics Today* now:

Musserism 26: *What do you think of Aage Bohr et al.'s letter in the October* Physics Today*?*

But, I'm not sure what you want to know.

Once upon a time, I wrote a rather mean-spirited note to Mermin about Ulbeck and Bohr's "genuine fortuitousness." I'll dig that up and paste it below, but I doubt it answers whatever you had in mind. [See 25-09-02 note "Ulfbeck and Bohr" to N. D. Mermin.] But who knows, maybe it does. `\bdm` and `\edm` refer to the beginning and end of a David Mermin quote; `\bq` and `\eq` refer to the beginning and end of a more general quote. I later cleaned that letter up and used it in Section 3 of my pseudo-paper "Delirium Quantum" which I sent you a while back.

If your concern has to do with the phrase "quantum world," and contrasting their use of the phrase with mine (I say it exists, they say it doesn't)—they're quite likely very different uses of the phrase—then it would probably take me longer to answer you.

15-10-04 *Genuine Fortuitousness* (to O. C. Ulfbeck)

I noticed from reading your letter (with Bohr and Mottelson) in the October *Physics Today* that you three have a new article: *Foundations of Physics* **34**, (2004) p. 405. Would it be possible for you to send me an electronic version of the paper (TEX, LATEX, PDF, MS Word, etc.)—our library at Bell Labs no longer carries that journal.

21-10-04 *Dowd on Faith and Certainty* (to me)

From Maureen Dowd, "Casualties of Faith," *New York Times*, 21 October 2004:

> When I was little, I was very good at leaps of faith.
>
> A nun would tape up a picture of a snow-covered mountain peak on the blackboard and say that the first child to discern the face of Christ in the melting snow was the holiest. I was soon smugly showing the rest of the class the "miraculous" outline of that soulful, bearded face.
>
> But I never thought I'd see the day when leaps of faith would be national policy, when the fortunes of America hung on the possibility of a miracle.
>
> What does it tell you about a president that his grounds for war are so weak that the only way he can justify it is by believing God wants it? Or that his only Iraq policy now – as our troops fight a vicious insurgency and the dream of a stable democracy falls apart – is a belief in miracles? [. . .]
>
> W.'s willful blindness comes from mistakenly assuming that his desires are God's, as if he knows where God stands on everything from democracy in Iraq to capital-gains tax cuts.
>
> As Lincoln noted in his Second Inaugural Address about the Civil War, one can't speak for God: "The Almighty has His own purposes."

21-10-04 *Words Aiming to Inspire* (to R. E. Slusher)

Finally a note that has nothing to do with the performance review . . . Spurred by our conversations yesterday, let me send you the little introduction I wrote for my Caltech summer school lectures a few months ago. It's pasted below. [See 17-06-04 note "Preamble" to H. Mabuchi.] I think it touches on quite a few themes of our discussion yesterday—both on the points where we seem to agree and the points where we seem to disagree.

I hope it paints a fairly evocative picture of what I see quantum mechanics indicating at its core: That the world is in continued (everlasting) creation. And that this creation comes about in an almost "sexual" way—that is, it is something that happens whenever two pieces of matter get together. (This is what was being talked about when *Scientific American* said I call my view the "sexual interpretation of quantum mechanics".) But I am careful to say quantum mechanics *indicates* this: Because another part of the lesson is that quantum mechanics itself is not a low-level physical theory making direct statements about physical reality. Rather it is a very high-level theory whose main concern is about an agent's (i.e., a man, a computer, whatever it is that is needed to make an agent) making enlightened decisions in response these little acts of birth that it itself is involved in.

Anyway, that's what the note below is about. Read it and see if it stirs any sympathy. I think you might see some resemblance between my idea of "birth" and your attention to the discreteness of quantum measurement outcomes that you were mentioning yesterday.

Just waiting for Russo to begin to speak.

21-10-04 *More on the S Interpretation* (to R. E. Slusher)

If the Caltech intro did anything for you, you might also be interested in this little pseudo-paper "Delirium Quantum" that I distributed at the Minnesota meeting (but never posted on the `arXiv`). [See "Delirium Quantum" `arXiv:0906.1968v1`.]

21-10-04 *Books, Books, Books!* (to J. B. Lentz & S. J. Lentz)

Thank you both for the fabulous birthday present!!! You certainly shouldn't have ... but I'll certainly take it! Tomorrow I'm taking the whole day off to go book shopping, and I suspect I'll be in the Strand for about six hours of that. This is really the best birthday present I've had in ages.

I've been joking lately about how there's no better insulation for thin walls than a good collection of American pragmatism! (In case you want to know more about the philosophy, go here: `http://www.pragmatism.org/`. William James is my hero of heroes.) Anyway, let's see how the library grows tomorrow. At present, that section of the library has 331 books. I'll give you an update on what it looks like by the end of the weekend.

22-10-04 *More from NY Times about Reality* (to me)

From Bob Herbert, "Bush's Blinkers," *New York Times*, 22 October 2004:

> Does President Bush even tip his hat to reality as he goes breezing by?
>
> He often behaves as if he sees – or is in touch with – things that are inaccessible to those who are grounded in the reality most of us have come to know. For example, with more than 1,000 American troops and more than 10,000 Iraqi civilians dead, many people see the ongoing war in Iraq as a disaster, if not a catastrophe. Mr. Bush sees freedom on the march.
>
> Many thoughtful analysts see a fiscal disaster developing here at home, with the president's tax cuts being the primary contributor to the radical transformation of a $236 billion budget surplus into a $415 billion deficit. The president sees, incredibly, a need for still more tax cuts.
>
> The United States was attacked on Sept. 11, 2001, by Osama bin Laden and Al Qaeda. The president responded by turning most of the nation's firepower on Saddam Hussein and Iraq. When Mr. Bush was asked by the journalist Bob Woodward if he had consulted with former President Bush about the decision to invade Iraq, the president replied: "He is the wrong father to appeal to in terms of strength. There is a higher father that I appeal to." [...]
>
> There are consequences, often powerful consequences, to turning one's back on reality. The president may believe that freedom's on the march, and that freedom is God's gift to every man and woman in the world, and perhaps even that he is the vessel through which that gift is transmitted. But when he is crafting policy decisions that put people by the hundreds of thousands into harm's way, he needs to rely on more than the perceived good wishes of the Almighty. He needs to submit those policy decisions to a good hard reality check.

25-10-04 *Son of Samizdat* (to H. Price)

I'm pleased to hear that you're enjoying my samizdat! If that's whet your appetite, I hope you'll move on to Son of Samizdat as you get a chance. More officially, it's titled *Quantum States: What the Hell Are They?* and it's posted on my website. I consider it the best work of my life (though it still needs distilling into a paper). It's where I took my strong anti-representationalist turn. It was a very hard turn to take, but I think if we stick with it the rewards could be very big.

25-10-04 *Inventory* (to J. B. Lentz & S. J. Lentz)

OK, here's the monster you helped create! The list of Friday purchases is pasted in below. If you want to borrow any of the books you funded, let me know . . .

More seriously, get this title: Michael Magee, *Emancipating Pragmatism: Emerson, Jazz, and Experimental Writing*. I didn't buy it, but the blurb on Amazon says:

> *Emancipating Pragmatism* is a radical rereading of Emerson that posits African-American culture, literature, and jazz as the very continuation and embodiment of pragmatic thought and democratic tradition. It traces Emerson's philosophical legacy through the 19th and 20th centuries to discover how Emersonian thought continues to inform issues of race, aesthetics, and poetic discourse. Emerson's pragmatism derives from his abolitionism, Michael Magee argues, and any pragmatic thought that aspires toward democracy cannot ignore and must reckon with its racial roots. Magee looks at the ties between pragmatism and African-American culture as they manifest themselves in key texts and movements, such as William Carlos Williams's poetry; Ralph Ellison's discourse in *Invisible Man* and *Juneteenth* and his essays on jazz; the poetic works of Robert Creeley, Amiri Baraka, and Frank O'Hara; as well as the "new jazz" being forged at clubs like The Five Spot in New York. Ultimately, Magee calls into question traditional maps of pragmatist lineage and ties pragmatism to the avant-garde American tradition.

Maybe that explains why I've been drawn to jazz since meeting you guys!

Thanks again for the birthday present.

1. John Dewey, *Experience and Nature*, Second Edition, (Open Court, La Salle, IL, 1971), HC.
2. John Dewey, *John Dewey—The Later Works, 1925-1953. Volume 12: 1938*, editor Jo Ann Boydston, textual editor Kathleen Poulos, introduction by Ernest Nagel, (Southern Illinois University Press, Carbondale, IL, 1986), HC.
3. William Joseph Gavin, *William James and the Reinstatement of the Vague*, (Temple University Press, Philadelphia, 1992), HC.
4. Philip Kitcher, *The Advancement of Science: Science without Legend, Objectivity without Illusions*, (Oxford University Press, NY, 1993), PB.
5. Arthur O. Lovejoy, *The Thirteen Pragmatisms and Other Essays*, (The Johns Hopkins Press, Baltimore, 1963), HC.
6. John McCormick, *George Santayana: A Biography*, (Alfred A. Knopf, NY, 1987), HC.

7. Ralph Barton Perry, *Philosophy of the Recent Past: An Outline of European and American Philosophy Since 1860*, (Charles Scribner's Sons, NY, 1926), HC.
8. Ralph Barton Perry, *Present Philosophical Tendencies: A Critical Survey of Naturalism, Idealism, Pragmatism and Realism, together with a Synopsis of the Philosophy of William James*, (George Braziller, Inc., NY, 1955), HC.
9. Philip P. Wiener, *Evolution and the Founders of Pragmatism*, with foreword by John Dewey, (Harper & Row, NY, 1965), PB.
10. Tulane Studies in Philosophy, Volume XII, 1963: Studies in Recent Philosophy, PB. (Bought it because of three interesting essays on George Herbert Mead and pragmatism.)

25-10-04 *Stages of Development* (to N. D. Mermin)

I went through a hernia operation in the winter of '94 and didn't much enjoy it. (Though, Carl Caves used to pride himself that it was his kids that likely caused the hernia—when I was playing with them one evening—and, by way of that, he could trace the cause of my meeting Kiki back to himself. You see, because of the operation, I actually stayed home for a while and stopped looking for a girl friend in the coffee shops and bars. And, lo and behold, in my boredom looking out the window, I first noticed this beautiful neighbor walking her dog. The rest is history ... and two kids.)

Which reminds me of something that I've been wanting to record. Lately when Katie (who is getting close to three years old) misplaces something, if I ask her where it is, she will reply, "anywhere." She doesn't say, "I don't know." Or "I can't remember," or indeed any other phrase that would put the blame on her incapacities. She just replies with "anywhere." What has struck me is how much this reminds me of some of the early expositions of the uncertainty principle that I was exposed to. Maybe in both cases, it's all about stages in our development.

25-10-04 *The Quantum and the Politic* (to N. D. Mermin)

By the way, as you can guess, I've been watching the presidential race with greater than usual interest this time around. Of course there's the issue of how our nation is in a quagmire now ... and my profound confusion about how so many Americans can still support Bush. But there's been an undercurrent that has interested me at the intellectual level: All the talk about belief, reality, and certainty. It's everywhere!

In the first presidential debate, I couldn't have gotten a better slogan for our own discussions of the last couple of years, than from a remark Kerry made to Bush: "But this issue of certainty: it's one thing to be certain, but you can be certain and be wrong."

Even in quantum mechanics.

25-10-04 *Ever More Interpretations* (to M. A. Nielsen)

Did you happen to read David Mermin's diatribe in the May issue of *Physics Today* about how he invented the "Shut Up and Calculate Interpretation" of QM? (I'm just getting caught up with my *Physics Todays*—they were all piled up in my Bell Labs mail box when I returned from Ireland.) Anyway, I faintly remember you and I having that very conversation a few years back at Caltech. We were in your office as I recall, and one of us said Feynman and one of us said Mermin. Or, at least I faintly remember that. Problem is, I don't remember which of us took which side! Do you remember this? I've been trying to replay this in my mind, and half the time I think I thought it was Feynman and half the time I think I thought it was Mermin! If you can help me out, I might be able to make up a good joke to play on Mermin.

Michael's Reply

> Funny, I remember having this discussion with someone, but I don't remember it being with you! Dave Bacon comes to mind. I'm not sure this helps you out in quite the way you hoped, although it does add to the story. So many cases of mistaken identity . . .

25-10-04 *More on Landauer* (to W. T. Grandy, Jr.)

Grandyism 1: *I was interested in the exchanges with Landauer, but his replies didn't reveal much. For a number of years now I've been wrestling with the definition of information – to paraphrase the title of one of your papers, "What the Hell Is It?" In contemplating Landauer's claim that "Information Is Physical" I've had the impression that he was not distinguishing between information and its representation, and was really talking about the latter, which is indeed physical.*

I'm inclined at this point to think that the concept of information – like randomness, probability, and entropy – is basically anthropomorphic, or at least brain dependent down to some level. But, then, anthropomorphic is certainly physical, though not in the sense usually envisioned by physicists, so maybe his characterization has something to it after all. This is a bit like punching a pillow, or wrestling with jello!

I am pleased to hear you are enjoying my samizdat. Better than the Landauer section, I think you might get more from the exchanges with Mermin and Peres—at least a lot more Bayesian flesh was put on my Copenhagen bones there.

You are right about Rolf confusing information with information carriers—I think that has caused quite a bit of trouble in our quantum-information community. I'll append a note below where I tell a little story about this. (This one comes from my second email samizdat "Quantum States: What the Hell Are They?" posted on my webpage.) [See 02-02-02 note to Timpson, titled "Colleague."]

26-10-04 *More "More on Landauer"* (to W. T. Grandy, Jr.)

Grandyism 2: *Thanks for your last note and the Bennett anecdote re: Landauer. Sometime, perhaps after a sufficient amount of wine, I'll have to share my copy of Ed Jaynes' rejection of Landauer's paper "Minimum Energy Dissipation in Logic." Other correspondence indicates that the editor was not at all happy with Ed's recommendation, but I'd guess the original still exists in the editorial archives of the IBM J. Research. It was published anyway, of course.*

I would very much like to see that, if you ever feel like divulging it! As you can see, I'm fashioning myself somewhat as a historian of quantum information. And your copy of the report sounds like it will ultimately be important historically.

Tom's Reply

> Upon reflection, and in the interests of history, I see no reason not to pass this on to you now, particularly since both Ed and Rolf are gone. I've attached a pdf file with both

the request and Ed's report. In connection with this you might also find interesting an excerpt from an email Ed sent to me on 7 July 1996: "Landauer just throws his readers off by insisting that erasing a memory requires a dissipation of energy of about kT per bit. It obviously requires NO dissipation of energy, because computer memories are never "erased"; they are just overwritten. Resetting every bit to zero does not destroy any information either – it merely moves it into correlations between the memory and its environment. It is when you fail to take note of the changed environment that information is lost; but still no energy is dissipated. I have been trying to explain this to Landauer for 35 years now (having refereed his first paper in which he said this and trying again whenever I meet him) without the slightest success."

An interesting point here is the discrepancy in dates in the above review and the "35 years", so there must be an earlier Landauer paper that Jaynes also refereed – unless Ed's memory here was faulty, but it usually was prodigious.

Letter from IBM Journal Editor to E. T. Jaynes

November 10, 1969

Professor E. T. Jaynes
Department of Physics
Washington University
St. Louis, Missouri 63130

Dear Professor Jaynes:

Thank you for agreeing to review the enclosed manuscript, ''Minimum Energy Dissipation in Logic'' by R. W. Keyes and R. Landauer. It has been submitted for publication in the *IBM Journal of Research and Development* and I would like to know your opinion of its originality, technical accuracy and general merit.

A list of suggested review criteria is enclosed, but these need not be followed on a one-for-one basis. I would also appreciate any comments or suggestions you think appropriate. Your report will be anonymous where the authors are concerned; may I hear from you by November 24?

There will be an honorarium and I have entered a complimentary subscription to the *Journal* in your name.

Sincerely,

R. J. Joenk
Associate Editor
IBM JOURNAL OF
RESEARCH AND DEVELOPMENT

RJJ/mgk
Enclosures

E. T. Jaynes' Referee Report

REPORT OF REFEREE

R. W. Keyes and R. Landauer, ''Minimum Energy Dissipation in Logic''

I feel that the weakest aspect of this article is its lack of convincing power; after reading it over several times, I remain unconvinced that computer operations have anything to do with entropy, or that there is <u>any</u> fundamental minimal energy dissipation in logic steps.

This doubt stems from two causes, both of which should be correctible if the authors' conclusions are correct.

Firstly, they place a great burden on the reader, requiring him to have read about a dozen previous articles from which they merely quote the conclusions without giving any supporting arguments for them. Surely, if these matters have been discussed so much, there must exist by now some simple argument, requiring only a paragraph or two, indicating to the reader <u>why</u> any fundamental limitation must exist. Presentation of <u>one</u> such argument would accomplish more for the reader than citing any number of ''authorities.''

Secondly, their analysis of one particular model does not seem entirely air-tight, and in any event it gives no reason to think that the conclusions are general. A different model might lead to entirely different conclusions. I illustrate this by two counter-examples.

On the next to last page of the text, the authors say that removal of a thin barrier leads to ''an unavoidable non-equilibrium process'' with an entropy increase of k ln 2. But this does not follow from the laws of physics; there must be some hidden assumption involved. For example, before the barrier is removed the particle might be in a definite quantum state $\psi(0)$. Removal of the barrier then leads to a problem which can in principle be solved to give the state $\psi(t)$ at any later time, in which the wave function will in general be non-zero on both sides. But it was initially in a pure state with entropy zero, and it remains, according to the Schrödinger equation, in a pure state with entropy zero. The supposed ''irreversibility'' must be the result of some unstated limitation of how deeply we are permitted to analyze the situation.

A switching operation does not have to take place via adiabatic changes in a potential well. We can have simply two well-separated holes A and B, into which a particle may be put. We switch by lifting the particle bodily out of a hole A, and placing it in hole B. Whatever energy was required to lift it out of A is recovered when we lower it back into B. If one considers a thermal situation, with the energy in holes A and B fluctuating by kT, this conclusion remains true on the average, for we have just as much chance of gaining energy as losing it when the transfer is made.

If I were a computer engineer, I think I would simply ignore articles that try to tell me what I <u>cannot</u> do; and just go ahead and do them anyway. Almost all progress in technology has been made in the face of theoretical predictions that it can't be done; and unless some limitation is derived rigorously from some undoubted law of physics, such as energy conservation, the lessons of history lead one to place his bets on the clever inventor, to circumvent imaginary problems.

Subsequent Letter from IBM Journal to E. T. Jaynes

February 10, 1970

Professor E. T. Jaynes
Department of Physics
Washington University
St. Louis, Missouri

Dear Professor Jaynes:

Thank you again for your critique of the manuscript, ''Minimal Energy Dissipation in Logic'' by R. W. Keyes and R. Landauer. A copy of the current, revised manuscript is enclosed. There were two other reviews of the original manuscript in addition to yours; one of these is included for your information.

You will observe, of course, that the authors have not adopted the reasoning of your counterexamples. In their opinion, the first example is not appropriate because it is based on a conservative system whereas, in fact, fluctuating external forces must be included in a relevant model of machine manipulations. The second example is thought to be incomplete due to neglect of ''prior information,'' or testing of information states, which is necessar for a realizable computer operation. Both the requirement of energy dissipation and that of testing states are described in Landauer's original paper, which is also enclosed.

If you would like to comment further on this manuscript, I would appreciate receiving your reply by February 18.

```
Sincerely,

R. J. Joenk
Associate Editor
IBM JOURNAL OF
RESEARCH AND DEVELOPMENT

RJJ:mgk
Enclosure
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27-10-04 *NY Times on Reality* (to me)

From Nicholas D. Kristoff, "Pants on Fire?," *New York Times*, 27 October 2004:

> Whenever I say that President Bush isn't a liar, Democrats hurl thunderbolts at me. And when I say Mr. Bush isn't truthful, Republicans erupt like Mount St. Helens.
>
> So what do I mean?
>
> Let me offer an example – not from Iraq but from Mr. Bush's autobiography. In it, he tells a charming little story involving his daughters in 1988, on the eve of the presidential debate between his father and Michael Dukakis:
>
>> "One night, Laura and I were out of town campaigning, and Barbara and Jenna spent the night at the vice presidential mansion. Dad had spent the day preparing for a debate with Michael Dukakis. Unfortunately, Barbara lost her sleeping companion, Spikey, her favorite stuffed dog. She complained loudly that she could not sleep without Spikey, so 'Gampy,' better known as Vice President Bush, spent much of the night before his debate searching the house and grounds of the vice presidential residence, flashlight in hand, on a mission to find Spikey. Finally, he did, and Barbara slept soundly. I don't know if my dad ever went to sleep that night."
>
> It's a heartwarming tale of family values. And while it's not malicious enough to count as a lie, it's laced with falsehoods.
>
> We know that because Mr. Bush's mother wrote about the same incident much earlier, in 1990, in *Millie's Book*, nominally written by her dog. For starters, the episode occurred when the girls were five and a half, in 1987, a year before the presidential debate.
>
> What's more, *Millie's Book* says that Spikey was a cat, not a dog. And instead of searching all night and finally finding Spikey, Vice President Bush gave up, grumbling: "I have work to do. What am I doing searching for a stuffed animal outdoors in the dark?" Anyway, little Barbara had already fallen asleep with another stuffed animal. Spikey turned up the next day behind the curtains. [...]
>
> The current president's hyped version of the incident reflects his casual relationship with truth. Like President Ronald Reagan, reality to him is not about facts, but about higher meta-truths: Mom and Dad are loving grandparents, Saddam Hussein is an evil man, and so on. To clarify those overarching realities, Mr. Bush harnesses "facts," both true and false.

28-10-04 *Enjoyed and No* (to M. Pérez-Suárez)

I read your paper and enjoyed it. And I think it is a great effort in getting out "the word." Also the discussion on probability is top-notch and makes a much better effort than I ever have personally to explain why we adopt the Bayesian notion of probability (even for quantum states).

But this paper really is your production, and you should de-list me as an author. With regard to my *own* research it doesn't contain anything new (unless you count the second part of Eq. 7 ... which I want to lay out in much more detail when I try to put together our springtime discussions in my own way for SHPMP). And stylistically, it is your presentation and personality. Which—actually—is a very good thing for the dissemination of the program: The more angles we all hit it from, the more chance I suspect it will stick in the public's craw. Without my name on it, also, it helps give the impression of an independent construction/exposition. And thus, I hope the technique will bring in a new set of readers.

I hope you won't be offended by my withdrawal: I mean to make no negative connotation by it. It's just that time is short, and when Fuchs gets involved with something, he tends to spend too much time trying to work it into a Fuchsian style (whenever he can get away with it, that is)—and that is a bad trait, especially with respect to the present project.

Now that my head is more clear, and I'm back in a more technical environment, I dearly want to get back into doing calculations. But I also must clear up all these old projects that are still waiting (like SHPMP and Kluwer and such). So, the clearer I can keep my plate the better ... the better for us all.

28-10-04 *Mathematical Metascience* (to D. J. Foulis)

Are you out there somewhere? Would it be possible to send me an electronic copy of your article "Mathematical Metascience"? If you can't, I can get Howard Barnum to mail me a xerox copy of it; he was already on the verge of sending it, but I thought this might be faster.

28-10-04 *Reference, 2* (to F. E. Schroeck)

Below are the references to all three of my quantumness papers. I hope you can understand clearly enough the archaic code that I used. Renes, by the way, in a couple of papers on `quant-ph`, now has a full-fledged quantum cryptographic application for these SIC ensembles.

I'm curious to see what you've written about. Please send me a copy of the paper when you can—by email if possible.

No need for an absentee ballot: We're back in New Jersey now!

- C. A. Fuchs, "On the Quantmness of a Hilbert Space," in *Quantum Information, Statistics, Probability: Dedicated to Alexander S. Holevo on the Occasion of His 60th Birthday*, edited by O. Hirota (Rinton Press, Princeton, NJ, 2004), pp. 65–77. `quant-ph/0404122`.

- K. M. R. Audenaert, C. A. Fuchs, C. King, and A. Winter, "Multiplicativity of Accessible Fidelity and Quantumness for Sets of Quantum States," *Quantum Information and Computation* **4**(1), 1–11 (2004). `quant-ph/0308120`.

- C. A. Fuchs and M. Sasaki, "Squeezing Quantum Information through a Classical Channel: Measuring the 'Quantumness' of a Set of Quantum States," *Quantum Information and Computation* **3**(5), 377–404 (2003). `quant-ph/0302092`.

29-10-04 *URGENT: Exact Quote* (to G. Brassard)

You're in luck. The following paragraph can be found in the JMO paper:

> The task is not to make sense of the quantum axioms by heaping more structure, more definitions, more science-fiction imagery on top of them, but to throw them away wholesale and start afresh. We should be relentless in asking ourselves: From what deep *physical* principles might we *derive* this exquisite mathematical structure? Those principles should be crisp; they should be compelling. They should stir the soul. When I was in junior high school, I sat down with Martin Gardner's book *Relativity for the Million* and came away with an understanding of the subject that sustains me today: The concepts were strange, but they were clear enough that I could get a grasp on them knowing little more mathematics than simple arithmetic. One should expect no less for a proper foundation to quantum theory. Until we can explain quantum theory's *essence* to a junior-high-school or high-school student and have them walk away with a deep, lasting memory, we will have not understood a thing about the quantum foundations.

I've got to run to lunch in a second, so I may not be around for the next couple of hours.

29-10-04 *Intrigue!* (to W. K. Wootters)

I was on the web trying to find the title I gave for a talk at Jeff Bub's "New Directions" conference last year, and I ran across a schedule already posted for next year's meeting. Beside your name, I saw the title: "Measurement-loops as Elemental Quantum Processes." I've got to say, that really, really intrigues me! What are you gonna talk about? If you've got the time to commit a sketch of the idea to email, I'd love to read it!

01-11-04 *Abstract Abstracts* (to C. Beisbart)

OK, here they are, 50 days late:

> Title: Some of the Phenomena of Quantum Information Theory
>
> Abstract: The no-cloning theorem. Quantum entanglement. Quantum teleportation. Quantum key distribution. Positive-operator-valued measures (POVMs). Quantum nonlocality without entanglement. The monogamy of quantum entanglement. Bell inequalities. The Kochen- Specker theorem.
>
> Title: Bayesian-Like Ways to Think of Those Phenomena
>
> Abstract: A Bayesian understanding of no-cloning. A Bayesian description of quantum teleportation. The connection between Bayesian conditionalization and POVMs. ... And so on down the line. Until, finally, the Kochen-Specker theorem: This one may actually be a statement about ontology rather than a statement about degrees of belief! And without some ontology, Bayesians would have very little reason to be.

01-11-04 *Rational Hilbert Space* (to S. Aaronson)

I'm sure you're already aware of this paper, but I just ran across it this morning:

> L. Adleman, J. Demarrais, and M.-D. A. Huang, "Quantum Computability," SIAM J. Comput. **26**(5), 1524-1540 (1997).

Anyway, it caught my attention because it shows that the answer to my bar-room question to you, Toner, and Childs may not have been completely obvious. At least someone found some aspects of it worth writing a paper about.

So, despite the fact that there is no usual Kochen-Specker theorem for rational Hilbert spaces, BQP survives unscathed for them. Just an interesting little factoid for me. Don't know what to do with it, but it's an interesting factoid for me.

I need to come to Princeton sometime soon to get a new library card (probably within the next two or three weeks). Maybe we could meet up for lunch or something.

01-11-04 *No Reductive Physicalism!* (to H. Halvorson)

I ran across your title for the College Park meeting next year, "No Reductive Physicalism, No Measurement Problem," and have become quite intrigued! It looks very, very interesting from my perspective. What sorts of things are you thinking now?

I never heard back from you after you requested some reading suggestions from me. Was any of that useful? Or did it all just look like a load of muck?

Anyway, I'm back in New Jersey again ... and this time I'd like to do things right. In particular, not pass up the opportunity of visiting or collaborating with interesting colleagues in the area. How are things with you? I presume you're at Princeton permanently now. It'd be great if I could give you a visit soon. I've been meaning to come to campus to renew my library card in any case, and I definitely would like to do that before Butterfield departs so I get a chance to see him a little. Might you have any time in the next two or three weeks?

02-11-04 *Election Day Blog* (to C. M. Caves, R. Schack, and N. D. Mermin)

The *New York Times* this morning started one of their columns in this way, and I thought it was particularly relevant for our collaboration:

> Every four years, by journalistic if not political tradition, the presidential election must be accompanied by a "revolution." So what transformed politics this time around? The rise of the Web log, or blog. The commentary of bloggers – individuals or groups posting daily, hourly or second-by-second observations of and opinions on the campaign on their own Web sites – helped shape the 2004 race. The Op-Ed page asked bloggers from all points on the political spectrum to say what they thought was the most important event or moment of the campaign that, we hope, comes to an end today.

So, let me do my duty as an avid reader of the *New York Times* and tell you what I think was the most important event of the campaign. It was near the end of the first presidential debate, 3 October 2004, when John Kerry said, "... this issue of certainty: it's one thing to be certain, but you can be certain and be wrong."

True of politics, true of human relations, true of what can be derived from a quantum state.

Unraveled, it is quite a deep statement, as we know. One might even say, a "revolution." Quantum certainty is not a state of affairs, but a state of mind. And we knew it even before the election year!

Maybe I'll cc this note to Mermin too. Even he, I bet, would agree that one of the key issues in the campaign has to do with the meaning of "certainty."

05-11-04 *I Had To Ask!* (to W. K. Wootters)

Well, I had to ask, didn't I? ...

I got both your notes, and I've read them several times over. But I have to confess that I'm confused about what you're up to.

Is a measurement loop this (at least in the quantum mechanical setting)? Ingredients: 1) a bipartite system comprised of identical components (and hence isomorphic Hilbert spaces), 2) a *fixed* initial state for that bipartite system, 3) a (unknown?) unitary operator on one of the systems (expressing their relative evolution), and 4) the set of all imaginable complete orthogonal measurements on the bipartite system?

Is part of your point—again, in the quantum setting ... I know you ultimately want to abstract away from that—that these ingredients are sufficient for doing tomography on the unitary? Just a question so I can get better oriented.

On a maybe related matter, do you know this phrase I like? That, "a quantum operation is just a quantum state in disguise." What I mean by this is: If from the Bayesian view, a quantum state is the analogue of a (unconditioned) probability distribution, one might ought to think of a quantum operation as the analogue of a conditional probability distribution. And just as the conceptual difference between an unconditioned and a conditioned probability distribution is not so very important, so one might imagine for the conceptual difference between quantum states and operations—namely that there isn't really one.

Anyway, this train of thought led me to try to get the structure of quantum time evolutions out of something like a Gleason theorem for measurements upon a "single system" posed at two points in time. I never could quite make that work the way I wanted it to (though I never completely gave up hope either).

Just thinking out loud now, and wondering whether there might be any connections between the two problems ... since they are both about tomography of time evolutions in way. But before I do more thinking, I should understand whether I've gotten your scenario right. I suspect I didn't.

07-11-04 *Mermin on Dirac Notation* (to A. Wilce, H. Barnum & J. M. Renes)

Taken from Chap. 1 of his lecture notes for his course on quantum computation:

> This is a good example of the primary point of Dirac notation: it has many built in ambiguities, but it is designed so that any way you chose to resolve those ambiguities is correct. In this way elementary little theorems become consequences of the notation. Mathematicians tend to loathe Dirac notation, because it prevents them from making distinctions they consider important. Physicists love Dirac notation, because they are always forgetting that such distinctions exist and the notation liberates them from having to remember.

10-11-04 *Being Bayesian in a Quantum World — Invitation* (to all BBQWers)

Dear Colleague,

We hope you will accept our invitation to the meeting detailed below. We expect to obtain some amount of funding to help cover your travel costs and expenses. Please let us know as soon as possible whether you think you are interested in the meeting and will be able to come. (I, of course, have already fielded the idea of the meeting with many of you: So, I expect we shall have a great turnout!) Once we hear back from you, we will provide more details as the organization proceeds.

On behalf of all the organizers,

Best regards,
Chris Fuchs

BEING BAYESIAN IN A QUANTUM WORLD
1-6 August 2005, Konstanz, Germany

Organizers:	Carlton M. Caves	(University of New Mexico)
	Christopher A. Fuchs	(Bell Labs, Lucent Technologies)
	Stephan Hartmann	(London School of Economics and University of Konstanz)
	Rüdiger Schack	(Royal Holloway, University of London)

To be a Bayesian about probability theory is to accept that probabilities, whenever used, represent subjective degrees of belief and nothing else. This is in distinction to the idea that probabilities represent long-term frequencies or intrinsic, chancy propensities "out there" in nature itself. But how well does a subjective account of probabilities mesh with the existence of quantum mechanics? To accept quantum mechanics is to accept the calculational apparatus of quantum states and the Born rule for determining probabilities in a quantum measurement. If there were ever a place for probabilities to be objective, one would think it ought to be here! This raises the question of whether Bayesianism and quantum mechanics are compatible at all. For the Bayesian, it suggests that perhaps we should rethink what quantum mechanics is actually about!

There is no doubt that we live in a quantum world. From transistors to lasers to nuclear warheads, the evidence is all around us. One could take from this that the individual elements in the quantum formalism give a mirror image of nature: That is, that the wave function is so successful as a calculational tool precisely because it represents an element of reality. A more Bayesian (or, at least, Bayesian-like) perspective is that if a wave function generates probabilities, then they too must be Bayesian degrees of belief, with all that such a radical idea entails. In particular, quantum probabilities have no firmer hold on reality than the word "belief" in "degrees of belief" already indicates. From this perspective, the only sense in which the quantum formalism mirrors nature is through the normative constraints it places on gambling agents trying to navigate through the world. To the extent that an agent should use quantum mechanics for his uncertainty accounting rather than some foil theory tells us something about the world itself—i.e., the world independent of the agent and his particular beliefs at any moment. The task of the quantum Bayesian is to make this argument explicit and rigorous and to reap any benefit such a clarification can give to philosophy and physical practice.

Hogwash or deep idea? The time seems ripe for a discussion, pro and con. At this meeting, in the pleasant surroundings of Lake Konstanz, we envision a roughly fifty-fifty mix of philosophers (who have thought long and hard about probability and quantum foundations) and quantum-information physicists (who have developed an impressive box of mathematical tools for prying apart the probabilistic structure of quantum mechanics) to set the tone. The goal is to make real progress on these issues through a complementarity of talents and some good-hearted debate. Might we better understand the power of quantum computation through Bayesian techniques? Does a Bayesian conception of the quantum state really make "the measurement problem" go away? How secure is quantum cryptography really if a quantum state is "just" a state of belief? Less secure? Or maybe more (honestly) secure? Does a Bayesian conception of the quantum state lead us closer to or further away from the Copenhagen interpretation? What about many-worlds? ... And so the list of topics goes on. We hope the discussions will go long into the night and fill the lakeside walks.

The proposed set of participants is as follows:

Marcus Appleby	(physicist, London, USA)
Guido Bacciagaluppi	(philosopher, Paris, France)
Howard Barnum	(physicist, Los Alamos, USA)
Hans Briegel	(physicist, Innsbruck, Austria)
Todd Brun	(physicist, U. Southern California, USA)
Jeffrey Bub	(philosopher, Maryland, USA)
Paul Busch	(physicist, Hull, UK)
Jeremy Butterfield	(philosopher, Oxford, UK)
Ignacio Cirac	(physicist, Max Planck Inst., Germany)
John Earman	(philosopher, Pittsburgh, USA)
Arthur Fine	(philosopher, Washington, USA)
Jerry Finkelstein	(physicist, San Jose, USA)
Branden Fitelson	(philosopher, Berkeley, USA)
Henry Folse	(historian, Loyola, USA)
Bas van Fraassen	(philosopher, Princeton, USA)
Nicolas Gisin	(physicist, Geneva, Switzerland)
Hans Halvorson	(philosopher, Princeton, USA)
Lucien Hardy	(physicist, Perimeter Inst., Canada)
Jim Hartle	(physicist, UC Santa Barbara, USA)
Jenann Ismael	(philosopher, Arizona, USA)
Norbert Lütkenhaus	(physicist, Erlangen, Germany)
David Mermin	(physicist, Cornell, USA)
Gerard Milburn	(physicist, Queensland, Australia)
Wayne Myrvold	(philosopher, Western Ontario, Canada)
Michael Nielsen	(physicist, Queensland, Australia)
Itamar Pitowsky	(philosopher, Hebrew U., Israel)
David Poulin	(physicist, Queensland, Australia)
Huw Price	(philosopher, Sidney, Australia)
Benjamin Schumacher	(physicist, Kenyon College, USA)
Abner Shimony	(jack of all trades, Boston, USA)
John Smolin	(physicist, IBM Research, USA)
Rob Spekkens	(physicist, Perimeter Inst., Canada)
Christopher Timpson	(philosopher, Leeds, UK)
Jos Uffink	(philosopher, Utrecht, Netherlands)
Bill Unruh	(physicist, British Columbia, Canada)

David Wallace	(philosopher, Oxford, UK)
Reinhard Werner	(physicist, Braunschweig, Germany)
Howard Wiseman	(physicist, Griffith U., Australia)
Anton Zeilinger	(physicist, Vienna, Austria)

We look forward to seeing you in Konstanz!

Best wishes,

Carl Caves, Chris Fuchs, Stephan Hartmann, and Rüdiger Schack

10-11-04 *Being NON-Bayesian in a Quantum World* (to R. Werner)

By now you'll have seen the invitation I sent you for our meeting "Being Bayesian in a Quantum World." I know that you're negative on Bayesian probability as a whole, but for precisely that reason I hope you'll still come to the meeting. Rüdiger Schack and I agree that you have given us some of the best runs for the money we've ever had! And we think you could help keep the meeting quite lively in that regard. Your arguments are sharp and to the point. Whatever the outcome (i.e., pro- or anti-Bayesian), I want the meeting to be a productive one, and you would be great in that regard.

So, I'm definitely hoping to hear back from you!

10-11-04 *$\psi^*\psi$, Subjective Probability?* (to J. Earman)

By now you'll have seen the invitation to our BBQW meeting that I sent you. I definitely hope you'll come! I remember at the end of the Minnesota meeting, you said: "Psi-star psi generating subjective probabilities? Could not be!" So, I know you already have an opinion! But it'd be great to see you engaged on the subject, arguing your point. You'd be a great addition to the meeting. And particularly, I have so many things to discuss with you that I first learned from your book, it'd be a great honor if you were there.

10-11-04 *Do Correlations Need To Be Explained?* (to A. Fine)

By now you'll have seen the invitation to the BBQW meeting I sent you. I so hope you'll come. That old paper of yours was quite influential on me several years ago. So I was thinking of that one of your many hats, when I argued for your inclusion in our list. It would be great to get your feedback, pro or con, on this whole movement as a foundational approach to quantum mechanics. Plus I think you'd just have a lot of fun!

10-11-04 *Even Better Than Your Talk Title* (to H. Halvorson)

Halvorsonism 2: *This project was motivated by your overall program (which, if I have interpreted it correctly, rejects the idea that there is a "measurement problem"). But I've added a bit of gloss that connects this idea with recent debates in philosophy of mind. (Debates about how to formulate "physicalism", etc..)*

In short, all philosophers of physics I know – with the possible exception of Bub – seem to think that the "measurement problem" is a problem that nobody can avoid, no matter what their metaphysical persuasion. But the more I re-read the standard derivations of the problem, the more

I suspect that some bogus metaphysics gets invoked at the last step – viz., reduction of mental events to physical events.

Now, I'm not committed to anti-reductionism, or to some sort of dualism. If pressed, I might say the whole physicalism versus dualism debate is ill-founded. (I think that was James' position; and it is certainly van Fraassen's position.) But I think now that only strong reductionists (namely, those who don't think that the problem is ill-founded, and who think that, in fact, mental events are identical to physical events) have a measurement problem. This is good news for those of us who have a sane view, because we can stop worrying about the supposed problem and get on to more interesting topics!

That sounds great! Now you have my mouth watering to hear more about it all. (I guess I shouldn't say my ears are watering ... that sounds disgusting.) Certainly you know I have read a lot of James by now; he's turned into one of my heroes. Anyway, it sounds like you've got some good meaty thought. Looking forward to your explanations.

Halvorsonism 3: *By the way, John Conway is giving a talk "Free Will, Elementary Particles, & the Kochen-Specker Paradox" on November 19th at 4pm. The talk is based on a recent theorem by Conway and Kochen. They tell me that they have decisively proven, once and for all, that QM is indeterministic. Hmm!*

Frankly, I think I'd almost give up all of PSA to see this one! My own feeling is that nothing in quantum mechanics gets closer to a (trial) ontological statement (or ontological relative to the theory) in quantum mechanics than the Kochen-Specker theorem. Here's the way I put it tongue-in-cheek in a couple of talk abstracts recently (for the summer school that will follow our Konstanz meeting):

> Title: Some of the Phenomena of Quantum Information Theory
>
> Abstract: The no-cloning theorem. Quantum entanglement. Quantum teleportation. Quantum key distribution. Positive-operator-valued measures (POVMs). Quantum nonlocality without entanglement. The monogamy of quantum entanglement. Bell inequalities. The Kochen- Specker theorem.

> Title: Bayesian-Like Ways to Think of Those Phenomena
>
> Abstract: A Bayesian understanding of no-cloning. A Bayesian description of quantum teleportation. The connection between Bayesian conditionalization and POVMs. ... And so on down the line. Until, finally, the Kochen-Specker theorem: This one may actually be a statement about ontology rather than a statement about degrees of belief! And without some ontology, Bayesians would have very little reason to be.

Anyway, I suspect Conway will be talking about another version of Kochen-Specker (or something similar). I'd like to see the mechanics of that.

Hope to hear a positive response from you soon on the BBQW meeting! I think this is going to be the best (most productive, most exciting, most memorable) meeting I've ever been involved in organizing!

10-11-04 *The BBQW Thing* (to N. Lütkenhaus)

By now you'll have seen the invitation I sent you to the BBQW meeting. I hope you'll seriously think about coming and will not feel out of place with so many philosophers. Particularly, I think you could make an important contribution to this meeting, by helping us understand whether our attempt for a Bayesian-like formulation of QM has any implication on information security (or vice versa). Or, even how to pose various protocols—Rüdiger, in particular, would like to get into this aspect of the discussion. Joe Renes is likely to be there too. (Also maybe you've seen the recent papers by the Maurer group on quantum de Finetti theorems and could comment, etc.) Anyway, I'm just saying, your inclusion in the list of invitees was very intentional, and we hope you can come.

11-11-04 *Free (Bayesian) Housecleaning!* (to A. Shimony)

Shimonyism 4: *The conference in Constanz is very tempting. I would learn a lot and would be forced to sharpen the presentation of my objectivist position. But I anticipate such a burden this coming summer that I cannot make a commitment to attend. If there is a change I shall inform you.*

Oh, I do hope that burden will go away, because I dearly want you at our meeting. I want to get so many of these quantum-probability issues settled in our community sooner rather later, and your input really is crucial for that. You've been thinking deeply about probability for so many years, I think it would be great if our participants could get a sense how those thoughts apply to our particular context. If it'd help you out, I could even come to Boston to clean your house this summer!

Seriously, if there's any way you can make the trip happen, I think our whole debate would be much better primed for progress with your presence. Please let me know if there's any way you can rearrange.

11-11-04 *BBQW and Unique Butterfields* (to J. N. Butterfield)

I'm glad you're intrigued by the conference! But I oh so hope you can come. Your presence would fill a unique role there, the way you strive to find common ground between seemingly opposed points of views. We need you actually. Seriously. So I do hope things will work out with your and your family's schedule.

11-11-04 *Making Every Effort* (to D. Poulin)

It's great to hear that you want to come to BBQW!

On your comment, actually I think Smolin, Gisin, and Hartle are all pretty good to being "open to discussion"—each in their own way, of course. I've had very good (and productive) discussions with all of them in the last year or two. But, in any case, rest assured that we're doing our best to round up some *productive skeptics* for the meeting. The notes below show a sampling of the effort specifically in that direction.

The point is, I think you'll have plenty of opportunity to sharpen your viewpoint, rather than either just a) preaching to the choir, or b) being buried asunder. It's gonna be a great meeting: Definitely, you gotta come!

12-11-04 *Philosopher Henry* (to H. J. Folse)

That is great news to hear that you can come! It's gonna be a good meeting, and I'm pretty excited about it.

Sorry to have called you a historian; for some reason, I thought that was your official job title. I should have checked.

Folsesm 22: *What are you looking for from participants, a 20–30 minute paper? I'll have to think up something to put on a power point.*

That's hard to say at this point. The note below is our last exchange on the subject (with which at least Stephan agreed), but I can't say yet whether it represents the final word.

Be thinking about what Bohr would have thought about de Finetti! Is there enough evidence in his work to give us any indication? It would be wonderful if you could give us a report on that. I'm thinking of say: B. de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989). Exempting the first couple of sections (where I can't quite figure out how far deF is really going), I think this is his definitive philosophical statement.

16-11-04 *Copenhagen Hoping* (to A. Zeilinger)

I just sent you an invitation for our meeting "Being Bayesian in a Quantum World", and I'm writing again because I hope, hope, hope you can come. The main thing is that I've had enough contact with the philosophers recently to see that we're starting to chip away at their resistance to the Copenhagen interpretation. And I'm hoping with this meeting we'll finally have the strength to make a full frontal assault. If you could be there that would be a great help, and I think you would find the experience very satisfying.

I hope you will give a reply as soon as possible.

16-11-04 *Symmetry Magazine and Article Request* (to D. J. Harris)

I apologize for keeping you waiting. I thought a lot about your proposal, but ultimately, I don't think I'm going to be able to find a way to tie in what I'd like to say about Einstein and quantum foundations with his photoelectric effect paper. If your call were to write a 650-word version of what I think was the main quantum foundational theme of his life (from 1932 to the end)—i.e., a story like I wrote up on pages 9–11 of `quant-ph/0205039`—then I think I could do it. And I'd do it because that's how the man should be remembered, and the widest possible community should recognize that of him: He had a very clear understanding—maybe more than anyone else—that the quantum state must represent incomplete information. And where we have come to nowadays—particularly in quantum informational approaches to the foundations—is in trying to explain why the information cannot be completed. But I just haven't found a clever way to tie that in with the photoelectric effect. So, I better desist at this point; I understand your deadlines.

17-11-04 *Bayes, Bennett, and Bull* (to M. Pérez-Suárez)

Sorry to hear you're not quite happy there. Just keep in mind the crowd you're hanging out with and learn what you can with regard to their specialties. There'll always be time to Bayesianize at other meetings.

On your Smolin query, it might well be John. He told me that he's going back to Cambridge for a week in December. Also it sounds like Charlie Bennett is back in town now. You'll find that he's not very philosophical (in fact he tries to be anti-philosophical), nor is he very sophisticated (he tries to be anti-sophisticated), but he can be great fun. And I venture to say he's probably the greatest genius (if there can be an overall mark for such things) in the whole city.

I hope things are on course for you to spend some time in Albuquerque.

01-12-04 *BBQW Responses — Progress Report, as I near the Pacific* (to S. Hartmann, C. M. Caves & R. Schack)

Friends in Barbecue,

Below is how it stands, just as I am poised for my jump over the Pacific to Sydney. We've had a couple more replies, and at least one E has turned into a Y. I think Carl was going to write Nielsen again; and Stephan was going to write Uffink. We can probably write off Fine and Werner at this stage, since I've written them secondary notes and still gotten no response.

Also, see the note from Milburn below. It looks like he will contact Stephan with some more funding ideas. Carl, is there something you're pursuing that I should help with?

To answer Stephan's question, I think the PSA session on quantum information and foundations went very well. Or at least I think so. The biggest bright eyes in the audience were Bas van Fraassen, Harald Atmanspacher, Steve Savitt, Jossi Berkovitz, someone named Antigone (prof in Minnesota?), John Norton, and maybe Michael Dickson. Berkovitz and Antigone were particularly interested in the quantum de Finetti theorem. In addition to that, though he wasn't at the session, I met Alan Hájek at the conference. He spoke at the Richard Jeffrey memorial session (along with Brian Skyrms, Lyle Zynda, and Persi Diaconis), and I got a pretty good impression of him. Afterward, I fielded the question of whether he would be interested in BBQW, and he was actually very enthusiastic. I think now I'm inclined to say he would be my top pick for the Fitelson spot. But why don't we invite both Hájek and Howson? (Also, does anyone know Urbach? What's he like?)

Beyond that, I wonder whether we might consider Diaconis? I got one heck of a good impression from his talk. On top of that, it's clear that he's got technical skills like no other: Skyrms may talk "radical probabilism", but Diaconis calculates "radical probabilism"! And I do think we could use some representation of that. Diaconis gave the impression that he may be particularly interested in foundations at this point in his life.

Are things in good shape for Stephan to apply to the VW foundation? Or should we send out the second round of invitations first. I could do that from Sydney this week: I'm hoping to get a lot of work done from there in the wee hours of the morning. We'd just need to decide upon who's next.

That's all I can think of at the moment.

04-12-04 *From Australia* (to J. Bub)

I'm sorry I forgot to give you the recommended reading list before I got involved with my brother's visit to NJ and then had to scuttle off to Australia. (I'm visiting Huw Price.) Here's what I can give you at the moment as a "must read":

B. de Finetti, "Probabilism," Erkenntnis **31**, 169–223 (1989).

It seems way off the mark in the first two sections (seemingly portraying that de Finetti is almost a solipsist ... maybe something got really screwed up in the translation), but it soon recovers after that to become what I would say is THE most powerful article on Bayesian probability I have ever read. It was the article that finally convinced me of the nonfactivity of all probability distributions.

Now, it took me four separate readings of the article over six years, and a lot of rumination in between, to get to that point. But with the hindsight of that experience, I hope I'm in a much better position to help you guys slog through it more effectively than I did.

You might also want to read Richard Jeffrey's accompanying article

> R. Jeffrey, "Reading *Probabilismo*," Erkenntnis **31**, 225–237 (1989).

to help set the historical context.

I also want to suggest to you some pages from Bernardo and Smith's book,

> J. M. Bernardo and A. F. M. Smith, *Bayesian Theory*, (Wiley, New York, 1994).

but it'll have to wait till I get back home Wednesday before I can suggest anything specific to you.

How does that sound? Is that enough to get you started until I can get back to you Wednesday or Thursday.

08-12-04 *BBQW Topics — Just Some Ideas* (to the BBQW Boys)

Here are some other topics for your VW Foundation proposal for our meeting, and more generally, for the conference itself. I guess I'll also send this to Gerard Milburn in case he might need it for his own funding efforts.

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1. Could the Everett Interpretation of quantum mechanics really give rise to a Bayesian derivation and interpretation of the quantum probability rule? (Wallace, Uffink, Barnum, ... and Greaves if we sneak her in as a discussant)

2. Can or cannot one contemplate doing quantum cosmology with a Bayesian interpretation of quantum states? Assuming one can, what does Bayesianism add for helping to solve existent technical problems? (Hartle, Unruh, Brun, Caves, ... and maybe Srednicki.)

3. How does a Bayesian interpretation of the quantum state impact information security in the sense of quantum cryptography? Is there a serious difference between subjective and objective interpretations of the quantum state in this regard? Does one really need an objective notion of randomness to make the idea of a quantum random number generator fly? Even if one will not buy into the idea of a Bayesian interpretation of the quantum state, will Bayesian techniques for standard statistical practice nevertheless make a difference for security analyses? (Lütkenhaus, Gisin, Schack, ... and maybe Renner and Renes.)

4. What does objective chance in the sense of David Lewis's "principal principle" or a propensity interpretation (as in Giere's early paper or as a special case of Popper's) add to the interpretational issues of quantum mechanics? Or does an objective interpretation of probability only impede quantum foundational progress? (Myrvold, Shimony, Butterfield, Gisin, Howson – pro objective chance; Appleby, Fuchs, Schack, Caves – con objective chance)

5. Quantum computing. How might quantum computational algorithms be understood as Bayesian inference engines? Do any of the known quantum computational models (unitary-gate implementations, measurement-based models, or adiabatic models) favor or disfavor a Bayesian interpretation of quantum states and operations? How to pose some known quantum computational algorithms as Bayesian updating questions? (Nielsen, Briegel, Cirac, Wiseman, Caves, Milburn, Barnum, Mermin, . . .)

6. What would Bohr think? How close is a Bayesian interpretation of quantum states *really* to anything Copenhagen like? Where is a Bayesian interpretation less radical and anthropocentric than the Copenhagen interpretations (in the Bohr, Pauli, and Heisenberg varieties)? Where is it more? (Folse, Fuchs, Mermin, and Zeilinger)

7. Bayesian interpretations of the quantum state tend to suggest novel quantum measurements to use in their representations (for instance, the symmetric informationally complete positive-operator-valued measures and complete sets of mutually unbiased measures). They also suggest interesting quantum-state and operation tomography questions (like the pure-state informationally (PSI) complete measurements and the de Finetti representation theorems). It would be nice to get some technical reports on these issues. (Appleby, Caves, Schack, . . . and Diaconis, Renner, and Wootters.)

8. To what extent does quantum mechanics come about solely through information-theoretic constraints? To what extent is it not much more than a theory of missing information? Bub-Clifton-Halvorson theorem; Spekkens toy models; Smolin objections. Is a further ontology underlying the information-theoretic constraints needed or not? Can Smolin really give an Everett interpretation to Spekkens's toy model? (Bub, Halvorson, Spekkens, Smolin, Poulin . . .)

9. Is quantum mechanics an extension of (Bayesian) probability theory? For instance, through C^*-algebras or effect algebras. Or is it rather a particular application with some special assumptions? For instance, might it be little more than a theory of priors? Or is it some combination of those two ideas? (Bacciagaluppi, Busch, Hardy, Fuchs, Barnum)

10. How well does quantum mechanics mesh with the "radical probabilism" of de Finetti, Richard Jeffrey, and Brian Skyrms? Does it require radical probabilism for its understanding? Does the natural noncommutivity that arises in more general kinds of conditioning (like Jeffrey conditionalization) have anything to do with quantum mechanical noncommutivity? (Hájek, Hartmann, Schack, Fuchs, . . . and Diaconis)

11. Quantum Bayesianism and wider philosophical issues, for instance from the philosophy of language to antirepresentationalism (i.e., the sorts of things in later Wittgenstein, Rorty, Davidson, later Putnam, etc.) How do the views of Fuchs and Schack on quantum certainty mesh with Wittgenstein's book *On Certainty*? In what ways is it useful to recognize the "non-factivity" (Timpson's term) of the subjective Bayesian's view of quantum states? (Timpson, Price, Fuchs, Appleby, Schack)

12. Quantum versions of foundational Bayesian arguments: Like Dutch book (synchronic and diachronic), Cox, de Finetti representations, decision theoretic axioms, etc. (Pitowsky, Uffink, Caves, Schack, Fuchs, Poulin, Brun, Barnum, Wallace)

13. . . . And I'm sure I forgot a ton of other interesting potential topics at this sitting. But I think this is enough for now, and 13 is an unlucky number anyway.

09-12-04 *Looking Forward* (to G. Bacciagaluppi)

Now for other, less urgent business.

Bacciagaluppism 1: *I have been thinking further about "the idea": I think it can make precise in what sense the measurement problem becomes more benign. I shall try to write down a few pages and send them to you.*

I'll look forward to seeing that.

On a somewhat related matter, I went to Hans Halvorson's talk at Princeton last night and came away very disappointed and self-reflective. The disappointment (this time) was not with myself but with Halvorson. I guess I put myself up for a let down: You see, Halvorson titled his talk "No Reductive Physicalism, No Measurement Problem"—a title that I could easily imagine myself writing to try to convey some of my most beloved ideas about quantum mechanics. To top it off, Hans had told me this in one of his emails:

Halvorsonism 4: *This project was motivated by your overall program (which, if I have interpreted it correctly, rejects the idea that there is a "measurement problem"). But I've added a bit of gloss that connects this idea with recent debates in philosophy of mind. (Debates about how to formulate "physicalism", etc.)*

So, I thought, "Great! He's going to make precise and much more convincing all this stuff I've been muttering about for the last couple of years." (For instance, I was quite pleased with the fine points of our program that Chris Timpson made clearer at the end of his thesis, where he points out that it is crucial for us that quantum states be "nonfactive." That seemed a worthwhile "gloss" that may help in several ways.) Thus, it was quite a shock to me when I saw that what Hans was talking about is diametrically opposed to almost everything I think of QM. His quantum states are still *ontic* states, and (physical!) collapse comes about from some mysterious interaction between (ontic) brain states and (epistemic? ephemeral? Platonic? in any case, not ontic!) mental states. It was almost like seeing the old von Neumann solution all over again: The measurement chain stops at consciousness. I.e., it's all about that mysterious last step. It was awful.

I motivated that? That's what disturbed me. One of the most hotshot of the hotshot young philosophers could read me no more carefully than that? Am I that absolutely unclear? It's easy for me to see how someone could disagree with me on quantum foundations if they've understood but reject the Bayesian point of view—and that's fair enough—but what I can't understand is how when I strive so hard to be clear, I can still be continually misread. Especially from someone like Hans, whose recent work with Bub might have indicated that he would have at least a nominal interest in trying to get these ideas straight.

The only thing I can think is that a large class of philosophers really don't take me seriously, or at least beyond a little lip service. Halvorson put no gloss on my program; he muddled it with some pretty bad (and certainly completely independent) ideas.

Thus I really am quite thankful for the clarity you help give me. With you, I've never had the impression that you misunderstand what I'm aiming for. On the contrary, I often feel quit dumb around you, feeling that your understanding (of my own program) far outstrips mine.

But all of this is just to repeat in a long-winded way what I said in the first line about the note you're going to send me: "I'll look forward to seeing that."

In what sense does the measurement problem become more benign? You know what I've been aiming for: A way to make convincing that quantum-state updating is no more (or if you rather, no less) philosophically interesting that the process of conditionalizing a probability. It is just technically different than usual conditionalization. The extra unitary rotations are not mysterious physical actions but simply different rules for conditionalizing. Thus the technical problem becomes trying to find a way (or modification to standard Bayesian arguments) to justify why *this* conditionalization rule rather than *that* one. But the overall point: One should *only* find quantum state change mysterious if one also finds standard Bayesian conditionalizing in need of some kind of further metaphysical explanation—i.e., if one deems it important to find a literal or mechanistic explanation for how facts change beliefs.

Also, you know that that's only half the coin for me. What I think *is* philosophically interesting is that the character of the event space for our probability functions changes in going to quantum mechanics: I.e., we shouldn't—from my view—think of the h's in a $P(h)$ as signifying unknown but pre-existent properties. Rather they are values that come about from our actions upon a physical system. All I'm trying to do is cleanly isolate what I view as two very distinct issues: Change of the conditionalizing rule? Ultimately not so interesting. And, to make use of your phrase, a kind of benign change from our old view. Change of the character of the event space from pre-existent properties to the results of an agent's actions? That—my intuition tells me—is where the really good stuff is!

But I blabber, blabber. At least it's made me feel better to write all this down.

10-12-04 *The Further Reading* (to J. Bub)

I had promised to send a further list of things to read upon my return from Australia. Here they are, I hope it's not too late.

From Jose M. Bernardo and Adrian F. M. Smith's book *Bayesian Theory* (Wiley, 1994):

1) Sections 1.1, 1.2, and 1.3.

2) The subsection titled "Subjectivity of Probability" on pages 99–102.

3) Section 4.8 to page 237.

Also I think Marcus Appleby's paper "Probabilities Are Single-Case, or Nothing" (`quant-ph/0408058`) could be quite helpful. Particularly, I like the argument of Section 3.

My plan for your seminar is mostly to have a free roaming discussion about the contents of these readings. Is that OK with you? Or did you have something else in mind?

13-12-04 *The Beast* (to S. Aaronson)

Aaronsonism 8: *Hope everything's well with you. Yep, my own big 400+ page "samizdat" is lumbering toward completion over the next couple days ...*

[Reference Scott's Thesis]

Would you mind reading it through by tomorrow night and sending me detailed comments? Ha ha, just kidding. But you're mentioned in the acknowledgments, and the prologue ("Aren't You Worried That Quantum Computing Won't Pan Out?") is intended to amuse. I'd love to hear your thoughts.

I loved it! And you have helped me immensely with your Chapter 1: Soon after the new year, I've got to construct a talk to give to Jeff Jaffe (the president of Bell Labs research) and Dave Bishop (the new VP over physical-science research) on why Bell Labs should imagine spending even a cent on quantum foundations research. Among other things, I have wanted to argue that it has been the foundational mysteries all along that have motivated so much of technical quantum computing and quantum information. And that these guys who want to suck off the governmental tit spurred by this field shouldn't forget its very roots! I.e., I'm going to partly lash out at the guys who call what I'm interested in "Sunday physics" but nevertheless (have to) cite my papers in their (self-proclaimed) "hard core" physics papers.

Anyway, I was going to quote liberally from Feynman, Deutsch, early Bennett and Brassard, Wootters, and the like. But now I can also see that I'll be quoting liberally from Aaronson! Even if you never prove another great theorem again, you're turning into a great writer.

BTW, I hope you will post your thesis on `quant-ph`. But before you do, compactify it with single-space, etc. Save the trees man!

I've still got to come visit you in Princeton. I'll try to do that soon after the new year too.

14-12-04 *Thoughts on Non-nonlocality* (to H. Price)

Finally coming back to your request for what to read within my writings on how a Bayesian conception of quantum states takes care of (or should take care of) issues to do with nonlocality in QM. Let me suggest you read Section 3, "Why Information?", and the beginning part of Section 4, "Information About What?", in my paper "QM as Quantum Information (and only a little more)", `quant-ph/0205039`. In that regard, let me also paste in below some correspondence I had with David Mermin and Jerry Finkelstein on those sections—I think they clarify things a bit further—and let me also attach an abridged version of the same paper that I never posted on the archive. In that version, I changed the "Why Information?" section somewhat to better reflect what I told Mermin below.

The key issue is, what do I mean when I say that nothing physically changes on Bob's side when Alice performs a measurement on her half of an entangled pair? In that regard, I think you might also get something out of my discussions on pages 175–176 and 184–189 of my samizdat *Quantum States: What the Hell Are They?*. These discussions were motivated by some standard dictionary (can't remember which) wanting to include a definition of quantum teleportation in its latest revision, and Bennett supporting a definition that said something like, "The transference of one particle's properties onto another particle via the assistance of . . . " The thing I objected to strenuously was the idea that any properties at all were transferred. Also, let me point you to some discussion on the same point in *Notes on a Paulian Idea*. See pages 465–466, starting at the words "Quote from Draft," and see pages 467–471 in the note titled, "Detailed Commentary." Getting a description of quantum teleportation that Asher and I could both agree on for the paper we were writing was a point of serious contention for us. And I went at great lengths to try to get my point across to him. It's exactly the same point that's relevant to our own discussion (i.e., Fuchs and Price).

I guess you could say the overarching idea is "no surprises, no reality." That is an idea I flesh out in pretty good detail in my paper "The Anti-Växjö Interpretation of Quantum Mechanics." See sections 4 and 5. (They are completely independent of the rest of the paper; so don't read any of the rest of the paper.) I think (I hope) actually, that you'll find the discussion there interesting independently of any issues to do with our discussion of nonlocality. In particular, if you read those sections, I don't think you could fail to give me the credentials of a "subject naturalist." Am I right?

Let's see, anything more? One last thing: Let me also suggest Chapter 8 of Chris Timpson's PhD thesis *Quantum Information Theory and the Foundations of Quantum Mechanics*, `quant-ph/0412063`, and also the "Envoi" at the very end. He does quite a nice job of explaining how the "nonfactivity" of our quantum states buys us the ability to stop talking about nonlocality.

There, that's one thing marked off my to-do list for you. (Certainly there's no rush to respond; I know you're traveling. Just archive this note until you have the time ... even if that's six months from now.)

14-12-04 *Backward Causation* (to C. H. Bennett and B. W. Schumacher)

Did you guys (or either of you guys) ever write anything up on the stuff that got you so excited in Japan a couple of years ago?: I.e., the stuff on time travel via quantum teleportation? If you haven't written anything up, do you have some PowerPoint presentations (or such) that you wouldn't mind sharing with me? (I know that some exist because I've seen you talk on this before.) I've been discussing this stuff with Huw Price, an Australian philosopher who is quite interested in exploring ideas about backward causation in the context of QM, and I've told him about your stuff. Price is quite a good guy, and I want to make sure I've got the story straight with all this stuff. Would you mind if I also share your slides with Huw in the course of our discussion? I don't see how it could hurt (since I promise he's a nice guy), but I understand you might have some objection if you haven't published this yet.

Anyway, the way I pick on all three of you (i.e., you two and Huw) is that I think you do something dangerous with a simple "chain of inference" by mystifying it with all this forward and backward time-travel talk. But now to compliment you: I think your Japan examples are great for illustrating that!

14-12-04 *Directions, Etc.* (to J. Bub)

Bubism 3: *Unfortunately, the only copy of Bernardo and Smith is out of the library, so probably no one will have read that. A free roaming discussion is fine, I think, but you should probably be prepared to talk a bit in general first (or perhaps second) about what specific foundational issues in quantum mechanics push one towards Bayesianism, and perhaps also about information in physics, since this is the topic of the seminar. Does that sound OK?*

I'll try.

But most importantly, you gotta realize that, concerning

> "what specific foundational issues in quantum mechanics push one towards Bayesianism,"

it's the other way around. It's clear thinking about probability and the use of it that leads to Bayesianism—nothing to do with quantum mechanics. *Then*, recognizing that a quantum state is (mathematically and conceptually) *nothing more than* a compendium of probabilities, one gets to the Bayesian conception of quantum states. In other words, it is Bayesianism that carries a hope of clarifying quantum foundations—not that something in quantum mechanics specifically pushes toward Bayesianism.

I'll try to see if I can get Bernardo and Smith scanned into my computer and turned into a text file. There's some chance that that might happen tonight.

15-12-04 *Favorite Bernardo and Smith Quotes* (to J. Bub)

Well, try as I might, I couldn't get my scanner connected up last night. It always takes much more time to install these things than one expects! Very frustrating. It just confirms what I've been thinking: I never want to go through another move again. *I never ever want to move again!* Moves just disrupt everything in life, and never for the better.

In light of last night's failure, let me do this as a stopgap. Here are two of my favorite Bernardo and Smith quotes that I already have in my computer. Chew upon them, because I think they especially express the flavor of the reconception of quantum mechanics I'm shooting for: That quantum mechanics is a *normative* theory of personal behavior (or uncertainty accounting) IN LIGHT OF the peculiar, particular world in which we are immersed. Different world, different normative theory for agents immersed within it. Moreover, because of the last point, though quantum theory is a theory about agents (rather than of objective reality per se), one can still hope to glean some hypotheses about objective reality itself from it. One just has to do so obliquely. But that's our task, and that's what we're setting ourselves up to do.

I'll try to emphasize these things in a preamble to our discussion tomorrow night. But if you don't mind, distribute this note to your students first so that it'll have had at least a little time to percolate in their minds.

15-12-04 *And Just a Little More To Read* (to J. Bub)

I know you guys have already read this stuff, but maybe it'd be worthwhile going over it again before my arrival: Namely Chapter 8 of Timpson's thesis (paying attention to the issues surrounding Footnote 4) and his "Envoi" at the very end. I think he does good justice to what (and why) we Bayesians are up to. It's all about breaking the "factivity" of quantum states.

15-12-04 *And Just a Little More To Read, 2* (to J. Bub)

Bubism 4: *Re Timpson, I'd be interested in your comments on his footnote on p. 181: 'Although he does not himself put it in these terms, Fuchs' awareness of the factivity of the terms "knowledge" and "information" and his related criticism of Mermin, mark the change from the objective Bayesianism of Fuchs (2001) to the more consistent subjectively Bayesian position of Fuchs (2002a).'*

Plan to. But also, it might be useful for you to look over the pages he cites from my "Quantum States: W.H.A.T.?" (namely, 19–25 and 42–51). I think I might read 'em too.

16-12-04 *Troubles of the Slusher Kind* (to R. E. Slusher)

I'm taking off to Maryland now to give a seminar to Jeff Bub's quantum foundations group at UMD. But I've been troubled all night about how I can get us past our point of disagreement from yesterday: I.e., to try to find a way to convince you that we Bayesians are not anthropomorphizing quantum mechanics in an unnecessary way. And to try to find a way to convince you that quantum mechanics is not like Newtonian mechanics, even in the broadest of all senses. I.e., to convince you that Newtonian mechanics is a theory of systems (as they are by themselves), and quantum mechanics is rightly a theory of observers (and only observers), not a theory of systems.

I intend to convince you of that—so you're good exercise for me. That'll be my new year's present. In the meantime, have you read Section 4 of my `quant-ph/0204146`, "The Anti-Växjö Interpretation of Quantum Mechanics"? It addresses the issue at least in a way that turned Preskill's head.

16-12-04 *The Doctrine of Preemption* (to C. H. Bennett)

Bennettism 22: *Reading the end of your letter I find the usual backhanded compliment: Presumably the last word "that" refers to the dangerousness of forward and backward time travel talk. I seem to have spooked you so much with my loose talk that now you are even afraid of forward time travel, which you never used to be.*

OK, you got me … in fact, preempted me. I loved the lines near the end of your presentation:

Q. Is it time travel?

A. It depends on what your definition of "is" is.

It was as if you had already heard my snide remark of 2004 in 2002. Had you?

I'm glad to hear the Newton meeting is almost over. I'm looking forward to having you back in the neighborhood. If you've got the time, I'd love to start dropping in from time to time sometime in January. In particular, at least at the outset, if I could interest you guys in the SIC-POVM problem (i.e., do they always exist?, and if they do, how are they constructed?, what can you do with them?, etc.) it'd be great. I need someone(s) to bounce ideas off of, and I'm awfully lonely here. I keep figuring there must be some simple solution, and that one really doesn't need to go hog-wild with abstract mathematics to get at it. It probably just needs some experimental mathematics like the Smolin minimizer could give, a little Bennettian playfulness with ideas, and a Fuchsian smile. (You've long known that I'm attracted to quantum mechanics because deep inside I think I can charm things into existence … and QM is the one scientific thing that gives me hope that there's no pre-existent.)

And thanks for the pictures!

20-12-04 *The Doctrine of Preemption, 2* (to C. H. Bennett)

Bennettism 23: *What's a SIC-POVM? Is it an actual POVM or a misspelled unwell POVM?*

It stands for "symmetric informationally-complete", and yes, the pun is intended. They're annoying critters. Take the space of operators over a Hilbert space of dimension d; it is a vector space of dimension d^2. Now ask, can one find a complete *orthogonal* basis A_i on that space consisting solely of positive, rank-one operators? The answer is no. So, ask the next best thing. If one cannot satisfy $(A_i, A_j) = 0$ for $i \neq j$, can one at least satisfy $(A_i, A_j) =$ constant for $i \neq j$? The answer *appears* to be yes, and sets of such operators that sum to the identity are dubbed SIC-POVMs. So, it's a really very elementary question … but it seems to require something clever for an actual proof.

Bennettism 24: *My "It depends on what your definition of 'is' is," was meant to be a quote from Bill Clinton, not a time-traveled reaction to you.*

I understood that. But in a world with backward causation, are you so sure you can draw such distinctions?

Have a safe trip home.

21-12-04 *Know Any Young German Bayesians?* (to P. Busch and H. J. Briegel)

I should apologize for my provocative title! But I wanted to get your attention!

First off, let me give you both an update on how our meeting in Konstanz is going. I'll do that by pasting two notes below. One lists the responses we've gotten so far, and one describes various suggested topics for the meeting. I know I don't need to clench your interest since you've both already said that you would come, but I figure it can't hurt to try to stoke the enthusiasm.

Now for the real purpose of my note. Soon I'll be sending out a second round of invitations, and I would like to ask your advice. In total, we're hoping for about 36 participants (excluding the students we'll probably slip in). Thus we've got room for maybe 10 more invitees. Here's the question: Can either of you suggest any good young Germans who might be a good fit for the subject matter of the conference? You can also suggest some older Germans too!

The issue is that it seems the VW Foundation has some interest in funding a substantial part (if not all) of the meeting, but they want an increased emphasis on 1) younger researchers (i.e., ones who have not yet obtained a permanent position) and 2) Germans. So, since there's some room anyway in our ranks, I don't mind trying to accommodate their wishes if I can . . . especially if I can do it while still having the meeting of my dreams.

Thus if you have some suggestions, they would be most useful. Who am I overlooking from the German quantum information, quantum foundations, or philosophy communities who could really add something to our meeting? There's no promise that we'll add their names to the roster, but we'll definitely take anything you say seriously.

Hope to hear back from you.

31-12-04 *Dylan and the New Year* (to J. B. Lentz & S. J. Lentz)

Well, I got a little time this morning to myself while the girls were out shopping, and I finally got a chance to finish reading Bob Dylan's *Chronicles*. I had wanted to do that before the new year started up. Thank you two so much for sending it. Ever since reading these lines in the Washington Post,

> The hype seems enough to turn the most faithful acolytes into unbelievers. How can it possibly be true, as the magazine claims, that this week's celebrity memoirist is "the most influential cultural figure now alive"?
>
> Oh, you thought he was once, back in the '60s. The image of that faraway decade has now been so magnified by myth and distorted by culture-war invective that you have to strain to recall what it felt like to be alive then, but you do remember this: Half the people you knew believed that if only they could figure out what Bob Dylan was saying, the secrets of the universe would be revealed.

I had wanted to sink my teeth into the book. That last line, especially, was something that caught me. I've been through that so many times myself: from listening to John Lennon to reading John Wheeler to listening to Paul Simon to reading William James to listening to John Coltrane . . . . And even now, to reading Bob Dylan: I knew that's how I'd tackle the book if I got my hands on it. The secrets of the universe are in the end, at the high level, not the low level: Quarks and electrons are ways that we can focus our attention so that we can distill simple truths here and there, but they're not what we're "made of". Sometimes I think we're made of poetry, if anything. Or, at least, that's closer to the right idea.

Anyway, it was a great book. I enjoyed whole swaths of it and took a lot of notes.

BTW, there's a little good news on my side with respect to these same musings (on poetry and such) that I don't think I've told you about before: I learned a few weeks back that the *American Heritage Dictionary of American Quotations* is going through a revision (it'll soon be the *Oxford Dictionary of American Quotations*), and they want to include a couple of lines from some of my writing. It's very flattering to think that my little poetry could be in the same book with William James's and Bob Dylan's very big poetry, but mostly it's a statement about raw luck—the good kind for me and the bad kind for them. Still, with everything one writes, whether it is preserved by luck or not, as long as it is preserved, it's a way of living a little a longer. And the idea of that always feels good.

Good luck to yourselves in this new year, and many happy returns to both of you.

31-12-04 *New Year's Eve* (to A. Peres)

Dear Asher,

As I write you these words, it is indeed New Year's Eve, but the title of the note is meant to be read more metaphorically. It has been a very long time since I have written you. I hope your recovery from your stroke is coming along as well or even much better than you had indicated in one of your notes. Maybe even everything is back to normal by now? Or at least I'd like to dream that.

I don't know where I left off in my conversation with you. I guess you know that I did not end up getting an offer for a faculty position, either in Copenhagen or Vancouver. Nor did the Mabuchi/Preskill ploy of trying to create an interdepartmental position (physics and philosophy) for me at Caltech work out either. Thus, with no place to go, I had to come back to Bell Labs. Of course, far worse things could have happened—I look at what happened to you, and I know how selfish I am—but I fell into a fairly deep, dark depression for over four months. An amazingly illogical depression. I've done enough to survive my wife (like buying us a house and getting the family moved into it), and I've done enough to survive Bell Labs (speaking when I needed to speak and doing the administrative things required of me), but I've done little beyond that. The depression has been much worse than even after our fire: At least after the fire I kept writing and doing science. But even my writing had come to a standstill this time.

For instance, after those very nasty problems with our email server—you probably don't remember, but I told you how reams of my email had been randomly eaten up for some undetermined time on the order of two months—I just used the opportunity to effectively slip out of the email ether, and very few have heard from me in these months.

Anyway, I'm writing you now because something has been jarred loose, and my self-esteem seems to have started to come back. Or maybe my body just got weary of being depressed. I'm not sure of the source of the optimism, but I'm now finding myself looking forward to an intense new year—intense with work and intense with thought. And it's New Year's Eve!

... All of that just so I could say "Happy New Year!" to you in clear conscience. I wish you and Aviva and the whole family all the best.

On another note—perhaps some welcome news for you—I had never written you about the completion of your festschrift in my period of silence ... but at least the festschrift itself went on. All the papers have been with *Foundations of Physics* for some months now. They're a fine lot of 26 papers (including your autobiography). I'll put the full list below. vdM has most recently told me that they're next in queue after the Cushing festschrift (which itself got delayed for some reason or other), and they should start appearing early this new year.

Expect more letters from me this year! I'll be back soon.

All the best,

Chris

Papers in the Peres Festschrift:

1. H. Bechmann-Pasquinucci, "From Quantum State Targeting to Bell Inequalities"
2. Jacob D. Bekenstein, "How does the entropy/information bound work?"
3. Paul Benioff, "Towards a Coherent Theory of Physics and Mathematics: The Theory – Experiment Connection"
4. Robin Blume-Kohout and Wojciech H. Zurek, "Redundancy of Information Storage in Multiqubit Universes"
5. Gilles Brassard, Anne Broadbent, and Alain Tapp, "Quantum Pseudo-Telepathy"
6. Časlav Brukner, Markus Aspelmeyer, and Anton Zeilinger, "Complementarity and Information in 'Delayed-choice for entanglement swapping' "
7. Dagmar Bruss and Chiara Macchiavello, "How the First Partial Transpose Was Written"
8. Adán Cabello, "Bell's theorem without inequalities and without unspeakable information"
9. S. Deser, "A Note on Stress-Tensors, Conservation and Equations of Motion"
10. Bernard d'Espagnat, "Consciousness and the Wigner's Friend Problem"
11. David P. DiVincenzo and Barbara M. Terhal, "Fermionic Linear Optics Revisited"
12. Steven T. Flammia, Andrew Silberfarb, and Carlton M. Caves, "Minimal Informationally Complete Measurements for Pure States"
13. Friedrich W. Hehl and Yuri N. Obukhov, "To consider the electromagnetic field as fundamental, and the metric only as a subsidiary field"
14. Karol Horodecki, Michał Horodecki, Pawel Horodecki, and Jonathan Oppenheim, "Information theories with adversaries, intrinsic information, and entanglement"
15. Michał Horodecki, Ryszard Horodecki, Aditi Sen(De), and Ujjwal Sen, "Common origin of no-cloning and no-deleting principles – Conservation of information"
16. Elena R. Loubenets, " 'Local Realism', Bell's Theorem and Quantum 'Locally Realistic' Inequalities"
17. N. David Mermin, "What's wrong with this criticism"
18. Asher Peres, "I am the cat who walks by himself"
19. Murray Peshkin, "Spin-Zero Particles Must Be Bosons: A New Proof within Nonrelativistic Quantum Mechanics"
20. Arkady Plotnitsky, "On the Precise Definition of Quantum Variables and the Relationships between Mathematics and Physics in Quantum Theory"

21. Abner Shimony, "An Analysis of Stapp's 'A Bell-type theorem without hidden variables'"
22. Henry P. Stapp, "Comments on Shimony's Analysis"
23. Anthony Sudbery and Jason Szulc, "Compatibility of subsystem states"
24. Daniel R. Terno, "Inconsistency of a quantum-classical dynamics, and what it implies"
25. William K. Wootters, "Quantum Measurements and Finite Geometry"
26. Mario Ziman, Martin Plesch, and Vladimir Buzek, "Reconstruction of superoperators from incomplete measurements"

Asher Peres died 1 January 2005.

01-01-05 *Our Dear Asher* (to D. R. Terno)

That is very sad news indeed. I feel knocked over. Asher was a great man—among the greatest I have ever known and ever read. I owe him my whole career in fact—from the things I learned from him to the way he supported me to the ways he lifted my life by taking me seriously like few others. He was one of the few people who actually read my samizdat, for instance, and I cherished his opinion. I feel that I betrayed him greatly with my comparatively petty problems of the last few months—they kept me silent when maybe he needed me most. Below is the letter I wrote him just yesterday, explaining what was up—finally breaking my long silence. I am sure he never saw it. I am very sad.

I think that you should send an announcement to the quantum information community, much like the one I sent when Jaynes, Landauer, and Lewis each passed away. If it would be useful to you, I could send you my email address book with maybe 300 or 400 quantum-information/foundations names in it.

I will call Aviva tomorrow when the time is more proper in Haifa. She is a gem, and I hope that she will keep shining.

Asher was a great man, a great friend, a great scientist.

Announcement I Sent Out Broadly 3 January 2005 on Behalf of the Authors

Quantum information science lost one of its founding fathers.

Asher Peres died on Sunday, January 1, 2005. He was 70 years old.

A distinguished professor at the Department of Physics, Technion – Israel Institute of Technology, Asher described himself as "the cat who walks by himself". His well-known independence in thought and research is the best demonstration of this attitude. Asher will be missed by all of us not only as a great scientist but especially as a wonderful person. He was a surprisingly warm and unpretentious man of stubborn integrity, with old-world grace and a pungent sense of humor. He was a loving husband to his wife Aviva, a father to his two daughters Lydia and Naomi, and a proud grandfather of six. Asher was a demanding but inspiring teacher. Many physicists considered him not only a valued colleague but also a dear friend and a mentor.

Asher's scientific work is too vast to review, while its highlights are well-known. One of the six fathers of quantum teleportation, he made fundamental contributions to the definition and characterization of quantum entanglement, helping to promote it from the realm of philosophy to the world of physics. The importance of his contributions to other research areas cannot be overestimated. Starting his career as a graduate student of Nathan Rosen, he established the physicality of gravitational waves and provided a textbook example of a strong gravitational wave with his PP-wave. Asher was also able to point out some of the signatures of quantum chaos, paving the way to many more developments. All of these contributions are marked by a surprising simplicity and unbeatable originality.

Of all his publications, Asher was most proud of his book *Quantum Theory: Concepts and Methods*. The book is an example of Asher's scientific style: an uncompromising and deep understanding of the fundamental issues expressed in a form which is as simple and accessible as possible. It took Asher six years to carefully weave the threads of his book together. The great quality of the work is acknowledged by anyone acquainted with the final result.

In a favorite anecdote, Asher told about a reporter who had interviewed him on quantum teleportation. "Can you teleport only the body, or also the spirit?" the reporter had asked. "Only the spirit," was Asher's reply. Our community has been privileged to know him and have been touched by his spirit.

"I am the cat who walks by himself" is the title of a charming twelve-page autobiography covering his life from his birth in the village Beaulieu-sur-Dordogne in France until his meeting with Aviva on a train to Haifa [`physics/0404085`]. The rest of his story is in his formal CV.

Netanel Lindner, Petra Scudo, Danny Terno

01-01-05 *A Backup for Us Both* (to J. W. Nicholson)

A Time to Mourn
(From *New York Times*, 1 January 2005)
by David Brooks

I have this week's front pages arrayed on the desk around me. There's a picture of dead children lined up on a floor while a mother wails. There's a picture of a man on the beach holding his dead son's hand to his forehead. There are others, each as wrenching as the last.

Human beings have always told stories to explain deluges such as this. Most cultures have deep at their core a flood myth in which the great bulk of humanity is destroyed and a few are left to repopulate and repurify the human race. In most of these stories, God is meting out retribution, punishing those who have strayed from his path. The flood starts a new history, which will be on a higher plane than the old.

Nowadays we find these kinds of explanations repugnant. It is repugnant to imply that the people who suffer from natural disasters somehow deserve their fate. And yet for all the callousness of those tales, they did at least put human beings at the center of history.

In those old flood myths, things happened because human beings behaved in certain ways; their morality was tied to their destiny. Stories of a wrathful God implied that at least there was an active God, who had some plan for the human race. At the end of the tribulations there would be salvation.

If you listen to the discussion of the tsunami this past week, you receive the clear impression that the meaning of this event is that there is no meaning. Humans are not the universe's main concern. We're just gnats on the crust of the earth. The earth shrugs and 140,000 gnats die, victims of forces far larger and more permanent than themselves.

Most of the stories that were told and repeated this week were melodramas. One person freakishly survives while another perishes, and there is really no cause for one's good fortune or the other's bad. A baby survives by sitting on a mattress. Others are washed out to sea and then wash back bloated and dead. There is no human agency in these stories, just nature's awful lottery.

The nature we saw this week is different from the nature we tell ourselves about in the natural history museum, at the organic grocery store and on a weekend outing to the national park. This week nature seems amoral and viciously cruel. This week we're reminded that the word "wilderness" derives from the word for willful and uncontrollable.

This catastrophic, genocidal nature is a long way from the benign and rhythmic circle of life in *The Lion King*. It's a long way from the naturalist theology of Thoreau's *Walden* or the writings of John Muir.

The naturalists hold up nature as the spiritual tonic to our vulgar modern world. They urge us to break down the barriers that alienate us from nature. Live simply and imbibe nature's wisdom. "Probably if our lives were more conformed to nature, we should not need to defend ourselves against her heats and colds, but find her our constant nurse and friend, as do plants and quadrupeds," Thoreau wrote.

Nature doesn't seem much like a nurse or friend this week, and when Thoreau goes on to celebrate the savage wildness of nature, he sounds, this week, like a boy who has seen a war movie and thinks he has experienced the glory of combat.

In short, this week images of something dark and unmerciful were thrust onto a culture that is by temperament upbeat and romantic.

In the newspaper essays and television commentaries reflecting upon it all, there would often be some awkward passage as the author tried to conclude with some easy uplift - a little bromide about how wonderfully we all rallied together, and how we are all connected by our common humanity in times of crisis.

The world's generosity has indeed been amazing, but sometimes we use our compassion as a self-enveloping fog to obscure our view of the abyss. Somehow it's wrong to turn this event into a good-news story so we can all feel warm this holiday season. It's wrong to turn it into a story about us, who gave, rather than about them, whose lives were ruined. It's certainly wrong to turn this into yet another petty political spat, as many tried, disgustingly, to do.

This is a moment to feel deeply bad, for the dead and for those of us who have no explanation.

02-01-05 *I Hope You Are OK* (to U. Mohrhoff)

I was reading the news tonight and learned that Pondicherry had significant damage from the tsunami. My knowledge of geography is very bad, and I did not know before that Pondicherry is near the sea. I so hope you are safe. I keep my fingers crossed.

Ulrich's Reply

> I am really touched by your inquiry. We (that is my wife and I) felt the earthquake as we sat down for breakfast (at this distance only a gentle swaying of the ground). We then monitored the news to know the location of the epicenter, and when we learned of the magnitude of the quake, we waited for the tsunami. Pondy town is sufficiently above sea level to have remained undamaged, but fishing communities nearby and some Auroville beach settlements were hard hit. One office near the beach had 20 computers flushed out.
>
> P.S. I still feel that our views on QM have so much in common that we should form a common front against the quantum state realists ... If you can spare 30 minutes, he a look at my PowerPoint presentation "Beyond causality...", which you can be downloaded from my website. (You are even quoted there. Favorably!)

02-01-04 *Omphaloskepsis* (to J. W. Nicholson)

I think you and Sumitra might get a kick out of how this dictionary described my behavior tonight.

> OMPHALOSKEPSIS: Contemplating one's navel as an aid to meditation.
>
> This word seems to be relatively new, at least the Merriam-Webster "Word of the Day" column claims it to have been invented only in the 1920s. It turns up in only a few dictionaries and seems to be a word that survives more for the chance to show off one's erudition than as a real aid to communication. If so, this article is a further perpetuation of its unreal status. It is formed from two Greek words, omphalos, "navel, boss, hub", and skepsis, "the act of looking; enquiry". The former turns up in words such as omphalotomy, "cutting of the umbilical cord", in the related omphalopsychic for one of a group of mystics who practised gazing at the navel as a means of inducing hypnotic reverie, and omphalomancy, an ancient form of divination in which the number of children a woman would bear was determined from counting the knots in her umbilical cord at birth.

03-01-05 *Safety* (to U. Mohrhoff)

I am so relieved to hear that you are safe. I became very worried. Some time good luck happens too (but I've gotten where I don't expect it much anymore).

Now that I know you are safe, let me tell you some sad news that I could have told you yesterday. Asher Peres passed away New Year's Day. It was either a stroke or a heart-attack; it happened very quickly. He was buried yesterday. I will distribute an obituary written by his last three students in a few hours; you are in the distribution list. I'm going to miss him very much.

With regard to your "P.S.", thanks for sending me your webpage link. Indeed for some reason I feel re-energized to try to sort our similarities and differences in point-of-view about QM (and metaphysics). I hope that I can come to a better understanding. I tried downloading the PowerPoint presentation you told me about, but had no luck. Could perhaps you just send it to me as an email attachment? Likely, I will be able to open that. I will also enjoy trying to read some of your papers again.

Ulrich's Reply

I really appreciate your concern. Thanks again.

Although I did not have the good fortune to meet Asher Peres, I am saddened by the news of his passing. I treasure his one (unsolicited) email in which he expressed appreciation for my AJP paper "What quantum mechanics is trying to tell us" (responding to Mermin's "What is quantum mechanics trying to tell us?"). My copy of his *Concepts and Methods* has disintegrated from overuse, and I loved him for his insistence that "there is no interpolating wave function giving the 'state of the system' between measurements". (It's echoing through all my papers.) If I have recently taken to criticizing the paper you coauthored with Asher Peres, it is because the closer one's views are, the larger loom the remaining differences ....

> [Quoting CAF:] ... for some reason I feel re-energized to try to sort our similarities and differences in point-of-view about QM (and metaphysics). I hope that I can come to a better understanding .... I will also enjoy trying to read some of your papers again.

I very much look forward to that. (Let's bear in mind the warning my grandmother gave to my father long before email made matters worse: "Letters don't smile.") My recent relatively short `quant-ph/0412182` (to appear in the Indian journal of physics *PRAMANA*) may be a good starting point. I also would like you to have a look at my class notes, once they are online. The trouble is that they keep changing all the time ... I can see why it took Asher Peres six years to write his book.

03-01-05 *The Alchemical New Year* (to M. Pérez-Suárez)

I apologize for taking so long to write you back; I had wanted to send you a timely New Year's greeting myself. But when I sat down at my computer New Year's Day, I found the news that my mentor, colleague, and friend Asher Peres had died. That led to a lot of sadness and also a lot of email in its own right. You'll be receiving an obituary by Asher's students that I'm helping distribute in a few hours.

Let me come back to our happy alchemical musings though. Your note intrigued me. I'm looking forward to whatever you're going to write up.

In the meantime, let me send you some of my own mumbo-jumbo. This comes from something I had written to a *Scientific American* reporter earlier in the Fall. Tell me if you can see the alchemical strain running through it—I think there's one there. Didn't the alchemists too see the big bang as all around us?

Best wishes for the new year! This year I'm planning to make it quite a productive one (to counterbalance last year).

05-01-05 *Fingers Crossed* (to T. Mor)

Morism 1 : *I was there in the funeral. Did you know he is now buried right next to Nathan Rosen? He bought that area, and even planned carefully that the height of the stone on his grave is a little <u>below</u> Rosen's stone. [This I learnt yesterday, when visiting Aviva and Asher's daughter Lydia.]*

That's a great story; I didn't know that.

07-01-05 *Krugman on Reality (again)* (to me)

From Paul Krugman, "Worse Than Fiction," *New York Times*, 7 January 2005:

> I've been thinking of writing a political novel. It will be a bad novel because there won't be any nuance: the villains won't just espouse an ideology I disagree with – they'll be hypocrites, cranks and scoundrels.
>
> In my bad novel, a famous moralist who demanded national outrage over an affair and writes best-selling books about virtue will turn out to be hiding an expensive gambling habit. A talk radio host who advocates harsh penalties for drug violators will turn out to be hiding his own drug addiction.
>
> In my bad novel, crusaders for moral values will be driven by strange obsessions. One senator's diatribe against gay marriage will link it to "man on dog" sex. Another will rant about the dangers of lesbians in high school bathrooms. [...]
>
> In my bad novel the administration will use the slogan "support the troops" to suppress criticism of its war policy. But it will ignore repeated complaints that the troops lack armor.
>
> The secretary of defense – another "good man," according to the president – won't even bother signing letters to the families of soldiers killed in action.
>
> Last but not least, in my bad novel the president, who portrays himself as the defender of good against evil, will preside over the widespread use of torture.
>
> How did we find ourselves living in a bad novel? It was not ever thus. Hypocrites, cranks and scoundrels have always been with us, on both sides of the aisle. But 9/11 created an environment some liberals summarize with the acronym Iokiyar: it's O.K. if you're a Republican.
>
> The public became unwilling to believe bad things about those who claim to be defending the nation against terrorism. And the hypocrites, cranks and scoundrels of the right, empowered by the public's credulity, have come out in unprecedented force. [...]
>
> Either way, when the Senate confirms Mr. Gonzales, it will mean that Iokiyar remains in effect, that the basic rules of ethics don't apply to people aligned with the ruling party. And reality will continue to be worse than any fiction I could write.

11-01-05 *Our Bayesian QM Meeting — Why You* (to P. Diaconis)

By now I hope you have seen the invitation I sent you last night for our meeting "Being Bayesian in a Quantum World" to be held in Konstanz later this year. In case you are wondering why I sent it to you, I should say that I heard your talk at the PSA meeting this year, and I liked what I saw. In particular, it struck me that you could be a very helpful participant in our meeting with regard to the technical side of things: I.e., helping us distill various philosophical issues into some well-posed mathematical questions ... and to help us see them solved!

So, I very much hope that you can come and will want to come. I think we've made great progress recently in inserting the Bayesian conception of probability into quantum mechanics and quantum information theory. But our effort is still in its infancy, and we need all the help we can get.

To help convince you that there is some serious work to be done, please allow me to point you to a few papers of mine along with colleagues that I hope will give you a flavor of the sorts of technical issues that you might help address or learn about etc., etc., through your participation at our meeting. These things are along the lines of quantum versions of various de Finetti representation theorems, new kinds of diachronic coherence, etc.:

- C. A. Fuchs and R. Schack, "Unknown Quantum States and Operations, a Bayesian View," `http://www.arxiv.org/abs/quant-ph/0404156`
- C. M. Caves, C. A. Fuchs, and R. Schack, "Conditions for Compatibility of Quantum State Assignments," `http://www.arxiv.org/abs/quant-ph/0206110`
- C. A. Fuchs, "Quantum Mechanics as Quantum Information (and only a little more)," `http://www.arxiv.org/abs/quant-ph/0205039`
- R. Koenig and R. Renner, "A de Finetti Representation for Finite Symmetric Quantum States," `http://www.arxiv.org/abs/quant-ph/0410229`

I hope that's enough to pique your interest, and enough to indicate that your presence at the meeting would be very valued!

11-01-05 *Wheeler, Bayes, Schleich* (to W. P. Schleich)

Thank you for sharing your memories about Asher. I have collected many words of remembrance in reply to our announcement, and I will pass them on to Asher's family after the collection looks complete.

I hope you will also note the conference invitation I sent you last night. I very much hope you will attend. Caves, Schack and I—and I especially—regard these efforts to get a Bayesian view of quantum mechanics as a detailed carrying-out of the program John Wheeler first set us onto: "law without law" and "it from bit". John Wheeler was a great influence on me, as he was on you. Your participation in this whole program would be a great addition to it, and our conference, I think, will really get you in the mood. So, please do say *yes* (so that we can hold you a spot), even if your schedule is only tentative at the moment!

As it turns out, I looked at your webpage yesterday (to make sure that I had the correct email address for you), and I was reminded of your early work on phase-space representations of quantum mechanics. Let me draw your attention to a couple of my own papers where I show how one can get something like a coordinate-space representation of the quantum state (on finite-dimensional Hilbert spaces) in terms of a *single* probability distribution. And by "probability distribution" I really mean that: It's not something that goes negative like the Wigner function. Instead, rather than through negativity, the unique quantum features of a system are expressed by other properties of the distribution. Anyway, here are the papers, and I'd enjoy any feedback you can give me:

- "Quantum Mechanics as Quantum Information (and only a little more)," `http://www.arxiv.org/abs/quant-ph/0205039`. (See particularly Sections 4.2 and 6.1 for the representations I'm talking about.)
- "On the Quantumness of a Hilbert Space," `http://www.arxiv.org/abs/quant-ph/0404122`. (See equations 28–32 for a particularly clean representation of the variety introduced in the other paper.)

I think we're at the beginning of something very big in physics; we're carrying the torch that John first lit. I hope you'll write to say, "Yes, I'll see you in Konstanz!"

Wolfgang's Preply, "Asher Peres"

Many thanks for your message concerning Asher Peres. I am deeply saddened and shocked by the news. The last time I have met Asher was in Rome during the celebration of De Martini's 70th birthday. At that time he did not look very good and he also looked rather confused. I was worried about him and had a longer talk with him. He told me that he will not travel again without his wife.

I have known Asher since 1984 when he came to Austin to give a talk on the influence of initial state to problems in quantum chaos. It is interesting that recently we have returned to just his approach again in some common work with Mark Raizen and Thomas Seligman. Asher was a great scientist and a wonderful human being who taught us all a lot in science and humanity. He will be certainly missed by the whole community.

13-01-05 *Another Chance . . .* (to G. Brassard)

Brassardisme 17: *Here's another chance for us to write the paper that we had planned for Asher's 70th birthday . . .*

This is a good point. I will try, but I can't make a promise at the moment: Almost all desire to write (even my usual nonsense) has left me for a while. I have to struggle with each and every word lately—and I've been dreaming of the day that I wouldn't have extant and unfulfilled writing projects hanging over me. But Asher deserves much, and I will try to find some way to accommodate. Still I don't venture a promise at the moment.

17-01-05 *Need Answer Quickly* (to R. W. Spekkens)

Could we get you to come out and give us a visit this Spring? Ideally I'd like to have you around for a week or more and give us a seminar on your noncontextuality work (pointing out connections between it and issues in quantum computing). I ask because Lov has some money to spend and we haven't been able to get rid of it in big chunks. So we're resorting to a more vigorous short-term visitor program for the short term (and are trying to lure some people here for sabbatical in the longer term). I'd very much like to have you out, and I've gotten Lov's approval.

The reason I'm asking for a quick answer is that DARPA will be out here on the 20th to review Lov, and he'd like to have as complete a list as possible of visitors to advertise. So, could you give me a quick reply letting me know if it's possible.

Spekkensism 28: *I would like to hear your thoughts on a result I just came up with the night before last (hot off the presses). I like to call it the "reverse Gleason theorem". The theorem states that if a functional f on the space of density operators satisfies*

$$0 \le f(\rho) \le 1,$$

and

$$f\left(\sum_k p_k \rho_k\right) = \sum_k p_k f(\rho_k)$$

for some probability distribution then it can be written as $f(\rho) = Tr(\rho E)$ for some effect E.

Yeah, I knew that one. One can prove it with essentially the same techniques that we proved the POVM Gleason theorem with, though you may have come across another way. In fact it really is the same theorem—certainly technically (except for little issues about scaling), but also conceptually (if one has already accepted that POVMs are refinements of knowledge full stop).

With hindsight, I also know that Holevo knew it sometime in the 70s. He told me about it as we were flying to Pasadena in 1997; he said I could read about it in his 1980s book. He kept emphasizing how difficult it was to prove the theorem in the infinite dimensional case, which I didn't care about, and, particularly, which obscured the content of what he was saying to me: I didn't realize any connection between this and POVM-Gleason until long after our/Busch's papers when I was writing a review on Holevo's second book. It brought the conversation back to me.

Anyway, Holevo's starting point for the theorem is the operational approach to quantum mechanics (of Davies and Lewis and himself, etc.), and I never liked that motivation. I.e., he starts by considering the probability of a preparation (identifying a preparation with a density operator), but for a Bayesian about quantum states, that's like talking about a probability of a probability. At the time I'd have none of that. It's nice as an expedient in speech, but for a Bayesian it shouldn't be a low-level way of talking. So more analysis is needed to get at the nub of the formulation. And that's where I left it at the time.

But who knows, maybe you'll have some spin on it that'll intrigue me afresh. Come out for a visit, and we can talk about all these things.

18-01-05 *Two More For You* (to L. K. Grover)

OK, I've got two more visitors for you to mark down for your presentation.

1) Rob Spekkens said he could come April 25th through May 4th, and he would talk on how the source of quantum computational speedup may be the Kochen-Specker theorem (or contextuality as he likes to call it) rather than entanglement per se, etc. So this is something you can properly call quantum *computation* research to the funding agents rather than quantum communication (as you had indicated to me earlier).

2) Walter "Jay" Lawrence, a professor at Dartmouth College, who works with Zeilinger from time to time, can come for two or three days in mid-March. So he'll be a cheap visitor—that doesn't help you spend a lot of money, but I'm trying my best. Anyway, he'll give us a talk on the mutually unbiased basis problem. And, at least in the case of he and I, we'll be collaborating on the same problem that Wootters and I will be collaborating on when he comes for sabbatical: representing quantum mechanics by way of these structures. By the way, for your own edification, you might have a look at Paz and collaborators' stuff on such representations of quantum *computing*:

- `http://www.arxiv.org/abs/quant-ph/0204149`
- `http://www.arxiv.org/abs/quant-ph/0204150`
- `http://www.arxiv.org/abs/quant-ph/0410117`

I plan to do something similar with my SIC-POVM representation of QM.

Also, Wootters reiterated in a note last night that he'll be coming for a couple of weeks in the Fall and a couple of weeks in the Spring.

I hope that helps you out.

24-01-05 *Proposal Criteria* (to G. J. Milburn)

Milburnism 1: *The other aspect that worries me a little are two rules regarding what they support:*

> participation in international activities, such as meetings and workshops, that are likely to move the research agenda of the field(s) forward, and shape collaborative and complementary research programs to address significant problems or capture new opportunities

but do not support:

> conference organisation or attendance where the purpose is the reporting of research findings.

It is my belief that our BBQW is a workshop of the kind that they do support. Is that also your view? I will need to make this clear in the application.

I *definitely* view the conference as a specimen of their first criterion: That's effectively the whole point of the meeting. However, I would have never thought that to be mutually exclusive to their second criterion (which seems to be what they are implying). It is true that I'm hoping we will get some presentations like:

> Spekkens
> `http://www.arxiv.org/abs/quant-ph/0406166`
> `http://www.arxiv.org/abs/quant-ph/0401052`
>
> Appleby
> `http://www.arxiv.org/abs/quant-ph/0412001`
>
> Renner
> `http://www.arxiv.org/abs/quant-ph/0410229`
>
> Wallace
> `http://www.arxiv.org/abs/quant-ph/0312157`
>
> Srednicki
> `http://www.arxiv.org/abs/quant-ph/0501009`
>
> Wootters
> `http://www.arxiv.org/abs/quant-ph/0401155`

etc., etc. To the extent that these are all relatively new works, this conference will be partially devoted to "the reporting of research findings." But the ulterior purpose of that is "to move the research agenda of the field forward, and shape collaborative and complementary research programs to address significant problems [and] capture new opportunities." That is, I want to see our various diverse efforts turned into a single agenda—the BBQW agenda—but for that to happen we will necessarily have to share our recent results with each other.

Does that help any for how to spin your proposal?

25-01-05 *MaxEnt 2005 Invitation* (to K. H. Knuth)

Thanks for the call yesterday. Your invitation to MaxEnt is very flattering. When I was a grad student, I used to pore over the old proceedings trying to get ideas—so a potential appearance at a MaxEnt would be a great honor. Also, as you seem to know, I think there is plenty of room to a

Bayesian about probabilities at the same time as rejecting the idea of a hidden-variable explanation underneath quantum mechanics. I like to spread the word of that view anytime I can … and I would particularly like to spread the word the attendees of MaxEnt, who would likely make the most progress with it.

But the trouble is, I've already committed myself to lecturing at the Konstanz summer school on "Philosophy, Probability, and Physics" and the dates of that are also August 7 to 13. [An aside: Also, in fact, the week before that, Aug 1–6, I will be in Konstanz running my own conference "Being Bayesian in a Quantum World" (with Carl Caves, Rüdiger Schack, and Stephan Hartmann) with about 20 philosophers and 20 quantum information theorists in attendance.] After that I was to rejoin my family for a week in Munich, where I they will deposited with my in-laws while I'm in Konstanz.

So, I'm torn. On the one hand I don't want to turn you down. But on the other, I did already commit to lecture at the summer school. Let me ask you a couple of questions. 1) Will your budget permit you to fly me to and from Germany, along with my local expenses? And, 2) would it be acceptable for me to attend only part of MaxEnt? If there's a yes answer to both of these questions, I could try to see if the Konstanz guys will schedule my lectures for one end or other of the time period, whichever is the more convenient for your meeting. Just an idea; I don't know if I've got enough nerve to really pull it off (i.e., all the quick flights), but if you're game, I would think about it harder.[79]

30-01-05 *My Last Email to Asher* (to Lydia Peres-Hari & Aviva Peres)

Below is the very last email I sent Asher. I don't know whether he saw it before passing away. I will share it with you, though it contains some fairly private information. Sadly, I did not write Asher much in the last three months; the note below explains why. I regret that very much now.

Asher was a great man, and in many ways, far beyond a friend to me—a father actually. I don't know whether he knew that, but I hope he did. In physics he had a great deal to do with my learning to stand on my own two feet. Without his support in 1994, I very likely would have stayed crawling. He gave me confidence in myself like no one else.

I looked back in my email records after talking to Aviva this morning. Since May 9, 1997, I saw that I had 1129 emails from Asher! And I myself had sent him 789 in that same period. We also had several hundred emails beyond that, written between November 7, 1994 and May 1997; unfortunately I used a different filing system back then, so it is not easy to figure out how many we actually exchanged. Still, by any measure, that's a lot of writing! (At least I have all our notes recorded, so they can be retrieved with a sufficient effort.)

I loved him, and love him.

31-01-05 *Quote of the Day* (to N. D. Mermin)

I just ran across the following quote of Nietzsche in Jauch's book on quantum foundations, and it reminded me of you:

[79]Indeed, my records show that I arrived in San Jose, California at 5:42 PM August 9, and departed 9:37 AM August 12 to head back to Munich, Germany and finish up the other trip! My travel schedules in those days were like Ezekiel's "wheels within wheels".

> That things have a quality in themselves quite apart from interpretation and subjectivity, is an idle hypothesis: It would presuppose that to interpret and to be a subject are not essential, that a thing detached from all relations is still a thing.
>
> — from *The Will to Power*

I don't know that you'll like the first part; but I'll bet that the second part makes the phrase "correlation without correlata" pop into your head (for whatever reason).

31-01-05 *Made My Reservations, Incompleteness, and (Irrelevance of) Decoherence* (to J. Bub)

OK, I made my reservations at the Jury's Normandy. For my own records, this is the confirmation number: 94740.

I've been meaning to write you ever since my visit to UMD to try to frame some of the issues you helped me think through with your questions. But my recent anathema to email has really gotten in the way of any writing bug I may have had. So, as a stopgap, and perhaps a starting point for further discussion when we get together again, let me attach a file with two old notes in it. The first concerns the sense in which I think it is fair to say that quantum mechanics is incomplete (even though I think hidden variable completions are the wrong way to go). The second comes out of my "Notes on a Paulian Idea" and concerns what I think is the more proper direction for study of "classicality" than notions to do with "decoherence." The point of the latter is that the classical or macroscopic world is not a state of nature in need of an explanation, but rather an artifact of our common states of knowledge. And so, as with much of quantum mechanics, take out the observer, and you take out whether some phenomenon can be classified as classical or not. Anyway, that's my point of view presently. Though admittedly it's only the hint of a program.

Luckily, I've been thinking very little about these things the last few weeks. Instead, I've been calculating, calculating, calculating. One finds a kind of calm in that that's found nowhere else.

09-02-05 *Measures of Our Field* (to R. Laflamme)

I am putting together a high-level presentation for the Bell Labs executives for next week to give some evidence of how "big" the field of quantum information and computing is, and I'm trying to do it by various measures. I wonder if I can ask you can to help me a little with some numbers? Is it possible to put an easy figure on how much money Canada is spending on quantum information and computing research each year lately? Do you know how that breaks down between theory and experiment? Also do you have an estimate on how many students and postdocs there are there working in the field? And if you can't answer these questions, do you know where I might try to find the answers?

I apologize for bothering you with this, but it'd help my presentation look spiffier if I had some numbers like this, and I'm guessing you may be able to help (and Gilles Brassard suggested the same). Thanks for any help you can give!

Ray's Reply

The only reasonable thing I can offer is a rough guess for the last 4 years:

- around \$40M from private sector (\$33M at IQC, \$7M at D Wave)

- infrastructure + personnel, about \$17M (\$12M at IQC, the rest at Montréal/Calgary/ Toronto and CIAR)
- \$1.5M/year for personal grants in the area (NSERC/MITACS/CIPI)

Total \$63M in the last 4 years

Hope that helps and good luck with your talk.

10-02-05 *Being Bayesian in a Quantum World – for Qubit News* (to Qubit News)

A previous post said, "It looks like they, in fact, are amidst quantum computing scientists." Indeed! And we're hosting our first international conference on the subject. The meeting is titled "Being Bayesian in a Quantum World," and will be held in Konstanz, Germany, August 1–6, 2005. It is now closed to further participants—because of space limitations and a desire to keep the discussion intimate—but I'll attach the meeting's announcement and the confirmed-participant list to give some flavor of the subject and to indicate whose writings to turn to if you want to learn more. [See 10-11-04 note "Being Bayesian in a Quantum World — Invitation" to the invitees.] Not all participants are by any means Bayesian (and also, there are many flavors of Bayesianism), but it shows that there are several quantum information scientists who consider the subject serious enough to entertain its potential.

18-02-05 *Lost Opportunity* (to R. E. Slusher)

I just read this quote in Feynman's 1982 paper on quantum computing, and boy how I wish I had used it to end my talks to Jaffe and Bishop. It might have also benefited Bernie when Bishop was being stubborn about quantum limits too. Here it is:

> ... full attention and acceptance of the quantum mechanical problem—the challenge of explaining quantum mechanical phenomena—has to be put into the argument, and therefore these phenomena have to be understood very well in analyzing the situation. And I'm not happy with all the analyses that go with just the classical theory, because nature isn't classical, dammit, and if you want to make a simulation of nature, you'd better make it quantum mechanical, and by golly it's a wonderful problem, because it doesn't look so easy. Thank you.

Let's make that road trip to Peirce's house sometime.

02-03-05 *Will You Find It Interesting?* (to M. Pérez-Suárez)

Thanks for bringing that book to my attention! It looks exciting.

I'm glad to hear that you got the approval for a very long stay in Albuquerque. I think you'll find the time productive (much more productive than your time with me).

I'm on my way to Poland at the moment to talk on quantum foundations. Why I'm going there, I'm not quite sure, ... I guess it's because I'm easily flattered. But you're right: aside from this anomaly, I've been spending most of my time trying to calculate something. (Unfortunately to little success.)

Marcos's Preply

I apologize for my silence. But I have found a moment to tell you that you should take a look at this book, which I have been browsing in the last days:

`http://www.springer.com/physics/quantum+physics/book/978-3-540-20856-3`.

William James, the "ideal of the detached observer", subjectivism ... a lot of subjects of your interest (even though you have readjusted yourself —or was it the world that made the readjustment on you?— a "true physicist" once again) are considered in the book (surprisingly enough, Harald's work is not mentioned).

I'll return to you in a few days (at least I hope so) in order to make my point on the "alchemical connection" and other issues.

Good news (at least, they are for me): The application for my stay with Carl has been accepted for the whole period of six months. I'll tell Carl about this, and I'll try to begin arranging things concerning it as soon as possible.

02-03-05 *And Yesterday* (to R. E. Slusher)

Slusherism 1: *I finished "The Sentiment of Rationality" and some other James stuff. Now I need more counseling from you! I am determined to understand the dynamics of measurement by an apparatus. I still don't see the human observer being involved (except to come by and readout the apparatus hard drive) — especially since I've been reading a lot about how complex the "SELF" is.*

You can't blithely say the parenthetical remark. The very most important thing we have learned from quantum mechanics is that there is no such thing as readout. Supplement your William James (particularly the essay *Pragmatism*) with:

N. D. Mermin, "Hidden variables and the two theorems of John Bell," Rev. Mod. Phys. **65**, 803 (1993).

That's one of the best articles on the Kochen-Specker theorem you can read.

But you're right, human is not the essential concept ... unless to bear probabilities and make decisions in the face of uncertainty is to be human. (But I don't think it is.)

I'll gladly talk about this when I get back.

07-03-05 *I Won't Be There Tuesday* (to J. B. Lentz)

What am I doing in Wrocław? It's the usual evangelical mission. They invited me to give some lectures on my point of view of quantum mechanics. When confronted with an offer like that, it's hard to turn down the invitation ... no matter where it is. It's the usual story we encounter in most aspects of life: One tries one's best to increase his children's chances of survival. Mostly I did it out of a sense of duty. The trip however, has turned out to be a pleasant surprise. They have treated me royally. The vice-president of the city even attended my first (more public) lecture, and presented me with a nice coffee-table book about the city. The food, the cognac, the jazz restaurants, have all been very nice too.

07-03-05 *No Laws at All* (to C. G. Timpson)

Indeed, I love that quote! Thanks for sending it. I have been told by someone (can't remember who at the moment) that I should read Cartwright's book, but until now I've put it off. However you have an awfully good track-record with your recommendations to me of books, and this quote definitely seals it. Better than reading it: I'll buy it and then read it!

I'm in Wrocław in Poland at the moment, finally enjoying a beer after all the hard work of this week. When I wasn't lecturing, I couldn't seem to stop my obsession with a certain calculation: It kept me up day and night, and thus the jetlag was much worse than usual. But at least I learned a lot by my random walk in Platoland.

Chris's Preply

> I am re-reading Cartwright's *How the Laws of Physics Lie* and am being struck how the position in that book seems in many ways sympathetic to the kind of view of science, and qm in particular, you'd like to adopt.
>
> Here in particular is a little passage I thought would please you:
>
>> Covering-law theorists tend to think that nature is well-regulated; in the extreme, that there is a law to cover every case. I do not. I imagine that natural objects are much like people in societies. Their behaviour is constrained by some specific laws and by a handful of general principles, but it is not determined in detail, even statistically. What happens on most occasions is dictated by no law at all. (OUP 1983, p.49)

08-03-05 *Spelled Wrocław, Pronounced Vrotswav, but Germans Still Say Breslau* (to D. B. L. Baker)

"It's been a long time since I've written you from a far-away place. I guess that's because far-away places have become little more than a pain for me. Trip after trip seems to drone on, and I've lost so much purpose. But tonight I feel like writing something. I'm captured by the charm of what I see around me. The snow coming down, the muffled sounds and the romance in this city square; a kiss I spied in a dark corner of the cathedral."

I wrote those lines in my head last night, thinking that I would go back to my hotel room, have a beer or two, and write you a long note. It didn't happen. I had the beer, you see, but I got wrapped up in music and the newspaper. Hans Bethe died yesterday at the age of 98, and I got sucked into stories about him. He was a great physicist and a great man. The last time I saw him, about 6 years ago, he was still reporting his research! In slow motion, but he was still reporting topical research! Amazing. Get on Google News if you can, and read about him. He was an important part of America and of all time; he brought the cores of stars into the hands of man. You read about someone like that, and I challenge you to think that man is an insignificant force in nature.

It's early morning, and I just arrived at Wrocław airport. I have a tortured route today: First to Munich, then to Paris, then to Chicago, and finally to Newark. Humbug, bah humbug. But at least I found last night romantic in the extreme. There were such big flakes of snow coming down, while a few people in the city square waited patiently for the Zamboni to smooth the ice in the skating rink; the couple I saw at the corner of the cathedral; the music trickling out of the basement clubs. It was those things combined with all that I had seen and heard this week; it came to a sort of fester. It's a strange world here. One that is still getting over World War II (everyone still talks about it, even the twenty-somethings), and one that reminisces the "funny" days of communism. It is a world a few years behind ours, at least emotionally.

I had the nicest experience and was treated royally by my hosts. What's the phrase? "Famous in Peoria," or something like that? The vice-president of the city even attended my first lecture and presented me with a big coffee-table book (of city things of course). There was a set of people that took a four hour train-ride to hear my lecture. And, I can't believe this one: There was a student who wanted my autograph! That's a first ... but then again it was Peoria ... I mean Wrocław. Ah, what's the difference. Still, it was touching. I wish I could live up to that young guy's dreams, but I don't have it in me anymore. Most of the time I feel like such a has-been.

But last night, everything felt right just briefly. In the city square I felt so small, but in just the right way. You know what I mean? I felt like ...

Well, once again, I didn't manage to finish my note to you before having to get on the plane. Now after all the delays and the nearly missed flights, the mood is broken. Munich has passed, Paris has passed, and I'm somewhere over the Atlantic. I strongly suspect however that my luggage isn't. Over the Atlantic, that is. Sad how some bad flights can shatter a day ... which I guess gets me back to where I started: "It's been a long time since I've written you from a far-away place. I guess that's because far-away places have become little more than a pain for me."

Maybe I'll have better luck the next time I try to write you.

11-03-05 *A Quiet Fuchs ... for once* (to H. Price)

I'm back from Poland, and I'm trying to get caught up on my email and other things. (Among them, emails from long before Poland!)

Question: Have you sent off your "Truth as Convenient Friction" to the memorial volume? Sorry I never sent you that list of typos. Trouble was, I wanted to reread it, and reread the relevant Rorty, and make some idiosyncratic comments at the same time ... maybe to show you that I really do have a philosophical streak. It was laid out beautifully in my mind: I would title the note "Value Added." But then a bad thing happened—I got obsessed with a certain calculation, and I haven't been able to let it go for over two months now!

Still, if you haven't sent your paper to the publishers, I might use that as an excuse to reread it this weekend (skipping Rorty, and making no promises to even attempt value-addedness, but at least refreshing myself of your arguments as a kind of payment for the editorial work).

11-03-05 *Chance* (to J. Ismael)

I'm just coming back to email after a long hiatus. (Even for a guy whose professional career is based on his knack for correspondence, sometimes a hiatus is overdue.) Anyway, thanks for the paper. I just printed it out and will *certainly* read it. Too bad you're not coming to Konstanz to read it to us!

There's a good (subjective) chance I'll be in Sydney at your meeting, but I don't want to give it a certainty yet.

Ismaelism 1: *But be patient; after hearing your talk in October, and rereading some of your work, I realized we share something perhaps more important than the surface disagreement: viz., the conviction that probability is information.*

Without doubt, I'll be patient. But it's hard at this point to imagine that our disagreement will only be a surface one: I have so much invested in taking a subjective interpretation of probability down into quantum theory's core—as a research program—that a reversal at this point might amount to schizophrenia. In fact, I think my ideas on quantum foundations hang together *only* because of a de-Finetti style interpretation of probability. But still, I know I'll learn a lot by studying your paper. And we shall see.

I'm curious to know which of my things you've read, so that I can know which version of me you've seen. I'm fairly fluid in some aspects. But, in particular, if I think I might further our discussion on chance by pointing you to some particular passage, it would be useful to know which part of my writings you've digested.

15-03-05 *Down with Properties!* (to S. J. van Enk)

- The wavefunction is not a property of the quantum system.
- Nor is a measurement outcome the signifier of some property (either one pre-existent or one created from the measurement process itself).
- States and POVMs play essentially the same roles: Subjective judgments.
- Measurement outcomes (I would guess) do have a kind of reality—they do come into existence via the measurement process—but they don't signify properties. Rather they are rawer than that: Kind of like the nodes in your graphs. Anyway, see "Preamble" at the end of the samizdat. New outcome, new graph? Just playing with ideas.

Steven's Preply

> When I read your email to Spekkens I saw why you're interested in the book about how Asians think.
>
> I liked Rorty's description of the world and his example of numbers. That somehow made me think of dictionaries, where words are defined only in relation to other words. Whereas I used to think a dictionary, therefore, doesn't define anything, thanks to Rorty I now think the opposite: it determines everything! In fact, I think if you give me a Swahili dictionary I could in principle translate almost every Swahili word into English by just looking at how all Swahili words relate to one another.
>
> That in turn made me think of graphs and the graph-isomorphism problem: a graph is defined completely by relations between all nodes, where the nodes only have completely meaningless labels. The graph-isomorphism problem is like finding the translation of a dictionary.
>
> And that, I suppose, is also the essence of Mermin's correlations without correlata.

16-03-05 *Stein* (to A. Radosz)

I've been meaning to write to you since my return, but I've only now gotten your email address.

I did find the Gertrude Stein quote you mentioned in one of my papers! It was in a review I wrote of Alexander Holevo's new book:

> The clear-cut purpose of the monograph is not to go through the mechanics of each theorem, but to paint a picture in modern art. Throughout, there is an emphasis on

> how each and every quantum result differs from its analog in the classical probabilistic model. One does, however, fear that a central illuminating thread is somehow missing from the exposition, though the author cannot be blamed: It is a thread no one has yet seen. A radical departure from classical probability theory, yes. But why? What is the all-important, undeniable physical fact that forces this revised calculus upon us? Can we state it in a way that does not consist of a dozen disparate theorems? One day we will get there. In the mean time, though, an exposition like this is part of the necessary journey. It is likely to be as Gertrude Stein said of the more classical kind of modern art, "It looks strange and it looks strange and it looks very strange; and then suddenly it doesn't look strange at all and you can't understand what made it look strange in the first place."

I'm sorry I forgot that. I was first alerted to that particular quote of Stein's by John Wheeler. To see another Gertrude Stein quote in action, see an old paper of mine with Carl Caves: `quant-ph/9601025`, "Quantum information: How much information in a state vector?". See page 26 and footnote 43.

It was great meeting you and dining with you, and thanks again for the lovely introduction you gave to my talk!

16-03-05 *Reply from the Lusitania* (to K. H. Knuth)

Knuthism 1: *I have been extremely interested in the logic of questions, and have recently found that they follow the free distributive algebra and possess a measure that is a natural generalization of information theory. I can now perform computations that determine the degree to which one question answers another, and I have been applying some of this work here at NASA to design intelligent instruments that decide which measurements to take next given the data that they already possess. You may be interested in the three papers I am attaching for your perusal .... I will be giving a tutorial on this work at MaxEnt 2005 on Sunday.*

I have been mightily impressed by a skim of some of your papers. I hope I get a chance to actually understand them over the course of this Spring and Summer. In particular, the two that most rise to my attention at the moment are "What is a Question?" and "Lattice Duality." For about three years now I've been emphasizing the direct analogy between the formal structure of quantum measurements (aka questions) and quantum states (aka Bayesian degrees of belief in the quantum context). If a quantum state is akin to a probability (and therefore subjective), a quantum measurement is akin to a conditional probability (and therefore subjective)—that's the kind of idea. You can find some of these thoughts tabulated in `quant-ph/0205039`, particularly in Section 6, but I go into much greater depth in *Quantum States: What the Hell Are They?* posted at my website. (Unfortunately, the latter has not been distilled into a proper paper yet, but maybe it's entertaining nonetheless.) As I understand it, these papers of yours (and the Cox work that started them off, which I did not know about) seem to indicate that this is a much larger phenomenon than I had contemplated. I'll definitely have to understand this stuff that you're uncovering.

17-03-05 *It's All About Gram Matrices* (to K. R. Duffy)

Dear Old Friend,

By the time you read this note, I know you'll have a horrible headache (assuming you read email at all the day after St. Patty's Day), but I thought I'd say hi anyway while the day is still here. Ireland has been on the whole family's mind all week—in lots of ways we all miss it, and we've been talking about it a lot and reminiscing. I even endured watching Bobby Flay (who I don't like) on the Food Channel last night so that we could see "A Taste of Ireland." The scenery made it worth it (at least whenever Flay wasn't in the same picture). Hey, we even put an Irish flag on the pole at the front of our house this week, finally retiring the University of Texas flag we put up when we first moved in.

How are things go with you? For myself, I've been scrambling the last two months to get a good result to present at the JTL meeting—not a lie. At the other meetings I've been going to recently, I've been presenting slight variations on the same-old-same-old, knowing that I could always get away with it. But at John's meeting, I want to present some mathematics that would have maybe put a smile on his face. I keep thinking I'm this close "", then it all vanishes. Still, there's a running chance! And I'm little if not determined.

Say hi to Jo and Dave and anyone else I'd remember if you see them. And take some Nurofen!

21-03-05 *Changing the World* (to C. H. Bennett)

Here's a little story I've got to tell you and Theo. Ever since you wrote me these lines soon after Katie's birth,

Bennettism 25: *Theo enjoyed your birth announcement for Katie. When I read her that Katie was beautiful in every way and would shape the world by her presence and will, she said she wasn't surprised. That is just what she would expect from another baby that came from the same parents as Emma. Theo told me that her grandfather, when told of the birth of a baby from someone he admired, would typically say "That baby will change the world." (After the world changes it a few hundred times, I couldn't help thinking.) Congratulations to all four of you from the two of us.*

I have from time to time told her that one day she will change the world—that that's her destiny. Last night I told her that as I was tucking her into bed and asked, "Do you know it?" How did she reply? "Yeah. Today I changed the batteries in my toy."

I loved it and had to record it.

25-03-05 *[SPAM?] At It Again: Delirium Quantum* (to H. M. Wiseman)

Wisemanism 34: *I'm writing a foundational paper (partly to prepare myself for your workshop, but its been on the go for years) and want to quote parts of the following:*

> Any single application of quantum theory is about ME, only me. It is about MY interventions, MY expectations for their consequences ... and MY reevaluations of MY old expectations in the light of those consequences. It is noncommittal beyond that. This is not solipsism; it is simply a statement of the subject matter. ... What quantum theory does is provide a framework for structuring MY expectations for the consequences of MY interventions upon the external world. At least that is what the formal structure is about. There is no "we," there is no "our."

from your "Delirium Quantum" collection. Is this going to be made public any time soon so I can give it a proper reference?

Hmm, what do you think? You think I should post that? How much more foolish than usual would I look with this one? [See "Delirium Quantum" `arXiv:0906.1968v1`.]

Another question. Guido Bacciagaluppi once told me that "Delirium Quantum" was an abomination of a title: It should be "Delirium Quanticum" so that both words are Latin (after I explained to him that I was thinking of "delirium tremens" when making the title). In a way he's right, but on the other hand I've got a certain redneck kind-of persona to preserve: I'm not sure I want to get too fancy with my titles. The present one seems just incorrect enough to fit me.

30-03-05 *Civil Disobedience* (to G. L. Comer)

Thanks for your notes bringing me up-to-date on your tripartite career: poetry, music, and physics. And I applaud your efforts to find linearity in each of them! (I.e., the place of the observer/actor/agent in each of them.) [...]

I look forward to reading your discussion of linearity in GR. Definitely, send it by me as it comes along.

For myself, I've been obsessed by a basic linear algebra for two months now, and I've thrown myself into it like I was a graduate student again. That is one of the great things about quantum information: One starts to realize how many very basic questions in finite-dimensional linear algebra have simply never been explored. And sometimes, they're really tough questions despite the finite dimensions. Take the linear vector space of $d \times d$ complex matrices. That's a d^2 dimensional vector space, and you can equip it with an inner product, say the Frobenius one $\mathrm{tr}(A^\dagger B)$ where $\dagger$ denotes Hermitian conjugation. Question: Can one always construct a complete orthonormal basis for this vector space consisting solely of unitary matrices? It's not obvious, but the answer is yes—it took Schwinger to note it. New question: Can one always construct a complete orthonormal basis consisting of Hermitian matrices? Much easier, and the answer is yes. New question: Can one always construct a complete orthonormal basis consisting of positive semidefinite Hermitian matrices? Much harder! But the answer is no. In fact, it looks like the closest one can come is to get a normalized basis with pairwise inner products of value $1/(d + 1)$. But proving it, that has turned out to be really hard. Even constructing a set of (positive semidefinite Hermitian) matrices with those inner products has turned out to be elusive. Constructions exist for $d = 2, ..., 8$ and 19, but there's only numerical evidence for the other dimensions up to 45 (and after 45 there's not even numerical evidence, the problem becomes too big for easily accessible computers). But dammit, it's an important question. For these are precisely the things we should have been using all along to express quantum mechanics, rather than expansions in terms of orthonormal bases.

But such is the life! Interpreting and reinterpreting the scripture.

06-04-05 *Internal Realism?* (to J. E. Sipe)

Finally, let me point you to `quant-ph/0204146`. The relevant sections for our Putnam discussion last night are 4 and 5. I'm curious to know whether that sounds like Putnam's internal realism to you (as I think Rob thought it did), or whether it sounds like a different beast? Of course, I should read Putnam myself ... but your opinion might tide me over until I get a chance to.

06-04-05 *Being Bayesian in a Quantum World* (to G. Musser)

I am sorry I've been out of contact for so long, but of course nothing has worked out on the schedule I had wanted to keep to this year. In particular, I hope you and *Scientific American* might still be interested in an article from me. I have decided that if I were to write one, I would like to title it "Being Bayesian in a Quantum World" and with it convey some of the excitement a rather large community of us are starting to feel for this aspect of the "Quantum Foundations in the Light of Quantum Information" program I described to you previously.

Now, what I think I'd like to do is start putting some words together right after our big meeting on the subject in Konstanz in early August of this year. That way I think I'd be better able to take into account all the things I see and hear around me there, which would probably quite benefit the article. The particular subject of the article would pretty much be along the lines of a little abstract I just wrote up for the MaxEnt and Bayesian Methods meeting, which will be held in San Jose just after BBQW: I'll place the abstract below so that you might get a feel for the subject matter.

Would *Scientific American* be interested in an article from me along these lines?

The abstract below is actually a variation on some paragraphs I wrote to announce our Konstanz meeting. Thus, in a separate email, I think I will send you the full (original) announcement. More particularly, if you're interested you can take a look at our website to see the final list of participants, the program, the research questions, etc. Here's the web address:

`http://web.archive.org/web/20090511060206/http://`
`www.uni-konstanz.de/ppm/events/bbqw2005/`.

At least given your seeming excitement in our discussions last year, you might just be interested—I hope so! Anyway, one idea that comes to mind is that if you think you might like to cover the meeting for a little description or blurb or whatever in *Scientific American*, you are certainly welcome to come. There will be plenty of good people to talk to there, and I doubt you'd be bored. You'd just have to let me know fairly quickly so that we can make sure there is still a room available at the conference hotel.

07-04-05 *Recovery of Philosophy* (to J. E. Sipe)

Sipesm 1: *Here's the quote by Dewey that I mentioned:*

> Intellectual advance occurs in two ways. At times increase of knowledge is organized about old conceptions, while these are expanded, elaborated and refined, but not seriously revised, much less abandoned. At other times, the increase of knowledge demands qualitative rather than quantitative change; alteration, not addition. Men's minds grow cold to their former intellectual concerns; ideas that were burning fade; interests that were urgent seem remote. Men face in another direction; their older perplexities are unreal; considerations passed over as negligible loom up. Former problems may not have been solved, but they no longer press for solution.

I find this really very amazing, especially since it was written in 1917. I don't think Thomas Kuhn ever knew about it (– while I certainly haven't read everything Kuhn wrote, I'm not aware of him acknowledging any debt to Dewey anywhere –) especially since the quote occurs in an essay ("The Need for a Recovery of Philosophy") that Dewey wrote about contemporary philosophy in America. But the approach is pure Kuhn (or rather Kuhn-before-Kuhn), with a clear articulation of what would later be called "normal science" and "paradigm shifts."

Hey, I've read that article—relatively thoroughly in fact. But I didn't catch the similarities to Kuhn in it! Below are the quotes I took from the article for my upcoming "The Activating

Observer: Resource Material for a Pauli'an-Wheeler'ish Conception of Nature" ... which pretty much reveals the parts of the article I was paying attention to at the time.

The quotes come from (and the page numbers refer to), J. Dewey, *John Dewey: The Essential Writings*, edited with introduction by D. Sidorsky, (Harper & Row, New York, 1977).

malleable world, pp. 70–71:

> [I] set out with a brief statement of some of the chief contrasts between the orthodox description of experience and that congenial to present conditions.
>
> (i) In the orthodox view, experience is regarded primarily as a knowledge-affair. But to eyes not looking through ancient spectacles, it assuredly appears as an affair of the intercourse of a living being with its physical and social environment. (ii) According to tradition experience is (at least primarily) a psychical thing, infected throughout by "subjectivity." What experience suggests about itself is a genuinely objective world which enters into the actions and sufferings of men and undergoes modifications through their responses. (iii) So far as anything beyond a bare present is recognized by the established doctrine, the past exclusively counts. Registration of what has taken place, reference to precedent, is believed to be the essence of experience. Empiricism is conceived of as tied up to what has been, or is, "given." But experience in its vital form is experimental, an effort to change the given; it is characterized by projection, by reaching forward into the unknown; connection with a future is its salient trait. (iv) The empirical tradition is committed to particularism. Connections and continuities are supposed to be foreign to experience, to be by-products of dubious validity. An experience that is an undergoing of an environment and a striving for its control in new directions is pregnant with connections. (v) In the traditional notion experience and thought are antithetical terms. Inference, so far as it is other than a revival of what has been given in the past, goes beyond experience; hence it is either invalid, or else a measure of desperation by which, using experience as a springboard, we jump out to a world of stable things and other selves. But experience, taken free of the restrictions imposed by the older concept, is full of inference. There is, apparently, no conscious experience without inference; reflection is native and constant.
>
> These contrasts, with a consideration of the effect of substituting the account of experience relevant to modern life for the inherited account, afford the subject matter of the following discussion.

time, pp. 73–74:

> The preoccupation of experience with things which are coming (are now coming, not just to come) is obvious to any one whose interest in experience is empirical. Since we live forward; since we live in a world where changes are going on whose issue means our weal or woe; since every act of ours modifies these changes and hence is fraught with promise, or charged with hostile energies—what should experience be but a future implicated in a present! Adjustment is no timeless state; it is a continuing process. To say that a change takes time may be to say something about the event which is external and uninstructive. But adjustment of organism to environment takes time in the pregnant sense; every step in the process is conditioned by reference to further changes which it effects. What is going on in the environment is the concern of the organism; not what is already "there" in accomplished and finished form. In so far as the issue of what is going on may be affected by intervention of the organism, the moving event is a challenge which stretches the agent-patient to meet what is coming.

Experiencing exhibits things in their unterminated aspect moving toward determinate conclusions. The finished and done with is of import as affecting the future, not on its own account: in short, because it is not, really, done with.

Anticipation is therefore more primary than recollection; projection than summoning of the past; the prospective than the retrospective. Given a world like that in which we live, a world in which environing changes are partly favorable and partly callously indifferent, and experience is bound to be prospective in import; for any control attainable by the living creature depends upon what is done to alter the state of things. Success and failure are the primary "categories" of life; achieving of good and averting of ill are its supreme interests; hope and anxiety (which are not self-enclosed states of feeling, but active attitudes of welcome and wariness) are dominant qualities of experience. Imaginative forecast of the future is this forerunning quality of behavior rendered available for guidance in the present. [...] Imaginative recovery of the bygone is indispensable to successful invasion of the future, but its status is that of an instrument. To ignore its import is the sign of an undisciplined agent; but to isolate the past, dwelling upon it for its own sake and giving it the eulogistic name of knowledge, is to substitute the reminiscence of old age for effective intelligence.

time and control, p. 78:

Experience, to return to our positive conception, is primarily what is undergone in connection with activities whose import lies in their objective consequences—their bearing upon future experiences. Organic functions deal with things as things in course, in operation, in a state of affairs- not yet given or completed. What is done with, what is just "there," is of concern only in the potentialities which it may indicate. As ended, as wholly given, it is of no account. But as a sign of what may come, it becomes an indispensable factor in behavior dealing with changes, the outcome of which is not yet determined.

The only power the organism possesses to control its own future depends upon the way its present responses modify changes which are taking place in its medium. A living being may be comparatively impotent, or comparatively free. It is all a matter of the way in which its present reactions to things influence the future reactions of things upon it. Without regard to its wish or intent every act it performs makes some difference in the environment. The change may be trivial as respects its own career and fortune. But it may also be of incalculable importance; it may import harm, destruction, or it may procure well-being.

control, p. 79:

As we have already noted, the environment is rarely all of a kind in its bearing upon organic welfare; its most wholehearted support of life activities is precarious and temporary. Some environmental changes are auspicious; others are menacing. The secret of success that is, of the greatest attainable success—is for the organic response to cast in its lot with present auspicious changes to strengthen them and thus to avert the consequences flowing from occurrences of ill-omen. Any reaction is a venture; it involves risk. We always build better or worse than we can foretell. But the organism's fateful intervention in the course of events is blind, its choice is random, except as it can employ what happens to it as a basis of inferring what is likely to happen later. In the degree in which it can read future results in present on-goings, its responsive choice, its partiality to this condition or that, become intelligent. Its bias grows reasonable. It can deliberately, intentionally, participate in the direction of the course of affairs. Its foresight of different futures which result according as this or that present factor predominates in the shaping of affairs permits it to partake intelligently instead of blindly and fatally in the consequences its reactions give rise to. Participate it must, and to its own weal or woe. Inference, the use of what happens, to anticipate what will—or at least may—happen, makes the difference between directed and undirected participation. And this capacity for inferring is precisely the same as that use of natural occurrences for the discovery and determination of consequences—the formation of new dynamic connections—which constitutes knowledge.

existence of reality, p. 81:

One of the curiosities of orthodox empiricism is that its outstanding speculative problem is the existence of an "external world." For in accordance with the notion that experience is attached to a private subject as its exclusive possession, a world like the one in which we appear to live must be "external" to experience instead of being its subject matter. I call it a curiosity, for if anything seems adequately grounded empirically it is the existence of a world which resists the characteristic functions of the subject of experience; which goes its way, in some respects, independently of these functions, and which frustrates our hopes and intentions. Ignorance, which is fatal, disappointment, the need of adjusting means and ends to the course of nature would seem to be facts sufficiently characterizing empirical situations as to render the existence of an external world indubitable.

Bergson, pp. 86–87:

What are the bearings of our discussion upon the conception of the present scope and office of philosophy? What do our conclusions indicate and demand with reference to philosophy itself? [...]

It is easier to state the negative results of the changed idea of philosophy than the positive ones. The point that occurs to mind most readily is that philosophy will have to surrender all pretension to be peculiarly concerned with ultimate reality, or with reality as a complete (i.e., completed) whole: with *the* real object. The surrender is not easy of achievement. The philosophic tradition that comes to us from classic Greek thought and that was reinforced by Christian philosophy in the Middle Ages discriminates philosophical knowing from other modes of knowing by means of an alleged peculiarly intimate concern with supreme, ultimate, true reality. To deny this trait to philosophy seems to many to be the suicide of philosophy; to be a systematic adoption of skepticism or agnostic positivism.

The pervasiveness of the tradition is shown in the fact that so vitally a contemporary thinker as Bergson, who finds a philosophic revolution involved in abandonment of the traditional identification of the truly real with the fixed (an identification inherited from Greek thought), does not find it in his heart to abandon the counterpart identification of philosophy with search for the truly Real; and hence he finds it necessary to substitute an ultimate and absolute flux for an ultimate and absolute permanence. Thus his great empirical services in calling attention to the fundamental importance of considerations of time for problems of life and mind get compromised with a mystic, nonempirical "Intuition"; and we find him preoccupied with solving, by means of his new idea of ultimate reality, the traditional problems of realities-in-themselves and phenomena, matter and mind, free will and determinism, God and the world. Is not that another evidence of the influence of the classic idea about philosophy?

reality, pp. 88–89:

It is often said that pragmatism, unless it is content to be a contribution to mere methodology, must develop a theory of Reality. But the chief characteristic trait of the pragmatic notion of reality is precisely that no theory of Reality in general, *überhaupt*, is possible or needed. It occupies the position of an emancipated empiricism or a thoroughgoing naive realism. It finds that "reality" is a denotative term, a word used to designate indifferently everything that happens. Lies, dreams, insanities, deceptions, myths, theories are all of them just the events which they specifically are. Pragmatism is content to take its stand with science; for science finds all such events to be subject matter of description and inquiry—just like stars and fossils, mosquitoes and malaria, circulation and vision. It also takes its stand with daily life, which finds that such things really have to be reckoned with as they occur interwoven in the texture of events.

The only way in which the term reality can ever become more than a blanket denotative term is through recourse to specific events in all their diversity and thatness. Speaking summarily, I find that the retention by philosophy of the notion of a Reality feudally superior to the events of everyday occurrence is the chief source of the increasing isolation of philosophy from common sense and science. For the latter do not operate in any such region. As with them of old, philosophy in dealing with real difficulties finds itself still hampered by reference to realities more real, more ultimate, than those which directly happen.

I have said that identifying the cause of philosophy with the notion of superior reality is the cause of an increasing isolation from science and practical life. The phrase reminds us that there was a time when the enterprise of science and the moral interests of men both moved in a universe invidiously distinguished from that of ordinary occurrence. While all that happens is equally real—since it really happens—happenings are not of equal worth. Their respective consequences, their import, varies tremendously. Counterfeit money, although real (or rather *because* real) is really different from a valid circulatory medium, just as disease is really different from health; different in specific structure and so different in consequences. In occidental thought, the Greeks were the first to draw the distinction between the genuine and the spurious in a generalized fashion and to formulate and enforce its tremendous significance for the conduct of life. But since they had at command no technique of experimental analysis and no adequate technique of mathematical analysis, they were compelled to treat the difference of the true and the false, the dependable and the deceptive, as signifying two kinds of existence, the truly real and the apparently real.

malleable world, p. 91:

The epistemological universe of discourse is so highly technical that only those who have been trained in the history of thought think in terms of it. It did not occur accordingly, to nontechnical readers to interpret the doctrine that the meaning and validity of thought are fixed by differences made in consequences and in satisfactoriness, to mean consequences in personal feelings. Those who were professionally trained, however, took the statement to mean that consciousness or mind in the mere act of looking at things modifies them. It understood the doctrine of test of validity by consequences to mean that apprehensions and conceptions are true if the modifications affected by them were of an emotionally desirable tone.

Prior discussion should have made it reasonably clear that the source of this misunderstanding lies in the neglect of temporal considerations. The change made in things by the self in knowing is not immediate and, so to say, cross-sectional. It is longitudinal—in the redirection given to changes already going on. Its analogue is found in the changes which take place in the development of, say, iron ore into a watch spring, not in those of the miracle of transubstantiation. For the static, cross-sectional, nontemporal relation of subject and object, the pragmatic hypothesis substitutes apprehension of a thing in terms of the results in other things which it is tending to effect. For the unique epistemological relation, it substitutes a practical relation of a familiar type—responsive behavior which changes in time the subject matter to which it applies. The unique thing about the responsible behavior which constitutes knowing is the specific difference which marks it off from other modes of response, namely, the part played in it by anticipation and prediction. Knowing is the act, stimulated by this foresight, of securing and averting consequences. The success of the achievement measures the standing of the foresight by which response is directed. The popular impression that pragmatic philosophy means that philosophy shall develop ideas relevant to the actual cries of life, ideas influential in dealing with them and tested by the assistance they afford, is correct.

11-04-05 *Another Query* (to A. Kent)

Kentism 7: *Another query on* `9512023`*. I'm puzzled by the comment on p. 12 which says that A_{rs00} implies $|Phi_{00}\rangle = 0$ and similarly for 11. That doesn't immediately seem to fit with eq (12), which suggests that you'd need $A_{00rs} = 0$ to imply $|Phi_{00}\rangle = 0$. Also, I'm struggling to see how the optimal Eve strategy in the paper defines a unitary evolution if $|Phi_{00}\rangle = |Phi_{11}\rangle = 0$.*

Am I confused or going mad? Or is it possible there's a glitch in the conventions somewhere? (Is the `arXiv` *version isomorphic to the published version, btw? I'm working from the* `arXiv` *one.) You probably won't remember after 10 years, I realize. But if you happen to be able to shed any light, I would be grateful.*

Wow, that was a long time ago! I'd have to relearn the paper—like you seem to be doing at the moment. All my notes (and there were substantially more calculations than made it into the paper) burned up in the Cerro Grande fire. As a first pass, does my paper `9611010` help any? I remember trying to tie up some loose ends there (though for some different measures). Also, concerning the conjecture—I presume you mean the conjecture that a two-state probe is good enough—I don't know that anyone ever proved it. I gave up trying after `9611010`, though I did make a little progress in the direction there (see the discussion in the right column of page 10). I think maybe Bob Griffiths tried his hand at it too, but didn't make headway—though I'm not sure.

Anyway, let me know if any of that helps, and if it doesn't I'll try to think about it next week (after I get the seminars I've got to give this week off my back).

12-04-05 *Chris Fuchs, Apr. 14, MH* (to H. J. Landau)

Here's something of an abstract. Sorry to be getting back to you so late.

Abstract: Sets of quantum states good for quantum key distribution. Discrete positive-operator-valued measures on finite-dimensional vector spaces. Representing quantum states as probability distributions. Gram matrices. Schur product theorem. Hadamard exponential. Schwarz inequality. If any of these words or phrases interest you, come to the seminar.

21-04-05 *Another Idea* (to R. W. Spekkens)

Another thing that might make your visit fun. Maybe we could drive to Princeton one day and have a visit with John Conway and get him to give us a presentation of this new KS construction he's rumored to have with Kochen. (I did a quick google search and found this, for instance, but haven't read it yet: `http://www.cs.auckland.ac.nz/~jas/one/freewill-theorem.html`.)

Do you know if there's anything new in this?

25-04-05 *Your Talk (or your talks)* (to R. W. Spekkens)

Spekkensism 29: *I'm thinking that it might be useful for us to discuss what sorts of information-theoretic axioms might get us to the tensor product structure of Hilbert space. Another interesting project would be to try to reduce Lucien's axioms about subspaces and subsystems to a single axiom. [...] Then again, we could try to work out the number of "mutually unbiased bases" in the toy theory. This might be easier to solve than the equivalent problem in quantum mechanics. Or ... something entirely different.*

I wouldn't mind talking about a range of things, as I've grown flaccid in my old age. Maybe something new from you could perk me up.

Though probably this is what I'll be most interested in: I'm presently in the process of working out what the qutrit state space looks like, solely in terms of a constraint on the probability distributions over nine possible events. Think "toy model." Similarly, I *think* that with enough coffee, I could explicitly work out the constraints for the two-qubit case. Say that I can actually get that done. I'd like to understand a) in what way or not those constraints look like your toy model's constraints, and b) if getting the quantum constraints exactly right (i.e., what I'm doing) adds any qualitative phenomena over the long list your toy model already recovers. Surely, it'll give entanglement, whereas your model cannot, but what does that amount to?

26-04-05 *Thanks!* (to W. E. Lawrence)

Lawrencesm 2: *Thanks for the great visit and kind hospitality. It was invigorating and informative.*

No, thank you. I got every bit as much out of it, if not more. You reminded me of how important it is to just role up your sleeves and get to work. For instance, to stop toiling with the abstract when you're stuck and see what comes out in the three-dimensional case—it's super-important to make that move every now and then, and I sometimes forget.

26-04-05 *Eagle Eyes* (to C. G. Timpson)

Wow, with a friend like you, who needs ... any other friends at all! Just one good one would do. Thanks for the defense! I read it and enjoyed it. You always have the power to lift my spirits simply by understanding the (essentially trivial) things I've been saying — you're a rare bird in the philosophical community. Even Jeff Bub and Hans Halvorson surprise me in these matters. Every time I see Jeff try to re-ontologize "information" I cringe—I sometimes wonder why he even bothered to go through the motions of the Clifton-Bub-Halvorson exercise, when information to him is just another ontic entity. With that misreading of the very idea of information, it seems to me at best one makes no progress at all; at worst one just mystifies the problem further and erodes any confidence in the community that this is an interesting research direction.

I haven't seen the Palge/Konrad paper. I wonder why they didn't send it to me too? Interesting. Probably, it was because I was a bad boy about replying to an email of questions Veiko had when he first started the critical project. He hit me at a time when I could hardly stand to lift a finger for email. It takes a toll trying to say and resay old things in better and better ways just to fend off another critical paper. Somehow I need to get re-energized to try to write a "definitive" paper on the view, and then let come what may in the aftermath.

BTW, yes Eq. (97) was a typo, just as you sensed. Try as I might—and I tried all weekend—I can't remember who first pointed it out to me. I was able to trace some correspondence about it back to Tony Sudbery, 18 December 2003, which went:

> **Tony:** I'm sure I'm not alone in seeing your dissolving of the "violent" state change (on p. 34) as a cheat. Calling it a "mental readjustment" doesn't make it any less violent. Incidentally, there's something wrong with eq. (97); these U_i are not unitary.
>
> **Chris:** Fair enough. It is violent—every bit as violent, discontinuous, and irreversible as a usual application of Bayes' rule. But the point is that it is on the inside of the agent's head, and no one cares what is going on in the agent's head. What physicists should be worried about is the physics of the external world (and not the agent's head). And who knows what really happens there ... especially if physics itself is only really about the interface.
>
> About the U_i's you are correct. I'm sorry, I've prepared an updated version with a lot of the typos, etc., fixed, but I haven't reposted it yet. Here's what the new edition says:
>
> That is to say, there is a sense in which the measurement is solely disturbance. In particular, when the POVM is an orthogonal set of projectors $\{\Pi_i = |i\rangle\langle i|\}$ and the state-change mechanism is the von Neumann collapse postulate, this simply corresponds to a readjustment according to unitary operators U_i whose action on the subspace spanned by $|\psi\rangle$ is
>
> $$|i\rangle\langle\psi|\ .$$

For the life of me, I can't remember who first brought up the point, and my high-tech desktop-search tools haven't been of help this time. I do distinctly remember it was someone before Tony. Anyway, little things like this bug me at times ... when I can't put my finger on them. If the person is not already in the acknowledgement list, he should be.

Seeing that someone made a big deal of that one line of bad exposition in a paper, though, tells me that I really should get the thing re-posted. Most of the other typos were even more minor—for instance, I originally placed Strasbourg in Germany rather than in France—but there is another one that's on the level of the one above—i.e., one with the potential to cause real confusion. If you look carefully at Eq. (61), you'll see that there's a slip-up of the same order there. I wrote the swap-operator on the wrong space. To do it right, I need to add two or three more sentences and change the notation. I should do that, and then re-post the thing.

All that said, I actually do look forward to the day that you'll write a critical analysis of the Bayesian program. I feel in my heart that that analysis will be worthwhile, and the flaws you dig up will force us to a new level of consistency. (Of course, your analysis could simply break us, but I'm betting—I can't do otherwise!—that the outcome will much more positive than that.)

28-04-05 *Infinite Limits* (to G. Valente)

Thanks for sending me your paper. I've printed it out and will bring it to Maryland with me today. I'll look forward to a personal explanation of its content when I see you.

I have to tell you right now though that I am skeptical of its conclusion. This comes predominantly from a long-standing belief that was made forcefully upon me when I was a student of Carlton Caves (who himself was influenced on this issue by Ed Jaynes)—namely, that the infinite cases *never* carry the *essence* of a problem. In the present context what this means is: If something works in all finite dimensional cases, but *apparently* not in the infinite dimensional case, my faith in the former won't be eroded at all. It just means something has gone awry with one's analysis of the infinite dimensional case. And this conviction comes about because, in my mind, the infinite is just a convenient approximation, of use in this or that calculation, but without any autonomy.

I dug up a great quote from Chapter 2 of Jaynes' book on probability that frames the issue, and I'll place it below.

You'll also have to tell me about how miserable College Park is. I suspected it would be so. But when I saw that your being there would be a grand opportunity for a fellow alchemist[80] to make some influence on Jeff, I closed my eyes to it. Perhaps I shouldn't have. Still, I am very happy that you are there to influence Jeff.

Jaynes quote below.

> It is very important to note that our consistency theorems have been established only for probabilities assigned on finite sets of propositions. In principle, every problem must start with such finite set probabilities; extension to infinite sets is permitted only when this is the result of a well-defined and well-behaved limiting process from a finite set. More generally, in any mathematical operations involving infinite sets the safe procedure is the finite sets policy:
>
> > Apply the ordinary processes of arithmetic and analysis only to expressions with a finite number of terms. Then after the calculation is done, observe how the resulting finite expressions behave as the number of terms increases indefinitely.
>
> In laying down this rule of conduct, we are only following the policy that mathematicians from Archimedes to Gauss have considered clearly necessary for nonsense avoidance in all of mathematics. But more recently, the popularity of infinite set theory and measure theory have led some to disregard it and seek short-cuts which purport to use measure theory directly. Note, however, that this rule of conduct is consistent with the original Lebesgue definition of measure, and when a well-behaved limit exists it leads us automatically to correct "measure theoretic" results. Indeed, this is how Lebesgue found his first results.
>
> The danger is that the present measure theory notation presupposes the infinite limit already accomplished, but contains no symbol indicating which limiting process was

[80] I remember fondly what you wrote in your very first letter to me: "I wake up thinking about our alchimistic touch to the nature yesterday morning. As soon as I started studying, I read the same idea (alchemy) in your first statement about Pauli. I closed my books and I run away to phone my mum in Vicenza. . . "

used. Yet as noted in our Preface, different limiting processes—equally well-behaved—lead in general to different results. When there is no well-behaved limit, any attempt to go directly to the limit can result in nonsense, the cause of which cannot be seen as long as one looks only at the limit, and not at the limiting process.

09-05-05 *Being Bayesian in a Quantum World — Conference Invitation* (to J. B. Hartle)

I'm sorry to hear you can't come after all. Particularly, with what would have been your talk plan, your participation seems even more urgent than ever. Too bad! Well, we will think of you—among other things, I will present my standard slide of the "The Jim Hartle 1964, Sect. 4, Interpretation of Quantum Mechanics (suitably modified)" in my introductory talk—and please pass on my best wishes to your daughter.

09-05-05 *Becoming in the World (much more important than being)* (to H. J. Briegel)

I am sorry to take so long to respond to you. There are a million things I need to get going on with the conference organization . . . and there'll soon be a flurry of activity in that direction.

The format of the meeting is that we hope to have less than about half of the attendees speaking: Two talks in the morning, two talks in the afternoon. Then after each set of two, the idea is to have open floor discussion somewhat related to their topics. It's based on the model of the Seven Pines meetings, where I've seen it work well. At some point soon, we'll start soliciting "volunteers".

Would you be interested in giving a talk on one-way quantum computing, and how it might shake up the "standard" ideas about what powers quantum computing. And—if possible—say something about how well that meshes (or does not mesh) with a Bayesian take on probability theory? I might put you back-to-back with Nielsen for a session.

Is that enough detail to help you in your planning? We actually have a conference web site that's very close to being posted. But we've been keeping it secret (and you should too) until some issues with a funding agency get resolved.

09-05-05 *Justifying Conditionalization* (to H. Greaves)

I'm always desperate; it's part of my character: And so . . . thanks a million for the paper! Indeed I'm quite curious about this "kick" you mention.

Are you sure we can't convince you to come to Konstanz if we can secure funds for you? In another act of desperation, I'll show you our secret website in case it might help tip the scales for you:

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`.

Please peruse it, and if you reconsider let me know. (BTW, when I say it's a secret website, I'm relatively serious: we can't go public with it until some issues with one of our funding agencies are resolved. So please do keep the address to yourself until we go public with it ourselves.)

Hilary's Preply

Good to see you again this weekend. I mentioned a paper by myself and David Wallace, justifying conditionalization (QM aside) via an expected-epistemic-utility proof. In case you're interested, it's at

`http://philsci-archive.pitt.edu/archive/00002212/`

Incidentally, the one type of updating rule that is allowed in principle, as a consequence of our framework (it dropped out of the proof and refused to go away — we didn't want it), consists of rules of the form conditionalization-followed-by-a-kick ...

... I would be astounded if this stuff had any relevance whatsoever for your project, but if you're desperate and/or curious, you might like to take a look.

10-05-05 *BBQW Things* (to S. Hartmann)

I hope that bug has now left your body; sorry for the late reply to your note. Indeed, I've been meaning to write you for quite a while now, so I'm running far later than you probably even perceive.

From looking at the conference website, it looks like you got the note from Hartle saying that he cannot attend after all. It is kind of a shame because his subject did look quite good. But in the light of this, let me cause some new trouble: Looking back at our attendee list, 47 starts to look like such an unsymmetric number! I fear to ask this, but might you consider upping it to 50? My reasons for inquiring are both scientific and political.

First the science. Two weeks ago, I discovered an *outstanding* new candidate. His name is Matthew Leifer, and he is a postdoc at the Perimeter Institute. If you look at his webpage, via this link ... or ... you'll see that among his interests are "the Bayesian approach to quantum probabilities" but that far understates his knowledge and ambition in the area. Indeed, I've come to the opinion that he would be one of our most interesting participants (when we can get him to open his mouth — he's a very quiet person in a crowd, though he definitely opens up in one-to-one talk and that too would be a good role for him). He has some extremely interesting ideas about how quantum states better connect up with de Finetti's more general notion of "previsions" than with probabilities per se, and he has started to explore some very concrete proposals in this direction. A very, very interesting guy; he spent a week here recently, and I was ashamed that I had not pushed for him at our meeting before. (Well, I couldn't really be ashamed — I didn't know him — but I'd be ashamed now if we didn't get him; we really could get a good bang for the buck from him.) And Matt would really like to be there.

Now, making the transition from science to politics, though still on the scientific end: [...]

Now, all the way on the political end. [...]

Thus my question is, would these new additions (or some portion thereof) be OK with you? Can the budget still afford it?

As you suggest, it is about time to start soliciting talks and scheduling. I think it would be most useful for the website to be accessible before I start making those requests large scale.

Concerning number of speakers, I think I would suggest that we have about 20 total, and indeed follow the Seven Pines model. Thus we could have four talks and two discussions per day, with two talks and one discussion on the day of the excursion. That would only leave us with two other talks to squeeze in somewhere else. Maybe we could have one evening session.

If it is honorable to suggest it, I think I would like to give the opening talk. What I would do is give some very general words about the Bayesian program for QM as a whole, and suggest discussion points and open questions. I'd also give a quick overview about how the various talks in the schedule fit together. In other words, I'd like my talk to serve the purpose of giving a direction to the meeting; it would not be particular to my own latest research efforts. As you hoped for me when I came to the LSE, I will try to be provocative.

Concerning your website questions, I hope to have some detailed comments for you in the near future. One thing I already know that I'd like to do is start a section that has various papers for download. For instance, I intend to scan in de Finetti's "Probabilism" and a Richard Jeffrey article on radical probabilism and issue these as suggested readings. Once the speakers have been decided upon, we can also ask them to post their own picks of relevant readings.

11-05-05 *LOL* (to C. P. Hains)

After all these years, I finally hear from you, and what do you send me??! National Orgasm Day? I like it. But you probably knew I'd be particularly receptive to the idea. It's appropriate. Since we haven't talked in so long, you probably don't know that I finally lighted on a name for the view of QM I've been trying to develop over the years. I now call it "the sexual interpretation of quantum mechanics"—Copenhagen and many-worlds interpretations are so dull in comparison. Anyway, it makes for a lot of good bawdy bar-room talk at conferences. Do a Google search on the term if you want to learn a little about it.

I hope things are going well for you—I imagine they are. The big excitement here is that Kiki decided to paint the house orange. ... I guess I should have seen it coming 11 years ago!

12-05-05 *Smelling Correlation without Correlata* (to N. D. Mermin)

I was listening to a talk of Robert Raussendorf yesterday, who is visiting us here at Bell Labs, on cluster-state computation, and I'm almost ashamed to admit it, but I just about blurted out "Correlation without Correlata!" right in the middle of the talk! There was something in all he said that made the idea come to the surface in a way it hadn't (for me) in years. Particularly, I was moved by his point (actually, the particular way he presented it because I had already known it) that in a direct simulation of a quantum circuit in this model, one could perform the measurement that represents the readout in the first step, in the last step, or anywhere in between. It's only the correlation, not the correlata (not the particular measurement outcomes themselves), that matter in this model of quantum computation.

Anyway, I found myself wondering if this model of quantum computation might not be a better home for your "Copenhagen Computation" ideas than the usual unitary model. Have you thought about that?

17-05-05 *Spontaneous Syntax Breaking* (to G. L. Comer)

I feel your pain ...

On a related subject (to which your note brings me back, as did a couple of sentences in this talk by Healey that I heard a couple weeks ago: `http://carnap.umd.edu/philphysics/healey.htm`), back in my early years at UNC I went through an obsession for a while of looking up formal developments of the idea of a differentiable manifold. The thing that I was troubled by at the time was that in all the developments I could find, the idea of a set *plus* a set of coordinate charts (along with a requirement that it be possible to paste them together) was *primary*. I could find no definition of differentiable manifold that didn't rely on a more primitive definition of coordinate chart. The reason that troubled me—these are all vague memories now, so please don't press me to try to defend any of the ideas anymore or even try to make them precise—was that when it came time to describe what general relativity was about *but in words*—and all books of course strived to do that—the differential manifold was taken as a *primary* construct with coordinate charts being something of a secondary nature. See the circularity in that? You can't get the idea of a differentiable manifold off the ground without the idea of a coordinate chart, but then you're told that the differentiable manifold is the primary idea and coordinate systems are only gratuitous additions.

Do you remember in discussions of ours along these lines (circa 1991/1992)? And if so, can you dig up any of the old mails/emails?

Friday I leave for beautiful Vienna. But my romanticism for the place is marred by the sad fact that I have no idea yet what I'll say in my talk! There'll be a lot of sleeplessness between now and then.

17-05-05 *Numbers and Deeper Things* (to S. H. Simon)

Going back to our conversation yesterday, it dawned on me that it might be useful for you to have the numbers I compiled for my presentation to Jaffe too.

Now to more fun things also related to our conversation yesterday: You might enjoy reading through the Andrew Steane paper I mentioned. Here's a link: `http://www.arxiv.org/abs/quant-ph/0003084`. I read it again over breakfast this morning, and it's as good as I remembered it. (The last version I had read of it was in 2002 when I edited it for a special issue of SHPMP.) It's easy reading, but it's deep reading. In particular, I think it contains the roots of a much better way to start thinking about quantum computation, and thus a better way to present the phenomena to anyone interested (like our executives and VPs). Steven's way of presenting things yesterday might be viewed as a stopgap in that direction, but clearly there's a lot of room for honing.

In any case, if we get the ideas right, results can't help but flow faster—and that's what we're about. I think this paper deserves to be better known.

17-05-05 *Steane Paper* (to R. E. Slusher & S. J. van Enk)

I read the Steane paper that I mentioned yesterday over breakfast this morning, and it's every bit as good as I remembered it! You two should have a look at it too: `http://www.arxiv.org/abs/quant-ph/0003084`. It's easy reading, but it's deep reading. And it touches on a lot of points that Dick was worried about yesterday.

Quoting Steane, and inserting my own snide remark,

> The answer to the question 'where does a quantum computer manage to perform its amazing computations?' [this is Deutsch's standard rhetorical question showing that he has little imagination beyond his cooked-up many-worlds imagery] is, we conclude, 'in the region of spacetime occupied by the quantum computer'.

The paper's great! And looking back, it clearly needs to be better known! Citebase shows that it has only accrued 9 citations in the last 5 years.

18-05-05 *Being Bayesian, Pro or Con* (to T. Rudolph)

Now that I've decided I wholeheartedly like you again (never again steal my badge young man!), I've also decided that I'd like to invite you to our meeting "Being Bayesian in a Quantum World," August 1–5, in Konstanz. If I haven't too offended you by this delayed invitation, I hope you'll consider and let me know as soon as possible. On the off chance that you'll say yes, we've gone ahead and reserved a hotel room for you. We can definitely pay your local expenses, and will most likely be able to pay your travel expenses too.

In a day or two I'm going to start soliciting volunteers for talks. Though we plan to max out with a participation of exactly 50 people, I'm hoping for no more than 20–22 actual speakers—the rest will be "discussants" during the long discussion sessions after each pair of talks. Thus, having to have a talk to actually give is by no means mandatory.

18-05-05 *Coordinate Charts* (to G. L. Comer)

The point I didn't emphasize in my last note—where at least it looks like I was successful in emphasizing that, in all the formal developments I've seen, "coordinate chart" is a more basic notion than "differentiable manifold" (otherwise the definition of manifold would not make use of coordinate charts in any way)—is that this may mesh very well with what you wrote me:

Comerism 8: *General covariance is the process through which results in one coordinate system can be translated into results for another coordinate system.*

From that point of view, coordinate chart should indeed be primary, with differentiable manifold only secondary: "Differentiable manifold" is only the formal object that falls out of the *physical* statement that all coordinate systems should be smoothly translatable into one another (locally).

A little bit of these thoughts were on my mind when I wrote that old draft/notes "an information theoretic hole in general covariance" (or with such title), circa '91. You probably don't remember it because you probably thought I was a quack then (maybe you still do). Anyway, the wacky idea on my mind at the time was that *I hoped* that if one restricted oneself to *only* the coordinate charts that were computably (in the Turing sense) generatable from one another, one might not be able to distill a full-fledged manifold from the process but a different, more loosely connected structure for spacetime. In this way, I hoped to make sense of EPR phenomena by explaining that the two particles in it are not so far from each other after all.

With the years and the thoughts that have intervened, I know that those ideas were indeed very misplaced: They were indeed quackery. But it laid the groundwork for me to better appreciate your point: Coordinate systems are a more basic mathematical structure than manifolds for a rather deep reason. They are proxies for potential observers (agents, in my favored language), and the key issue is translatability.

20-05-05 *Room for One Very Last One* (to A. M. Steane)

Carl Caves, Stephan Hartmann, Rüdiger Schack and I are organizing a rather large (but invitation only) conference in Konstanz this summer, August 1-5, titled "Being Bayesian in a Quantum World." The reason I'm writing you is because we have one last spot and, as it turns out—literally by coincidence—I happened to read your paper "Quantum Computing Only Needs One Universe" again the other day. I was struck once again by how much I think you're on the right track in that paper, and I was re-energized to think we might have a good discussion along those lines if you were to be in Konstanz. I had already planned a discussion of measurement-based quantum computing, as Hans Briegel and Mike Nielsen will be there. But your participation would round out things even better.

Below, I'll paste in the original announcement. Also, let me refer you to the "secret" webpage for the meeting (we haven't gone public with it, and can't until a certain funding agency makes a decision): There you can read more about the topics that will be explored at the meeting, and also see the almost-final participant list. It's really—I think—going to be a fabulous meeting. There will be no more than 20-22 talks—i.e., not every participant will be speaking—so you won't be overloaded with talks. Plenty of discussion and thinking time.

If you're interested in coming, we can fund you completely. Just let me as soon as possible what your decision is.

23-05-05 *Whitehead and the Quantum* (to R. E. Slusher)

Too bad you're not here: I think you would have enjoyed the talks and exchanges between Abner Shimony and Shimon Malin about Whitehead's philosophy and what it might have to do with quantum mechanics.

I didn't remember that Bill Wootters had once written me about Malin's book until I ran across his old note with Google Desktop by accident a while ago. I'll paste it below as you might get something out of it too.

My talk went well today; an audience of 250 or so. Not too many fainted when I said, "Roughly speaking, a Bayesian is someone who believes there cannot be probabilities without gamblers."

04-06-05 *Quantum Bayesians & Anti-Bayesians* (to all BBQWers)

Thank you all once again for planning to participate in our upcoming meeting Being Bayesian in a Quantum World, Konstanz, 1–5 August 2005. It's going to be a great time, and I think we're going to get a lot done.

With that in mind, it's time to start planning the talks. There is now a conference webpage set up at

`http://web.archive.org/web/20090511060206/http://`
`www.uni-konstanz.de/ppm/events/bbqw2005/`

and there, along with various pieces of practical information, you can catch a glimpse of your fellow participants and some suggested research topics and questions. (However, please do keep the webpage private at the moment, making no links to it, etc., as certain issues with our funding agencies must be settled before we can go public. Nevertheless, the webpage is already there for your use.)

The final number of participants at the meeting is 47, and we think we have struck a good balance of Bayesian pro and con, physicist and philosopher, introvert and extrovert, and so on. From this group, ideally we'd like to have about 20–22 speakers—enough to give some focus to the meeting, but not so many as to make all our heads swim. In fact, the plan is to follow the method of the Seven Pines meetings that some of you have participated in before: Each day there will be four talks, two in the morning and two in the afternoon. The talks will be 45 minutes each, and following each pair, there will be a coffee break and then an extended roundtable discussion (45 minutes to 1 hour) to further explore the topic at hand. There may be one evening session to help compensate for an afternoon off. The organizers will try to pair up the talks so that they complement one another and make for an even discussion afterward.

With that in mind, we'd like to start asking for volunteer speakers. If you would like to speak, please send me a tentative title and enough of an abstract that the organizers can get a feel for the content of your proposed talk. If there are too few volunteers, you can count on us to do some arm-twisting and guilt-tripping until we get what we want. If there are too many—the much preferred option—we will have to make some tough decisions, excising overlap, etc. In any case, it is expected that all participants will have a significant impact on each other, either through their lively exchanges at the roundtables or via their good-hearted debate over a beer or three.

Please let us know your thoughts as soon as possible. It'll be great to hear from each of you.

04-06-05 *Being Bayesian in a Quantum World* (to A. Cho)

I don't know if I ever told you how much I enjoyed the article you wrote for Science covering the discussions at the Seven Pines meeting last year. I mailed it to all my relatives saying, "Quail and pheasant? I thought it was chicken." But, particularly, I loved the napkin you pictured that showed a figure from my lecture in it!

Anyway, I'm writing you to see if you might be interested in repeating the experience in Konstanz, Germany this summer? I and a few others have organized what I think is a very nice meeting on information theoretic views of the quantum state—i.e., views centering around the view of the quantum state you depicted in that napkin. If you were interested in attending to cover the meeting, you would certainly be welcome!

To try to whet your appetite, I'll place the original conference announcement below. Also, you can have a look at our "secret" website (we haven't gone public yet) to see the final list of speakers, the conference program, the talk topics, etc. I think you'll note that we have some of the best of the best in quantum information and computing there—you'll have to take my word for it if you don't know the field—but in any case you'll probably at least know some of the more famous names (like Milburn, Unruh, Mermin, Nielsen).

Anyway, if you're interested, please do come. I think it is fair to say you would not be bored! Just let me know as soon as possible if you would like to come, so that we can make sure that there is still room at the conference hotel if you're interested.

04-06-05 *BBQW Sauce* (to H. M. Wiseman)

No, I'm just going to have to drop out of the Sydney workshop: I'm way overbooked, I've realized. I intend to write Huw about this in the next few hours. (I'm in Miami, making my way to Stockholm; plenty of email time.)

Anyway, to answer your question: See the mass mailing I just sent out.

Concerning Delirium Quantum, I'll post it soon … especially if you send me one more note nudging me in that direction. I wish you were going to be at this meeting in Sweden: I'm going to unveil the story of two islands (and its impact on the quantum Bayesian view): The Island of Nurturing Wives and the Island of Bad Girlfriends. (There is an alternative universe where the two islands are the Island of Supportive Husbands and the Island of Annoying Boyfriends. I'll be sure to mention both universes.) Anyway, I think you would find it efficacious, and I would value your feedback. Too bad.

In a hurry …

05-06-05 *MaxEnt 2005 Invitation, 2* (to K. H. Knuth)

I am so sorry I am running almost a month behind on replying to your email!

Please title my talk "Being Bayesian in a Quantum World" and maybe use the two paragraphs below as my abstract. I hope it is not too long, but now that I've spent a while working on it, I rather like it.

I hope to be able to drop this email off in London, on my way to Sweden. At the moment, I'm somewhere over the Atlantic.

> Abstract:
>
> To be Bayesian about probability theory is to accept that probabilities represent subjective degrees of belief and nothing more. This is in distinction to the idea that probabilities represent long-term frequencies or objective propensities. But, how can a subjective account of probabilities coexist with the existence of quantum mechanics? To accept quantum mechanics is to accept the calculational apparatus of quantum states and the Born rule for determining probabilities in a quantum measurement. If there were ever a place for probabilities to be objective, one might think it precisely here! (And many do.) This raises the question of whether Bayesianism and quantum mechanics are compatible at all. For the Bayesian, it only suggests that we should rethink what quantum mechanics is actually about. Is it "law of nature" or really more "law of thought," though "law of thought" conditioned by the particularities of our world?
>
> From transistors to lasers, the evidence abounds that we live in a quantum world. However, one should not confuse the quantum WORLD with quantum THEORY. In particular, one should not jump to the conclusion that wave functions are as successful as calculational tools as they are because they mirror some kind of elements of reality. A more Bayesian-like perspective is that if wave functions generate probabilities, then they too must be Bayesian degrees of belief, with all that such a radical idea entails. In particular, quantum probabilities have no firmer hold on reality than the word "belief" in the phrase "degrees of belief" already indicates. From this perspective, the only sense in which the quantum formalism mirrors nature is through the normative constraints it places on gambling agents who wish to better navigate through this (quantum) world in which they are immersed. It might be thought that this is rather thin information about nature itself—and thus that the whole view collapses into a kind operationalism or positivism—but the information is not insubstantial! To the extent that an agent should use quantum mechanics for his uncertainty accounting rather than some other theory tells us something about the world itself—i.e., the world independent of the agent and his particular beliefs at any moment. In this talk, I will try to shore up these ideas by showing what quantum mechanics looks like when represented using probability simplexes rather than Hilbert spaces. It can be done, and when done, one starts to get a feeling for how little quantum theory deviates from Bayesianism after all.

08-06-05 *Classical No-Cloning* (to J.-Å Larsson)

Two references:

1. C. M. Caves and C. A. Fuchs, "Quantum information: How much information in a state vector?" in *The Dilemma of Einstein, Podolsky and Rosen – 60 Years Later (An International*

Symposium in Honour of Nathan Rosen – Haifa, March 1995), edited by A. Mann and M. Revzen, Annals of The Israel Physical Society, vol. 12, pp. 226–257, 1996. `quant-ph/9601025`.

2. H. Barnum, C. M. Caves, C. A. Fuchs, R. Jozsa, and B. Schumacher, "Noncommuting Mixed States Cannot Be Broadcast," Physical Review Letters **76**(15), 2818–2821 (1996). [Reprinted in *Quantum Information and Quantum Computation*, C. Macchiavello, G. M. Palma, and A. Zeilinger eds. (World Scientific, Singapore, 2000), pp. 195–198.] `quant-ph/9511010`.

The '96 paper with the basic point was actually written first, but posted later.

09-06-05 *The Bell Quote* (to M. O. Scully)

What was that God, Buddha, Bell quote you challenged me to find? If you give it to me again, I'll bet decent odds I find it.

Marlan's Reply

> Bell said something like:
>
> > "What if all this led us to the existence of God or Buddha; wouldn't that be very, very interesting?"
>
> If you can find anything like that I would be grateful (and surprised).
>
> Concerning your book and Pauli—does the connection with Pauli have to do with his ideas on the objective and mystical as complementary (Bohr) opposites?

10-06-05 *Film (!) from 1927 Solvay Conference, on Web* (to H. Barnum)

Barnumism 11: *A good friend of mine, Alex Wilce (Univ. of Susquehana, Mathematics) shared the following link with me; the site has short home-movie film clips from the 1927 Solvay Conference:*

`https://www.youtube.com/watch?v=8GZdZUouzBY`

Here's the description from the website:

> *Following is a "home movie" shot by Irving Langmuir, (the 1932 Nobel Prize winner in chemistry). It captures 2 minutes of an intermission in the proceedings. Twenty-one of the 29 attendees are on the film. The film opens with quick shots of Erwin Schrödinger and Niels Bohr. Auguste Piccard of the University of Brussels follows and then the camera re-focuses on Schrödinger and Bohr.*

That is really great! It cheered me up in this annoyingly delayed-flight time while I twiddle my thumbs in the American Airlines club.

13-06-05 *Wonderful Meeting* (to A. Y. Khrennikov & C. Eriksson)

Now that I'm home (... though I am just about to turn around to go to Europe again, for the J. T. Lewis Memorial Meeting), I wanted thank you both for a very fine meeting. I got lots of ideas at this one, and think it's rejuvenated my research—I have not felt this good in a long time. You guys are performing a valuable, valuable service to the physics community.

19-06-05 *Philosopher's Stone* (to G. L. Comer)

[...] The week turned out to be quite productive, and ideas started flowing.

The main thing on my mind has been viewing the quantum measurement process as a kind of modern version of alchemy. I guess I've been talking about this on and off for several years now, having been exposed to the analogy through Pauli's letters to Jung and Fierz, but the transformation in Sweden was that the stuff really got into my way of thinking, and I could start to see how to formalize it. I'm toying with the idea of writing a paper titled, "The Consequences of Our Interventions," to try to redo my paper with Asher "Quantum Theory Needs No Interpretation" from this new perspective. In that paper, we had written,

> The thread common to all the nonstandard "interpretations" is the desire to create a new theory with features that correspond to some reality independent of our potential experiments. But, trying to fulfill a classical worldview by encumbering quantum mechanics with hidden variables, multiple worlds, consistency rules, or spontaneous collapse, without any improvement in its predictive power, only gives the illusion of a better understanding. Contrary to those desires, quantum theory does *not* describe physical reality. What it does is provide an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions. This strict definition of the scope of quantum theory is the only interpretation ever needed, whether by experimenters or theorists.

First off, I wish I had never said, "quantum theory does not describe physical reality"—I really only meant "the wave function does not describe reality" and should have stuck with that formulation. But more importantly, what precisely are these "consequences of our interventions"? From the wording we used, one surely gets the impression that, whatever they are—we said "detector clicks," but what a glib phrase!—they somehow live outside of the agent performing the experiment. And I guess that's what I thought at the time.

Now I'm quite internal about it all; those "consequences" really live in the agent himself, not in the quantum system or even in some "detached" measurement device. And I think I now have a clear enough formulation of the idea that I ought to go public with it.

Quantum THEORY (I emphasize theory to draw a distinction between quantum theory and the quantum world, i.e., that stuff outside of us which is independent of the agent) is predominantly about the changes we bring about within ourselves by interacting with the external world. Very little of quantum theory is about the external world itself. Particularly, the clicks are not "out there," but rather "in here."

I've been scouring the Pauli quotes I have in my computer to see exactly where I got these ideas from, and so far I haven't read nearly as forceful of a version of it as I seem to remember—so I'm pretty sure I still haven't quite found the right passage. The only thing that comes close, so far, is this passage from Pauli's article on Kepler:

> Now, there is a basic difference between the observers, or instruments of observation, which must be taken into consideration by modern microphysics, and the detached observer of classical physics. By the latter I mean one who is not necessarily without effect on the system observed but whose influence can always be eliminated by determinable corrections. In microphysics, however, the natural laws are of such a kind that every bit of knowledge gained from a measurement must be paid for by the loss of other, complementary items of knowledge. Every observation, therefore, interferes on an indeterminable scale both with the instruments of observation and with the system observed

and interrupts the causal connection of the phenomena preceding it with those following it. This uncontrollable interaction between observer and system observed, taking place in every process of measurement, invalidates the deterministic conception of the phenomena assumed in classical physics: the series of events taking place according to pre-determined rules is interrupted, after a free choice has been made by the beholder between mutually exclusive experimental arrangements, by the selective observation which, as an essentially non-automatic occurrence (Geschehen), may be compared to a creation in the microcosm or even to a transmutation (Wandlung) the results of which are, however, unpredictable and beyond human control.

> In this way the role of the observer in modern physics is satisfactorily accounted for. The reaction of the knowledge gained on the gainer of that knowledge (Erkennenden) gives rise, however, to a situation transcending natural science, since it is necessary for the sake of the completeness of the experience connected therewith that it should have an obligatory force for the researcher (für den Erkennenden verbindlich). We have seen how not only alchemy but the heliocentric idea furnishes an instructive example of the problem as to how the process of knowing is connected with the religious experience of transmutation undergone by him who acquires knowledge (Wandlungserlebnis des Erkennenden). This connection can only be comprehended through symbols which both imaginatively express the emotional aspect of the experience and stand in vital relationship to the sum total of contemporary knowledge and the actual process of cognition. Just because in our times the possibility of such symbolism has become an alien idea, it may be considered especially interesting to examine another age to which the concepts of what is now called classical scientific mechanics were foreign but which permits us to prove the existence of a symbol that had, simultaneously, a religious and a scientific function.

Or maybe I picked it up from this passage in Heisenberg's article, "Wolfgang Pauli's Philosophical Outlook,"

> The elaboration of Plato's thought had led, in neo-Platonism and Christianity, to a position where matter was characterized as void of Ideas. Hence, since the intelligible was identical with the good, matter was identified as evil. But in the new science the world-soul was finally replaced by the abstract mathematical law of nature. Against this one-sidedly spiritualizing tendency the alchemistical philosophy, championed here by Fludd, represents a certain counterpoise. In the alchemistic view "there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only 'Deo concedente.'" The governing symbol for this magical view of nature is the quaternary number, the so-called "tetractys" of the Pythagoreans, which is put together out of two polarities. The division is correlated with the dark side of the world (matter, the Devil), and the magical view of nature also embraces this dark region.

Or finally, maybe from this letter from Pauli to Fierz:

> All of this then led me onto further, somewhat more phantastic [*sic*] paths of thought. It might very well be that we do not treat matter, for example viewed in the sense of *life*, "properly" if we observe it as we do in quantum mechanics, *specifically when doing so in complete ignorance of the inner state of the "observer."*
>
> It appears to me to be the case that the "after-effects" of observation which were ignored would *still* enter into the picture (as atomic bombs, general anxiety, "the Oppenheimer case" e.g. etc.), but in an *unwanted form*. The well-known "incompleteness" of quantum mechanics (Einstein) is certainly an existent fact somehow-somewhere, but certainly cannot be removed by reverting to classical field physics (that is only a "neurotic misunderstanding" of Einstein), it has much more to do with *integral relationships between "inside" and "outside" which the natural science of today does not contain* (but which alchemy had suspected and which can also be detected in the symbolics of my dreams, about which I believe them specifically to be characteristic for a contemporary physicist).
>
> With these vague courses of thought I have reached the border of that which is recognizable today, and I have even approached "magic." (From this standpoint observation in quantum mechanics might even appear to someone as a "black mass" after which the "ill-treated" matter manipulates its counter-effect against the "observer," thereby "taking its revenge," as a "shot being released from behind"). On this point I realize well that this amounts to the threatening danger of a regression into the most primitive superstition, that this would be much worse than Einstein's regressive remaining tied to classical field physics and that everything is a matter of holding onto the positive results and values of the *ratio*.

Still I have the nagging feeling that there is something much better if I could just dig it up.

Anyway, the way I view quantum measurement now is this. When one performs a "measurement" on a system, all one is really doing is taking an ACTION on that system. From this view, time evolutions or unitary operations etc., are not actions that one can take on a system; only "measurements" are. Thus the word measurement is really a misnomer—it is only an action. In contradistinction to the old idea that a measurement is a query of nature, or a way of gathering information or knowledge about nature, from this view it is just an action on something external—it is a kick of sorts. The "measurement device" should be thought of as being like a prosthetic hand for the agent—it is merely an extension of him; in this context, it should not be thought of as an independent entity beyond the agent. What quantum theory tells us is that the formal structure of all our possible actions (perhaps via the help of these prosthetic hands) is captured by the idea of a Positive-Operator-Valued Measure (or POVM, or so-called "generalized measurement"). We take our actions upon a system, and in return, the system gives rise to a reaction—in older terms, that is the "measurement outcome"—but the reaction is in the agent himself. The role of the quantum system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.

Reciprocally, there should be a transmutation of the system external to the agent. But the great trouble in quantum interpretation—I now think—is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory actually talks about, the "measurement outcome") takes place. It should be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. It is only a hint of that more interesting transmutation. And, as you know, somehow out of all this I think of the agent and the observer as being, together, involved in a little act of creation that ultimately has an autonomy of its own—that's the sexual interpretation of quantum mechanics. (However the ideas in that last sentence are a little murkier than what I'm trying to get at now.)

Does "measurement" in this new sense explicitly require consciousness (whatever that is)? I don't think so. But it does require some kind of nonreductive element—some kind of higher-level description that cannot be reduced to a lower-level one. Here's the way I've been putting it in my last three lectures, when talking more particularly about quantum mechanics from the Bayesian perspective. I point out that a Bayesian is, roughly speaking, someone who believes that without gamblers, there cannot be probabilities. Probabilities are not external to gamblers. Then someone always asks, must you have consciousness to have probabilities? And I say, "No." Take as an example my laptop computer loaded with a Bayesian spam filter. It is a perfectly good gambler in the Bayesian sense, but I think most people would be hard-pressed to call it conscious. Similarly, I think we're going to ultimately learn an analogous lesson about all this transformation/transmutation/creation/measurement business.

On the other hand, I do find myself being tickled toward a more Whiteheadian-like view that, whatever this higher-level description is, every piece of nature has more or less of it, from people all the way to stones and atoms. It's just that you have to be on the inside of the philosopher's stone to see it.

30-06-05 *Wittgensteinian Arm Twisting* (to H. Price)

One of the people I was really hoping would volunteer to talk at Konstanz was you! Since, however, I haven't heard anything from you, maybe it's time to do a little arm-twisting. Particularly, I wonder if I could entice you to give a talk that shuffles around in some way or other, or gives an introduction to, Wittgenstein's book *On Certainty*? As I recall, you'll be going to the Wittgenstein symposium the very next week, so he'll certainly already be on your mind. This aspect of his thought may not be on your mind for *that* meeting, but without doubt you'll be the closest we have to an expert on the subject at *this* meeting!

Still more particularly, I would love some sort of discussion to build up about the ties (or contradictions) between Wittgenstein's notion of certainty and the radical Bayesian notion of certainty (à la de Finetti and Richard Jeffrey)—i.e., that even "certainty" is a state of mind, just like a 50/50 assignment (or any other one, for that matter). (Compare how Wittgenstein says certainty is a "tone of voice".)

If you'd take the task, I'd be forever grateful. And I can supply you with relevant passages in Jeffrey's writings and even some of our debate with Mermin to give you something of a foil to work against, if you'd like.

It'd be really great to have a discussion of this subject, and there's no doubt you'd be the right man to lead it.

Huw's Reply

> I'm afraid there's no way I could talk about *On Certainty*. I'm not much of a Wittgenstein scholar in any case, and what little I do know (certainly) doesn't include anything about *On Certainty* (I've been invited to the Kichberg meeting as an expert on the arrow of time, not Wittgenstein!) Sorry!

30-06-05 *Flattered* (to A. Duwell)

I'm flattered by your abstract! A couple of typos though that maybe you can fix:

> **Sorting the objective from the subjective: remarks on Fuchs**
>
> Chris Fuchs seeks to identify the features of quantum theory that reflect the structure of the world and are not artifacts of the quantum formalism. To this end, Fuchs advocates attempting to interpret as many elements of quantum mechanics as reflective of subjective beliefs about quantum systems [as possible??]. The elements of the theory that resist this subjective interpretation are thereby candidates for reflecting elements of reality. The obvious problem with this methodology is that just because one can interpret an element of a theory subjectively, doesn't mean one ought to interpret it subjectively. In this paper I advocate the use of eliminative or demonstrative induction as a partial solution to the problem of sorting subjective from objective elements of theories. I discuss the application of this method to quantum theory and the limitations of the [what comes here??]

30-06-05 *Konstanz Talk?* (to J. B. M. Uffink)

You were one of the guys I was really hoping to hear from regarding a talk at the BBQW meeting in Konstanz, but I haven't heard anything from you! If you'd take the task we'd certainly be delighted! Particularly, even something like the talk you gave at LSE last year could be very useful. Might we entice you to give your thoughts on anything around the subject matter—it could be pro or con, it doesn't matter—I just want to get a lively discussion going. Or if you wanted to use this as an opportunity to formalize your thoughts on Everettian attempts to derive the quantum probability rule that could be very useful too. Whatever. The main thing is, it'd be great to see your clarity of thought in action with respect to this cluster of ideas.

Please let me know as soon as possible whether you think you could give us a talk, or whether you're definitely going to decline (despite my heartfelt plea!).

30-06-05 *A BBQW Talk?* (to A. Hagar)

Since I haven't heard back from you following my call for "volunteers", I wonder if we might entice you to submit a talk proposal for the BBQW meeting? Particularly, if you still think the BBQW way is "doing quantum mechanics in the dark" it'd be great if you'd organize your ideas on the subject afresh and give us a presentation. It could be a lively discussion point. If you have a tentative title and abstract, it'd be great to throw it in the pot of possibilities. We'll probably be making our talk decisions by the end of next week.

30-06-05 *New Delirium* (to H. M. Wiseman)

I'm sorry: I've been very slow to reply to your proposal for a talk at BBQW. I think it sounds great, and almost surely we'll pick it from the pool of potential talks. We'll let everyone know the final verdict next week (but as I say, you might as well start planning your talk now).

I like the phrase in your abstract, "... accepting that quantum mechanics is incomplete" because if you nuance it enough, and take out the idea of hidden variables, I'd even agree with it. Thus, thinking about that, I decided to extend Delirium Quantum a little with two more excerpts before posting it for you. Particularly, the new Sections 6 and 7 might give you a way of thinking "quantum mechanics is incomplete" in a way that you haven't thought before. I'll attach the new version. [See "Delirium Quantum" `arXiv:0906.1968v1`.]

Looking much forward to this meeting and sparring with you there.

01-07-05 *Bell, Buddha, Pauli* (to M. O. Scully)

Thanks for the Bell phrase to be on the lookout for. I'll keep my eyes peeled, and if I find something I'll let you know.

Scullyism 1 : *Concerning your book and Pauli—does the connection with Pauli have to do with his ideas on the objective and mystical as complementary (Bohr) opposites?*

My original interest in Pauli had more to do with a point he made over and over about how A) the quantum mechanical observer takes *part* in a little act of creation with each quantum measurement (very John Wheelerian also), yet B) there remains a kind of objective reality within the quantum description in that the observer cannot control the outcome of that creation. The observer is participatory, but not overpowering. Here it is, in one version, in Pauli's own words:

> In the new pattern of thought we do not assume any longer the detached observer, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.
>
> Nevertheless, there remains still in the new kind of theory an objective reality, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen.
>
> Like an ultimate fact without any cause, the individual outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case . . .

It's the conjunction of all that that I call "the Paulian idea" (and thus the title of my book, which is really an elaboration of those ideas). The part after the "Nevertheless" is particularly important.

However, I am aware of the complementarity you mention, though with Pauli it was more precisely a complementarity between "physical" and "psychic" or "psychological". He devoted a lot of discussion with his assistant Markus Fierz and with the psychologist Carl Jung to try to flesh out a "neutral language" that would somehow be able to drop the distinction between "physical" and "psychic". I'll give you an example of the sort of stuff he was up to, by pasting in a letter that Pauli had written to Jung below. I've got a lot of material like that in my upcoming posting to `quant-ph`, "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature."

Anyway, that sort of idea is starting to intrigue me more now than it used to. Another interesting fellow to read along those lines is William James, the great American pragmatist.

I'll look for the Bell quote.

Letter from Pauli to Jung, 31 March 1953:

The labeling of ideas as either of spiritual origin or physical (or physiological) origin and your corresponding definition of physics as a science of ideas of the second kind has revived memories of my youth.

Among my books, there is a somewhat dusty case containing a Jugendstil silver goblet, and in this goblet there is a card. A gentle, benevolent, and cheerful spirit from days of yore seems to be issuing forth from this goblet. I can see him shaking your hand in a friendly way, welcoming your definition of physics as a pleasing, albeit somewhat belated, indication of your insight and understanding; he goes on to add how suitable the labels are for his laboratory, and expresses his satisfaction at the fact that metaphysical judgments in general (as he was wont to say) "have been relegated into the realm of the shadows of a primitive form of animism." This goblet is a baptism goblet, and on the card it says in an old-fashioned ornate script: "Dr. E. Mach, Professor at the University of Vienna." It so happened that my father was very friendly with his family, and at the time totally under his influence mentally, and he (Mach) kindly agreed to take on the role of my godfather. He must have had a much stronger personality than the Catholic priest, with the apparent result that I was thus baptized in an antimetaphysical manner rather than in a Catholic one. Be that as it may, the card remains in the goblet, and despite all the great mental changes I went through later on, it remains a label that I myself bear—namely: "of antimetaphysical origin." And in fact, to put it in a somewhat simplistic way, Mach regarded metaphysics as the root of all evilin this world—in other words, in psychological terms, as the Devil himself—and that goblet with the card remained as a symbol of the *aqua permanens* that keeps evil metaphysical spirits at bay.

I do not need to describe Ernst Mach more closely, for if you look at your own description of the extroverted sensation type, then you will see E. Mach. He was a master at experimentation, and his apartment was crammed full of prisms, spectroscopes, stroboscopes, electrostatic machines, and the like. Whenever I visited him, he always showed me some neat experiment, already completed, partly so as to eliminate unreliable thinking, with the ensuing illusions and errors, and partly to support it and correct it. Working on the assumption that his psychology was a universal one, he recommended everyone to use that inferior auxiliary function as "economically" as possible (thought economy). His own thought processes closely followed the impressions of his senses, tools, and apparatus.

This letter is not meant to be a history of physics, nor the classical case of type opposites: E. Mach and L. Boltzmann, the thinking type.[81] I last saw Mach just before the First World War, and he died in 1916 in a country house near Munich.

What is interesting in connection with your letter is Mach's attempt to fall back on psychic facts and circumstances (sensory data, ideas) within the realm of physics as well and especially to eliminate as far as possible the concept of "matter." He regarded this "auxiliary concept" as grossly overrated by philosophers and physicists and viewed it as a source of "pseudo problems." His definition of physics basically coincided with

[81] I should like to recount an anecdote here because I am sure you will really enjoy it. Mach, who was by no means prudish and was most interested in all the intellectual trends of the day, once pronounced judgment on the psychoanalysis of Freud and his school. He said: "These people try to use the vagina as if it were a telescope so that they can see the world through it. But that is not its natural function—it is too narrow for that." For a while this became a popular quotation at the University of Vienna. It is very typical of Mach's *instrumental* way of thinking. For him, psychoanalysis immediately conjures up the vividly concrete image of the wrongly applied instrument: namely, the female organ where it does not belong.

the one proposed by you, and he never failed to stress that physics, physiology, and psychology were "only different in the lines of investigation they took, not in the actual object," the object in all cases being the constant psychic "elements" (he exaggerated their simplicity somewhat, for in reality they are always very complex). I was surprised that despite your sweeping criticism of what later came to be called *"Positivism"* (Mach used this term a great deal), there are nevertheless also fundamental similarities between you and this line of thought: In both cases there is the *deliberate elimination of thought processes.* And of course there is nothing at all wrong with these labels for ideas and the corresponding definition of physics, especially as it accords perfectly with the idealistic philosophy of Schopenhauer, who consciously uses "Idea" and "Object" synonymously. But it all depends on *how one proceeds.* What Mach wanted, although it could not be carried out, was the total elimination of everything from the interpretation of nature that is "*not* ascertainable *hic et nunc.*" But then one soon sees that one does not understand anything—neither the fact that one has to assign a psyche to others (only one's own being ascertainable) nor the fact that different people are all talking about the same (physical) object (the "windowless monads" in Leibniz). Thus, in order to meet the requirements of both instinct and reason, one has to introduce some *structural elements of cosmic order*, which "in themselves are not ascertainable." It seems to me that with you this role is mainly taken over by the archetypes.

It is right that what one does or does not call "metaphysics" is, to a certain extent, a matter of taste. And yet I agree with you totally that in practical terms, great value is to be attached to the demand that metaphysical judgments be avoided. What is meant by that is that the "not in themselves ascertainable" factors (concepts) that have been introduced do not completely escape the controlling, checking mechanism of experience, and that *no more of them may be introduced than is absolutely necessary*: They serve the purpose of *making statements about the possibility of ascertainments hic et nunc.* This was the sense in which the concept of "possibility" was meant, and it was in this sense that I called such concepts *"symbolic things in themselves"* and the *"rational aspect of reality."* As you rightly point out, *there is absolutely no need to make statements of Being in the metaphysical sense about these "things in themselves."* In the natural sciences, one makes the *pragmatic statement of usefulness* about them (in order to understand the ordering system of the ascertainable); in mathematics there is just the *formal logical statement of consistency.* In psychology, those "not in themselves ascertainable" concepts include, the unconscious and the archetypes, and in atomic physics, they include the totality of the characteristics of an atomic system that are *not all simultaneously* "ascertainable *hic et nunc.*"

In my last letter, I referred to that which is actually "ascertained *hic et nunc*" as "concrete phenomenon" and the "irrational aspect of reality." It is always present in the psyche of an observer, whatever the "label of origin" might be. At this point, however, the question arises of whether the description "psychic" or the term "psyche" can go *further* than the "ascertainable *hic et nunc.*" I am inclined to reply to this question in the negative and to take the "not in themselves ascertainable" structures, which are introduced as conceptual indications of possibilities of the ascertainable, and give them the definition *"neutral"* and *not the definition "psychic."*

To me, this view also seems to be supported by Plato's expressions *meson* (middle) and *tritoneidos* (third form), which both meet my requirements for "neutrality" (=middle position), nay, actually seem to *emphasize* it. Plato certainly had the word "psyche" at his disposal, and if he opts to use a different word instead, then it must be one with a deeper meaning, one that calls for careful consideration. For me, this deeper meaning lies in the need to make a clear distinction between the experience of the individual, which exists in his psyche as something ascertainable *hic et nunc*, and the general concepts, which, "nonbeing ascertainable in themselves," are suitable for taking up a middle position. Your *identification of psyche = tritoneidos* thus seems to me a *retrograde step*, a loss in terms of conceptual differentiation.

With my call for "neutral" general concepts, I find myself in agreement with your article "The Spirit of Psychology," which struck me as fundamental, especially when you say: "The archetypes have ... a nature which one cannot definitely describe as psychic. Although by the application of purely psychological considerations I, have come to question the solely psychic nature of the archetypes, etc." I feel that you should certainly *take these doubts seriously and not once again* make too much of the *psychic factor*. When you say that "the psyche is partly of a material nature,"[82] then for me as a physicist this takes on the form of a metaphysical statement. I prefer to say that psyche and matter are governed by common, neutral, "not in themselves ascertainable" ordering principles. (Unlike the psychologist, the physicist has no problem, for example, with saying "the U field" instead of "the unconscious," which would thus establish the "neutrality" of the concept.)

But I wish to make it quite clear that my hope that you might agree with this general point of view is based on the impression that some of the *pressure needs to be removed* from your analytical psychology. The impression I have is of a vehicle whose engine is running with overloaded valves (expansion tendency of the concept "psyche"); that is why I should like to relieve some of the pressure and let off steam. (I shall come back to this later on p. 10 below).

I would also hope that a clarification of the scope of the concept of the psyche might include your *de iure* recognition of the fact that the heart is not just a psychological symbol but also a conception labeled "of physical origin." Economy with the inferior function on the lines of E. Mach often serves to fulfill a function, even if it is not actually that of thinking!

07-07-05 *Hook or Crook* (to K. H. Knuth)

OK, I booked some flights now. But I had to get tricky to trim the price down. The way I did it was to book TWO separate tickets: One roundtrip between Munich and Brussels, and one roundtrip between Brussels in San Jose. [...]

Here's the schedule: [...]

Another request: I know there are going to be a lot of pressures on you by being the organizer of this meeting, but might you be able to find some time August 10 to give me a private rendition of the tutorial "Probability and Entropy: Statements and Questions," you're giving the first day. Or at least a gentle introduction to your work on the logic of questions? I'd like very much to use this opportunity to get those ideas pumped into my head.

I think that's it for now. I'll try my best to give you guys a rousing talk. At these travel prices, I certainly won't be giving you your money's worth, but I'll try my best to inspire the young ones that there is a wide-open research field out there for being Bayesian in a quantum world.

[82] Your letter, p. 6. This statement seems to me alarmingly close to the definitely *meaningless* statement that "everything is psychic"! For "psychic" to acquire any meaning, there must also be something nonpsychic involved, and this is where the "not in itself ascertainable" seems to me suitably *neutral*.

Just because you're Bayesian, it doesn't mean you have to believe in hidden variables underneath quantum mechanics: The world is way more interesting than that.

08-07-05 `Your Touch, My Touch` (to M. S. Leifer)

Concerning:

Leiferism 1 : *The main point is that it is not necessary to include "mental readjustment" unitaries in a world that only includes a single agent. They are only required if a measurement is made by a second agent who has incompatible information. To summarize, I would consider replacing your motto "quantum mechanics describes a world that is sensitive to our touch" by "QM describes a world in which my information is sensitive to your touch, your information is sensitive to my touch, but my information isn't necessarily sensitive to my touch and yours isn't necessarily sensitive to your touch." (less catchy I know).*

But as you say, more accurate.

Anyway, this sounds strongly akin to something I once wrote in the introduction to `quant-ph/0009101` (a paper with Kurt Jacobs). Read that intro—as you get a chance!—and tell me whether that meshes with your motto. Particularly, note the stuff that starts up at the passage:

> Note the two ingredients that appear in this formulation. First, the information gathering or measurement is grounded with respect to one observer (in this case, the eavesdropper), while the disturbance is grounded with respect to another (here, the sender). In particular, the disturbance is a disturbance to the sender's previous information-this is measured by his diminished ability to predict the outcomes of certain measurements the legitimate receiver might perform. No hint of any variable intrinsic to the system is made use of in this formulation.

Don't forget the footnotes.

Speaking of mottos, since I still haven't written that note about the Växjö insight, let me try to put it in a single sentence. The extra unitary captures the agent's *judgment* of how he should conditionalize believing what he does about how he interacts with the "measured" system. The unitary is literally about the extent he judges he should reject Goldstein conditionalization. We know from Richard Jeffrey's remarks that even the method of updating is a judgment (that's why he mentions in one of those papers even wilder things than Goldstein). That's the idea anyway—that diachronic coherence relies itself on a prior judgment—and I'd like to see if that can be made precise (and applicable to the quantum case).

08-07-05 *The Bell Quote, at your request* (to M. O. Scully)

It took some devious ways, but I found it. Here's the part you were most interested in:

> Suppose for example that quantum mechanics were found to resist precise formulation. Suppose that when formulation beyond FAPP is attempted, we find an unmovable finger obstinately pointing outside the subject, to the mind of the observer, to the Hindu scriptures, to God, or even only Gravitation? Would not that be very, very interesting?

It's probably best, however, to look at the extended quote below to get a more complete sense of what he was getting at. I'll place the extended quote at the end of this note.

The source is Bell's article: J. S. Bell, "Against 'Measurement'," Physics World 3, pp. 33–40 (1990).

I believe the advertised reward for finding this stuff was \$100? Maybe I have the exact number wrong. In any case, I'd be happier to take a faculty appointment at Princeton instead.

Extended Bell Quote:

SURELY, after 62 years, we should have an exact formulation of some serious part of quantum mechanics? By 'exact' I do not of course mean 'exactly true'. I mean only that the theory should be fully formulated in mathematical terms, with nothing left to the discretion of the theoretical physicist ... until workable approximations are needed in applications. By 'serious' I mean that some substantial fragment of physics should be covered. Nonrelativistic 'particle' quantum mechanics, perhaps with the inclusion of the electromagnetic field and a cut-off interaction, is serious enough. For it covers 'a large part of physics and the whole of chemistry' (P A M Dirac 1929 Proc. R. Soc. A **123** 714). I mean too, by 'serious', that 'apparatus' should not be separated off from the rest of the world into black boxes, as if it were not made of atoms and not ruled by quantum mechanics. ...

[Dirac] divided the difficulties of quantum mechanics into two classes, those of the first class and those of the second. The first-class difficulties concerned the role of the 'observer', 'measurement', and so on. Dirac thought that these problems were not ripe for solution, and should be left for later. He expected developments in the theory which would make these problems look quite different. It would be a waste of effort to worry overmuch about them now, especially since we get along very well in practice without solving them.

Dirac gives at least this much comfort to those who are troubled by these questions: he sees that they exist and are difficult. Many other distinguished physicists do not. ...

I agree with ["the most sure-footed of quantum physicists"] about [this]: ORDINARY QUANTUM MECHANICS (as far as I know) IS JUST FINE FOR ALL PRACTICAL PURPOSES.

Even when I begin by insisting on this myself, and in capital letters, it is likely to be insisted on repeatedly in the course of the discussion. So it is convenient to have an abbreviation for the last phrase: FOR ALL PRACTICAL PURPOSES = FAPP.

I can imagine a practical geometer, say an architect, being impatient with Euclid's fifth postulate, or Playfair's axiom: of course in a plane, through a given point, you can draw only one straight line parallel to a given straight line, at least FAPP. The reasoning of such a natural geometer might not aim at pedantic precision, and new assertions, known in the bones to be right, even if neither among the originally stated assumptions nor derived from them as theorems, might come in at any stage. Perhaps these particular lines in the argument should, in a systematic presentation, be distinguished by this label — FAPP — and the conclusions likewise: QED FAPP.

I expect that mathematicians have classified such fuzzy logics. Certainly they have been much used by physicists. But is there not something to be said for the approach of Euclid? Even now that we know that Euclidean geometry is (in some sense) not quite true? Is it not good to know what follows from what, even if it is not really necessary FAPP? Suppose for example that quantum mechanics were found to resist precise formulation. Suppose that when formulation beyond FAPP is attempted, we find an unmovable finger obstinately pointing outside the subject, to the mind of the observer, to the Hindu scriptures, to God, or even only Gravitation? Would not that be very, very interesting?

Marlan's Reply

Very impressive—well done!

A 200$ check "in the mail". How about coming down to Princeton next week for lunch?

14-07-05 `hput diracqphsub.tex` (to H. Barnum)

Barnumism 12: *Make sure you mention to your boss that the paper primarily concerns states that as far as we know, aren't physical!*

I think he's already well aware that almost everything I do is unphysical . . . by his standards of the word "physical" that is. But he's good to me nonetheless. (We're even planning a pilgrimage to the home of C. S. Peirce together.)

15-07-05 *Krugman on Reality, Again* (to me)

From Paul Krugman, "Karl Rove's America," *New York Times*, 15 July 2005:

> John Gibson of Fox News says that Karl Rove should be given a medal. I agree: Mr. Rove should receive a medal from the American Political Science Association for his pioneering discoveries about modern American politics. The medal can, if necessary, be delivered to his prison cell.
>
> What Mr. Rove understood, long before the rest of us, is that we're not living in the America of the past, where even partisans sometimes changed their views when faced with the facts. Instead, we're living in a country in which there is no longer such a thing as nonpolitical truth. In particular, there are now few, if any, limits to what conservative politicians can get away with: the faithful will follow the twists and turns of the party line with a loyalty that would have pleased the Comintern.
>
> I first realized that we were living in Karl Rove's America during the 2000 presidential campaign, when George W. Bush began saying things about Social Security privatization and tax cuts that were simply false. At first, I thought the Bush campaign was making a big mistake – that these blatant falsehoods would be condemned by prominent Republican politicians and Republican economists, especially those who had spent years building reputations as advocates of fiscal responsibility. In fact, with hardly any exceptions they lined up to praise Mr. Bush's proposals.
>
> But the real demonstration that Mr. Rove understands American politics better than any pundit came after 9/11.
>
> Every time I read a lament for the post-9/11 era of national unity, I wonder what people are talking about. On the issues I was watching, the Republicans' exploitation of the atrocity began while ground zero was still smoldering.
>
> Mr. Rove has been much criticized for saying that liberals responded to the attack by wanting to offer the terrorists therapy – but what he said about conservatives, that

they "saw the savagery of 9/11 and the attacks and prepared for war," is equally false. What many of them actually saw was a domestic political opportunity – and none more so than Mr. Rove.

A less insightful political strategist might have hesitated right after 9/11 before using it to cast the Democrats as weak on national security. After all, there were no facts to support that accusation.

But Mr. Rove understood that the facts were irrelevant. [...]

[W]hat we're getting, instead, is yet another impressive demonstration that these days, truth is political.

15-07-05 *Arm Twisting* (to N. Lütkenhaus)

We never heard back from you when we made a call for speakers at BBQW in Konstanz. Thus maybe it's time to do a little arm twisting now—particularly as we have been studying the mix of volunteered talks and it would be nice to have one or two more on "applications." Could we get you to give a talk on something to do with quantum crypto (predominantly) and any thoughts you have (even tangential or wildly speculative) on how the Bayesian view of quantum states might make a difference (or not) in the planning, design, or implementation of quantum cryptosystems? It would just make our program a little more solid and less philosophical to see some of these ideas in action.

If you can work up the nerve to take the task, it would be great! Remember this is a very informal meeting—only about science, and not about egos—so really you have nothing to lose, and you might spur some great discussions or applications with your talk.

Whatever your decision, please let me know asap.

17-07-05 *Take the Task!* (to D. M. Appleby)

I apologize for taking so long to get back to you. As it turns out, we finally made all the final talk decisions today (since Caves, Hartmann, Schack and I all happen to be in Waterloo). Anyway, the decision is Yes, please do give us a talk. And particularly, yes please do give us your passioned defense of the subjectivist account of probability. So, you've got a task ahead of you: But remember you basically asked for the challenge.

Your slot is on the first day, the third talk down the line. Speakers are in order:

Morning:

- Spekkens (toy model, and arguing that quantum states should be viewed epistemically)
- Schack (not only epistemically, but subjectively—because that's the only sensible account of probability and "objective chance" plays no role anyway—and the consequences of that view for quantum operations)

Afternoon:

- Appleby (you give your talk just as you had outlined; subjectivist ideas are so foreign to some it would be good to hear them twice even if there is any overlap with Schack; so basically don't worry about overlap)
- Myrvold (rebutting, tries to argue that objective chance really is needed, otherwise Bayesianism would collapse when confronted with a fundamentally probabilistic theory like QM)

- Timpson ("if the quantum state is to be associated with any cognitive state, it must be belief not knowledge," ... if you want to solve the "measurement problem")

Thus ends our arguing-for-the-subjective-view day.

Anyway, I hope that gives you a feeling for how you'll fit.

We'll post the full schedule in a couple of days; I'll send out a general announcement soon. In the meantime, could you please send us an official title and a short abstract ASAP (preferearably before Thursday).

Certainly thanks for doing this Marcus! As I've told you before, I think your papers on probabilities are gems.

18-07-05 *Your BBQW Talk* (to T. A. Brun)

Sorry to be getting back to you so late. Sadly, we've only finally made our final plans for the talks in Konstanz (Carl, Stephan, Ruediger and I are all together this week at a meeting in Waterloo).

Anyway, yes please do plan to give a talk on compatible state descriptions. You'll be talking on a more technical, less philosophical day, along with Mermin, Briegel (both on quantum computing things), Renner, and Srednicki (on de Finetti representation stuff). But don't let that deter you from making foundational statements too. Since we don't have any other representation of compatibility questions, your talk will be ideal—much needed as an intro to the question(s) and subject. More than likely, you can expect that Caves, Schack, or I will make some comments along the lines of our paper in the discussion session: so that could be a nice discussion.

Looking forward to seeing you soon!

18-07-05 *Your BBQW Talk* (to D. Wallace)

Thanks again for offering to give a talk at BBQW. I hope you don't mind my scratching my earlier approval of your "global criticism" suggestion: We've finally gotten to making decisions on the talks, and believe me, there's more than enough criticism of the Bayesian approach! (Myrvold, Mermin, Wiseman, Hagar, and probably a couple others).

Anyway, we think it'd be much more productive for you to talk about your decision theoretic derivation of the probability law in Everettian QM—actually that subject sorely needs some representation. Your talk will be the Wednesday morning. In the same session, we're hoping to also get Barnum to talk a little dually on your stuff and on Zurek's recent derivation. Also Carl Caves may talk on the frequency operator approach of Hartle and Farhi-Goldstone-Gutman.

We'll send out an announcement with the full talk schedule soon. However, in the meantime, if you could send an official title and a short abstract ASAP that'd be great!

I hope you'll find this meeting a productive one; in any case, I'm looking forward to seeing you again.

25-07-05 *Sessions and Chairs* (to V. Palge)

Below you'll find the session titles and chairs. Also, please change the title of my opening talk to "Fuchsian BBQ(w), a Sampler of Meats and Sauces". After lunch, I'll send you some extra abstracts from some of the speakers that I have accumulated. I'll be back in about an hour.

- Monday Morning
  "Epistemic Quantum States and the Paths They Lead To"
  Chair: Stephen Bartlett

- Monday Afternoon
  "Subjective vs. Objective Probabilities in QM"
  Chair: Jeffrey Bub

- Tuesday Morning
  "Implications from Quantum Computing"
  Chair: Mauro D'Ariano

- Tuesday Afternoon
  "Technical Bayesian Theorems"
  Chair: Norbert Lütkenhaus

- Wednesday Morning
  "Bayesian Probabilities in Everettian Worlds"
  Chair: Huw Price

- Thursday Morning
  "Updating Beliefs and Diachronic Coherence"
  Chair: Jos Uffink

- Thursday Afternoon
  "Updating Beliefs and a Touch of Copenhagen"
  Chair: Holger Lyre

- Friday Morning
  "Separating Subjective from Objective in QM (if possible!)"
  Chair: Paul Busch

- Friday Afternoon
  "Changing the Course of Physics, Some Ideas"
  Chair: Joseph Renes

25-07-05 *Bad Jokes* (to N. D. Mermin)

Regarding:

Merminition 152: *Please use the full title I sent you:*

> Does being Bayesian illuminate the quantum world?
> Or is the quantum world an embarrassment to the Bayesian?
>
> Abstract: The speaker will meditate on what he likes and what he dislikes about taking a Bayesian view of quantum theoretic probabilities. The talk will be very informal if only because, as we all know, there is no quantum world.

As you should know, I think there is ONLY a quantum world. Did you forget about me when you wrote your abstract?

Merminition 153: *Just an example of the kind of bad jokes I hoped I'd be able to string together. I had in mind the Bohr quote. Nothing more. But I did think it was a Law of Thought for you. Not the same as a world.*

I knew you had the Bohr quote in mind; I was trying to make my own bad joke.

Concerning, "But I did think it was a Law of Thought for you." ... you never cease to shock me .... And you never cease to cause me to strive to try to convey the very simple little idea more effectively! I wonder when I'm gonna finally hit the sweet spot? Quantum THEORY, a law of thought: Yes. Resoundingly yes. But the quantum WORLD—i.e., that situation, that world, that reality, which conditions us to choose THIS law of thought rather than THAT law of thought (in other words some alternative or imaginary law of thought)—is something else entirely. It's the stuff that's here whether there are any law-of-thoughters around or not. That's what I really want to get at; that's what I've always really wanted to get at.

26-07-05 *Am I Turning Into You?* (to S. Aaronson)

Aaronsonism 9: *Consider the terrifying evidence at*

`http://www.scottaaronson.com/papers/are.ps`

and in particular, the following sentence:

> "If exponentially-long strings were rocket fuel, and probability distributions were grape juice, then quantum states would be wine – the alcoholic 'kick' in this analogy being the minus signs."

Headed for an `arXiv` *near you (though there's still time to revise, and thereby reduce the foolishness quotient ...).*

For God's sake let's hope not! I've seen what it looks like from the inside, and I can tell you it ain't pretty.

However, I very much like this turn I'm seeing in you of dispelling *some* of the mystery of quantum computing by comparing quantum states to probability distributions. The exponentiality in the latter is something Caves and I have played up here and there over the years. Have you ever read our old paper, `quant-ph/9601025`? Caves probably has something better by himself by now, but we laid the groundwork there.

Anyway, you don't realize it, but you might just be starting to fall down the slippery slope of becoming a quantum Bayesian.

Incidentally, I'm starting to think there must be something wrong with the idea that it's the minus signs that give the kick. For the minus signs are representation dependent. Take the representation of QM that I keep playing up, where the quantum state is represented as a single probability distribution full stop. No minus signs at that level. Now it is true that the minus signs then reappear in the time evolution equations. But if one talks about the one-way computational model of Raussendorf and Briegel, even those minus signs go away. So a kind of probabilistic model for quantum computation with no minus signs ever appearing. What appears instead is an interesting relation between the marginal and joint event spaces. One that apparently can't be sustained classically. I wish I had your brain so that I'd be better equipped to explore these kinds of questions.

I'm finally going to go to Princeton today. I guess you won't be there!

27-07-05 *Wow!* (to M. Dickson)

I just saw your talk proposal for BBQW,

> Subjective and Objective in Quantum Theory
>
> Chris Fuchs has proposed to understand the quantum state in terms of the "Bayesian idea that the probability one ascribes to a phenomenon amounts to nothing other than the gambling commitments one is willing to make on it" (Fuchs 2003). Correctly predicting that jaws will drop in reaction to such an idea, Fuchs goes on to emphasize that it is part of a larger project of disentangling the subjective from the objective in quantum theory. This talk will be a reflection on that project. First, drawing on philosophical movements from the late 19th and early 20th century, specifically surrounding the philosophy of geometry and the role of analytic truths in physical theories, I will suggest that the interplay between subjective and objective in physical theory is complex and subtle, and itself depends on certain choices made by us. (Fuchs drops hints in the same direction. I will try to flesh out my own understanding of these subtleties.) Second, I will illustrate this idea, drawing on themes from Bohr (and possibly other Copenhagen-type approaches to quantum theory, but I make no claims on that point). Specifically, I will consider, as (I would argue) Bohr did, the question of how we make 'contact' between quantum state-ascriptions and actual experimental observations. The result is suggestive of a neo-Kantian (or neo-Carnapian) philosophical foundation for the 'subjectivist' approach to quantum theory, and in any case it might contribute to Fuchs's project.
>
> Reference: C. A. Fuchs, "Quantum Mechanics as Quantum Information, Mostly," J. Mod. Opt. **50**, 987 (2003).

and wow! I'm flattered that you'd take me seriously enough to talk about some of my stuff (even if only to speak to its difficulties).

I guess I've talked more about the specific subject of your abstract in several places (other than the paper you cite, that is), but maybe let me draw your attention to the most relevant one in case you haven't seen it yet—I think it's a substantial development of what you've seen (if that's all you've seen). It's a weird pseudo-paper titled "The Anti-Växjö Interpretation of Quantum Mechanics". You can find it here `http://xxx.lanl.gov/abs/quant-ph/0204146`, and the most relevant sections for you would be Sections 4 and 5.

I've had only one significant response to it from a philosopher—it was from Michel Bitbol. Since you will also talk of the neo-Kantian tradition, let me place it below. [See preply to 10-12-03 note "First Meeting" with M. Bitbol.]

I don't know if these materials will help you in any way—particularly at this late date—or only paint a more inconsistent picture of myself, but in the interest of making as much progress as we can in our limited time, I thought I should point them out to you.

I'm definitely looking forward to meeting you; I've only seen you in the distance before. And I'm looking forward to learning from you.

28-07-05 *Asynchrony!* (to A. Plotnitsky)

Plotnitskyism 14: *I assume that you got an invitation from George Welch for the Snowbird conference, since I just got one myself. I did manage to get in touch with David from Australia, who cannot come–too bad of course.*

No, I did not get anything from George Welch. Could you have him send whatever he sent again? (I don't know George Welch.) Coincidentally, I just had lunch with Marlan Scully a couple of days ago. We talked mostly about his son's book and about free will. Marlan has quite a religious side too—I had not realized the extent of that before.

At the very least, looking forward to seeing you in Snowbird … but I hope we hook up again before that.

Maybe I should give as a title for my talk, "Not the Incompleteness of Quantum Mechanics, but the Incompleteness of Bohr's Reply to EPR". Who are you going to get as a replacement for David?

28-07-05 *Calendar Marked* (to W. E. Lawrence)

OK, I've marked out my calendar for Sept 29 through Oct 1. I'll plan to drive in the Thursday morning (well before the afternoon talk), and take off Saturday morning after breakfast. Maybe there's some chance I can get my family to come with me; if I do, I'll certainly pay any extra hotel costs that incurs. I'll let you know their decision as the time draws nearer.

Have fun on your vacation. We're just taking off for Germany ourselves today. The family will be there for 3 weeks, as I shuttle about here and there for meetings. I'm very proud of the meeting I organized on Bayesian probability in quantum mechanics. You might snoop around the webpage if you get a chance:

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`.

I suspect there are several talks you would have enjoyed there. So, anyway, that part of my shuttling about will be a labor of love. The ridiculous part of my shuttling comes from my jumping back to San Jose for a two-day visit to the MaxEnt conference in the middle of all that! Now that there's no way for me to get out of it, I'm really starting to regret it! Thus much is now for sure though: From August 19, when we return to New Jersey, to September 29, when I come to Hanover, I'm not traveling anywhere else!!

So I should be fresh and happy again by the time I give your seminars.

28-07-05 *Bernardo and Smith and Savage* (to S. Hartmann & V. Palge)

Do either of you have a copy of Bernardo and Smith's book *Bayesian Theory* and Savage's book *Foundations of Statistics* or the book *Studies in Subjective Probability* (can't remember the editors)? Anyway, I'm bringing a few books, like Jeffrey's two books and de Finetti's collected papers, but I can't fit much more into my suitcase. If you guys have the above-said books (and maybe Howson and Urbach too), could you bring them to the meeting? We could put things like this out on a display table for perusing or if anyone needs something for a sleepless night. Also it'll make it easier for us to dig up a quote if need be.

Also, we should have a display table for the participants to lay out copies of recent papers, etc., if possible.

09-08-05 *Slides* (to H. M. Wiseman)

Would you mind sending me a copy of the talk you gave at BBQW? I'd like to study the slides and ultimately reply.

(You know, if I were a solipsist, I wouldn't have to ask you for the slides ... nor would I ever have to use the Born rule, as I'd know the outcomes of all measurements in advance. Solipsists have no need to make requests or gamble.)

09-08-05 *Figure Supplement* (to H. Poirier)

It dawned on me that this figure from my talks might be a useful supplement for you with respect to the paper I sent you last week. Unfortunately I don't have the latest version of it scanned into my computer, but this might still help. (The stick figure now says, "Ouch, *d*!" rather than "Aha, *d*!" Small conceptual difference, as "aha!" conveys a discovery, while "ouch!" conveys the idea of a simple physiological reaction.)

Anyway, the figure is attached. Looking forward to your article.

11-08-05 *So Slow Fuchs* (to A. Wilce)

Sorry to take so long to reply to you; I've fallen by the wayside again with this exhausting schedule of travels and particularly the Konstanz meeting. As I write to you, I'm returning to Munich from the MaxEnt meeting in San Jose.

Wilce-ism 1: *The conference has left my head buzzing with ideas, some of which I'd like to discuss with you at some point.*

Glad to hear that. And I always enjoy my discussions with you; I'm always impressed by your simultaneous mix of precision and tolerance (particularly for the imprecise likes of me).

Wilce-ism 2: *Also – I've finally unearthed an ancient, but possibly very interesting, doctoral dissertation on Bayesian methods in generalized probability theory (read, quantum logic), by Marie Gaudard, a student of C. H. Randall. Would you like to have a copy?*

Yes please. I have this dream of traveling no more until the new year come August 19. If I can follow that, there some chance I might actually a paper or two read this Fall!

12-08-05 *Per Diem?* (to K. H. Knuth)

Thanks again for inviting me. I got a lot out of interacting with you, Ariel, Carlos, John Skilling, Andy Charman, Mike West and others. We definitely should have had you all at our QM-Bayesian meeting in Konstanz; that was a big oversight, and I won't make it again.

12-08-05 *Thanks* (to R. Renner)

Rennerism 1: *I am writing you to thank you again for the invitation and also for the organization of the nice BBQW workshop. I really enjoyed it and, clearly, I learned a lot. (Unfortunately, I had to run for the train last Friday, so I missed to say you goodbye personally.)*

I'm glad you enjoyed it.

Homework Assignment: Go to Bernardo and Smith's book *Bayesian Theory*, turn to Chapter 4 on "modelling." Prove quantum versions of all the representation theorems in there.

Take care, and I hope to see you soon.

12-08-05 *Quantum Bayesians and Anti-Bayesians* (to W. C. Myrvold)

Thanks for including me on the distribution list of your long note. This is something I want to eventually come back to, but at the moment I'm going to have to plead "organizer's exhaustion". As I write to you, I'm on my way back to Munich from the MaxEnt conference in San Jose. Soon after completing this note, my plan is to simply fall into a deep, deep sleep—it may last a couple of weeks. (First I plan to vacation a little in Munich, but then it'll probably take me a week to get my life established again in New Jersey upon my family's return home.) Plus by the time I finally open my sleepy eyes, I'll have the benefit of being able to review everyone else's reply to you before composing my own thoughts.

Keep up the good work!

Hey, here's a challenge to you. I think the single most significant reading for sending me toward the radical-probabilist end of Bayesianism was de Finetti's paper, "Probabilism," in *Erkenntnis* **31**, pp. 169–223 (1989). I'm pretty sure the same is true for Rüdiger, and possibly even for Appleby. Here's my challenge: Pinpoint the places where you think de Finetti errs in that paper. If you can convince me of some significant errors in that paper, you might just turn me back toward the temple of objective chance (from whence I originally came into this game). Maybe Unruh, Duwell, and Harper could take the challenge too.

But as I say, for now I close my sleepy eyes.

Wayne's Preply

> I don't know about you guys, but I found the Konstanz conference very valuable, and I believe I have a better grasp now of the views on the 'quantum Bayesians', and my conversations with people at the conference helped me to sharpen my own thoughts. Following is meant to be an attempt to continue the discussion, and to get clearer in my own mind what I think. Response (and disagreement) welcome!
>
> We're all interested, I think, in the question: What do we accept when we accept qm? And accepting qm means, at minimum accepting the quantum rules as the ones to adjust one's credences to. And so I'm going to couch everything that follows in terms of what accepting certain credences commits one to.
>
> Start with a case that has nothing distinctively quantum about it. I'm thinking of a quantum experiment with a fixed experimental setup, but that won't be essential. Suppose there is a sequence of events over which Bayesian Peter (a character I borrow from Bas van Fraassen) has an exchangeable credence function (and here I'm departing a bit from deF's terminology, which makes "exchangeable" modify "sequence"; from deF's own point of view, this is misleading, because exchangeability has to do with a credence function and not with the events themselves). What does such a credence function commit Peter to?
>
> (Though I'm going to assume exchangeability, pretty much everything I'm going to say goes through if Peter's credences can be written as a mixture over a richer set

of distributions, including, perhaps, correlations between elements of the sequence or between elements of the sequence and other events; invoke the appropriate generalization of the de Finetti representation theorem).

Suppose that Peter has an opportunity to bet on the 101st element of the sequence, and he is given a choice between betting according to his current credences, and learning the outcomes of the first 100 elements, and then betting on the 101st. Unsurprisingly, he can't prefer the former; he must regard the more informed credences as being at least as good as the less informed, and, provided that he attaches nonzero credence to the proposition that his credence in the 101st will undergo some change as a result of conditionalizing on the first 100, he will strictly prefer the informed credences.

Of course, we could save Peter the trouble of conditionalizing and just tell him what his credences would be if he were to conditionalize on the first 100, and he would gladly swap these credences for his current ones. Moreover, we can write his current credences regarding element #101 as his current expectation value of what his credences will be after learning the first 100 elements of the sequence.

And so on; Peter will prefer credences conditional on more data to credences conditional on less, in the sense that he will pay some money to be told what they are, and will adopt them once he knows what they are. Consider the set of credence functions consisting of all the credence functions that would result from Peter's priors by conditionalizing on finite initial segments of the sequence. Peter believes — that is, he assigns credence one to it — that this set has a limiting distribution. Moreover, if he knew what this limiting distribution is — if God could hand it to him on a platter — he would prefer it to all the members of the set.

Of course, what Peter would prefer to all of these, would be for God to hand him on a platter the results of the whole infinite sequence, once and for all. Moreover, if Peter is a determinist, he believes that these results are already determined by the current state of the world, which is of course imperfectly known to him. Since Peter will, at any given time, only have a finite body of data, data about past events, all he will ever have, with regards to future events, is credences conditional on finite initial segments. The limiting distribution is the least upper bound, in Peter's preference ordering over things that God could hand him on a platter, of the set of finitely attainable credences.

Peter has certain credences about what this limiting distribution is, and his current credences about future events are his epistemic expectation values of the probability they have on the limiting distribution. (That is, though Peter doesn't know what the limiting distribution is, he has some opinions about it, and his current degree of belief in an event E is his estimate of what probability is given to E by the limiting distribution).

What status does this limiting distribution have, for Peter? It is *not* epistemic, in the sense of being anyone's degree of belief. It's also the sort of thing once can have degrees of belief about, and Peter regards his process of updating on data as becoming better and better informed about what the limiting distribution is.

Now suppose we have a situation like that envisaged by Lucien Hardy in his 5 axioms papers: we have a preparation device, a transformation device, and a measurement device, all with various knobs on it. Peter starts fiddling with the apparatus, doing various experiments with the knobs at various settings, and conditionalizing on the results. If his initial credences concerning the outcomes of the experiments are reasonable, he will, with enough experimentation, start to converge towards the quantum rules concerning correlations of outcomes with the settings of the various knobs. For any setting of the knobs on the devices, Peter's credences will be a mixture over possibilities of an ideal limiting distribution associated with those settings. This ideal limiting distribution has the same status as it did before. It is not Peter's or anyone else's actual belief state, but an imperfectly known, ideally optimal belief state for a given configuration of knob settings. ('Optimal' meaning judged by Peter to be optimal, in the sense that he prefers having it to any finitely attainable belief state.)

If Bayesian Alice starts with priors different from Peter's, no finite amount of data will bring them into exact agreement. But, provided neither regards the other as excessively dogmatic, they both believe (attach probability one to) the proposition that experimentation indefinitely continued would lead both of them to converge to the same limiting distribution. And they can both regard their current differences in credences to be differences in opinion about what that ideally optimal limiting distribution is. (And this last remark would be true even if the conditions for convergence were not satisfied. Both could regard their differences in opinion as differences in opinion about the ideally optimal distribution, but each regard the other of incapable of properly learning about it)

So — at what point in the above train of thought would the quantum Bayesians want to get off? Because we've gotten close to how I characterized objective chances. They are associated with chance set-ups (knob settings), and are optimal credences in the sense that they cannot be bettered on the basis of information that is in principle available to agents making wagers on the events in question. They are the sorts of things one can have degrees of belief about, have differences of opinion about, and regard ourselves as learning about.

Wayne's Reply

Chris suggested that I look at "Probabilismo," and it so happened that, when I got that e-mail, I had just come back from the library, where I was copying it. I've since finished reading it, and I must say that last paragraph came as a nasty shock! (I had previously known nothing of de Finetti's political convictions). It should come with a disclaimer: Warning: Political content may offend some reader!

But of course it's not de Finetti's politics that are at issue. Here are a few preliminary comments on "Probabilismo."

1. It is simply presumed throughout that "probability" is univocal; the notion that (as Poisson pointed out) "probability" is used in two distinct senses is wholly absent. Hence there is a presumption that we are looking for a single notion of probability to cover all its uses. This is a royal road to subjectivism regarding probability. If the choice had to be between holding (a) "probability" always refers to degrees of belief, and (b) "probability" always refers to objective chance, then (a) wins hands down and in fact (b) doesn't even get out of the gate.

This isn't a minor point; I think that a major source of confusion in the literature on the interpretations of probability has been the idea that we have to pick *one* interpretation as the sole legitimate notion. It's as if someone were reading my e-mails over the past few months, unaware that there are two cities named "London"; such a person could easily come to the conclusion that London is a strange and peculiar place or that at least I have bizarre or contradictory beliefs about it (E.g, I got on a plane in London for a 7-hour plane ride to Toronto, after which I took a 2-hour bus ride to London.)

Having decided that probability is always epistemic, de Finetti calls the notion of an unknown probability of heads that we are trying to estimate nonsensical and metaphysical. As indeed it would be if we were talking about an unknown credence. An unknown chance that we have credences about is less obviously nonsensical.

The closest the de Finetti comes to offering an argument against admitting chances is the assertion that the hypotheses that the chance of heads has a certain value is not directly verifiable. Which is true; we test such hypotheses by constructing a sequence of events that we are reasonably certain all have close to the same chance, and update our credences on the results of that sequence. I count this as a measurement of the value of the chance. Rejecting this as excessively indirect would be to reject far too much, as the procedure is not essentially different from the measurement of *any* physical quantity using noisy data (and *all* data is at least somewhat noisy). Any epistemological considerations that could be held against one could equally well be held against the other. In particular, it does no good to point out that one must start the procedure with certain assumptions, or that no amount of data will dictate a unique value as our estimate of the parameter or a unique set of posterior credences regarding that value of the parameter; this holds for *any* parameter estimation via statistical techniques, not just to estimation of chances.

2. De Finetti's subjectivism seems, at least in part, to be motivated by a fallacious inference from the fact that every judgment is subjective — that is, made by and belonging to a subject — to a conclusion about the content of the judgment, namely, that the subjective judgment cannot be a judgment *about* anything that is not subjective. This inference, fortunately, is not a valid one, because, if it were, we would have to conclude, not merely that all probabilities are subjective, but that everything is, and we would land in idealism or even solipsism. To his credit (?), de Finetti does indeed take this extra step. In section 30 (p. 214) he denies the existence of "external reality" (his scare-quote), and in section 2 (p. 171) he quotes Tilgher, apparently with approval: "All the objects, men, and things of which I speak are, in the last analysis, only the content of my present act of thought: the very statement that they exist outside and independently of me is an act of my thought . . . ." This is Tilgher's paraphrase of what Berkeley commentators call Berkeley's "master argument" for idealism. And it's no better here than it is in Berkeley.

3. De Finetti frequently draws an analogy between judgments of probability and matters of taste. This is not part of any argument but merely part of the rhetoric of his presentation. But in case this rhetoric is having any persuasive effect, perhaps I can curb the appeal of this analogy by pointing out that, from the point of view of Bayesian decision theory, the analogy is not a very strong one. Matters of taste come into decisions via their influence on utilities; and utilities and probabilities play essentially different roles in decisions. One important difference is that we are obliged to regard new information as potentially improving our probability judgments; given the choice of choosing according to my present credences, or first updating on information relevant to my choice and then making my choice, an expected utility calculation tells me to choose the latter. There is nothing comparable when it comes to utilities. Suppose I am reasonably certain that Alice started out with priors similar to mine, and then gained new information, so that her current credences are close to what mine would be if I were to update on the information she has. Then, judged by my own current credences, I would be better off following her choices than making the choice based on my own. If, on the other hand, I know that Alice started out with tastes similar to my own and then underwent a process that made her prefer castor oil to cognac, I will still order cognac after dinner; I do not regard the fact that Alice chooses castor oil as relevant to

my decision. One way to put this is: we can regard change of credences as pure learning experiences. A chance of tastes (utilities) is *never* a pure learning experience.

12-08-05 *Your Summary* (to W. G. Unruh)

Thanks for sending me the summary of your concerns. As you'll see from the other note that I just wrote to Wayne Myrvold (and cc'd you on) though, I'm going to drop out of discussion for a couple of weeks. So I don't have a detailed reply for you at the moment.

I did however take ten minutes to compile a couple of emails that I wrote to Mermin a couple of years ago on the very subject you bring up. The notes are somewhat inadequate as a reply to you, but they're along the lines to a beginning of a reply, so I'll go ahead and attach them to the present email. You'll see their relevance if you have a read, but I can predict in advance that I didn't say anything that will convince you.

More later, but it may be a couple or three weeks.

Bill's Preply

Hope you had a good trip to Munich, got there safely and had a good birthday celebration with your wife.

Just to summarize my concerns. You want the probabilities to be a state of belief because you want to argue that when, in the Bell problem, A measures her S_x and finds $+1/2$, then the change in the probability at B of S_x from 50-50 to "certainty" for $+1/2$ is simply a change in the state of belief of the person who who has this additional fact. That change in belief, of the person, has nothing to do with the world itself, and such a change in belief thus does not (non-locally) affect the world itself. I would say that the main problem with that line of argument is that then there is an additional fact, namely if you bet on the basis of that belief, then you keep winning. Now, the belief may be personal, but the fact that that that belief keeps you winning is more than just personal, it also tells you something about the world. There is some matter of fact of the world which puts its behavior into compliance with your state of belief. That compliance, that correlation is something that needs explanation, just as in your opinion, in the Bell case, where the probabilities are considered objective facts about the world, the change in the probabilities for B in the face of the measurements by A is something that requires explanation (and you explain by non-locality). I.e., it is not at all clear to me that making the probabilities to be beliefs and subjective buys you much (anything) except to change what it is that needs explanation. Saying that that correlation between your beliefs and outcomes at A requires no explanation seems to me to be as problematic as saying that the change in objective probabilities at B due to the measurement at A requires no explanation.

On the commutation of the informations, I guess I would take the state of two two level particles to be $\sqrt{.51}|11\rangle + \sqrt{.49}|00\rangle$. This is all that B knows and for him the state which best describes the second system is diagonal in the above $|0\rangle$,$|1\rangle$ basis, with .51 and .49 on the diagonal. However A measures the first particle in the $|+\rangle$,$|-\rangle$ basis (call it the z basis $|+\rangle = 1/\sqrt{2}(|1\rangle + |0\rangle)$, and finds its value to be $+$. For A, the optimal state of the second particle is in the state $\sqrt{.51}|1\rangle + \sqrt{.49}|0\rangle$ Whose projector does not commute with B's density matrix.

The 0 probability case is more difficult (I think I misunderstood that constraints on the problem when on the train), and I am not sure what to think in that case. Have you actually come up with a case where it is zero (i.e., where the density matrix for B times that for A is 0)?

18-08-05 *The Big Bang Happened Here* (to T. Duncan)

Not much time for a long note at the moment: My wife, children, and I are on the last day of our vacation in Munich (seeing the grandparents), and we've got a dinner date soon … followed by packing up for an early morning flight tomorrow. So I can't write much. But I've been enjoying getting to know you a little today.

First off thanks for the note—which I'll study and reply to eventually—and thanks for the links at the bottom of it that led me to the SII pages and others. I've got to say, I'm quite intrigued by your life and efforts—what a beautiful idea the SII is!

As I say, I'll try to address your note specifically in the near future; you have a point worth mulling over in more detail (about what one might mean more specifically by "incomplete information" in a world without hidden variables). In the meantime though, let me attach a little something somewhat on the subject that I have been contemplating posting on `quant-ph` (at least Howard Wiseman wants me to, so that he can reply to it officially). It's attached as a PDF file; the title is "Delirium Quantum". I titled the present email after your textbook's title because it reminded me of a line I use in Section 7 of this paper: "That (metaphorically, or maybe not so metaphorically) the big bang is, in part, right here all around us?"

Listen, I didn't give you a copy of my book *Notes on a Paulian Idea* in Sweden, did I? At least I don't have your name recorded in my list. If you'd like a copy, I'll have one sent to you: I still have loads of copies of the VUP edition to give away (before the Springer edition comes out), and you might enjoy perusing it given your interests.

Todd's Preply

> It was good to meet you in Växjö back in June — thanks for the references you pointed me to. The Spekkens paper helped clarify the vague question I was trying to articulate after your talk, about how interference effects arise in the epistemic view of quantum states. From the correspondence between spin states and epistemic states in the toy model, I see now how the incompleteness of maximum knowledge makes it possible to define a binary operation that behaves as a coherent superposition (and hence produces interference). This is appealing since it's a glimpse of a deeper principle that could underlie the coherent superposition of quantum states.
>
> I'd still like to gain a more concrete understanding of why this happens and what it means. It's difficult to get direct intuition into what's going on because much is buried in whatever "nature" (in the toy model) must be doing in order to enforce the knowledge balance principle. I guess this is essentially the point you make for the real universe, in asking (of the fact that maximal knowledge of a system is incomplete knowledge), "Why can it not be completed?" The heart of the matter lies in that question. As I think about it, I'm not even sure what it means for knowledge to be incomplete other than in relation to hidden variables in the spirit Einstein described. But as you said, that cannot be the type of incompleteness that's really going on.
>
> This leads to a point that is still muddy to me. It may be a naive question and I'm not quite sure how to phrase it, but let me try this to get started: Given that we must ask what the ontic states can be, such that they force this restriction of incomplete knowledge on the epistemic states, why not just place the entire restriction

directly on the ontic states themselves? Why not simply say there is a limit to what we can *know* about the state of the world because there is a fundamental limit to how much information can actually be "stored" about its state? As I understand it, this is something like the view expressed e.g. by Zeilinger ("A Foundational Principle for QM", *Found. Phys.* 1999). I realize this is not what you're saying. So, I'd like to better understand why you emphasize the distinction between epistemic and ontic states, when it seems possible that all of the restrictions on our knowledge could be absorbed as consequences of the restrictions on the ontic states. Anyway I'd appreciate any thoughts you have to point me in the right direction here.

On a more general note, I've read and pondered more of your writing. Your ideas are inspiring — I found myself cheering you on as I read, even (perhaps especially) the asides and footnotes. (I'm thinking particularly of your replies to Preskill and to Wootters, in "The Anti-Växjö Interpretation of QM.") I have no idea if you make these comments for the same underlying reasons that I make similar comments, but I'd very much like to learn more about your philosophical perspective behind them. So I'll highlight a few points that struck me, and if you have time/interest, I'd love to hear your additional thoughts.

First, thanks for leading me back into William James' writing. I remember reading several of his essays and finding encouragement when I was in grad school trying to articulate the deeper motives and questions guiding my work. I especially like the opening pages of "The Present Dilemma in Philosophy" because he articulates so well the practical importance of one's view of the universe: "The philosophy which is so important in each of us is not a technical matter; it is our more or less dumb sense of what life honestly and deeply means. It is only partly got from books; it is our individual way of just seeing and feeling the total push and pressure of the cosmos." (*Pragmatism*, lecture 1 – The present dilemma in philosophy)

Next, your reminder that the usefulness of theories does not mean they are part of the blueprint of the universe is well articulated — and very important, I think. Have you read Edward Harrison's book, *Masks of the Universe*? My background is in cosmology so I'm used to thinking of the issue in that language, but it's very similar. Harrison expresses it by highlighting the distinction between the real Universe (capital U) and our model universes (lowercase u) which are masks that we try to fit to the real Universe. Here's a passage from Harrison which captures the essence of the point:

> The universes are our models of the Universe. They are great schemes of intricate thought — grand belief systems — that rationalize the human experience. They harmonize and invest with meaning the rising and setting Sun, the waxing and waning Moon, the jeweled lights of the night sky, the landscape of rocks and trees, and the tumult of everyday life. Each determines what is perceived and what constitutes valid knowledge, and the members of a society believe what they perceive and perceive what they believe. A universe is a mask fitted on the face of the unknown Universe.

Of course it's one thing to acknowledge superficially that, yes, of course, our models are limited and don't really capture everything about reality, ... but then going about our day-to-day work acting as if we can capture everything. (And as Wootters mentioned, there probably is some practical value in pretending this as a matter of methodology.) But what particularly intrigues me about what you're doing is that you seem to be bringing this awareness directly into the practical methodology of doing science. I'm not sure I said that very well —I guess what I mean is that there could be situations where there is practical value in NOT pretending that theories can perfectly mirror reality. Explicitly building that point into a theory (as you seem to be doing for QM, if I understand correctly) is tremendously important I think.

This email is already getting long, so I'll stop here and save comments about other points from your writing for another day.

22-08-05 *Exercise 1.image* (to T. Duncan)

Duncanism 1: *Thanks for the "Delirium Quantum" paper, which I've started browsing. I love your idea that new facts are being created around us all the time. The* Big Bang Happened Here *title evolved from a somewhat weaker point I like to make in lectures — that when we learn about the history of the cosmos, we are not just learning about distant things detached from us. Rather, we are discovering the history even of the space we are immersed in right here. A little bit of space we can hold in our hands once glowed with the heat of the big bang. Your point is a stronger one, that the process is ongoing even now. And yes, it is spine-tingling to think about that, to feel that our actions may "count" in some truly fundamental way. (BTW the title of our textbook has changed now to* Your Cosmic Context*, so it's funny that you found an old reference to it. I've attached a draft of the first chapter since it has some comments on models of the universe that might be of interest to you.)*

OK, so you already know how I'd answer your Exercise 1.image: "I am a cosmic artist – a contributor to a universal creative process." Thanks for sending Chapter 1 of your book, *Your Cosmic Context*. I just enjoyed reading the first 13 pages of it.

I sent a note the secretary at VUP to have a copy of *Notes on a Paulian Idea* sent to you. Per my promise, I will eventually get back to your more substantial points (from your first email), I just have to have a thought or two before embarking on the mission. (And I've got to get caught up a lot, after having been absent from Bell Labs for almost a month!)

23-08-05 *The Article for Science & Vie* (to H. Poirier)

Poirierisme 1: *Hardy proved that QM can be seen as a Generalised Probability Theory, or in other words, as a tool to calculate the probability associated with each outcome of any measurement that may be performed on a system prepared by the associated preparation.*

As far as I understand your position, this redefinition of QM is not so far from yours (when you say that the quantum state is solely an expression of subjective information about the potential consequences of our experimental interventions into nature).

So my problem is: If all the characteristics of QM can be seen as generalised probability tools, how can you think that there will remain a piece of quantum theory with no information theoretic significance?

The formal structure of quantum mechanics is like a sacred text, isn't it? There are so many ways to read it, so many diverse and conflicting meanings to be found in its pages. At weaker moments, one starts to wonder how one is going to reconcile all those meanings.

You ask a good question: "If all the characteristics of QM can be seen as generalized probability tools, how can you think that there will remain a piece of quantum theory with no information theoretic significance?"

This is a place where Hardy and I presently diverge, I think. There is great stuff in his derivation, and a lot that we've all learned from it—particularly, for me, I'm thinking of the origin of the multiplicative structure of Hilbert space dimension—but by thinking of quantum mechanics as a kind of *generalized* probability theory, I think Lucien swings too far toward a purely operational interpretation of quantum mechanics. It becomes a theory *purely* of knobs and transformations and clicks, and stops saying anything particular about reality itself.

What is the origin of the divergence between us? I think it is in his move of thinking that probability theory and information theory are malleable or fluid or empirical to some extent. In my approach, on the other hand, I think of probability theory as a kind of "a priori," much like simple arithmetic is "a priori"—its structure does not depend upon the particular details of the world. In other words, probability theory is not something that can be generalized; it is what it is.

Now, from that point of view, if we find that the content of quantum mechanics is, say, 1) a restriction on the set of probability distributions used to describe one's expectations about measurement outcomes, and 2) a modification from Bayesian conditioning—these are the two ingredients I standardly advertise in my own rewrite of the sacred texts—then those two ingredients are saying something about empirical reality. They are not part of the a priori structure of probability theory.

Another way I sometimes say it in my lectures is that, in the past there has been a lot of work in trying to view quantum mechanics as a kind of larger structure than probability theory, and one recovers standard probability theory in the commutative case. (Hardy's work is along the lines of that tradition.) What I am shooting for, however, is something different. I want to view quantum mechanics as a proper subset of probability theory to the extent that I can. To the extent that I can't, then that is saying something about reality.

I think Hardy's derivation has some element of that approach in it too, but in order to disentangle it all, I think there's still some work to be done. In particular, we've still got to get a better handle on what part of his derivation is actually *generalizing* probability theory—perhaps some of what he was doing has actually been misidentified. I.e., in the end, maybe he wasn't generalizing probability at all, but applying it to a specific physical context.

Anyway, that's my take on it at the moment.

I hope that helps. Good luck with your writing.

25-08-05 *De Finetti's Fascism* (to W. C. Myrvold)

Myrvoldism 3: *I've since finished reading it, and I must say that last paragraph came as a nasty shock! (I had previously known nothing of de Finetti's political convictions). It should come with a disclaimer: Warning: Political content may offend some readers!*

Sorry I didn't mention that detail—his (early-in-life) fascism is certainly an embarrassment. In any case, in that regard, you should have a look at the disclaimer at the end of page 31 of Carl's "Resource Material for Promoting the Bayesian View of Everything" posted at

`http://info.phys.unm.edu/~caves/thoughts2.2.pdf`.

30-08-05 *From Mineralarians to Pastafarians* (to C. H. Bennett and J. A. Smolin)

I'm guessing you guys have already heard about the Pastafarians, but in case you haven't, let me forward on this link that I just got from my friend Jeff.

Nicholsonism 10: `http://www.venganza.org/`

May you be touched by His noodly appendage. RAmen.

01-09-05 *Jazz in Westfield* (to J. W. Nicholson)

Nicholsonism 11: *Now where is my Geritol?*

That reminds me of a time when I was visiting Charlie Bennett and Herb Bernstein in Amherst. Bernstein stopped the conversation to take a break when he remembered that he should take his Metamucil. Then he came back into the room with the bottle and a spoon asking if anyone else would like some.

01-09-05 *BBQW Report* (to S. Hartmann)

How's this? Hope it fulfills your needs pretty well.

Being Bayesian in a Quantum World: A Report

"We've got to change the course of physics; nothing less will do!" That was the rallying cry for our meeting, *Being Bayesian in a Quantum World*, in Konstanz, August 1-5, and the VW Foundation can be sure that we tried our best to live up to this call. With a gathering of nearly 50 of the world's best quantum information theorists and philosophers of quantum mechanics, the sparks were certain to fly and they did. There arose a palpable sense that something new and powerful is brewing in the foundations of quantum mechanics with this Bayesian turn, and the posing of clear-cut research problems became the task of the day. The city of Konstanz provided the perfect setting for the meeting, with just the right variety of restaurants, biergartens, cafés, and walks to keep everyone stimulated from beginning to end. The discussions, often heated, literally started every day in the hotel's breakfast room with the first cups of coffee and often lasted late into the night, moving with the participants from coffee break to lunch to dinner to a café here or there.

The meeting was organized around sets of two or three formal talks at a go—for instance, two talks in the morning, followed by three in the afternoon, most days—with long audience-wide discussions following each set. In total, there were 24 formal talks, with the remainder of the invitees playing the role of session chairs and official discussants. In the following pages, we attempt to give a sense of the subject matter of these talks along with some of the discussion that surrounded them.

For instance, the first three talks of the conference went as follows.

- Christopher Fuchs: This talk tried to set the tone of the meeting by demonstrating that much of the content of finite-dimensional quantum mechanics reduces to two simple modifications of Bayesian ideology—1) the setting of a theory of prior probabilities with regard to the outcomes of a single special quantum measurement, and 2) a modification of the standard Bayesian conditionalization rule for updating probabilities in the light of new information. From this perspective, the formal structure of quantum mechanics becomes *mostly* a "law of thought" (in the same sense that George Boole called probability theory a "law of thought") rather than a "law of nature." Where nature still rears its head—i.e., makes its contingently given empirical content known—is through the higher-level set of reasons for why decision-making agents in this world should use *this* law of thought (i.e., quantum mechanics) rather than *that* law of thought (i.e., some foil theory other than quantum mechanics). Much of this talk was based on `http://www.arxiv.org/abs/quant-ph/0205039`, but there were also many newer points that have not yet been published. For instance, it was shown that the theory of priors mentioned above could be written as two simple restrictions on a probability distribution $p(i)$:

$$\sum_i p(i)^2 = \text{constant} \qquad \text{and} \qquad \sum_{i \neq j \neq k} C_{ijk}\, p(i)p(j)p(k) = \text{constant}$$

for a certain set of coefficients C_{ijk}. A research problem posed to the audience was to give an information theoretic reason for such constraints, particularly for the second one.

- Robert Spekkens: This talk set itself the task of giving a very strong argument for the view that quantum states should be viewed epistemically (i.e., as having to do with states of mind) rather than ontically (i.e., as having to do with states of nature, independently of any agent or observer). The structure of the argument was based on delineating the properties of the epistemic states about a certain toy-model universe and showing that (qualitatively at least) a large number of the most significant results of quantum information theory can be recovered in this model. The reason for introducing the toy model, rather than argue for the epistemic nature of the quantum state directly, is that the toy model allows a means for making a clear-cut distinction between ontic and epistemic states at the outset—that is, it leaves no room for confusion between the two concepts by its very construction. Much of this talk was based on the paper `http://www.arxiv.org/abs/quant-ph/0401052`, but the case was made even more convincing with several new examples exploring the structure of time evolution laws for such toy models.

- Rüdiger Schack: This talk attempted to go still further than the last by arguing that not only should quantum states be viewed epistemically, but more particularly as personalistic Bayesian degrees of belief. That is, even the idea of "a quantum state as a state of knowledge," with knowledge interpreted as "justified true belief," does not go far enough for a satisfactory Bayesian interpretation of quantum states. The argument was made quite thoroughly and generally by mapping the uses of quantum states to the uses of probability itself and then arguing for a "radical probabilist" interpretation of probabilities along the lines of Bruno de Finetti and Richard Jeffrey. Following that setting of the stage, a list of more technical results was shown to the audience. For instance, it was demonstrated that a nearly identical repertoire of theorems are available to the "quantum radical probabilist" as to the "classical radical probabilist" for responding to frequentists and propensitists who feel that quantum experiments can be doing nothing other than manifesting objective chancy propensities—a good example is the quantum de Finetti representation theorem for exchangeable density operators. This talk was in part based on `http://www.arxiv.org/abs/quant-ph/0404156`, but also contained much new material.

Thus went the first evening and then the first morning of the first full day. And the participants saw that it was good! In the afternoon, they came back for more—this time, the subject turned to refining the debate set forth by Professor Schack in the morning: Namely, does or does not the Bayesian project of interpreting quantum states require the notion of "objective chance" in sense of David Lewis or perhaps in the sense of something like Karl Popper's "propensities"?

To that end:

- Marcus Appleby, in his talk, gave a detailed argument for why probabilities should be viewed in the radical probabilist way and significantly expanded a line of thought started in his paper http://www.arxiv.org/abs/quant-ph/0402015, where he compares probabilities to qualia in the old primary-secondary quality distinction.

- Wayne Myrvold, in his talk, argued strenuously that even Bayesians need objective chance to make sense of what they are doing, along with a guiding idea like Lewis's "principal principle" for connecting objective chance to subjective credence. This work is presently being prepared as a paper with William L. Harper, "Why Bayesians Should Countenance Objective Chance."

- Christopher Timpson, in his talk, still further firmed up the debate by using various definitions and techniques drawn from the philosophy of language. Part of his argument can be read at http://www.arxiv.org/abs/quant-ph/0412063 where the main conclusion is: If one is going to get any interpretive traction in solving issues like the quantum measurement problem or the issue of nonlocality in quantum mechanics, "the cognitive state with which one must associate the quantum state is the state of belief, not that of knowledge."

The second day of the meeting, the presentations turned to far more technical and less foundational material. The first two talks concerned the revolutionary subject of quantum computing.

- N. David Mermin, in his talk, considered the model universe of a quantum computer in the way that he presents it in his Cornell course on quantum computing for computer scientists. The main conclusion he draws from this is that one is very naturally led to something like Bohr's view of quantum mechanics; see http://www.arxiv.org/abs/quant-ph/0305088. In the present context, he particularly focused on how a radically probabilist Bayesian view of the quantum state could contend with what he now considers the main mystery of quantum mechanics—those situations where quantum mechanics can make predictions with certainty.

- Hans Briegel, in his talk, gave a detailed description of a model of quantum computation (different from Mermin's) where the computation is enacted solely from quantum measurements (rather than unitary time evolution) on an array of quantum systems initially prepared in a fixed entangled quantum state; see http://www.arxiv.org/abs/quant-ph/0301052 and http://www.arxiv.org/abs/quant-ph/0504097 for two reviews of this very deep idea. In the present context, it was explored how this model seems to demonstrate that it is nonclassical *correlations* that lie behind the power of quantum computation, rather than the sensationalistic imagery used by the popular press and David Deutsch—namely, that quantum computation attains its power from parallel computations in parallel universes. To the extent that the idea of correlation can be given a purely Bayesian treatment, one can start to search for deep connections between a Bayesian interpretation of quantum states and the power of quantum computation.

The afternoon talks concerned various Bayesian-like representation theorems for quantum states:

- Mark Srednicki addressed the quantum de Finetti representation theorem, exploring whether one could indeed make sense of the idea of a "probability of a probability" and, consequently, the probability of an unknown quantum state.

- Renato Renner gave a representation theorem for finitely exchangeable quantum states (along the lines of a similar classical theorem of Diaconis and Freedman) and then showed that the theorem was crucial for proving the unconditional security of certain quantum cryptosystems.

- Todd Brun displayed a criterion (and proved a uniqueness theorem) for when two separate agents with distinct quantum states for a single quantum system can pool their expectations to form a new and improved quantum state. He also explored several notions for quantifying the amount of compatibility of distinct states, and posed various technical questions to the participants along those lines. Some of those problems were solved by Michael Nielsen and will eventually lead to publication.

Thus the afternoon and the morning of the second day passed. And the participants saw that it was still good! So too went so much of the rest of the conference. The third day saw in the talks of David Wallace, Howard Barnum, and Carlton Caves an eager, and very technical, debate on whether the quantum probability rule can be derived (or even made sense of!) in the context of a many-world interpretation of quantum mechanics (which is so popular in the quantum computing community). The fourth day brought attention back to some of the ideas explored in the opening talk of Fuchs: Namely, the extent to which the quantum updating rule can be viewed as a variation on Bayesian conditionalization. The talks of Veiko Palge, Matthew Leifer, Guido Bacciagaluppi, and Thomas Konrad all dealt with various details and critiques of this idea. On a more philosophical note, the talk by Michael Dickson—editor of the prestigious journal *Philosophy of Science*—placed the discussion of a Bayesian view of quantum mechanics within a neo-Kantian or neo-Carnapian framework. On the fifth day of the meeting, Armond Duwell and Amit Hagar opened the morning session by exploring the extent to which one might really hope to make a clear-cut separation objective and subjective aspects of quantum theory. The afternoon talks by David Poulin, Howard Wiseman, and Lucien Hardy explored the extent to which Bayesian and operationalistic interpretations of quantum theory can be motivating factors in finding new physics—one particular item of concern was in aiding the formation of a quantum theory of gravity.

In general, the meeting was a significant success. As already stated, the debate was strong, but the sense of a certain momentum forward was even stronger. It is fair to say, for instance, that a significant fraction of the participants will be having their students, graduates students, and postdocs exploring subjects brought to the fore in this meeting for a significant time to come.

Finally, we mention that a further sense of the excitement felt at this meeting can be read about in the French magazine *Science et Vie*, which is devoting its October 2005 cover story to our meeting, and also in an upcoming article in the American magazine *Scientific American* by one of the organizers.

06-09-05 *A Homer Simpson Drool* (to J. W. Nicholson)

Look at this baby, `http://www.pcmag.com/article2/0,1895,1850249,00.asp`. I'm toying with the idea of getting something like this.

I picked out a book for you, I think: Michael Polanyi's *Science, Faith, and Society*. It's a small one. I might try to read it again before I hand it off to you (to see if I'm able to believe it now, like I think I did before).

07-09-05 *EPR* (to A. Plotnitsky)

Plotnitskyism 15: *I am not sure, however, that I am quite ready to make any serious statements concerning entanglement in qft theory, and for the moment I mostly doing some reading. There is not that much actually.*

Let me try remedy your last sentence, a little at least, by giving you a few more references. First and foremost let me recommend a tutorial paper by my colleague here at Bell Labs, Steven van Enk. You can find it at: `http://www.arxiv.org/abs/quant-ph/0403119`. His discussion there is quite in line with the sorts of things I was telling you the last time we met (was it in Sweden?).

Steven also recommended the following two papers by Werner (and a student maybe)—S. J. Summers and R. Werner, J. Math. Phys. **28**, 2440 (1987); ibidem **28**, 2448 (1987)—though he warned that they are quite technical.

Finally, for the heck of it, I used the ISI index to see what papers have cited the Summers-Werner paper, and I paste in my findings below. Articles 1, 3, and 4 may be particularly useful to you. And within that you might get the most from the Peres-Terno paper. That paper can also be found on `quant-ph` at: `http://arxiv.org/abs/quant-ph/0212023`. Have a look at the section titled "Entanglement in quantum field theory" starting on page 21; that may give you some useful pointers.

Good luck in your thinking.

07-09-05 *Quantum Theology* (to M. O. Scully)

Now that Labor Day has passed, it's time for me to get back to work in a serious way. I apologize for taking so long to comment on your son's draft—and I'm still not going to do so in any detail today—but after my schedule of constant meetings this summer, I found myself so mentally drained that I seem to have lost the ability to speak for a while. But my tongue is getting loose again.

I read Chap. 11—the one on quantum theology—of Rob's draft on a plane somewhere between Brussels and San Jose a while ago. First off, I caught a lot of typos [. . .]

Second, before I comment in any detail, I want to read it again. So, if there have been any major revisions, maybe it'd be a good idea to email me the new version before I plunge ahead.

Concerning one thing you seemed to ask about particularly at our lunch meeting—i.e., about the spaceship example, where the uncertainty principle is amplified up to a level where it may affect practical human decisions—I think I like it. *But* I wouldn't call it a "proof" of free will as you seemed to want to do at lunch that day. Instead I think its import is that it forces us to contemplate as ubiquitous (rather than rare) the kind of situation William James talks about in two quotes that I'll attach as a PDF file. Particularly, look at the second quote. Its message is that free-willed agents can "shape" reality (in a way) when they have to, and what Rob's example shows is that it is a consequence of quantum mechanics that we have to do such a thing all the time.

But as I say, I'll try to comment in detail later. In the meantime, you and Rob might want to read (and start thinking about) these great James quotes. [See 21-11-01 note to Schack, "Pragmatism versus Positivism".]

07-09-05 *Heisenberg Quote* (to M. O. Scully)

Right, I had also promised to send you a quote of von Weizsäcker's about meeting Heisenberg. One such quote is below, but it is not quite as I remembered it. I thought I remembered von Weizsäcker as saying that Heisenberg told him that he had "proven free will" that day—a much more dramatic statement than what he says below. So, either I was thinking of a different version of the same story—and I'll have to work harder to dig up that quote—or maybe my memory is simply failing me finally. I'll let you know if I come to a conclusion.

From C. F. von Weizsäcker, "Physics and Philosophy," in *The Physicist's Conception of Nature*, edited by J. Mehra (D. Reidel, Dordrecht, 1973), pp. 736–746:

> I remember very well how I met Heisenberg for the first time when I was a boy of fourteen. We happened to be in Copenhagen at the same time. Soon afterwards, in a taxi in Berlin in April 1927, he told me about the uncertainty principle. I was fourteen years old, and I was greatly moved by this new idea. I got the impression that if this was physics, one must study physics. This was the first moment when I saw that there was a hope of bringing together the two different parts: the objective world described by classical mechanics and the world of man. I didn't know how, but somehow it meant that there was a connection between the two. And this was the way in which Heisenberg himself was expressing it when he said that the sharp distinction between subject and object was no longer possible in quantum theory.

07-09-05 *The Activating Observer* (to M. O. Scully)

While I'm sending you multiple emails today, let me go ahead and send you another. I call it "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature," and it's been the source of some of the quotes I've sent you (more accurately, it's where I've been storing them). With regard to Rob's book-writing project, he may find some useful material in there.

As you'll see, I've long had an interest in the idea that quantum mechanics may show that nature is a little more malleable to the presence of decision-making agents than had previously been conceived. Part of Rob's Chapter 11 seems to be concerned with that too, so that's why I say he may find some useful resources in the present draft. Sorry John Wheeler is not yet better represented in this draft, but the problem there is a historical one: I had read all those papers of his that I cite a long time ago, before I started collecting quotes. Now I need to go back and scan that material in with my scanner. It'll happen eventually; it just hasn't been done yet.

21-09-05 *Egregiously Ingenuous* (to G. Brassard)

Brassardisme 18 : *According to my dictionary,*

> **egregious**: adjective
> 1. outstandingly bad; shocking: egregious abuses of copyright.
> 2. archaic: remarkably good.

Here's another word I've always loved as having dual and contradictory meanings:

> **ingenuous** (în-jèn′yi-es) adjective
> 1. lacking in sophistication or worldliness; artless.
> 2. obsolete: ingenious.

And just in case you don't know:

> **ingenious** (în-jên′yes) adjective
> 1. Marked by inventive skill and imagination.
> 2. Having or arising from an inventive or cunning mind; clever.

I learned the meanings of ingenuous (it wasn't a word I had known before) when Göran Lindblad wrote in his paper "A General No-cloning Theorem"

> A number of different versions of no-cloning theorems have been published. In particular, the recent no-broadcasting theorem of Barnum et al. is a very general result. The proof is ingenuous, and the method used is far from obvious. Their result inspired me to try to find another approach which uses standard results on operator algebras and systematically exploits the structure of completely positive maps.

I was always very proud of Lindblad's saying that, but who knows!

Compost is finished now, and the new grass is already coming up. Rüdiger Schack is visiting and we're working on our latest Bayesian screed. (You tell me which meaning.) And in particular, I'm heartened again to get at the question, when it comes to quantum states, if they are nothing more than information, then "Information about what?"

By the way, we're also dropping in on Hans Halvorson and Bas van Fraassen's course today at Princeton. Have a look at: `http://www.princeton.edu/~hhalvors/teaching/phi538_f2005/`.

23-09-05 *Bayesians at FPP?* (to A. Y. Khrennikov)

Khrennikovism 8: *I would like to try to stimulate you to submit a paper to our conference proceedings which will be published by American Institute of Physics.*

I'm sorry, I would like to, but I just can't get a paper out at this time. I will definitely make a promise to give you a paper for your next proceedings.

Speaking of that, as I understand, you want me to be one of the organizers of your next meeting on Foundations of Probability and Physics. Do you know when that will be held yet? Anyway, I have an idea: What would you think of having a session devoted to the Bayesian or "radical probabilist" approach to quantum probabilities? I can think of several very good people I could invite (including Persi Diaconis). You'll note I organized a rather large international meeting on this subject just this year with a lot of relatively famous people there (look at

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`.

So, I hope to impress on you that this is a growing and important subject, and it would be nice if you had some representation of it beyond me at your FPP meeting. As you can guess—as I did before for you—I'll try to bring the American entrepreneurial attitude to my organizing of this session to maximize its success.

Also, there is a very large Bayesian meeting in Valencia, Spain from June 1–6, 2006. See `http://www.uv.es/valenciameeting`. There will be over 600 attendees there, they say, and maybe that will help us draw in some of the bigger names as they will be flying to Europe during a time very similar to the time of your meeting anyway.

28-09-05 *Visitor Exhaust* (to J. W. Nicholson)

Did you have any luck with the Polanyi book? Believe it or not there were couple of times this and last week when I wanted to be able to quote one of the passages in it to Rüdiger. (Probably just a function of it being fresh on my mind.)

05-10-05 *Nobel* (to J. W. Nicholson)

BTW, you must be pleased with this year's picks for the Nobel physics prize . . .

05-10-05 *Too Long Draft* (to A. P. Ramirez)

I'll be in tomorrow at 9:00, and I'll drop by your office. Attached is my "first draft" of a Form 1. Somehow I got confused and thought the rules had changed this year and that we were supposed to write 2 pages front and back. Dick set me straight today, so I'm going to condense significantly ... but not tonight. Anyway, maybe the present draft will give us something to talk about tomorrow, and I'll attach it forthwith. Also forgive me for the too flowery style—I'll be fixing that too—or at least tolerate me for a while.

> EMPLOYEE REPORT ON ACTIVITIES AND ACCOMPLISHMENTS
>
> This year, unfortunately, I talked far more than I wrote . . . but at least I calculated more than I talked, if that's any consolation! Allow me to report my progress in that order: 1) talks and organizational efforts, 2) completed and near-completed scientific papers, and finally 3) the yet unpublished calculations that consumed much of my attention.
>
> 1) Much of this year represented a whirlwind of travel for me. I made 10 (invited) international conference appearances and gave 6 external physics colloquia and seminars. I had to turn down at least 6 colloquia and conference invitations for lack of time or scheduling difficulties—one invitation even a Caltech colloquium. (In dollar terms, I tabulated that my hosts spent over \$19,000 on my behalf; in contrast, I spent only \$1,500 of my allotted \$3,000 of Lucent travel funds.)
>
> My overwhelming reason for taking on all this travel was to establish and entrench a certain point of view about quantum states within the quantum information community. This point of view—sometimes called the Bayesian approach to quantum probabilities—is my strongest intellectual child and has finally started to command serious attention around the world. For myself, I truly believe it is an engine that will take quantum information theory and computing to new heights. The key idea is that *almost* all of the formal structure of quantum theory (complex Hilbert spaces, state vectors, unitary time evolution, etc.) represents not so much a *direct* description of the quantum world itself, but rather a modification of probability theory induced upon the gambling agents (a.k.a. experimentalists) immersed within that peculiar world. Thus the research task becomes to find the residue of quantum theory that *is not* merely a modification of probability theory—the suspicion is only therein lies the real source of power in quantum information and computing. In fact, only once that part of quantum theory is uniquely identified will we have a firm grasp on the most interesting things that can be done with quantum information.
>
> In Andrew Steane's seminal paper "A Quantum Computer Only Needs One Universe" [*Studies in History and Philosophy of Modern Physics* **34B**, 469–478 (2003)], he fairly well demolishes the idea that the power of quantum computers comes from massively parallel computation (in parallel universes). In its place, he puts the following conjecture:
>
>> A quantum computer can be more efficient than a classical one at generating some specific computational results because quantum entanglement offers a

> way to generate and manipulate a physical representation of the correlations between logical entities without the need to completely represent the logical entities themselves.

Or, as he goes on, "[T]he basic fact which quantum computers take advantage of is that multi-partite entanglement offers a way to produce some computational results without the need to calculate a lot of 'spectator' results. For example, we can find the period of a function without calculating all the evaluations of the function." This idea rings true within the Bayesian approach to quantum information, with its emphasis on 'correlation' as just another species of the peculiar probability calculus the world forces upon us. Fleshing out ideas like this are indeed a prime area for exploration in the Bayesian approach.

Particularly to get the community thinking in these directions, I, along with Carlton Caves, Stephan Hartmann, and Rüdiger Schack, organized one of the more significant quantum information meetings this year to explore just such issues. The meeting title was *Being Bayesian in a Quantum World* and it was held in Konstanz, Germany, August 1–5, by way of generous funding of the Volkswagon Foundation (67,000 euro). We had 54 participants drawn from some of the best of the best of quantum information theory—the names Michael Nielsen, David Mermin, Gerard Milburn, John Smolin, William Wootters, Carlton Caves, Lucien Hardy, Norbert Lütkenhaus, William Unruh, and Hans Briegel will hopefully ring a bell or two. For five days the talks and discussions centered on the issues raised above, and the verdict is that this point of view is definitely on the map. For instance, the French magazine *Science et Vie* made a report of the meeting as their cover story for this month's issue, and *Scientific American* has invited me to write a full article on the subject in the coming couple of months.

In total, I am quite pleased about the extent to which my talks and organizational efforts have gotten the quantum information community thinking along these directions.

2) With regard to publications, let me mention the two most significant pieces of *new* work—one recently posted on the archive and submitted to *Physical Review A*, and one in draft form, soon to be posted.

The first—titled "Influence-Free States on Compound Quantum Systems" and coauthored with H. Barnum, J. M. Renes, and A. Wilce—explores the connection between a previously published derivation of mine of the tensor-product rule for combining quantum systems and the idea that measurements on one side of an entangled quantum state cannot be used to communicate instantaneously to the other. In particular it is shown that a kind of Bayesian updating of the remote system's quantum state after a localized measurement goes hand in hand with the idea of no-signalling. Beyond that, we further elucidate a trouble that came up in the previous derivation of the tensor product rule: Even though a no-signalling assumption specifies a kind of Bayes rule, and indeed even specifies the tensor-product rule for combining quantum systems, it is not strong enough to uniquely pin down that the joint probabilities for the outcomes of localized measurements must be derived from a standard quantum state (i.e., a bipartite positive-semidefinite operator of unit trace). It only specifies the weaker requirement that the probabilities be derived from bipartite operators that are "positive on pure tensors." Thus an extra assumption is clearly needed to get all the way back to quantum mechanics. What that extra assumption is, unfortunately, we do not yet know. However, we were able to show in this paper that this more general class of operators are pathological in the sense that they are not well behaved when they are incorporated into a quantum teleportation scheme. In general, this work is part of a larger effort to get a the root of the idea of quantum entanglement.

The second paper — tentatively titled "Accurate Quantum State Estimation via 'Keeping the Experimentalist Honest'" and co-authored with R. Blume-Kohout and P. Hayden — takes another look at the process of quantum-state tomography, but via Bayesian ideas and methods. It is based on first defining an "honesty" function that derives from the old (Bayesian) mathematical puzzle of "keeping the expert honest." In this puzzle, an expert is hired to give his opinion of whether one or another event i will occur, and he expresses that opinion in terms of a probability assignment p_i, where $p_i \geq 0$ for all i and $\sum_i p_i = 1$. But *opinions* are not objectively testable—certainly not by simply looking at the actual event that occurred. So how would one ever know whether the expert is lying about his opinions? For instance, the expert might actually believe p_i, but because he does not particularly like his customer and ultimately wants to see him fail, he might offer the distribution q_i instead. The trick is to choose a payment scheme that gives an incentive for the expert to keep honest. For instance, if the client agrees only to pay the expert after the actual event i happens, and only then an amount $f(q_i)$ for some function f acting on the expert's offered opinion q_i, then one can see that a good incentive scheme should have the property that

$$\sum_i p_i f(q_i) \leq \sum_i p_i f(p_i) \ ,$$

with equality if and only if $q_i = p_i$. In other words, if the expert's real opinion is p_i, then he should expect to lose money any time he offers an opinion different from his true one. Remarkably, it can be shown that this inequality alone uniquely specifies the function f up to a constant and it is $f(x) = -\log x$. (I believe this was first shown by Aczél in the mid 1970s.) A consequence of this result is that when an expert is honest, he can expect to be paid (up to a constant) an amount that is equal to the Shannon entropy $H(p) = -\sum_i p_i \log p_i$ of his true opinion.

In any case, on these ideas, we build the concept of an honesty function and generalize it to the quantum case, where an expert offers as an opinion a density operator ρ rather than a probability distribution. In particular, we show that Bayesian updating in the usual sense (for an exchangeable density operator assignment) is the most honest strategy a quantum-state tomographer can use before passing his opinion on.

3) Now let me come to my most passionate work for this year—it is work that is still not complete and certainly not published. What I have been seeking is a well-behaved and *insightful* representation for quantum states (in finite dimensional Hilbert spaces) that is purely in terms of a single probability distribution for each state. It is easy enough to see the starting point for such an endeavor: One merely chooses a generalized measurement or positive-operator-valued measure $\{E_h\}$, $h = 1, \ldots, d^2$, for a Hilbert space of dimension d, where all the operators are linearly independent to be a kind of fiducial measurement. Then the probability distribution $p(h) = \mathrm{tr}\, \rho E_h$ for the outcomes h of such a measurement will uniquely specify the density operator ρ. This can done easily enough, as there are many constructions $\{E_h\}$ that will do the trick. But which of these constructions will lead to the most insight with regard to the source and ultimate power of quantum information processing? The hope here is to find a transparent way to see the extent to which the structure of Hilbert space arises from a kind of information theoretic constraint on the expectations any experimentalist should adopt for the outcomes of his quantum experiments.

One promising candidate for an insightful fiducial measurement comes from my work in defining a measure of "quantumness" for sets of quantum states that I reported in last year's Form 1 (and was published in the journal *Quantum Information and Computation* this year). There, it was shown that if any set of d^2 one-dimensional projectors $\Pi_i = |\psi_i\rangle\langle\psi_i|$ exist with the property that $\operatorname{tr}\Pi_i\Pi_j$ is constant for all $i \neq j$, then such a set would fulfill a criterion for being the most quantum a set of states can be. Concerning the present issues, a nice corollary of the old work is that if such a set exists, then the operators $E_h = \frac{1}{d}\Pi_h$ would be linearly independent and thus the set $\{E_h\}$ would be adequate for giving rise to a fiducial measurement.

Ostensibly another promising measurement comes about by simply trying to choose a set of E_h to be as orthogonal to each other as possible—i.e., one would like to get as close as possible to an orthonormal basis of operators while still preserving that the set of operators describes a potential measurement. One of the things I proved this year is that this other class of candidate measurements actually turns to be identical to the previous one!

Thus the more reason to try to work out quantum mechanics in the language of this particular class of fiducial measurements. But do they exist? Computational work from several groups seems to confirm that they do (for dimensions up to $d = 45$ at least). But despite a handful of papers on the subject now posted on the `quant-ph` archive by various groups and a couple hundred pages of my own calculations, an actual proof—and more interestingly an analytic construction—is still up for grabs. I have, however, made plenty of progress ... and in directions that no one else seems to have taken. For instance, to record my strongest result yet, I have reduced the problem to simply showing the existence of a $d^2 \times d^2$ Hermitian matrix G with constant diagonal entries and off-diagonal entries all of the form $e^{i\theta_{hk}}$ such that

$$\operatorname{tr} G^3 = \operatorname{tr} G^4 = d\ .$$

If one can prove the existence of any solution whatsoever to those two equations, then the existence of such a measurement will be ensured.

On top of that I have been able work out what the full set of quantum states looks like in this language if these very nice measurements always exist—that is, I am getting very close to my real goal. For instance, in this language a pure quantum state is any probability distribution $p(h)$ over d^2 outcomes satisfying the following two equations:

$$\sum_h p(h)^2 = \frac{2}{d(d+1)}\ ,$$

$$\sum_{jkl} p(j)p(k)p(l)\operatorname{tr}(\Pi_j\Pi_k\Pi_l) = \frac{d+7}{(d+1)^3}\ .$$

The first of these equations is very clearly an information theoretic constraint, in terms of a Rényi entropy. But what of the second? This strikes of the sort of "residue" mentioned of the first section of this report. The numbers $\operatorname{tr}(\Pi_j\Pi_k\Pi_l)$ represent universal constants whose values are deeply intertwined with what quantum mechanics is actually about. At the moment, I think there is no more exciting question than pinning down what exactly this means.

SUMMARY: I hope the three sections above give some flavor of my research activities this year. As it turns out, yesterday I was heartened to hear that Roy Glauber won part of this year's Nobel prize in physics . . . not because I think I could ever aspire to those heights, but because Glauber's work reminds us of the power that can be had by finding just the right representation for certain physical phenomena—in his case recognizing the importance of coherent states for representing quantum states of the electromagnetic field.

Quantum information is still in search of the right representation for its most fundamental phenomena, and I hope the work described above is a significant contribution in that direction.

13-10-05 *Nature Physics Article* (to G. Brassard)

Brassardisme 19: *Ever heard of Frieden, Soffer or Brown?*

Frieden's stuff is pretty soft—i.e., not to be taken seriously. The basic thing he gets is the time-independent Schrödinger equation to come out of a kind of stochastic (hidden-variable) model. But that's a far cry from quantum mechanics . . . and not even nearly as complete as Bohm's hidden-variable theory (of which I wouldn't consider a real derivation of QM either). The only minor similarity between Frieden's work and our program is that we both use the word "information"—there the resemblance stops.

17-10-05 *Stomach Bug* (to S. L. Braunstein)

I remember ending our conversation last Friday by saying, "I'll almost surely be in next Monday." Teaches you something about the concept of certainty (and its fallibility). I've been up much of the night with a stomach bug and there's every indication this morning that it still hasn't abated. So, I think in good conscience, I probably shouldn't come in today . . . for myself and everyone around me. Hope that doesn't screw up your day's plans too badly, and I hope you get this note before you get on the road.

18-10-05 *Hello* (to C. Hewitt)

Thanks for all that; I'm flattered. (Though, too bad you—or, your admiration at least—weren't around when I interviewed with the MIT EECS dept in '97 or '98! I might be living in Cambridge, MA rather than Cranford, NJ!) I'll have a look at those links; I had never heard about actor theory before. But then again, I know almost nothing about programming—I've got a lot to learn.

If there really is a similarity between the actor model and my thoughts on quantum mechanics, then I suspect there is an even larger similarity between it and the "radical pluralism" of William James (i.e., a particular flavor of American pragmatism). If you haven't discovered James yet, you might enjoy reading him—it's a whole philosophy built on the very idea that there's no "global state."

Carl's Preply

I am an admirer of your work. In fact I made use of it in the following Wikipedia article (hopefully not distorting your views too much):

`http://en.wikipedia.org/wiki/Actor_model_history#Relationship_to_physics`

Also you might be interested in the following article:

http://en.wikipedia.org/wiki/Actor_model_theory

Your comments, questions, and suggestions are greatly appreciated.

Carl's Reply

Sorry that I missed your job talk. But since I am CS my admiration might not have been of much help!

Meanwhile, your ideas have proved too radical for some of the current physics moss backs at the Wikipedia so the ideas have migrated to[83]

http://en.wikipedia.org/wiki/Actor_model%2C_mathematical_logic%2C_and_quantum_physics

You should be able to read most of the above article in conjunction with

http://en.wikipedia.org/wiki/Actor_model_theory

even without any background in software.

I would greatly appreciate your comments, questions, and suggestions.

PS. Yes, James is wonderful.

18-10-05 *Lazy Abstract* (to H. C. von Baeyer)

With time running short today, I think I'll just send you an old abstract (with only some very minimal modifications), rather than writing you something new. Hope you don't mind.

Title:

Drawing Quantum Mechanics on a Probability Simplex

Abstract:

What is the difference between a quantum observer and a weather forecaster who uses classical probability theory? Not much. But where there is a difference, there lies quantum theory's most direct statement about properties of the world by itself (i.e., the world without observers or weathermen). In this talk, I will try to shore up this idea by writing quantum mechanics in a way that references probability simplexes rather than complex Hilbert spaces. By doing so, the connection between quantum collapse and Bayes' rule in classical probability theory becomes evident: They are actually the same thing up to a linear transformation depending upon the details of the measurement method. Looking at quantum collapse this way turns the usual debate in quantum foundations on its head: only local state changes look to be a mystery. State changes at a distance (as after a measurement on one half of an EPR pair) are completely innocent—they simply correspond to applications of Bayes' rule itself, without the extra transformation; that is, collapse-at-a-distance is nothing more than the usual method of updating one's information after gathering data. Thus the idea develops that if a quantum reality is to be found in the quantum formalism, it will be found only in the formalism's *deviations* from classical probability theory: Reality is in the difference.

[83] Apparently they are not represented there anymore either; maybe they were migrated into oblivion, or this is what was being referred to: https://en.wikipedia.org/w/index.php?title=History_of_the_Actor_model&oldid=29339394.

18-10-05 *Flight Schedule?* (to H. C. von Baeyer)

von Baeyerism 1: *The question I am most interested in concerns Zeilinger's principle:*

`http://www.univie.ac.at/qfp/publications3/pdffiles/1999-10.pdf`.

I would say the most serious technical development of an idea along those lines is Rob Spekkens' "toy model" even though, by its very construction, it can only go part of the way toward quantum theory. Download: `http://arxiv.org/abs/quant-ph/0401052`. In any case, I think it is one of the best, most convincing arguments for the epistemicity of quantum states around. It's worth the study, both for what it does and what it can't do.

Is there something to Zeilinger's principle? Maybe, but at present I think it's still more of an "idea for an idea" than a full-blown idea itself. Certainly it seems important to better understand how (and then find the reasons for why) our uncertainties scale with the amount of quantumstuff that they do—there's strong indications that this is indeed a fruitful direction for research. It'll be fun discussing these things with you.

24-10-05 *Books, Bookcases, and Information* (to J. B. Lentz & S. J. Lentz)

Thanks for the nice electronic birthday card. The kids and I both loved it. I had a nice and thoughtful day for my birthday. I went in to New York City and acquired 10 more books for the "pragmatism collection." If we could just find the right bookcases, the front room—i.e., the library—would look great. Who would have thought the books (given the subject) would be easier to get than the bookcases?

Thursday I fly to Newport News to give the physics colloquium at William and Mary in Williamsburg. In preparation for that, I've been reading Hans Christian von Baeyer's book, *Information: The New Language of Science*, this week. I figure it is a nice courtesy since von Baeyer is the fellow who invited me (never met him before). The book has turned out to be quite a pleasure though—accurate and entertaining, both; so I'm glad I took on the project. Brad, I think this is one you might enjoy if you're looking for some leisure reading—the kind of thing, at least, that I might imagine as leisure reading for a Chief INFORMATION Officer. Von Baeyer is exceptional in that his research field as listed by his department is literally "public understanding of science." Anyway, he does a very good job on everything from Bayesian probability to quantum computing in this book—you might find it an enjoyable introduction.

25-10-05 *GOBs, Bobs, Steering & Teleportation* (to H. Halvorson & B. C. van Fraassen)

... and if you're patient there's even a little bit about perspectivalism at the end.

Thanks again for letting me attend your seminar last Wednesday. I had a good time, and I hope I will push myself to continue coming. (If it only weren't for that darned long drive: 45–65 minutes, depending on traffic.) The main thing I get out of the deal, of course, is not so much the quantum-information material, but that the lectures and discussion points give me a window into how philosophers are starting to think about this subject. That's valuable for me.

Anyway, spurred by what I heard at the last seminar, I put together a few notes, and they're pasted below. Feel free to not read any further than this sentence if you're getting tired of my emails: These exercises of sentence construction are always useful for me, even if for no one else; so certainly I won't be hurt if you send this email to the recycle bin and don't reply. However, since you were the discussion leaders, I figure I might as well share the notes with you, even if only out of courtesy—I only hope they won't cause you to give me a failing grade for the semester!

Maudlin

First off, let me tackle something Maudlin said near the end of the seminar. As I recall, he basically asserted that the phenomenon of "steering" is a very real effect (I'm tempted to put the word "ontic" into his mouth here), that Bell had taught us such, and that the phenomenon has very useful consequences—for instance he tried to make the last point dramatic by putting a gun to Bob's head. As I recall, the point that started all this drama was Hans' statement that he was reluctant to accept the idea that a localized measurement could change the global *quantum* state for a bipartite system.

The main point I want to make here is that Maudlin's drama really carries no force as far as saying something *unique* about the quantum world. Let me give a contrived example that is a) not quantum mechanical, but b) serves the role Tim sought to fulfill via quantum entanglement (and quantum entanglement only).

Suppose Bob has in front of him four buckets, labeled 1, 2, 3, 4, and that there is a ball in precisely one of them—the rest of them are empty—but Bob has no clue which one. Furthermore unfortunately for him, these buckets along with the one ball are all themselves hermetically sealed in a GREAT OBFUSCATING BOX, or GOB. The definition of a great obfuscating box is that one can query it to ask a yes-no question of the ball's position within the collection of buckets, but it will never let the questioner know the position more precisely than two buckets' worth. For instance, one might ask "1v2?", meaning "is the ball in bucket 1 or 2?", to which the GOB will answer yes or no. If it answers "yes", then one is assured that the ball is indeed in bucket 1 or bucket 2; if it answers "no", then the ball must be in either bucket 3 or 4. Similarly one could ask the GOB "2v3?" A "yes" answer narrows the ball's position down to 2 or 3, and a "no" answer narrows it to 1 or 4. And similarly still, one could ask the GOB "1v3?" to get an appropriate answer for that question. The nasty trick is that only one of these three questions can be asked and never another one.

So, with a GOB in place of a qubit, let's now go back to the rest of Maudlin's scenario. Charlie holds a gun to Bob's head and says, "I'm going to ask the GOB this question "XvY?", and if you correctly predict the outcome before I get the result from the GOB itself, then I'll let you go free; but if you get it wrong, then BAM!, a bullet in the head! By the specification of the scenario, without any further help, there's very little Bob can do but make a guess and say his prayers.

But suppose Bob's friend Alice happens to overhear what's going on and can surreptitiously talk to him. If it turns out that she knows she has an identically manufactured GOB—assured by the factory to have a ball in precisely the same bucket as Bob's—then everything changes. She can help her friend by asking her GOB the question Charlie is about to ask his GOB and then slipping the answer back to Bob. Bob's GOB, we know, will give the same answer if she asks the same question. Alice, of course, still won't know precisely what bucket her ball is in (and consequently which bucket Bob's is in), but the information she gained is enough to keep Bob from getting shot.

So there: All the drama of Maudlin's example, all the same conclusions operationally, but where's the quantum mechanics? Where's the nonlocality? There is none. The example is powered solely by Bob's having a friend who is capable of relieving him of some of his uncertainty. Alice passes off her own incomplete information to Bob, and that saves the day. In the context of quantum mechanics, Schrödinger called that first action of Alice's (before passing off the information) "steering," but that seems like such a weird term for an example like this: Alice isn't steering anything at all; she's just trimming her uncertainty one way or another by making the choice to ask one question or another.

Having a hidden variable—i.e., the actual position of the ball in the buckets—serves only as a dramatic device in this example: For it makes it absolutely clear that Alice's action of learning at her site changes nothing about the reality at Bob's site. The reason the situation *seems* to look different in quantum mechanics is because we have all become convinced (through Bell-like arguments) that there can be no local hidden-variable theories underlying quantum mechanics. But from my own perspective, that's an over-hasty conclusion. In fact, it's a *non sequitur*. One can have uncertainties about many things, from localized variables to relations between very distant systems to the consequences of one's own actions. And in all of those cases, one can invent "passing off privileged information" scenarios like the one above to make dramatic a kind of "steering phenomena." Ruling out localized hidden variables (which I definitely believe has been done in quantum mechanics) in no way pushes the further consequence that "steering" is an ontic phenomenon rather an epistemic one.

In fact, I have always hated the word "steering" as a name for this phenomenon in the quantum context because it already loads the dice toward an ontic interpretation of quantum states. Indeed, one can see that from the very beginning in Schrödinger's early 1935 papers and his correspondence with Einstein. Schrödinger *starts* with the assumption that quantum states are ontic states (rather than epistemic states), and is therefore led to introducing some ad hoc rules for entanglement-decay so as to get out of a kind of action at a distance. On the other hand, Einstein (as exhibited in correspondence with Schrödinger reprinted in Fine's book *The Shaky Game*) starts with the rejection of action at a distance, and concludes that "steering" must be an epistemic phenomenon.

I side with Einstein, of course. Thus, to load the dice toward my own interpretation, I much prefer the words "conditioning" or "conditionalizing" (as from simple probability theory) or even "updating" over "steering." These words much more adequately convey the passive nature of what is going on in updating the quantum state of a far away system.

By the way, now that I've said all that, let me report that I'm doing nothing with my GOBs that Rob Spekkens isn't already doing with his toy model: `quant-ph/0401052`. I think that is conceptually the most important paper written concerning the interpretation of quantum information written in long while, and well worth everyone's understanding.

Teleportation

But isn't quantum teleportation the indication of something much deeper going on with quantum steering than with epistemic updating? No, it is just about the same thing fancied up into a more complicated situation.

Let me try to make that clear by writing down an example that I told Hans about in conversation at our second-to-last meeting. In usual teleportation, the cast of characters includes an Alice and a Bob who share two systems in a maximally entangled state, and implicitly, a Charlie who prepares a third system in the state of his choice and then hands it off to Alice. Alice then performs a measurement on the two systems in her possession and announces the result of the measurement to Bob. The teleportation process is completed with Bob performing an operation on his system conditioned upon his newly acquired information.

In what sense is it completed? Only in this: If Charlie has the promise that Alice and Bob went through all the actions described above, then he can safely ascribe the same quantum state to Bob's system that he had originally ascribed to the system he handed off to Alice.

Here's a corresponding classical example. In place of entanglement, let us equip Alice and Bob each with a coin (oriented heads or tails) encased in a magical opaque box. These magical opaque boxes have the following properties: 1) though one cannot see how a coin is oriented within it, one can nevertheless reach inside a box and turn the coin over if one wishes, and 2) if one touches two of these boxes together, they will glow green if the coins within them have the same orientations, and they will glow red if they have opposite orientations—the glowing reveals nothing about the actual orientation of either coin, only about their relationship. Finally let us stipulate the following for Alice and Bob: That their opaque boxes contain identically oriented coins, but Alice and Bob (or anyone else for that matter) know nothing more about the coins beyond that. In other words, Alice and Bob possess HH or TT, but they do not know which.

Now, as in quantum teleportation let us introduce a third character, Charlie. Charlie has an opaque box of his own. But let us give him some partial certainty about the orientation of his coin. Particularly, let us suppose he ascribes a probability p for his coin to be heads. This is a real number between 0 and 1, and in principle it might take an infinite number of bits to specify.

Here's the protocol. Charlie hands off his coin (encased in a magical opaque box) to Alice. Alice touches her newly acquired box to her old box. The two glow red or green, and she communicates the result to Bob. If the result was green, Bob leaves his coin alone. If the result was red, he reaches into his opaque box and turns the coin over. At that point the "teleportation" process is completed.

In what sense is it completed? Only in this: If Charlie has the promise that Alice and Bob went through all the actions described above, then he can safely ascribe the same probability p to the coin in Bob's box (i.e., p that it will be heads) that he had originally ascribed to the coin in his own box. In other words, Charlie has everything it takes to update his epistemic state about the orientation of the coin in Bob's box to what he had originally thought of the coin in his own box.

Is this wildly exciting? The stuff that would make headlines in papers all around the world and be called "teleportation"? At the material cost of transferring a single bit from Alice and Bob, an infinite number of bits (in the form of the real number p) has been transferred between the two sites instantaneously? Not at all! The only thing that was materially transported from one site to the other was a single bit (that the boxes glowed red or green). The rest was just "conditionalizing" or "updating". And there is no mystery whatsoever in that. Dumb and dull, it would never make a headline.

And so too, I would say with quantum teleportation, even though it has built my career. (Our Caltech experiment of it has over 600 citations now.)

At the end of this note, I'll place some of the correspondence I had with Asher Peres as we were writing our ill-fated paper "Quantum Theory Needs No 'Interpretation'." (Of course the whole paper is about a particular interpretation, but most critics never read beyond the title.) In it, we had a paragraph on quantum teleportation and how it helps to illustrate the epistemic nature of quantum states. And boy did we fight about how to write that paragraph, and even—at that time—about the meaning of teleportation. Seeing some of our own wrangling may be of use to you, if you care to explore this idea further. The main points of the part attached below are 1) how there is no THE quantum state for a system (there as many as there are potential observers) and how teleportation makes use of that idea, and 2) when Alice performs her actions, nothing physical changes on Bob's side.

Finally, let me comment on:

Hans's Seemingly Black-and-White-ism or All-or-Nothing-ism

Two or three times now, I have heard Hans equate the idea of interpreting quantum states epistemically (i.e., as knowledge or like a Bayesian degree of belief or whatever) with giving up on the project of thinking that *physics* has something to say about the real world (i.e., presumably the world as it is without any knowers or even Bayesians). That is altogether too glib of a position, and I want to do whatever I can to ease him out of thinking it.

The stuff that Caves and Schack and Spekkens and Appleby and Timpson (and sometimes Mermin and Unruh and Milburn and whoever else) have been talking about is much more subtle than that, and it should be recognized as such—this field of thought is at a respectable enough level now that it shouldn't be caricatured. I for one, for instance, pretty much characterize myself as a realist in the time honored sense: That there is a real world out there beyond our whim and fancy, and it is the task of science to hypothesize about its attributes and properties. I am not a philosophical idealist and certainly not a solipsist. And I don't think I am a positivist or an empiricist. Predominantly, I think I lean toward a kind of materialism (though tempered by a lot of pragmatic subtleties).

The main way I think Hans errs is that when he hears a phrase like "quantum states are states of knowledge," he thinks that that throws *everything* away to knowledge (or Bayesian degree of belief)—that nothing is actually hypothesized about the world. But quantum theory contains so much more than simply quantum states: There are Hilbert spaces; there are Hilbert-space dimensions; there are Hamiltonians; there are eigenvalues; there are notions of *separate systems* whose joint description is worked up through a tensor product. The quantum state is only one lonely piece of quantum theory.

When it comes to quantum states, our point is simply scientific open-eyedness and conceptual clarity: 1) Open-eyedness. We see so very many analogies between quantum states and incomplete knowledge—just look at Spekkens's paper for more than 20 such—that it would be scientifically foolish to not take those analogies absolutely seriously and explore them for all they're worth. But 2) Clarity. We think we get even more in return, via easy solutions to some of quantum foundations' main supposed conundrums. From the epistemic view, there is no measurement problem, there is no nonlocality.

Loads of questions still remain, but one shouldn't be frightened of the methodology … which is, as Spekkens put it,

> The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

For instance, if you would ask Carl Caves, he would classify quantum states as epistemic, but he thinks that Hamiltonians represent a decent chance of remaining ontic terms within the theory (i.e., as representing an agent-independent reality). If you ask Spekkens, his own gut feeling is that quantum states represent incomplete knowledge of *relations without relata* (whatever that would mean), but particularly those relations are to be viewed ontically. As for myself, I tend to think that the ontology lies somewhere in a quantum system's receptivity or sensitivity to external interventions (whatever that would mean), and that receptivity is to be viewed ontically.

So, there is a range of ways of staying hard-headed about the idea that *quantum states are states of mind* without becoming a postmodern or a deconstructionist—without taking a walk with Derrida. And that is what our research program is about. Here's the way I put it in my `quant-ph/0205039`:

> This, I see as the line of attack we should pursue with relentless consistency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

Now, look in that, and tell me where is this view of yours that we drop the idea that "physics should be about reality"? I'll just pose that as a challenge to you.

Hopefully I'll see you both again tomorrow.

27-10-05 *It Wasn't You* (to H. C. von Baeyer)

I just looked it up again. At the web site,

http://web.archive.org/web/20060104094204/http://
www.physics.byu.edu/research/theory/papers/

someone posted this comment:

> Our own Dr. Evenson recently had dinner with Anton Zeilinger, and asked him what he thinks about the radical ideas of Christopher Fuchs. Professor Zeilinger replied that "he is not radical enough."

I don't know who Evenson is, but he's not von Baeyer.

30-10-05 *Correlation without Correlata in the Strangest Places* (to N. D. Mermin)

I read these lines in Charles Krauthammer's column in the Washington Post this morning,

> This coldbloodedness is a trademark of this nation's most doctrinaire foreign policy "realist." Realism is the billiard ball theory of foreign policy: The only thing that counts is how countries interact, not what's happening inside. You care not a whit about who is running a country. Whether it is Mother Teresa or the Assad family gangsters in Syria, you care only about their external actions, not how they treat their own people.

and thought, "Is that really the billiard ball theory or rather correlation without correlata?"

31-10-05 *One World* (to C. H. Bennett and J. A. Smolin)

On another subject, I'd be curious to know your opinion(s) on Andy Steane's article `quant-ph/0003084`, "A Quantum Computer Only Needs One Universe." I found a fair number of his points in there insightful ... but I'd be willing to listen if your opinion is that I'm not being critical enough. I talked to Sam Braunstein last week about it, and he thought it was "utter tosh," but he didn't say much that put a dent in my opinion. Just the opposite, in fact: It strikes me that there are some ideas in that paper worth really trying hard to develop, if I could just get a handle on what the next step ought to be.

Charlie's Reply

> I rather like Steane's paper, which I hadn't read before. How delightfully put when he says the amount of computation is not properly measured by the number of steps that would have been required to do a computation some other way. I quite agree with him that it generates great confusion to imagine that quantum computers are doing many computations at once. I say they are doing one computation, and can get from input to output faster because of the extra maneuvering room Hilbert space provides during the intermediate stages. Toward the end of his piece, Steane speaks of entanglement as "representing correlations between logical entities without representing the entities themselves". I think this is a less felicitous way of speaking, rather like Mermin's "correlations without correlata". The trouble with both of these, in my view, is that they treat entanglement as a kind of correlation. Rather entanglement should be viewed as the primary thing, and correlation as one of its manifestations. Speaking in the liturgical language of the Church of the Larger Hilbert Space, correlation is a sacrament, the outward and visible sign of entanglement. Alas, your seduction at a tender age by the atheistic doctrine of Jaynes probably prevents you from thinking in this satisfying and clear-headed manner. To retreat from sectarian proselytizing, let me recommend my attached talk on publicity and privacy, which you might enjoy.

31-10-05 *QMech Interpretation & a Web-Thing Against the War* (to H. J. Bernstein)

Thanks for including me as a recipient on this note, though I'm not quite sure what it's all about. Did it have something to do with your last lines? To wit,

Bernsteinism 2: *What is YOUR best argument for those who take comfort (despite the clear-cut differences between statistical ensemble "lack – of – complete knowledge" kind of probabilities and "complex square roots that superpose" kinds of probability present even in the purest of PURE quantum states)?*

Anyway, just to record one of my latest strivings for clarity: I've gotten out of the habit of regarding probability as expressing "lack of complete knowledge." For that phrase effectively already implies that there's something out there that the user of the probability just doesn't have hold of. Instead now, along with de Finetti's usage, I just think of probability as quantifying "uncertainty" full stop. Uncertainty for whatever reason.

31-10-05 *Cover to Cover* (to N. D. Mermin)

By the way, do you know this quote of Bohr's? I've been meaning to send it to you for a while (but kept forgetting), in case it complements the one I usually hear you quote (i.e., the one about "track[ing] down ... relations between the manifold aspects of our experience").

> The extension of physical experience in our own days has ... necessitated a radical revision of the foundation for the unambiguous use of elementary concepts, and has changed our attitude to the aim of physical science. Indeed, from our present standpoint, physics is to be regarded not so much as the study of something a priori given, but rather as the development of methods for ordering and surveying human experience.

01-11-05 *Martha White's All-Purpose Correlation* (to C. H. Bennett)

I'm glad you give me some evidence that I'm not crazy for liking Steane's paper.

Bennettism 26: *Toward the end of his piece, Steane speaks of entanglement as "representing correlations between logical entities without representing the entities themselves". I think this is a less felicitous way of speaking, rather like Mermin's "correlations without correlata". The trouble with both of these, in my view, is that they treat entanglement as a kind of correlation. Rather entanglement should be viewed as the primary thing, and correlation as one of its manifestations. Speaking in the liturgical language of the Church of the Larger Hilbert Space, correlation is a sacrament, the outward and visible sign of entanglement.*

I'm not in disagreement with that ... particularly as the Grand Orgy of the Sexual Interpretation (of QM) would say the same thing. (Ask John.) Entanglement is a property of a quantum state with respect to a given tensor-product structure on the quantum state's Hilbert space. Correlation, on the other hand, is something that arises only through consideration of probabilities for the outcomes of (two-indexed) quantum measurements. Entangled states, consideration of measurements with only one index, NO notion of correlation; simple as that. So correlation can't be viewed as the primary concept.

Here's something I wrote along those lines in a now long forgotten document (of 1998):

> The quantum world brings with it a new resource that senders and receivers can share: quantum entanglement, the stuff Einstein-Podolsky-Rosen pairs and Bell-inequality violations are made of. This new resource, of all the things mentioned so far, is the most truly "quantum" of quantum information. It has no classical analog, nor might it have been imagined in a classical world.
>
> What is quantum entanglement? It is *not* probabilistic correlation between two parts of a whole. Rather it is the *potential* for such a correlation.
>
> In a quick portrayal:
>
> *classical correlation*— Alice and Bob entered a lottery for which they were the only players. They have not opened their "winnings" envelopes yet, but the messages in them say that one is the winner and one is the loser. Which is which, they do not know—they only know the correlation—but the answer is there, objectively existent, without their looking.
>
> *quantum entanglement*— Alice and Bob will eventually perform measurements on the EPR pair their envelopes contain and the outcomes *will* be correlated. However, before the measurements are performed, there are no objectively existent variables already there. Different measurements can and will lead to different correlations.

> In a certain sense, entanglement is a kind of *all-purpose correlation* just waiting to be baked into something real—a quantum "Martha White's Flour" `\cite{Flatt}`. The uses for this all-purpose correlation are manifold within Quantum Information Theory.

[The citation `\cite{Flatt}` goes to Lester Flatt and Earl Scruggs for their *Martha White Theme* song. What the heck, let me include the words to that too; it's at the end of the note.]

What I'm really interested in is whether we might dream up some technical problems to try to work out based on Steane's paper. Any ideas?

By the way, let me know if you get this note; I'm a little worried that your spam filter might pick it in retribution for my indiscretions above.

> **Martha White Theme Song**
>
> Now you bake right,
> (band response:) ah ha,
> With Martha White,
> (band response:) yes ma'am,
> Goodness gracious, good and light,
> With Martha White.
>
> Now you bake better biscuits, cakes, and pies,
> With Martha White Self-Risin' Flour,
> (band response:) that one all-purpose flour,
> With Martha White Self-Risin' Flour
> You've done all right!

01-11-05 *Thanks, Shelves, and Expenses* (to H. C. von Baeyer)

Now that Halloween is over, I have a little time to reply to some emails finally. Thanks so much again for the invitation to W&M. I had a great time, and it was lovely to see so many eyes sparkle from the ideas Friday. (Though I wish I had had more of a chance to talk to Gene Tracy—his eyes particularly sparkled.) And, I'm pleased that my "job interview" seemed to go well at the crabcake place ...

Finally, a further thanks for the detailed description of your bookshelves. My wife and I spent Sunday morning intermittently staring at the walls in our (to be) library, trying to figure out just what we're going to do. With my "pragmatism" collection now just shy of 400 volumes (and growing at an even pace), we've really got to get going on a solution. I want that room to be the intellectual center of the house, and what better theme for it than that "reality is on the make" (one of James' and FCS Schiller's themes, particularly).

01-11-05 *01 + 10* (to T. Duncan)

Thanks for the latest note, and thanks for keeping your eyes peeled.

(The simple existence of) Your note reminded me that I still had not replied to your original query:

Duncanism 2: *This leads to a point that is still muddy to me. It may be a naive question and I'm not quite sure how to phrase it, but let me try this to get started: Given that we must ask what the ontic states can be, such that they force this restriction of incomplete knowledge on the* *epistemic states, why not just place the entire restriction directly on the ontic states themselves? Why not simply say there is a limit to what we can know about the state of the world because there is a fundamental limit to how much information can actually be "stored" about its state? As I understand it, this is something like the view expressed e.g. by Zeilinger ("A Foundational Principle for QM",* Found. Phys. *1999). I realize this is not what you're saying. So, I'd like to better understand why you emphasize the distinction between epistemic and ontic states, when it seems possible that all of the restrictions on our knowledge could be absorbed as consequences of the restrictions on the ontic states. Anyway I'd appreciate any thoughts you have to point me in the right direction here.*

Somehow I never got inspired to give you the answer you deserve—I guess it would require something of an essay on why this distinction should be made at all, and I never mustered the energy. But let me do this by way of a partial answer: I'm just returning from having given the physics colloquium at William and Mary, and because Hans von Baeyer is there, I have also just finished his book, *Information: The New Language of Science*. It's really very, very good, I think. Particularly, Chapter 2, "The Spell of Democritus," I think goes a little toward answering your question. Let me recommend then that you give that a shot first. If it helps, let me know. And if it doesn't, maybe try re-posing your question, and I'll try my best (with a layer of guilt for pushing it off this long) to give you some feedback.

Thanks also for bringing Louise B. Young's book to my attention. Believe it or not, I've had that book on my shelf for five years and never opened it once. (I remember picking it up at a hospital fundraiser.) I'll certainly open it now.

Todd's Preply

> [CAF wrote:] OK, so you already know how I'd answer your Exercise 1.image:
> "I am a cosmic artist – a contributor to a universal creative process."

You can probably guess that I would answer similarly. ☺ I remember reading a book in high school by Louise B. Young, *The Unfinished Universe*. Some of the technical details in the book are questionable, but the spirit of it resonated deeply with me. That spirit is pretty well expressed by the final paragraph: "The universe is unfinished, not just in the limited sense of an incompletely realized plan but in the much deeper sense of a creation that is a living reality of the present. A masterpiece of artistic unity and integrated Form, infused with meaning, is taking shape as time goes by. But its ultimate nature cannot be visualized, its total significance grasped, until the final lines are written."

02-11-05 *von Baeyer* (to A. Y. Khrennikov)

I had the good fortune to meet Hans C. von Baeyer the other day and to read his new book, *Information: The New Language of Science*. It's a very good book, and I recommend it if you haven't read it yet. Anyway, he told me that you had invited him to your next meeting in Växjö. I'm very glad you did, and it is excellent to have another Bayesian among us there!! Have you been able to give any thought to my idea of a special session of "Bayesians" attending the meeting?

02-11-05 *Zeilinger, Me, and von Baeyer Makes Three?* **(to H. C. von Baeyer)**

I'm back, though "lunch" turned out to be effectively a two-day affair! To business now:

von Baeyerism 2 : *I have been dipping into your marvellous book all weekend, and have a question (or seventeen). . . .*

In January I will give an invited talk at the 75th anniversary of the American Association of Physics Teachers, in Anchorage, Alaska. The occasion is my receiving the Gemant Award of the AIP. My title is: "How I learned to stop worrying about Schrödinger's cat." I intend to chronicle my gradual adoption of your point of view about QM. Now the question:

It seems to me that the search is on for the irreducible kernel of QM, you call it the Zing, Maxwell, as a boy, would have called it "The GO of it." You and Zeilinger approach it from opposite ends: You by stripping away all "merely" information theoretic baggage, and he by guessing the answer. You have not yet succeeded in getting down to the bottom, and he has a long way to go before he builds up to the full theory. Analysis v. synthesis, top-down v. bottom-up.

Is this a fair way to characterize your approach?

Thanks by this; I'm certainly flattered. And I hope you'll ask me all seventeen eventually. The way to do good physics is to write good physics, and in my case, I need good questions before I can even hope for the latter.

And, by the way, speaking of good books, I really did enjoy reading yours last week. I think, whether it was intended to be or not, it is a great service to the quantum Bayesian community—sort of a mortar to soften up the popular beach—and I'm grateful for that. For the heck of it, below, I've decided to type in the little notes I made on my bookmark as I read it—they record my agreements and disagreements and a little of my study of the English language. Particularly, you'll note a remark I made about something on page 38. It refers mostly to your sentence,

> If the wave function is nothing but a storehouse of information needed to make correct predictions, then the stuff of the world is really, at bottom, information.

I hope my talk at W&M emphasized that I would be in disagreement with that.

And in fact, to start to answer your question, I think that disagreement probably captures the greatest gulf between the views Anton and I are each striving to develop. It's not a difference of technique (analysis vs. synthesis) per se, but of our *guesses* of an ontology—I think they are opposed to each other.

If you can give me a few days, I plan to come back to you with a much more complete answer. But first I want to re-read all of Zeilinger and Brukner's stuff. This will be a good opportunity for me to flesh out the similarities and differences of what we're hoping to get at. In any case, I'll get back to you long, long before your January talk.

By the way, congratulations on the teaching award. But I hope Anchorage isn't too cold in January!

— — — — — — — — — — — — — — — —

Notes on H. C. von Baeyer, *Information: The New Language of Science*

vB asks "why information not like energy" – 9, 10, 33
special relativity as inserting the observer into physics – 12
Wheeler, subjectivity – 13
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physics as comparison – 203
Boltzmann titles – 223
correct phrase on teleportation! – 225
Zeilinger or Rovelli first? – 226
information about what? whose information? – 227
entanglement not necessarily the key to quantum crypto – 230

02-11-05 *Don't Forget Fierz!* (to H. C. von Baeyer)

In fact, I'm back again already. Just as I was sending off the last note, I realized that I hadn't mentioned the Fierz project again—surely I don't want to forget about that. I wrote my friend Greg Comer today that my mouth is already watering to see your results.

Did by chance you read the Fierz article reprinted in my 29 August 1999 letter to Rüdiger Schack in *Notes on a Paulian Idea*? If so, what did you think of it?

After sending off this note to you, I'll write Harald Atmanspacher with a brief note of introduction about you and the story of our meeting and similar interests, and then I'll put you two in contact with each other.

02-11-05 *And Holladay* (to H. C. von Baeyer)

I just can't get my email right today; I keep remembering things that I forgot to reply to.

von Baeyerism 3: *Full disclosure: Wendell Holladay was my thesis adviser, but he died last year. De mortuis nihil nisi bene. (Of the dead, say nothing but good things.)*

I am sorry to hear about that. Were you close? Probably. In reviewing what I wrote about him in *Notes on a Paulian Idea*, at least I notice that a reply to his letter seemed to be the hardest part of the paper for me to construct—i.e., apparently he gave me a run for the money. He was probably a great guy. Again, I'm sorry.

02-11-05 *Carry Cameras, not Guns* (to C. H. Bennett)

I really enjoyed thumbing through your talk. Particularly, I enjoyed this quote

> Could it be that every major past phenomenon, say Sappho's other poems, or Jimmy Hoffa's murder, can be recovered from physical evidence in principle, if not in practice?
>
> To believe otherwise is venturing dangerously close to the deconstructionist view, abhorred by most scientists, that history is not what "actually" happened, only what we think happened.

as food for thought. It put the importance of the black-hole information question in a new light for me.

But also it reminded me of some stuff I once read about the interesting things that can happen when one tries to mesh the idea of "physical indeterminism" together with the ideas of "history" and "archeology" as autonomous sciences. As it happens, I have an old PDF file which includes a description of George Herbert Mead's thoughts on the subject. I'll attach it for your fun. (Ignore the stuff I wrote at the beginning of it, and just go straight to the copied description of Mead himself on page 2.) By the way, Mead—one of the founders of American pragmatism—was born in South Hadley, MA in 1863. So I suspect his ghost is practically a neighbor to you guys in Wendell on the weekends.

07-11-05 *The Nauseating Terminus* (to G. L. Comer)

I thought your note was on the mark. What it shows is that even in our state of relative ignorance about the goings on of the universe, we should still expect life to arise somewhere/somehow simply due to the vast size of this universe.

But also keep in mind that, from the Bayesian view, probabilities (and the surprise or lack of surprise they entail when one sees the outcome of a trial) represent nothing other than one's degrees of beliefs. Probabilities are not part of the furniture of the universe. In particular, I suspect if we understood chemistry a lot better, then—conditioned on that knowledge—life wouldn't look so surprising at all: That is, *with respect to* a better state of knowledge, one wouldn't have to shore up one's argument for the inevitability of life by statements of the vastness of the universe. It'd be a simple consequence of chemistry as such (and not rare initial conditions).

So that in the end, as scientists we would say: Because our fundamental fields have these Hamiltonians, life is effectively inevitable. The religious-minded at that point should say, "So who ordered those Hamiltonians? You yourself contemplate that they could have been otherwise. Hence, at just that point, there's the concrete expression of an intelligent design. From it, as you have just proved to me dear scientist, everything inevitably flows." And how do you combat that? We each have our terminus of inquiry: ours is an equation, and theirs is an old man behind the equation. By why should either of us terminate? We should both have a horrible sense of dissatisfaction.

07-11-05 *The Second Terminus* (to G. L. Comer)

Comerism 9: *I'm also not sure that I've assumed that probabilities are real. I readily admit that I introduced them via the question asked.*

I didn't mean to imply that you weren't being a good Bayesian. I was just changing the subject a bit ... so that I could make the point that the religious can always find a place for their old man in the sky. For they can always pick up wherever we (the scientists) say THIS is the fundamental equation or THAT is the necessary class of initial conditions. On the other hand, they think they're getting somewhere deeper by positing their old man in the sky than we are with our fundamental equation or initial condition (or both). To that I ask, why have they gotten any deeper: It looks to me to be of effectively the same level. For I can ask of their god, who ordered that? In fact, one can turn their own argument upon themselves. If they ever say that the beauty or the complexity of the world around us cannot be explained without a creator, then I say that the beauty or complexity of that creator could not be explained without an even more powerful and harmonious creator. And so on ad infinitum. The only leg I've ever see them stand on is to reply that, "But God needs no explanation" ... end of argument. And then I retort, "But then why do you demand it of my equations." And of course it just goes round and round.

Anyway, sorry, I wasn't disagreeing with you ... just changing the topic a little. But also, I wasn't even sticking to one topic really. The point about my personal *belief* that life is largely independent of initial conditions (just like planet formation seems to be, for instance), was really a side point.

Or to say it another way, mostly in my last note I was just ranting because the religious right have pissed me off too.

07-11-05 *Zeilinger, Orange House, and Halvorson* (to C. G. Timpson)

You've been on my mind recently, as I was forced to rethink "Zeilinger's Foundational Principle" for a visit with Hans C. von Baeyer at the College of William and Mary last week. It still doesn't fare better in my mind (even though I know it helped inspire Rob Spekkens' "toy model," for which I have great respect). Thus I recommended your paper on the subject to von Baeyer.

Von Baeyer is an interesting guy—his profession now is in the *accurate* popularization of science—and I enjoyed his book *Information: The New Language of Science* quite a bit as a really good attempt in that direction.

Anyway, I'm writing this note because I understand you'll be in New Jersey in mid-December. Where will you be flying into and out of? Newark, presumably? If you have any extra time, it'd be great to have you in the Murray Hill / Cranford area for a day or two (or more). We could put you up in our house and fatten you up with some of Kiki's home cookin' while you and I try to come up with something new in quantum mechanics. (Perhaps I should also mention my single-malt collection? Or would that defeat the purpose?) Finally, I wouldn't mind showing off the best book stores in NY City if you happen to bring an empty case. Just let me know your plans and if you have any time. I can get you from Princeton to Cranford, and then from Cranford to the Newark airport when the time comes.

The reason I know you'll be in NJ is because I've been attending van Fraassen and Halvorson's seminar on quantum information for a few of the sessions. Connected with that, in the next note I'll forward you something I wrote to vF and H after listening to Halvorson's lecture on teleportation. [See 25-10-05 note titled, "GOBs, Bobs, Steering & Teleportation" to H. Halvorson and B. C. van Fraassen.] It's a bit long, but maybe you'll find some parts of it entertaining. To two of the questions in your Budapest slides, "How is so much information transmitted?" and "Just how does the information get from Alice to Bob?," my own answer would be: *No* information is transmitted in the process of teleportation (excepting the two bits that tell Bob which action to be performed). The only nontrivial thing transferred in the process of teleportation is *reference*. Charlie's information (in the sense of Bayesian degrees of belief) stops being *about* the qubit he just handed off to Alice and starts being *about* Bob's. I don't know what you say in your paper "The Grammar of Teleportation," but I just printed it out and hope to have it read in the next couple of days. If we disagree substantially, you can guess I'll be writing you again!

07-11-05 *Charlie Bennett Stories* (to C. H. Bennett)

My friend Greg Comer asked me to remind him of a story I had once told about coin tossing. I dug up the note below and sent it off to him to fulfill his request. Remarkable thing dawned on me though: I've been telling stories about you for at least five years. I should replenish my supply.

From a 28 July 1998 note to G. L. Comer titled "Macaroni"

> I've just been enjoying a Barcelona morning, taking care of some e-mail odds and ends. Today is my last day in Spain; tonight I spend the night in London, finally to be on my way home tomorrow. Thank you very much for your stories, especially the ones about your dad.
>
> The last time I was in Europe (last month), I met up with Charlie Bennett as usual. I don't know how it happened, but we got into a conversation about coin tossing (real coin tossing, not the philosophical issue). Charlie said, "You know, there's a really easy method for predicting someone else's tosses; I'll show you." I said, "Oh bull." Then he pulled a quarter from his pocket and gave it to me. I flipped; he called heads. It was heads. So I did it again. He called heads. It was heads. I said, "Oh you just got lucky again." "Well, do it again," he says. So I flip. He calls heads. It was heads!! In astonishment, I say, "OK what's the trick you have up your sleeve? Did you give me a fixed coin?!?" He bursts out laughing, "No, not at all. One time in eight you get lucky, and when you do, it's a really good joke!"
>
> But still, synchronicity happens. The evidence is our two stories. I'll never forget mine; maybe you'll never forget yours.

Charlie's Reply

> Your version is partly apocryphal. I was demonstrating Martin Gardner's 3-cup trick, which he calls mathematical 3-card monte, to Merrick Furst at CMU. He didn't see how the trick worked, and thought it amazing that I had "guessed" the correct cup–the one holding the little piece of crumpled paper–3 times running. When he asked me how the trick worked, I noticed that, by chance, the correct cup had always been the leftmost one, so I said, "I just always choose the one on the left." This amazed him even more.

08-11-05 *Closet Haecceity* (to H. Barnum)

By the way, I was amused with your comment

Barnumism 13: *"Well, his theory's got haecceity, and that's been déclassé since the late middle ages. You just can't have a theory like that."*

Truth is, I guess I'm a closet haecceitist. I've probably told you that before. Indeed I love this quote by William James,

> Let me give the name of 'vicious abstractionism' to a way of using concepts which may be thus described: We conceive a concrete situation by singling out some salient or important feature in it, and classing it under that; then, instead of adding to its previous characters all the positive consequences which the new way of conceiving it may bring, we proceed to use our concept privatively; reducing the originally rich phenomenon to the naked suggestions of that name abstractly taken, treating it as a case of 'nothing but' that concept, and acting as if all the other characters from out of which the concept is abstracted were expunged. Abstraction, functioning in this way, becomes a means of arrest far more than a means of advance in thought. It mutilates things; it creates difficulties and finds impossibilities; and more than half the trouble that metaphysicians and logicians give themselves over the paradoxes and dialectic puzzles of the universe may, I am convinced, be traced to this relatively simple source. *The viciously privative employment of abstract characters and class names* is, I am persuaded, one of the great original sins of the rationalistic mind.

And you yourself also have some of the blame: When I retrace the footsteps that led me to this fetish, the point of departure seems to have been a conversation with you—when you first convinced Rüdiger and me (through de Finetti's argument) that even "exchangeability" is a subjective judgment. The root of the issue is all right there.

I know you've seen an earlier version of this "Delirium Quantum" thing of mine [`http://arxiv.org/abs/0906.1968`], but I'll send it again: Section 3 on "snowflakes" expands a little on this point in the quantum context. Is it so bad being a closet haecceitist?

08-11-05 *Quibbles, Actions, and Reading* (to H. C. von Baeyer)

von Baeyerism 4: *Re Bayesian: I have a couple of the volumes of the Pauli letters here on my desk. If you look up Bayes in the index, you come to a letter from P. to F. dated 12 July 1952. It is distinctive in including the dream-drawing of quantum mechanics and deeper reality that you are going to paste into your* `PauliProject.pdf`*. Where did you see it? It also appears in a letter to Jung a year later. Is Pauli's letter available in English?*

Fierz wrote at least two detailed drafts of a reply. One bit that you would love is this: Pauli brings up people who believe – or disbelieve – a theory so much that they remain unconvinced by statistical evidence. They stubbornly continue to wait for a measurement that disagrees significantly with the theory. Pauli suggests that the state, or society, could then declare that these people belong in an insane asylum. But he himself doesn't dare to make such rules.

Fierz replies:

> I am totally against the putative opponents, in the name of the state or of society, to hold their views. Rather, one should challenge them to a *bet*. If they accept it, they

> will *lose their money*, which proves that they have not lost their reason, but everything else, in particular, money.
>
> On the other hand, if they reject the bet, it is demonstrated that their conviction is without practical consequences, in other words, that they they don't really believe what they claim.
>
> I agree with your claim, and have always shared it, that purely logically nothing follows from the laws of Bayes and Bernoulli about a concrete single event. This is the answer to your question 2.
>
> However, the maxims of our practical actions can never be justified logically. If that were the case, empirical observations could be reduced to logic. Every action requires a decision, and that is an act of will.

Betting good money, that's where it's at!!!

I like it! What a great quote. Find me a thousand more like that, and I'll be forever in your debt.

Concerning your earlier Zeilinger question, I did take the time to read the Zeilinger and Brukner-Zeilinger papers. Also I reread Hall's and Timpson's commentary on them. (I suspect Mana's commentary is also good, but I didn't discover that one until this morning; so I can't say that I read it, though I know him and think he is a very promising student.) Timpson's comment in particular, I think is very relevant for what you were wanting to know. So I would suggest you have a look at it, if you haven't already. Here are the coordinates: `quant-ph/0112178`. The paper is written in too much of an argumentative style for my tastes, but I do agree with the substantive points.

Particularly, I agree that there's nothing to Zeilinger's principle *as presently sketched* without already invoking quantum mechanics itself. See Timpson's Appendix A. This is not to say that we may not gain some insight from these ruminations. As I said before, I think Spekkens' toy model is a great example in that regard. So Zeilinger's ideas are certainly important ideas, even if ultimately flawed. Indeed, I think what Spekkens' paper really shows is that the heart of quantum mechanics is something beyond the Zeilinger principle. For one might say that Spekkens' toy model already effectively embodies the Z principle—formally through its "knowledge balance principle"—and yet the model is *not* quantum mechanics. Rather it is a local hidden variable theory. As Rob says in his abstract, "A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with this research program."

For my own money, I now think the real heart of the theory is in the Kochen-Specker theorems; and the Z principle just doesn't capture that. In particular, I think the KS theorems are likely the most direct statements we yet have that the "observer cannot be detached" in an ultimate account of reality. Particularly, that's where I think our research needs to focus most: Finding a way to justify a Kochen-Specker-like theorem directly from the idea of engaged/activating observers, and bypassing the details of quantum mechanics. But I just don't see how to go that route yet.

Finally, let me go back to what I view as the more fundamental disagreement in outlooks for Zeilinger and me at the moment. (The disagreement may not be a permanent thing, but it is definitely here for the moment.) The point is made by Timpson pretty clearly:

> Before stating the Foundational Principle, it is helpful to identify two philosophical assumptions that Zeilinger's position incorporates. The first is a form of phenomenalism: physical objects are taken not to exist in and of themselves, but to be mere constructs relating sense impressions [Footnote: Here I take phenomenalism to be the doctrine that the subject matter of all conceivable propositions are one's own actual or possible

experiences, or the actual and possible experiences of another.]; the second assumption is an explicit instrumentalism about the quantum state:

> The initial state ... represents all our information as obtained by earlier observation ... [the time evolved] state is just a short-hand way of representing the outcomes of all possible future observations.

Zeilinger and I are certainly in agreement on the second assumption, but it is the first that drives a wedge between us presently. In fact, just last week, I wrote Bas van Fraassen and Hans Halvorson these words:

> I for one, for instance, pretty much characterize myself as a realist in the time honored sense: That there is a real world out there beyond our whim and fancy, and it is the task of science to hypothesize about its attributes and properties. I am not a philosophical idealist and certainly not a solipsist. And I don't think I am a positivist or an empiricist. Predominantly, I think I lean toward a kind of materialism (though tempered by a lot of pragmatic subtleties).

That's the key point: As far as I can tell, I think I almost lean toward a kind of materialism. Metaphorically: Outside of the alchemist is the philosopher's stone. Without it, there is nothing to enact the transmutation of the baser metals or of the alchemist himself.

If you decide after reading a good portion of the stuff I've already sent you that I'm not completely crazy, and you're interested, then I'll go on a limb and tell you more about what I meant in the last two lines above. And I'll tell you why I'm really so interested in this Pauli-Fierz correspondence (and Fierz's own writings). But if you're already thinking I'm a little crazy, I don't want to fuel the fire.

08-11-05 *A Few More Things* (to H. C. von Baeyer)

At your house, you asked why I called my compendium "The Activating Observer" rather than "The Participating Observer" or some such. Looking in the LaTeX file, I see that I marked out at least this many other tentative titles:

> The Undetached Observer:
> The Catalyzing Observer:
> The Malleable Reality:
> A Malleable Reality:
> The Malleable Substrate:

von Baeyerism 5 : *While I am happy that you are reading my book carefully, I won't spend much effort on defending it. I have learned a lot since I wrote it, and am much more of a Bayesian now.*

If you want to go to the real extreme—the place where Caves, Schack, and I now sit in the spectrum of Bayesianisms—then let me recommend you read these things:

1. Bruno de Finetti, "Probabilism: A Critical Essay on the Theory of Probability and on the Value of Science," Erkenntnis **31**, 169–223 (1989).

2. Bruno de Finetti, *Theory of Probability* (Wiley, New York, 1990), volume 1.

3. Richard Jeffrey, *Subjective Probability (The Real Thing)* (Cambridge University Press, Cambridge, UK, 2004).

4. Richard Jeffrey, *Probability and the Art of Judgment* (Cambridge University Press, Cambridge, UK, 1992).

The two de Finetti pieces are phenomenal, and they were life-changing for me (particularly the first reference, *after I read it the fourth time*!). (It's this life change that is recorded in my samizdat, *Quantum States: What the Hell Are They?*) My only warnings are: 1) to take his hint of solipsism in Section 2 of the first reference above with a grain of salt—he backs off on it by the time he gets to his book—, and 2) and to disregard his support for Mussolini and fascism in the last section of the same paper. Those things are independent of the rest of the development, and one should not get distracted by them—the rest of the paper really is phenomenal.

von Baeyerism 6: *If you look up Bayes in the index, you come to a letter from P. to F. dated 12 July 1952. It is distinctive in including the dream-drawing of quantum mechanics and deeper reality that you are going to paste into your* `PauliProject.pdf`*. Where did you see it? It also appears in a letter to Jung a year later. Is Pauli's letter available in English?*

Those figures that I'm going to paste in refer to two letters from Pauli to Jung: One from 27 May 1953 and one from 31 March 1953. The Pauli-Jung correspondence has been translated into English; that's where I got those long quotes from. They start up on pages 146 and 144 of `PauliProject.pdf`, respectively.

There. Now I think I've answered everything you've ever asked me.

10-11-05 *Kaine, Corzine, Kochen, Krazy Kats and All That* (to H. C. von Baeyer)

von Baeyerism 7: *By the way, the reason I am no longer worried about Schrödinger's cat is not that I think that q.m. is now understood, but that I feel, for the first time in my career, that the problem is in good hands with young folks like you and your friends. I think that where I can make a contribution is in explaining to other physicists and to the public that y'all are NOT crazy, that progress HAS been made, that new ideas ARE being brought to bear, and that there actually IS a hope of realizing Wheeler's sibylline predictions.*

I am very happy to hear that. I didn't get a chance to write you this morning, but you made my day.

10-11-05 *Delirium Quantum* (to B. C. van Fraassen)

I've got to say, I really, really enjoyed today's discussion. Now, I'm getting my money's worth! I thought you did a great job of giving a kind of (perhaps momentary) suspension of disbelief about Rovelli's ideas—i.e., making an honest attempt to get your head around them—and I thought Maudlin did a great job as skeptical counterpoint. I was very pleased with the food for thought it all gave me, and it made the drive back to Cranford go like a flash.

Particularly I'm struck that there really is more of a similarity between Rovelli's outlook and our quantum-Bayesian outlook than I had thought. Of course, there are plenty of differences too (and likely big ones). But I think it is much worthwhile for me to rethink all the various issues surrounding his view.

Anyway, let me attach a file that I hope will amuse you; it's titled `DeliriumQuantum.pdf`. The parts I'm interested in your reading connect mostly to your remarks today about Rovelli's view of the *domain* of quantum theory. In particular, I shoot for some explication of how I can at once say "quantum mechanics is incomplete"—I do that a lot now—and still emphatically deny that it would be worthwhile to try to complete it with a hidden-variable theory. Particularly, I like the analogy to do with a map vs. a globe that you'll see at the end of this file. In total, the only parts that I really care for you to see are Section 4 "Me, Me, Me," Section 6 "Preamble," and Section 7 "B." Of course, you can look at all the rest if you got nothin' better to do, but I don't think the other parts are connected to your discussion today.

I apologize in advance that the pieces I'm recommending were written in a more overtly William Jamesian style than usual (even by my own standards). Sadly, I find more clarity for myself when I do that, but I suspect not all my readers do! I remember I once gave some lectures at Case Western Reserve U., and one of the professors wrote to John Preskill, "During Fuchs's lectures everyone became convinced that quantum information theory was the most important thing ever. But when he left, no one could remember why!" You may have a not-unconnected reaction to these writings.

I'm taking the family on a little "road trip" vacation to New Hampshire for the next four days—starting in about six hours if I ever get to sleep—but I suspect I'll be dropping in on you electronically over the next few days. I'm quite excited about this, and I will probably spout things from time to time as they come to mind. I hope you don't mind: As always, feel free to ignore these notes and chalk them up to being my diary entries if you wish.

10-11-05 *"Action" instead of "Measurement"* (to B. C. van Fraassen)

Let me also elaborate a little on the spiel I gave today condoning the word "action" more than the words "measurement" or "question" in the context of "quantum measurement."

The following explanation comes from an email to another friend last Spring. I do think the very word "measurement" is fraught with trouble, particularly for what Rovelli and us quantum-Bayesians are trying to get after.

> Anyway, the way I view quantum measurement now is this. When one performs a "measurement" on a system, all one is really doing is taking an ACTION on that system. From this view, time evolutions or unitary operations etc., are not actions that one can take on a system; only "measurements" are. Thus the word measurement is really a misnomer—it is only an action. In contradistinction to the old idea that a measurement is a query of nature, or a way of gathering information or knowledge about nature, from this view it is just an action on something external—it is a kick of sorts. The "measurement device" should be thought of as being like a prosthetic hand for the agent—it is merely an extension of him; in this context, it should not be thought of as an independent entity beyond the agent. What quantum theory tells us is that the formal structure of all our possible actions (perhaps via the help of these prosthetic hands) is captured by the idea of a Positive-Operator-Valued Measure (or POVM, or so-called "generalized measurement"). We take our actions upon a system, and in return, the system gives rise to a reaction—in older terms, that is the "measurement outcome"—but the reaction is in the agent himself. The role of the quantum system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.
>
> Reciprocally, there should be a transmutation of the system external to the agent. But the great trouble in quantum interpretation—I now think—is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory

actually talks about, the "measurement outcome") takes place. It should be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. It is only a hint of that more interesting transmutation. And, as you know, somehow out of all this I think of the agent and the observer as being, together, involved in a little act of creation that ultimately has an autonomy of its own—that's the sexual interpretation of quantum mechanics. (However the ideas in that last sentence are a little murkier than what I'm trying to get at now.)

Does "measurement" in this new sense explicitly require consciousness (whatever that is)? I don't think so. But it does require some kind of nonreductive element—some kind of higher-level description that cannot be reduced to a lower-level one. Here's the way I've been putting it in my last three lectures, when talking more particularly about quantum mechanics from the Bayesian perspective. I point out that a Bayesian is, roughly speaking, someone who believes that without gamblers, there cannot be probabilities. Probabilities are not external to gamblers. Then someone always asks, must you have consciousness to have probabilities? And I say, "No." Take as an example my laptop computer loaded with a Bayesian spam filter. It is a perfectly good gambler in the Bayesian sense, but I think most people would be hard-pressed to call it conscious. Similarly, I think we're going to ultimately learn an analogous lesson about all this transformation/transmutation/ creation/measurement business.

On the other hand, I do find myself being tickled toward a more Whiteheadian-like view that, whatever this higher-level description is, every piece of nature has more or less of it, from people all the way to stones and atoms. It's just that you have to be on the inside of the philosopher's stone to see it.

10-11-05 *Wheeler's 20 Questions and Nordheim* (to B. C. van Fraassen)

Below is Wheeler's account of the original incident. It comes from

> J. A. Wheeler, "Bohr, Einstein, and the Strange Lesson of the Quantum," in *Mind in Nature: Nobel Conference XVII, Gustavus Adolphus College, St. Peter, Minnesota*, edited by R. Q. Elvee (Harper & Row, San Francisco, CA, 1982), pp. 1–23, discussion pp. 23–30, 88–89, 112–113, and 148–149.

I'll work on digging up that Nordheim / von Neumann reference I told you about. I don't think it is the Hilbert / Nordheim / von Neumann paper that one easily finds on the web, but something later. But maybe I'm wrong (and in more than one way).

> What is the difference between a "participatory" reality and a reality that exists "out there" independent of the community of perceivers? An example may illustrate a little of the difference. Edward Teller and I, and a dozen other guests, were sitting in the living room of Lothar Nordheim in Durham after dinner. From general conversation we moved on to the game of twenty questions. One, chosen as victim, was sent out of the room. The rest of us agreed on some implausible word like "brontosaurus." Then the victim was let back into the room. To win, he had to discover the word with no more than twenty yes/no questions. Otherwise, he lost. After we had played several rounds, my turn came and I was sent out. . . .

10-11-05 *Nordheim?* (to A. Wilce)

Could you give me the reference to the Nordheim / von Neumann paper you mentioned in your talk (at Perimeter? and maybe Växjö?)? Also can you refresh me on what you actually said about it? I ask because I was telling Bas van Fraassen today about Nordheim's involvement in Wheeler's famous story about playing the game of twenty questions in reverse. And I remarked to vF about how it was interesting that someone involved in quantum logic would have been the instigator of that.

Below is the beginning of Wheeler's account of what happened. [See 10-11-05 note "Wheeler's 20 Questions and Nordheim" to B. C. van Fraassen.]

10-11-05 *Our Own Rovellian Analysis* (to B. C. van Fraassen)

I particularly got a lot out of your "puzzle" analysis at the end. There were some points there, particularly Puzzle 1, that I don't think I've thought much about (or maybe any) before.

About the issue of consistency in Wigner's friend scenario, here's how Asher and I put it in our *Physics Today* piece:

> Does quantum mechanics apply to the observer? Why would it not? To be quantum mechanical is simply to be amenable to a quantum description. Nothing in principle prevents us from quantizing a colleague, say. Let us examine a concrete example: The observer is Cathy (an experimental physicist) who enters her laboratory and sends a photon through a beam splitter. If one of her detectors is activated, it opens a box containing a piece of cake; the other detector opens a box with a piece of fruit. Cathy's friend Erwin (a theorist) stays outside the laboratory and computes Cathy's wavefunction. According to him, she is in a 50/50 superposition of states with some cake or some fruit in her stomach. There is nothing wrong with that; this only represents his knowledge of Cathy. She knows better. As soon as one detector was activated, her wavefunction collapsed. Of course, nothing dramatic happened to her. She just acquired the knowledge of the kind of food she could eat. Some time later, Erwin peeks into the laboratory: Thereby he acquires new knowledge, and the wavefunction he uses to describe Cathy changes. From this example, it is clear that a wavefunction is only a mathematical expression for evaluating probabilities and depends on the knowledge of whoever is doing the computing.
>
> Cathy's story inevitably raises the issue of reversibility; after all, quantum dynamics is time-symmetric. Can Erwin undo the process if he has *not yet* observed Cathy? In principle he can, because the only information Erwin possesses is about the consequences of his potential experiments, not about what is "really there." If Erwin has performed no observation, then there is no reason he cannot reverse Cathy's digestion and memories. Of course, for that he would need complete control of all the microscopic degrees of freedom of Cathy and her laboratory, but that is a practical problem, not a fundamental one.

Of course that's not very satisfactory, but it was a *Physics Today* piece. This is extended and done much better on pages 322, 324(bottom)–325, and 19–23 of my *Notes on a Paulian Idea*. I like those discussions much better.

Still the whole thing can/should be done much, much better and more formally, and your discussion today inspired me to try do it from my perspective with all i's dotted and t's crossed.

Then it'd be nice to compare whatever comes out to your analysis of Rovelli's. Certainly the developments are going to be similar, but there may be differences here or there.

14-11-05 *American Private Equity* (to H. Mabuchi)

> My firm recently spoke with Hideo Mabuchi in the quantum theory group at Cal Tech and David Deutsch at Oxford University among other leading minds, and I thought I would drop you a quick note. My firm is currently looking to fund companies within the quantum technology sector. I spend a portion of my day speaking to experimentalists and theorists in the quantum theory looking for applications.

Is this is a whole new style of crackpot coming at me ... Or by wild chance—not a chance!—is there something to this? Is it time to fund our quantum information retreat in the woods of New England? (Don't forget, you pledged $100K!!)

14-11-05 *Desperately Seeking Postdoc (Funds)* (to G. Brassard)

I'm sure you can guess the pressure Bell Labs is putting on all the researchers here to obtain outside funds for this and that. Well, they've stepped it up a notch again ... and this is partially why I'm writing.

I had an idea (they don't come that often). Can you think of any way we might be able to fund a kind of "joint postdoc" between the two of us to work on our qm-foundational program? If we could do that—in such a way that Lucent could pick up some overhead from the project—something nice might come out of this pressure they're putting on me. Indeed, I'd love to have a postdoc focused on this sort of stuff, rather than the more mundane issues in quantum information, and could make a promise to you to steer the guy right. But funding is not my forte! (As you already know.)

Any ideas?

14-11-05 *Questions, Actions, Answers, & Consequences* (to B. C. van Fraassen)

> But physicists are, at bottom, a naive breed, forever trying to come to terms with the 'world out there' by methods which, however imaginative and refined, involve in essence the same element of contact as a well-placed kick.
>
> — Bryce DeWitt and Neill Graham, 1971

> I think that the sickliest notion of physics, even if a student gets it, is that it is 'the science of masses, molecules, and the ether.' And I think that the healthiest notion, even if a student does not wholly get it, is that physics is the science of the ways of taking hold of bodies and pushing them!
>
> — W. S. Franklin, 1903

Thanks for the note. In my own case, I didn't get nearly as much reading and thinking done over the weekend as I had hoped to—indeed, the time turned out to be a great vacation for the kids, but not so much for dad.

van Fraassenism 3: *I thought I would after all not follow you in replacing the term "measurement", despite all the bad effects old connotations have had in various discussions.*

Fine. That's your business. I just offered my two cents because Maudlin made one of his impassioned spiels precisely on the point of what "measurement" could mean in the Rovelli context—it seemed that some clarification was in order, and particularly worthwhile reiterating the point that if better language were used, quantum "measurement" might stop looking so unfamiliar.

van Fraassenism 4: *We need to bracket the old connotations such as that a measurement result reveals a pre-existing value for the measured observable. But I think we can do that because:*

Not trying to sway you anymore, but let me comment on a couple of these.

van Fraassenism 5: *there is a certain kind of retrodictive inference possible also on the basis of qm measurements. For a long time the paradigm was a source preparing a stream of particles in a certain state – measurements on samples taken from the stream give a good basis for conclusions about just what state the source was preparing, and these conclusions can then be used to predict the outcomes of further measurements made on later samples of the stream*

This is what we in quantum information call quantum-state tomography. One can indeed think of a quantum measurement outcome as *giving information* in the old standard sense in that case and not simply being the "unpredictable consequence of one's action." But then "giving information" is quantified by Shannon's "mutual information," $I(X,Y)$ and not simply by his entropy function $H(Y)$. That is, one has two random variables in the game—one treated classically, namely the "unknown preparation" X, and the other one purely quantum mechanical, the result Y of the measurement interaction. Those two variables have quite different roles, and one indeed would not want to think of X as the "consequence of one's interaction." On the other hand, without making explicit mention of X one has no means for thinking of the elicitation of Y as giving information about anything at all. Before seeing the value of Y, one can expect to be *surprised* to the extent quantified by $H(Y)$, but that's where the story stops.

[For a more detailed Bayesian-like development of this point, you might have a look at our paper "Unknown Quantum States and Operations, a Bayesian View," `quant-ph/0404156` and some of the references therein. Particularly the Introduction and Concluding section might be of some interest to you with regard to the present discussion.]

The only point I want to make to you with regard to your remark above is that, for these reasons, I would say it has no bearing on the issue at debate: I.e., whether it is better to think of a "quantum measurement" as simply an action with an unforeseeable consequence, or rather as a kind of "question-asking" or "information-gathering." It is tangential.

On a completely different subject,

van Fraassenism 6 : *Writers on the subject have emphasized that the main form of measurement in quantum mechanics has as result the value of the observable at the end of the measurement – and that this observable may not even have had a definite value, let alone the same one, before.*

your phrase "MAY NOT even have a definite value" floated to my attention. I guess this floated to my attention because I had recently read the following in one of the Brukner/Zeilinger papers,

> Only in the exceptional case of the qubit in an eigenstate of the measurement apparatus the bit value observed reveals a property already carried by the qubit. Yet in general the value obtained by the measurement has an element of irreducible randomness and therefore cannot be assumed to reveal the bit value or even a hidden property of the system existing before the measurement is performed.

I wondered if your "may not" referred to effectively the same thing as their disclaimer at the beginning of this quote. Maybe it doesn't. Anyway, the Brukner/Zeilinger disclaimer is a point that Caves, Schack, and I now definitely reject: From our view all measurements are generative of a NON-preexisting property regardless of the quantum state. I.e., measurements never reveal "a property already carried by the qubit." For this, of course, we have to adopt a Richard Jeffrey-like analysis of the notion of "certainty"—i.e., that it too, like any probability assignment, is a state of mind—or one along (my reading of) Wittgenstein's—i.e., that "certainty is a tone of voice"—to make it all make sense, but so be it.

I'm curious to understand whether Rovelli's writings already specify an opinion on the issue.

14-11-05 *Nordheim?, 2* (to A. Wilce)

Thanks for the reference, and also for bringing Kalmbach's book to my attention. Nicely enough, we have the latter here in our library, and I just had a cursory look at it. From it, I could tell that von Neumann had at least one paper where he mentioned a logical approach to quantum mechanics (a 1936 paper) before the Birkhoff-vN paper, but do you know if that is the earliest mention? Also I found similar information in this paper by Redei, "Why John von Neumann did not Like the Hilbert Space Formalism ..." which can be found on the web. Redei, in fact, quotes a November 1935 letter from vN to Birkhoff which seems to even indicate that vN had some understanding of a Gleason-like result even at that time. Do you know anything about this?

14-11-05 *Nordheim Again* (to B. C. van Fraassen)

Following up. The paper Alex Wilce talked about was apparently,

> Hilbert, D., von Neumann, J., and Nordheim, L., "Uber die Grundlagen der Quantenmechanik," Math. Annalen **98** (1927) 1–30.

It looks now, however, like I was wrong about these guys talking about the possibility of a quantum logical approach. Instead what this paper seems to be famous for is simply giving some guidelines for what it would mean to axiomatize quantum theory. Von Neumann, as best I can tell, only first talked about lattices (in the quantum context) in a 1935 letter to Birkhoff and first in print in a 1936 paper (by himself, not the one with Birkhoff).

Sorry for feeding you some misinformation.

14-11-05 *Fierz Translator* (to H. Atmanspacher)

I hope you are OK, and that your back is better. We definitely missed you in Konstanz.

Recently I've met a very interesting man, and I want to tell you about him. He is Prof. Hans C. von Baeyer at the College of William and Mary here in the United States. The last several years his interest has been in writing popularizations of science, and he recently won a couple of prestigious awards in that regard; have a look at `http://www.aip.org/aip/awards/gemawd.html` and `http://www.wm.edu/as/physics/news/von-baeyer-receives-american-institute-of-physics-andrew-gemant-award.php`. I myself have read his latest book *Information: The New Language of Science* and was quite pleased with its presentation.

Anyway, the main reason I write you is that it turns out that his godfather was Markus Fierz's brother, and I have gotten him interested in translating the Pauli-Fierz foundational/philosophical correspondence into English. I think he would be great for the task—as he has personal reasons because of his relationship, he is fluent in English, German, and French, he knows quite a bit about our "informational turn" in quantum foundations, and he is looking for an "easy" project to fill the cracks of his larger writing projects (as he goes into retirement this coming year).

The main questions on his mind are 1) whether there is anyone else taking on a project of this nature, 2) who might be a good publisher for this kind of material, and 3) what are the best, most complete sources of this correspondence. I told him you that would be the one most likely to have answers, and I told him something of your institute. I'd like to know the answers too.

If you are interested, I could put you two in contact. Or I could pass on any information you tell me to.

15-11-05 *Canned Answers* (to B. C. van Fraassen)

> One does not infer how things are from one's own certainty.
>
> Certainty is as it were a tone of voice in which one declares how things are, but one does not infer from the tone of voice that one is justified.
>
> — L. Wittgenstein, *On Certainty*, par. 30

> But de Finetti's philosophical view does see determinism as a state of mind masquerading as a state of nature, and sees causality as a fancied magical projection into nature of our own patterns of expectation. Beneath the mask of determinism is a state of mind—certainty—that is intelligible enough ...
>
> Certainties are not the only states of mind that are made to masquerade as states of nature. Probabilistic previsions are also magically projected into nature, to produce "real" probabilities, i.e. "objective chances" or probabilistic "propensities", which de Finetti rejects as firmly as he does deterministic causality.
>
> — R. Jeffrey, "De Finetti's Radical Probabilism"

Addressing your questions:

van Fraassenism 7: *Suppose that an observer assigns eigenstate $|a\rangle$ of A to a system on the basis of a measurement, then predicts with certainty that an immediate further measurement of A will yield value a, and then makes that second measurement and finds a. Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had? He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?*

It is not going to be easy, because this in fact is what Schack and I are actually writing a whole paper about at the moment—this point has been the most controversial thing (with the Mermin, Unruh, Wootters, Spekkens, etc., crowd) that we've said in a while, and it seems that it's going to require a whole paper to do the point justice. But I'll still try to give you the skinny of it:

- Q) He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?
- A) No he doesn't.
- Q) Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had?
- A) No I don't.

The problem is one of the very consistency of the subjective point of view of quantum states. The task we set before ourselves is to completely sever any supposed connections between quantum states and the actual, existent physical properties of the quantum system. It is only from this—if it can be done, and of course we try to argue it can be done—that we get any "interpretive traction" (as Chris Timpson likes to say) for the various problems that plague QM. (In that regard, you might look at Timpson's explanation of the point in the Envoi at the end of his thesis, starting page 223, in connection with his discussion of the problem of the "factivity of knowledge" on pages 176-182, particularly footnote 4.)

This may boil down to a difference between the Rovellian and the Bayesian/Paulian approach; I'm not clear on that yet. I'm looking at the first box on page 3 of your last week's handout at the moment. Rovelli relativizes the states to the observer, even the pure states, and with that—through the eigenstate-eigenvalue link—, YOU SAY, the values of the observables. I'm not completely sure what that means in Rovelli-world yet, however.

I, on the other hand, do know that I would say that a measurement intervention is always generative of a new fact in the world, whatever the measurer's quantum state for the system. If the measurer's state for the system HAPPENS to be an eigenstate of the Hermitian operator describing the measurement intervention, then the measurer will be confident, CERTAIN even, of the consequence of the measurement intervention he is about to perform. But that CERTAINTY is in the sense of Jeffrey and Wittgenstein above—it is a "tone of voice" of utter confidence. The world could still, as a point of principle, smite the measurer down by giving him a consequence that he predicted to be impossible. In a traditional development—with ties to a correspondence theory of truth—we would then say, "Well, that proves the measurer was wrong with his quantum state assignment. He was wrong before he ever went through the motions of the measurement." But as you've gathered, I'm not about traditional developments. Instead I would say, "Even from my view there is a sense in which the measurer's quantum state is WRONG. But it is MADE WRONG by the ACTUAL consequence of the intervention—it is made wrong on the fly; its wrongness was not determined beforehand." And that seems to be the main point of contention.

I think I say some of this better, and give better argumentation for it, in the attached document, but I'll let you be the judge of that. Particularly, I hope the long de Finetti quote helps here. The file is `Certainty.pdf`.

I think I have more to say in a positive vein on Rovelli, but I'll come back to that after lunch.

15-11-05 *February* (to A. Wilce)

Actually, if the slot is available, why don't we see if we can do it sometime between Feb 8 and 15 (inclusive) if you have a speaking date available then.

I would talk about symmetric informationally complete POVMs—what's known about them and what's not.

For our own personal pleasure, I have some new ideas about how working with them may help us out on our tensor-product derivation problem. Hopefully I'll have something worked on the qubit and qutrit cases by then, so that we might be able to think about what to do in the general-dimensional case.

For my talk, I'll try to follow Hans C. von Baeyer's "sawtooth model": plotting difficulty vs time, it should look like a saw blade, lots of peaks, lots of troughs. The idea is to give everyone a little something, whatever their level of sophistication, and in periodic doses. (By the way, I just met von Baeyer at William and Mary, when I gave the physics colloquium there.) I'll certainly keep in mind the undergraduate side of the audience, and I'll ask you more about what that really means as the time draws near!

16-11-05 *Your Phrase* (to B. C. van Fraassen)

What was that thing you said about torture in your last lecture? It was in the context of measurement and Rovelli. I want to get it right. If I'm recalling it right, it might serve a purpose even in my own account of Wigner's friend.

Bas's Reply

> I said that we also should not expect that, if we put someone to the question under torture, we would get an answer that he already had beforehand.

16-11-05 *Phone Message and Relationalism* (to H. Barnum)

On Wednesdays, I go to Princeton to a) partly to get away, but b) partly to attend Halvorson and van Fraassen's seminar. The last couple of sessions have really turned me on to van Fraassen. He's been discussing Rovelli and potential relational interpretations of QM. The part that I'm finding useful is that he is somewhat convincing me that, if one puts the interpretation of probability on the side for a moment, then there may not be that large of a gulf between our views (at least in any nonmetaphysical way). This has taken me a little by surprise, and presently I'm trying to digest it all.

16-11-05 *Red* (to R. W. Spekkens)

Spekkensism 30: *Is "red" ontic or epistemic? On the one hand, it refers to a certain sensation which is certainly subjective and on the other it refers to a particular wavelength of light, which is certainly objective. What good does it do us to argue about this? It's much better to try and articulate the precise sense in which colour vision involves an interplay between the objective and the subjective. Similarly for quantum theory.*

Well put.

17-11-05 *Dutch Meeting* (to A. Y. Khrennikov)

By the way, concerning,

Khrennikovism 9: *This is also related to the conference "Beyond QM" that we organize with G. 't Hooft and other people in Netherlands, 29 May – 2 June, so we plan that a group of people will move from Netherlands to Sweden.*

can you tell me more about the meeting? I might be interested in coming there too if possible. Where will it be? Who else will be there? Is there a website? ... And most importantly, is there any chance I could get invited? (Even the Bayesian program I promote hopes to one day go "beyond QM"!! In fact, that's the whole aim.)

17-11-05 *Rovelli Again* (to R. W. Spekkens)

Spekkensism 31: *I understood that the comparison was between you and Rovelli. In fact, I may even have mentioned this to you myself at some point. I have a standard overhead listing a variety of papers that defend the "epistemic view of quantum states", and Rovelli's paper is on it. Of course, it's not clear to me that he would agree with this characterization of his work, but the techniques are certainly similar. He even has something akin to the knowledge-balance principle. Others have tried to leverage what he's done into an axiomatic derivation of the formalism of quantum theory, but they have yet to be free of a few highly mathematical axioms. This work tends to be in the operational or quantum logic tradition.*

Can you give me references to some of those? My frank opinion is that Rovelli really didn't even get off the ground for that part of the project. (Much of the rest of the paper I like though.) I am starting to think there is a deeper connection between Rovelli's "even measurement outcomes are personal, i.e., relational" and my "any time two things get together, something new happens—quantum measurement and quantum theory just happens to concern only the cases when one of those things is me" (i.e., F-theory), as van Fraassen has been trying to convince me. But we'll see.

17-11-05 *Cash Value* (to H. Halvorson)

I probably laid too many cards on the table yesterday when you attempted to elicit my opinion on Bohmian mechanics. For, one of the arguments I gave was essentially one I had made in a referee report some time ago. Still it's always hard to keep these kinds of things secret—they generally so infect one's way of life that surely everyone can already guess the perpetrator.

Anyway, in principle, I am not a priori against the introduction of extra, unobservable entities into a theory (over and above, say, its rawest statement). But to make me care at all about them, they have to have a "cash value." This is why I gave the example of electromagnetic potentials. In classical electromagnetism, they too are unobservable; only the fields themselves are observable. But often it is quite useful to explicitly introduce the potentials and work in terms of them when solving a problem (if one wishes, one can even believe they're as real as real can be)—their facilitating my problem solving is their cash value and my reason for taking them seriously.

My argument is in the same vein when it comes to Bohmian mechanics.

Who knows, the Bohmians might one day indeed win out with me by giving me a good dollop of cash value. But I'm not banking on it. And life is too short to pursue every wacky theory *simply* because it is consistent. One has to follow one's instincts, and my instinct is that our biggest confusions with quantum theory come about because the only way we know how to word it presently is through the use of *too many* structures, not too few. That is, the number of distinct concepts in the axioms needs trimming, not fattening.

But I don't say anything that I haven't already said a thousand times. And in your case, I'm preaching to the choir anyway!

> The Bohmians are nothing if not an honest and faithful bunch. They are faithful to a theory that, though it has more structure than standard quantum mechanics (i.e., some extra elements over and above it), nevertheless—at least they strive to show rigorously—is empirically equivalent to quantum theory in every way. I am reminded of the secularist who complained to the priest that this is a godless world. The priest replied that the world couldn't possibly make sense without a god suffused through all its parts: The world only *looks* godless, but it only makes *sense* with a god. The priest recommended that the secularist supplement his daily study of the newspaper with a daily reading of the Bible. The difference between the Bohmians and the priest is that they have an equation to supplement standard quantum mechanics, whereas the priest only has unverifiable, humanly constructed words to supplement the daily events. As far as this reviewer can tell, that's the only difference.
>
> Is that difference enough to make Bohmian mechanics a science? It is a structure within mathematics, by construction, and maybe one more or less worthy of study. What will decide its support there are the whims and tastes of mathematicians, whose culture it is to explore various logical structures for reasons more detached than those of the physicist. But is Bohmian mechanics mathematical physics? Or is it more akin

to an equationified religion? This reviewer cannot help but feel that the answer is "no" to the former and "yes" to the latter.

There are so many tough and important problems in basic quantum mechanics waiting to be solved. For instance, for two independent quantum channels (i.e., the tensor product of two trace-preserving completely-positive linear maps, say on finite dimensional Hilbert spaces), is the quantum entropy of the output minimized by an entangled or unentangled input quantum state in general? No one knows the answer, but people have been working on the problem for almost 10 years now. The standing conjecture is that it is an unentangled input that minimizes the entropy, but no one has proved it yet, nor has anyone found a counterexample. Work like that, I would say, is an example of mathematical physics with respect to basic quantum mechanics. If a problem like this can be posed without the burden of the extra structure of Bohmian mechanics, one has to wonder whether adding the extra structure could possibly help in its solution. Maybe it can. But my attitude is, "Show me." If the Bohmians can be the first to the finish line for one or more problems like the one above—i.e., one posed independently of Bohmian considerations, but badly in need of its guidance—I will certainly rethink my stance. It would be a lesson for me, and I can already foresee that I would come to think of myself as misguided as Mach was when he refused to take atoms seriously because of their unobservability. But, show me. And I will be a very different reviewer the next time I am asked to judge a Bohmian research proposal.

19-11-05 *The Pleasures of Checking Consistency* (to H. Halvorson & B. C. van Fraassen)

I'm having my Saturday coffee, reviewing last week's seminar in my head, and every now and then looking up to this week's episode of *Ask This Old House*. Good combination!

Anyway, thinking about the point I tried to make to Hans about the utility of checking for consistency of epistemic or instrumentalist views of the quantum state, here's the way I put it in `arXiv:quant-ph/0104088v1`:

> What is a quantum state? Since the earliest days of quantum theory, the predominant answer has been that the quantum state is a representation of the observer's knowledge of a system. In and of itself, the quantum state has no objective reality. The authors hold this information-based view quite firmly. Despite its association with the founders of quantum theory, however, holding this view does not require a concomitant belief that there is nothing left to learn in quantum foundations. It is quite the opposite in fact: Only by pursuing a promising, but incomplete program can one hope to learn something of lasting value. Challenges to the information-based view arise regularly, and dealing with these challenges builds an understanding and a problem-solving agility that reading and rereading the founders can never engender. With each challenge successfully resolved, one walks away with a deeper sense of the physical content of quantum theory and a growing confidence for tackling questions of its interpretation and applicability. Questions as fundamental and distinct as "Will a nonlinear extension of quantum mechanics be needed to quantize gravity?" and "Which physical resources actually make quantum computation efficient?" start to feel tractable (and even connected) from this perspective.
>
> In this paper, we tackle an understanding-building exercise very much in the spirit

> of these remarks. It is motivated by an apparent conundrum arising from quantum information theory. The issue is that of the *unknown* quantum state.

Indeed I'm getting quite a bit out of this exercise of going over Rovelli.

21-11-05 *Dutch Meeting, 2* (to A. Y. Khrennikov)

Khrennikovism 10: *This is very preliminary information about coming conference "Beyond QM", it will be one of "Lorentz workshops." In fact, I do not know yet where in the Netherlands this series of workshops will take place. The main strategy for selection is that one should really go BEYOND QM. So people who just work on foundations of QM, but think that QM is the final theory are not welcome. I already tried to discuss a few names from "Växjö-conferences team", but they were rejected on the basis that there is nothing "beyond." So this is the situation. Among organisers there are G. 't Hooft, Theo (he was in Växjö), I, two people from SED community (they also were in Växjö, Ana and Lois) and some people from France (Balian and one two more names).*

If "going beyond quantum mechanics" in their sense means the unimaginitive move of *going back* to a hidden variable theory, then you're right, the meeting is not for me. If there were instead some room for exploring other ideas—say, the idea that quantum mechanics is *only our first hint* of a fantastic new kind of reality (one even whose laws of physics can evolve like Darwinian species)—then I might get excited. But looking at the list of the other organizers outside of yourself, it looks to me to be a more closed-minded, less-creative lot. I'd probably stick out like a sore thumb.

Attached is a pseudo-paper of mine, that I might polish into a contribution for this year's Växjö proceedings if you would like. Section 7 expands on what I was saying in the last paragraph.

21-11-05 *Pull!* (to R. Schack)

I remember when I was a kid in Texas, when one went skeet shooting, the command for sending the clay pigeon into the air was "Pull!"

Pull!

It'll be a busy month at Orange House Hotel (first Valente next week, then you the following one, and Timpson the week following that), but so much the better for quantum mechanics.

I think I now see the Wigner-friend issue with complete clarity. It is not so much the subjectivity of unitaries or the definition of a man, but **a)** things-happen-ism, + **b)** extreme personalism (i.e., not only personal probabilities, but personal outcomes for quantum measurements), + **c)** the analogy between quantum measurements and torture. Ingredients **a)** and **c)** are somewhat related to your ideas, so this is not completely independent work, but I think I finally see it all in a total package.

I know you're sick of reading "Me, Me, Me," "Preamble," and "B" in *Delirium Quantum*, but with hindsight, it really was already there. I think that was what was trying to come out of me the day we were watering the lawn. So, I'm going to send you that document again. Also, along with it, I'm going to send several notes I sent Bas van Fraassen last week to help bolster where I'm coming from. They give something of the flow.

Later in the week, I hope to write up a non-poetic version of the whole thing (particular to Wigner) for you.

21-11-05 *Notes to van Fraassen* (to R. Schack)

[[This refers to my 10-11-05 notes "'Action' instead of 'Measurement'" and "Our Own Rovellian Analysis" to Bas van Fraassen.]]

One final point, not in these notes but crucial: I had an epiphany of sorts last week. Von Neumann's description of quantum measurement (and consequently the root of the Wigner conundrum) is nothing more than the "correspondence theory of truth" (that William James always assaults) suitably formalized to the quantum situation. What I learned from Rovelli's paper (or vF's discussion of it, I don't know which) was how to break the instinct to formalize things that way.

21-11-05 *Seeing the Light* (to R. Schack)

Schackcosm 84: *It's the subjectivity of the sample space, stupid!*

I am not sure this is what you are getting at, but your brief lines on Wigner's friend suddenly made me see what could be the light. The friend's sample space is {pointer to the left, pointer to the right}*. Wigner's sample space is different. By contemplating the superposition* $|\text{friend sees left}\rangle + |\text{friend sees right}\rangle$*, Wigner does not treat "pointer to the left" and "pointer to the right" as measurement outcomes. In order to give a proper analysis of the so-called paradox, we must first know Wigner's sample space. My mistake was to identify the "left" in Wigner's superposition with the "left" in the friend's set of outcomes. They are not the same thing. The latter is what the friend sees. The former is a label in an abstract Hilbert space. Wigner and his friend are not talking about the same quantum measurement. There is no paradox.*

Yep, that's essentially it. The only thing further is that with all this "Me, Me, Me" business, one gives a gloss for why the sample spaces are different: The sample spaces are tied to the agents by definition.

It's really nothing that Asher and I didn't already say in our *Physics Today* thing that I quoted for Bas (and sent to you in the compilation). It's just that now I feel much more comfortable with what I would do if confronted aggressively with a question like, "Do you or do you not think a human intervention is necessary for a quantum measurement to come about? And related, do you or do you not think things happen independently of human intervention?"

Reversing the friend's wavefunction may change the story Wigner will coerce out of him (thus the bit about torture), but it will not change what has happened in the world. They are two different things. Quantum mechanics is not about what happens in the world; it is only about what might come about from a ME's interventions into it—and that is just a very small part of reality.

21-11-05 *Update on Schack and Wigner* (to C. G. Timpson)

Let me give you an update on what Schack is up to. He is, after all, coming to New Jersey, but the dates he finally settled on are December 3-8. Thus, if during your trip, you come by the Orange House Hotel, I'll have you all to myself.

By the way, I'm now also completely prepared for your visit too. That is: I'm not completely sure which troubles you had with a quantum-Bayesian account of the Wigner's friend issue (though Rüdiger gave me something of a sketch), but I am now quite confident that I can handle anything you throw at me. (See my note to Rüdiger below.) [See 21-11-05 note "Pull!" to R. Schack.]

Sometimes you have to rework these things in your head a thousand times, but in the end it's definitely all worth it. Thanks for sewing a seed of doubt in me: It caused me to strive as I thought I would, and I'm fairly pleased with the result.

We have a lot to talk about when you come.

21-11-05 *AJP Resource Letter* (to R. H. Stuewer)

The Anchorage meeting you mention must be the same one that Hans Christian von Baeyer wrote me about recently, at which he'll be receiving the Gemant Award (I think it was called). He wrote me this very nice note saying that he would title his talk, "How I Learned To Stop Worrying About Schroedinger's Cat," and said,

von Baeyerism 8: *I intend to chronicle my gradual adoption of your point of view about QM. … By the way, the reason I am no longer worried about Schrödinger's cat is not that I think that q.m. is now understood, but that I feel, for the first time in my career, that the problem is in good hands with young folks like you and your friends. I think that where I can make a contribution is in explaining to other physicists and to the public that y'all are NOT crazy, that progress HAS been made, that new ideas ARE being brought to bear, and that there actually IS a hope of realizing Wheeler's sibylline predictions.*

I'm so very happy he's giving exposure like that to our quantum foundational program (essentially the stuff I talked about at Seven Pines when you invited me) to such a good audience. With exposure, we might just get a workforce … and then who knows how things would really fly!

22-11-05 *MGM Curiosity* (to C. M. Caves)

What is his trouble with quantum information?

This kind of reminds me of the time I heard Willis Lamb talking about quantum information in the same breath as Bohmian mechanics, GRW theories, and the like. I recall he ended the diatribe by saying "quantum teleportation is beneath contempt."

22-11-05 *Difference Between Classical & Quantum* (to C. M. Caves & R. Schack)

I enjoyed finding these words in Joel Achenbach's piece in the *Washington Post* today:

> Reality gets fuzzier under closer scrutiny. It must be some kind of law of physics. (Actually, I think it's called quantum mechanics.) You would think that abundant evidence, steadily compiled, would make everything clearer, but the opposite is true. Partial knowledge gives us a simple picture of the world and its phenomena – an extension of the ignorance-is-bliss rule. Look deeper and you wind up scratching your head.

That's about the only truth I've ever seen in decoherence.

22-11-05 *From the Poetaster* (to R. Schack)

Schackcosm 85: *If a Bayesian spam filter can be an agent, then a quantum computer can act as an agent performing a quantum measurement.*

I see two possibilities now: You don't agree with this in some essential way, or your poetry is in trouble (I am hoping for the latter).

Poetry can always be made to work; that's the beauty of it. If ostensibly it can't, then it must not have been poetry to begin with, but poetastry.

I haven't gotten my head around what you're writing yet. I'll keep trying (as time permits today).

But come on, do you really believe that a man (and consequently a Bayesian agent) CANNOT be built in a laboratory by the hand of another man? That is what is at issue. I believe there is no difference in principle between a natural human body and one that can be put together in the laboratory. And if there is no difference, then we need a de-anthropocentrized way of putting what an agent is.

Yes, we are all already potential quantum computers. As the only difference between being a classical or quantum system—as treated in physical theory—is someone's state of mind about it (i.e., one's degrees of belief as captured by their quantum states for it). I can't accept that that there's any intrinsic difference: that would be the myth of Zurek's decoherence program.

But I have to go to an annoying meeting at the moment.

Rüdiger's Preply

Note on Wigner's friend, for Chris from RS.

If a Bayesian spam filter can be an agent, then a quantum computer can act as an agent performing a quantum measurement. Consider a joint system of two qubits, and let $s_{\rm prior}$ be a bit string that uses some code to describe the prior quantum state of the joint system. Now perform a projective measurement on the first qubit and let $s_{\rm post}(j)$ ($j = 0, 1$) be bit strings that encode the post-measurement state of the second qubit, conditioned on the measurement outcome, j. Consider a composite Hilbert space,

$$\mathcal{H} = \mathcal{H}_{\rm qubit1} \otimes \mathcal{H}_{\rm qubit2} \otimes \mathcal{H}_{\rm prior} \otimes \mathcal{H}_{\rm post} \otimes \mathcal{H}_{\rm agent} \ , \tag{43}$$

where $\mathcal{H}_{\rm qubit1} \otimes \mathcal{H}_{\rm qubit2}$ is the Hilbert space of the two qubit-system above, $\mathcal{H}_{\rm prior}$ is a quantum register large enough to hold the string $s_{\rm prior}$ in the form of a computational basis state, similarly $\mathcal{H}_{\rm post}$ for the strings $s_{\rm post}(j)$, and $\mathcal{H}_{\rm agent}$ houses the working space/internal state of the quantum computer/agent.

Let $|\phi\rangle, |\phi_0\rangle, |\phi_1\rangle \in \mathcal{H}_{\rm agent}$ and U be such that

$$U\Big(|\psi\rangle \otimes |j\rangle \otimes |s_{\rm prior}\rangle \otimes |0\rangle \otimes |\phi\rangle\Big) = |\psi\rangle \otimes |j\rangle \otimes |s_{\rm prior}\rangle \otimes |s_{\rm post}(j)\rangle \otimes |\phi_j\rangle \tag{44}$$

for any one-qubit state $|\psi\rangle$ and $j = 0, 1$.

To repeat, if a spam filter is an agent, then this quantum computer is an agent. It starts from a prior judgement and a sample space, interacts with the system, brings about a measurement outcome and records the post-measurement state in a register.

Is this any different from Wigner's friend? No. Wigner sets up this device in the initial state

$$|\Psi_{\rm ini}\rangle = \frac{1}{\sqrt{2}}(|00\rangle + |11\rangle) \otimes |s_{\rm prior}\rangle \otimes |0\rangle \otimes |\phi\rangle \ . \tag{45}$$

He applies U and writes down a superposition state. Does this mean that the friend is in a funny state? Not at all. Ask him (by looking, e.g., at the register encoding the posterior) and he will give you a simple answer. Reverse him, then ask him, and he will show you a register in the erased state $|0\rangle$, ready for the measurement.

I see two possibilities now: You don't agree with this in some essential way, or your poetry is in trouble (I am hoping for the latter). Does your poetry (hands in the form of meters extending into the world, the sexual interpretation of quantum mechanics, etc) work for a Bayesian spam filter?

22-11-05 *The Point: Inanimate Object, Wigner, his Friend, or Agent — It Doesn't Matter* (to R. Schack)

> I think the greatest lesson quantum theory holds for us is that when two pieces of the world come together, they give birth. [Bring two fists together and then open them to imply an explosion.] They give birth to FACTS in a way not so unlike the romantic notion of parenthood: that a child is more than the sum total of her parents, an entity unto herself with untold potential for reshaping the world. Add a new piece to a puzzle—not to its beginning or end or edges, but somewhere deep in its middle—and all the extant pieces must be rejiggled or recut to make a new, but different, whole. That is the great lesson.
>
> But quantum mechanics is only a glimpse into this profound feature of nature; it is only a part of the story. For its focus is exclusively upon a very special case of this phenomenon: The case where one piece of the world is a highly-developed decision-making agent—an experimentalist—and the other piece is some fraction of the world that captures his attention or interest.

It's things-happen-ism, man. But the point is, that shouldn't be confused with what quantum mechanics is about. QM is only about that other point of view, what-I-happen-to-cause-ism.

22-11-05 *Spoke Too Soon?* (to R. Schack)

OK, maybe I spoke too soon when I said we were in essential agreement yesterday after you said, "It's the subjectivity of the sample space, stupid!"

I looked at your LaTeXed note over and over today, but couldn't figure out what you were trying to get at. Do you want to try again?

Was your note from today intended to be in disagreement with what I had written you in reply yesterday?

22-11-05 *Story of Bohr's Office* (to G. Brassard)

Below is a story I wrote about my own visit to Bohr's office. [See 03-02-04 note titled "The Land of Bohr" to G. L. Comer.] There is a certain spirit there, isn't there?

22-11-05 *An Amusement* (to T. Maudlin)

Maudlinism 1: *I wrote this little piece earlier in the semester, after there had been some discussion in Bas and Hans's class about using the classical notion of information in quantum contexts. I thought you might find it amusing. I somehow think you will object to something in it, but I don't know quite what…*

Thanks for sending me your piece. I gave it a once-over this afternoon, and as my six-year-old likes to say, "I didn't like it … (pause) … I loved it!" I thought it was great; quite creative. And I was particularly pleased to learn that, "Dr. Psi, like all evil geniuses, is a perfect Bayesian."

But you were right, I don't agree with everything in the document. *However*, at least at my level of thoroughness for this once-over, I do think I agree with everything in the first 7.5 pages. That's gotta count for something! Particularly, I agree with the very first paragraph of the paper, and then the way you laid everything out and unrolled it thereafter in the rest of those 7.5 pages:

> Quantum teleportation is a very interesting physical phenomenon, predicted by quantum theory and confirmed in the lab. There has been some question about what the proper information theoretic analysis of this phenomenon is. In particular, there have been doubts about whether "classical information theory" (i.e. Shannon's theory) can be applied to such a quantum situation. These worries are misplaced. Shannon's theory is "classical" only in the sense that it is a well-established, canonical theory, not in the sense that it applies only to "classical" physics (whatever that is). I hope the following little playlet will be instructive.

Where we start to part company is where you, indirectly, invoke a comparison between pure quantum states and classical phase space points in your discussion of the no-cloning theorem. I say that's a comparison that shouldn't be made; it's like comparing apples to oranges, Frank Sinatra to Mötley Crüe, beliefs to facts. And then we diverge still further with the discussion of nonlocality.

But I really did like everything else! I remember in high school once writing a paper on Thoreau. When my teacher returned it with an A+, I was really humbled when she furthermore said, "Chris, you seem to have a feeling for Thoreau!" Well, from the devious insight and style of conversation you put into this document, I almost want to say, "Tim, you seem to have a feeling for Dr. Psi, the evil genius!" But maybe that goes too far …

See you tomorrow in class.

22-11-05 *On the Fly* (to G. Musser)

By the way, I should also tell you about how I have consciously adopted some of your phraseology in my own explanations. It comes from this thing you once wrote me:

Musserism 27: *Let me see whether I understand your vision of a world in continuous creation. Ernst has bequeathed me his cat; it arrives in a box. I open it to find the cat is … alive. Before I do, the cat is neither alive nor dead; it is not even in a purgatorial superposition. In fact, the categories of cat alive and cat dead don't even exist yet. When I open the box, I create the fact of a live cat. That fact had to come into existence* <u>*sometime*</u>*. In the classical deterministic universe, it came into existence when the universe did. All the arbitrariness was in the ICs. The quantum universe makes up facts as it goes along. The arbitrariness is spread out over time. It's the distinction between my wife, who makes plans and carries them out, and me, who decides step by step on the fly.*

The main point is the one in the last sentence, that the quantum universe makes up facts on the fly. I can't tell you how many times I've used that phrase since, but the last example I can think of came last week in a letter to Bas van Fraassen (a philosophy professor at Princeton).

I'll paste it below for your amusement. [See 15-11-05 note "Canned Answers" to B. C. van Fraassen.]

23-11-05 *Cathy, the Friend, and the Device-Agent* (to R. Schack)

OK, I start to appreciate what you're trying to get at.

Among other things, you call into question how I think I can make the inference from quantum mechanics (which is about what-I-happen-to-cause-ism) to "the greatest lesson quantum theory holds for us" (which is about things-happen-ism). Is that correct?

That indeed is a good question. It is something I should unroll a little more and be a little less declarative about.

But I also think your question (or maybe it is simply an independent question) has to do with the consistency of our Wittgensteinian notion of certainty (i.e., that one can have certainty and still not have it fulfilled).

Let me read and reread your two notes a little more, now that the kids are out of the house!

23-11-05 *The Skinny Answer* (to R. Schack)

Schackcosm 86 : *There is no big problem with the following bit of poetry:*

> The role of the quantum system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.

The qubit is a catalyst which brings about a transformation of the device-agent.

But how about the part where the world external to the agent makes one of the outcomes happen? How does Wigner's description of the device- agent tie in with the poetic tale that nothing prevents the external world from smiting the device-agent if it bets its life (let's say, its power supply) on the outcome? Doesn't Wigner have to conclude that the device-agent is wrong in thinking that the external world has made one of the outcomes come about?

Answering the last question in particular: No.

You see, if I answered "yes" it would be equivalent to saying that it is the external view that is always the right one. And that leads one down the path of many-world-ism, or at least Everettism. Instead, the move here is to say, *all* internal views are equally valid—the device-agent's (who sees an actual outcome following his interaction), Wigner's (who sees an actual outcome following his interaction), Wigner's friend's (who sees . . .), etc. When all is said and done, the "Me, Me, Me" facts actually are disembodied facts—that's the things-happen-ism part. But there is no contradiction with quantum mechanics because Wigner's degrees of belief are not degrees of belief about the device-agent's internal view. His degrees of belief are about what he might coerce out of the device-agent or even the system (or both) if he were to interact with them.

That probably obfuscated things even worse for you. But give me a reaction, and that will be a useful datum.

23-11-05 *Wigner's Impotence* (to R. Schack)

Schackcosm 87 : *To ask it in a yet different way, can Wigner undo the outcome that the world has made come about?*

No, he cannot. That is the disconnect between reality and Wigner's degrees of belief (including his certainty). Wigner can only undo *state-vector* time evolutions for systems external to himself.

23-11-05 *After-Shower Thought* (to R. Schack)

I'm going to have to pack up and get on the Princeton trek soon, but let me just record one phrase that came to mind while I was in the shower.

Schackcosm 88: *I don't feel, though, that*

> When all is said and done, the "Me, Me, Me" facts actually are disembodied facts—that's the things-happen-ism part.

gives a satisfactory connection to the things-happen-ism part. What and who determines the moment at which "all is said and done"? You certainly don't want to say it's when Wigner has queried the device-agent. To repeat, what connects the "actual outcomes" or facts that the players see to the things that happen?

Quantum theory has no more power within it to say when (quantum) events happen than classical probability theory has the power within *it* to say when its own events happen. Maybe this is why some developments of classical probability theory are careful to say that the h's in a probability assignment $P(h)$ are propositions. Propositions don't "happen", they are simply either true or false. And thus, by fiat, one never has to address the question of when h "happens". But we know that we shouldn't try to make that further move (i.e., viewing the h's as propositions with timeless truth values) in the quantum setting. Quantum events do "happen." But that doesn't mean that quantum theory should have the burden on it of explaining or determining "the moment at which 'all is said and done'."

This is ultimately, I suspect, why our view will be greeted with the same revulsion that William James's theory of truth was by Russell and Moore and the like: Because for James, too, truth is something that is made to happen; it is not something that is just there in a timeless way.

> The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication*. Its validity is the process of its valid-*ation*.

Have we made any progress?

23-11-05 *Basil* (to A. Y. Khrennikov)

Khrennikovism 11: *I shall read your paper. I hope you are not against if I also give it Basil Hiley. He is visiting me this week.*

I meant I might polish into a real paper for you upcoming proceedings, by adding an introduction, etc. Of course it is fine to share it with Basil.

Khrennikovism 12: *More or less you are right. It is more or less about HV, or more generally about realism as the basis of QM. I think it is the continuation of the debate between Mach and Boltzmann. You are definitely Machian, he would like your new idea about the evolution of laws of nature. But do not forget that Mach rejected Boltzmann's ideas on atoms. There was no experimental evidence of the existence of atoms. Mach considered them as unuseful elements of knowledge. See! But finally Boltzmann was right!*

It's not true that I'm Machian. In fact, I just wrote some stuff to Hans Halvorson and Bas van Fraassen the other day saying the opposite. I'll paste it in below; it clarifies the point (and note the reference to Mach). [See 17-11-05 note "Cash Value" to H. Halvorson.] However, it is true that I think that toying with hidden-variable extensions of quantum mechanics is a waste of time. Trying to make an analogy between Boltzmann's introduction of atoms and Bohm's introduction of trajectories is not a good one. I think you were much closer to the mark when you were still talking about "contextual probabilities." The story of quantum mechanics, I think, is much more like that.

Read the stuff below. And don't forget to read Section 7 in the pseudo-paper I sent you; I think—but maybe I'm wrong—it's a position that both you and Basil could respect. (For it says, in the end, quantum mechanics is incomplete.)

24-11-05 *Howard's Scream!* (to H. Barnum)

Quoting you:

Barnumism 14: *I tend to think that Many-Worlds (despite my having spent a lot of effort in my life playing devil's advocate for it) gets things backwards: stuff happens, we do scientific (i.e. some variety of roughly Bayesian in a very general, not necessarily conscious, sense) inference about the stuff that happens, the definite results we experience for measurements, we come up with a theory that systematizes the resulting beliefs (as evidenced by our willingness to bet, in a very generalized sense, on the outcomes of experiments and such). This systematization of our betting behavior faced with experiments can be represented in terms of probabilities given by the Born rule. Rederiving the Born probabilities from a part of the formalism that was cooked up, and especially, further developed and held onto, in part to give a good representation of just these probabilities, seems somewhat backwards. Without the probabilities, and the terrific guidance they give to our actions, who would have bothered with quantum mechanics anyway? I guess one can say that the rederivation is a sophisticated attempt to keep the probabilities and solve other problems that came along with quantum mechanics. But it still raises, for me, a serious problem of: what then of the formal and informal scientific reasoning, based on measurements having definite results, that brought us to the QM formalism and Born rule in the first place? Must we reconstruct it all in terms of Everettian branchings, with never a definite result?*

Hear, hear! I like that line of argument. And I wonder if you can put some real flesh on it. It'd be nice to derive an honest-to-god contradiction.

But here's maybe something to think about. It's a report from Heisenberg of something Einstein said to him:

> It is quite wrong to try founding a theory on observable magnitudes alone. In reality the very opposite happens. It is the theory which decides what we can observe. You must appreciate that observation is a very complicated process. The phenomenon under observation produces certain events in our measuring apparatus. As a result, further processes take place in the apparatus, which eventually and by complicated paths produce sense impressions and help us to fix the effects in our consciousness. Along this whole path—from the phenomenon to its fixation in our consciousness—we must be able to tell how nature functions, must know the natural laws at least in practical terms, before we can claim to have observed anything at all. Only theory, that is, knowledge of natural laws, enables us to deduce the underlying phenomena from our sense impressions. When we claim that we can observe something new, we ought really

> to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our consciousness—function in such a way that we can rely upon them and hence speak of "observation."

Particularly, I'm thinking about the part, "When we claim that we can observe something new, we ought really to be saying that, although we are about to formulate new natural laws that do not agree with the old ones, we nevertheless assume that the existing laws—covering the whole path from the phenomenon to our consciousness—function in such a way that we can rely upon them and hence speak of observation."

Could it be that this sort of thing helps save the move from "definite outcome empirical world" to "nothing happens at all Everett world" that the Everettians would like to make? I.e., that the idea of definite outcomes is just some old-theory scaffolding that eventually falls away after the shining tower of Everett is open for public service?

Happy Thanksgiving, by the way.

28-11-05 *Your Visit* (to G. Valente)

Thanks again for pushing me about Ramsey. I've been reading on and off about him over the holiday weekend, and I've learned much in the process—it's given me a lot of food for thought.

Next project, your paper!

29-11-05 *Abstract for APS March Meeting – Trial 1* (to me)

> Title:
> Quantum Mechanics in Terms of Symmetric Measurements
>
> Abstract:
> In the neo-Bayesian view of quantum mechanics that Appleby, Caves, Pitowsky, Schack, the author, and others are developing, quantum states are taken to be compendia of partial beliefs about potential measurement outcomes, rather than objective properties of quantum systems. Different observers may validly have different quantum states for a single system, and the ultimate origin of each individual state assignment is taken to be unanalyzable within physical theory—its origin, instead, ultimately comes from probability assignments made at stages of physical investigation or laboratory practice previous to quantum theory. The objective content of quantum mechanics (i.e., the part making no reference to observers) thus resides somewhere else than in the quantum state, and various ideas for where that "somewhere else" is are presently under debate—there are adherents to the idea that it is purely in the "measurement clicks," there are adherents to the idea that it is in intrinsic, observer-independent Hamiltonians, there are adherents to the idea that it is in the normative rules quantum theory supplies for updating quantum states, and so on. This part of the program is an active area of investigation; what is overwhelmingly agreed upon is only the opening statement. Still, quantum states are not simply Bayesian probability assignments themselves, and different representations of the theory (in terms of state vectors or Wigner functions or C^*-algebras and the like) can take one further from or closer to a Bayesian point of view. It is thus worthwhile spending some time thinking about which representation might be the most propitious for the point of view and might, in turn, carry us the most

quickly toward solutions of some of the open problems. In this talk, I will present several results regarding a representation of quantum mechanics in terms of symmetric bases of positive-semidefinite operators. I will also argue why this is probably the most natural representation for developing a Bayesian-style interpretation of quantum mechanics.

30-11-05 *TGQI Nomination Votes ...* (to H. Mabuchi)

Sorry I dropped out of sight almost as soon as you last heard from me: Mr. Giovanni Valente arrived for his visit and has kept me fascinated with all kinds of "noncommutative probability" ever since. But I'll forward on my picks in a couple of minutes, while he takes a breather from our debate.

01-12-05 *Focus Session Quantum Foundations* (to R. W. Spekkens)

OK, I did it. Thanks for keeping me abreast of this.

Below is the abstract I originally wrote up before the submissions page made me trim it. (I include it here so that I have a record of it.)

Title: Quantum Mechanics in Terms of Symmetric Measurements

Abstract: [See report of it in 01-12-05 note "The Swedish Bayesian Team," and add these words to its end:] In this talk, I will present several results regarding a representation of quantum mechanics in terms of symmetric bases of positive-semidefinite operators. I will also argue why this is probably the most natural representation for developing a Bayesian-style interpretation of quantum mechanics.

01-12-05 *The Swedish Bayesian Team?* (to D. M. Appleby & Others)

I'm writing this note to see if you might be available to come to a meeting in Växjö, Sweden, June 4-9, 2006? The general meeting is *Foundations of Probability and Physics – 4* organized predominantly by Andrei Khrennikov along the lines of his previous meetings, but he has given me a small budget to contribute toward trying to attract a group of quantum-Bayesians. If this meeting goes like the ones I've been associated with before, it will turn out to be a pleasant and productive time for the group I hope to attract there. (It is a nice village with a couple of bars and a nice lake to walk around and discuss things.)

Ideally, I'd like to have you, Itamar Pitowsky, Richard Gill, Matt Leifer, Ariel Caticha, and Rüdiger Schack there to contribute toward a discussion. [...]

The sort of idea I have in mind for a focused discussion can be read in some of the words I used in a recent abstract I wrote for my talk at the APS March meeting. I'll place those words below to give you a kind of feeling.

> Opening from Fuchs's APS abstract:
>
> In the neo-Bayesian view of quantum mechanics that Appleby, Caves, Pitowsky, Schack, the author, and others are developing, quantum states are taken to be compendia of partial beliefs about potential measurement outcomes, rather than objective properties of quantum systems. Different observers may validly have different quantum states for a single system, and the ultimate origin of each individual state assignment is

taken to be unanalyzable within physical theory—its origin, instead, ultimately comes from probability assignments made at stages of physical investigation or laboratory practice previous to quantum theory. The objective content of quantum mechanics (i.e., the part making no reference to observers) thus resides somewhere else than in the quantum state, and various ideas for where that "somewhere else" is are presently under debate—there are adherents to the idea that it is purely in the "measurement clicks," there are adherents to the idea that it is in intrinsic, observer-independent Hamiltonians, there are adherents to the idea that it is in the normative rules quantum theory supplies for updating quantum states, and so on. This part of the program is an active area of investigation; what is overwhelmingly agreed upon is only the opening statement. Still, quantum states are not simply Bayesian probability assignments themselves, and different representations of the theory (in terms of state vectors or Wigner functions or C^*-algebras and the like) can take one further from or closer to a Bayesian point of view. It is thus worthwhile spending some time thinking about which representation might be the most propitious for the point of view and might, in turn, carry us the most quickly toward solutions of some of the open problems.

01-12-05 *State Spaces* (to H. Halvorson)

Luckily, our library had the right volume of Alfsen and Shultz, and I checked it out. This new kind of characterization of complete positivity has me quite intrigued: Now if I can only translate the stuff I find in this book to finite dimensional language so I can see what it means!

What were the additional references you were going to give me on the relation between convex (state-space) structures and algebraic properties?

By the way, in times past, I've had the hope of deriving the idea of complete positivity from a Gleason-like theorem for the joint probabilities over measurement outcomes for measurements on a single system at two separate times. I've had some concrete ideas about how to articulate that (and a good load of calculations), but I never could quite get a theorem to work. If you'd like to throw in on a project like this, maybe we could discuss this too in the coming weeks.

01-12-05 *Finite Sets (then limits) Policy* (to H. Halvorson)

Also, here's the quote from Ed Jaynes that I almost read aloud in your seminar yesterday. I suspect nothing deeper than this is going on with Redei's criticism of *whatever he is imagining is* the Bayesian approach. (I.e., there are many different approaches that one might think of as more or less Bayesian, and whichever one Redei is attacking, I doubt his problems are of any origin other than what Jaynes expresses below.)

> It is very important to note that [the theorems we have just proven] have been established only for probabilities assigned on finite sets of propositions. In principle, every problem must start with such finite sets of probabilities; extension to infinite sets is permitted only when this is the result of a well-defined and well-behaved limiting process from a finite set. More generally, in any mathematical operations involving infinite sets the safe procedure is the finite sets policy:
>
> > Apply the ordinary processes of arithmetic and analysis only to expressions with a finite number of terms. Then after the calculation is done, observe

how the resulting finite expressions behave as the number of terms increases indefinitely.

In laying down this rule of conduct, we are only following the policy that mathematicians from Archimedes to Gauss have considered clearly necessary for nonsense avoidance in all of mathematics. But more recently, the popularity of infinite set theory and measure theory have led some to disregard it and seek short-cuts which purport to use measure theory directly. Note, however, that this rule of conduct is consistent with the original Lebesgue definition of measure, and when a well-behaved limit exists it leads us automatically to correct "measure theoretic" results. Indeed, this is how Lebesgue found his first results.

The danger is that the present measure theory notation presupposes the infinite limit already accomplished, but contains no symbol indicating which limiting process was used. Yet as noted in our Preface, different limiting processes—equally well-behaved—lead in general to different results. When there is no well-behaved limit, any attempt to go directly to the limit can result in nonsense, the cause of which cannot be seen as long as one looks only at the limit, and not at the limiting process.

02-12-05 *Princeton Quantum Informatics Conf* (to M. O. Scully)

I'd be happy to help you organize a Princeton Quantum Informatics conference, particularly if it will involve in some part a tribute to John Wheeler (as you had mentioned previously). Anyway, whichever way, I have a lot of friends in that community, and I think we could make a good meeting . . . if I didn't try too many of their patiences with my quantum Bayesian conference in Konstanz last year!

As it turns out, I'll be in Princeton December 14 to attend Chris Timpson's lecture at Bas van Fraassen's seminar. (Then Timpson will come back with me to Bell Labs for a couple of day visit.) How about we get together then for a little planning session: You can tell me much more about what you're thinking and what your constraints are, etc.

02-12-05 *Gleason Frames* (to I. Bengtsson)

Bengtssonism 1: *I'm looking for a topic for a Master's Thesis, and I am wondering whether I can ask the undergraduate in question to study Gleason's theorem, more specifically to understand the "general POVM" version, to get some feeling for why the "only projective" version is hard, and then to ask what happens if one places oneself somewhere in between—say admit ONLY tomographically complete but otherwise minimal POVMs, or something like that (I might ask her to invent restrictions herself). As a way to start, first study Kochen-Specker for $N = 2$, which has been done for cubes inscribed in a dodecahedron (Aravind, I believe), and then try to do it with tetrahedra inscribed in a dodecahedron (I don't think I have seen that).*

Question (that the supervisor ought to ask himself, really): Is the answer trivial, or known, or obviously too hard?

Any advice that you care to give would be appreciated.

I think the research project you suggest for your student is a great one! (I wish I were your student, in fact, for this one.)

The question you ask is by no means trivial. Have you looked at our paper `http://www.arxiv.org/abs/quant-ph/0306179`?

See Section IV in particular. My original expectation was that the set of symmetric informationally complete POVMs would be sufficient for deriving the probability rule, but it was not! (In fact, I'm still disappointed that that's not true: It still seems that it should be.) So, one question I might ask is how much asymmetry must be introduced before a Gleason theorem becomes viable again? For instance, is it enough to consider one very asymmetric frame and all unitarily equivalent ones?

Keep me up to about your progress. I think this is an exciting question.

02-12-05 *Notes to van Fraassen and Delirium Quantum (some nested)* (to G. Valente)

Contained within the attachments are all the notes I had promised to send you. [See the 10-11-05 notes "'Action' instead of 'Measurement'" and "Our Own Rovellian Analysis" to Bas van Fraassen.] You will see why I say they touch on our view of the "stability condition" with respect to quantum measurement, and why I sometimes say that the very existence of repeatable measurements is something of a mystery from our perspective.

Thanks again for spending some time at the Orange House. I got a lot out of the visit; you gave me much food for thought.

02-12-05 *Sounds of Silence and CBH* (to G. Brassard)

OK, I'll accept your sounds of silence for a while ... until my own stress levels start to go over the top about how I might fund this magically gifted postdoc who wants to work on these things ...

Too bad you couldn't have been around here the last three weeks or so. Bas van Fraassen and Hans Halvorson have been going over the CBH theorem in their seminar. And Jeff Bub has been making the three- and four-hour drives (each way) to attend the sessions! A lively discussion has ensued, with the obstinate Tim Maudlin in the audience. Anyway, you should have been there. I've gotten a lot from the discussion.

03-12-05 *Subjective Quantum Information* (to M. Bahrami)

Bahrami-ism 1: *I have studied the articles as you told me. There is a core stone in your attitude which can be called Subjectivness of Information.*

I pretended I am on your side and accept this vision. Then I tried to exploit this vision in a real experimental setup. So I choose Interaction-Free Measurement as proposed by Elitzur-Vaidman. To be honest I feel I can say nothing about why the existence of an object in one of the ways causes such a strange result. I have checked the journals to find other physicists interpretations on this subject, but I could find just tow different attitude: 1) many-world interpretation of this experiment, mainly by Vaidman, 2) objective interpretation of information by Auletta (Foundations of Physics, May 2005) Let alone the first one, but the second is in great contrast with the common beliefs of physicists like you on subjectiveness of information.

I would be grateful if you let me know how you interpret (or explain) this experiment with a subjective attitude toward information.

To be honest, that is a thought experiment that I never gave too much attention to, because I always thought it was hype without too much substance. The concept of "interaction-free measurement" uncovers what had in earlier days been called *interference* and gives it a new, more dramatic name, but that's qualitatively as deep as it goes. (Quantitatively, there are some differences with previous techniques, and this may be thought of as a clever interferometric technique.)

But now that you push me on it, I think this would be an excellent project for you to take on. Why not let this be the subject of your Master's thesis? Give a detailed quantum information theoretic analysis of this phenomenon. And particularly, test your understanding of the Bayesian approach to quantum states with respect to it. I, for instance, might learn a lot from your thinking, and then you can call the analysis your own.

12-12-05 *The Great Pumpkin* (to J. W. Nicholson)

I think you'll love this thing Greg Comer is going to do in his physics class today. Read below.

Comerism 10: *I can't believe I'm going to give the following question on my physics exam today:*

> Charlie Brown tells Linus that he has developed a theory of gravity that is based on mass and energy causing curvature in the spacetime continuum. He also shows Linus his experimental data which confirms that the theory works within the accuracy and precision of the experiments. Linus tells Charlie Brown that, regardless of the agreement with the data, the new theory is wrong, because it is known that the Great Pumpkin is responsible for gravity, and the new theory makes no reference to the powers of the Great Pumpkin. How should Charlie Brown respond? Assume a rational and respectful discourse from both.

19-12-05 *The Detached Fuchs* (to H. C. von Baeyer)

I'm finally replying to your nice note: Several visitors and Montezuma's revenge have gotten in my way since your writing me.

von Baeyerism 9: *I came across a footnote specifically about the term "der losgelöste Beobachter: the detached observer" in Wolfgang Pauli, Wissenschaftliche Briefwechsel, Vol. IV part I, page 343, footnote 13 to the commentary on letter [1263] from Bohm to Pauli, which is of course in English.*

Yes, of course, I would like that quote translated very much; as far as I know I don't already have it. All in the greater cause!

19-12-05 *Thanks and Question* (to H. Halvorson)

This is a belated thanks for sending me the Alfsen, Schultz, and Mielnik references. I've got to get on this!!

Now a question. I noticed that you're on the PSA 2006 program committee. The reason I was snooping there is because I had a flash that maybe I'd like to organize a session or workshop at the meeting comparing and *contrasting* epistemic views of the quantum state to Bayesian views of probability. It'd be nice if I could get, for instance, Persi Diaconis or Brian Skyrms and Abner Shimony to give talks, along with, say, Rob Spekkens and/or Huw Price and/or Itamar Pitowsky and/or Bill Demopoulos, etc., etc.—I'm just starting to think about it all. The trouble is, the deadline for proposals has already passed. Do you know whether this deadline is really strict? I noticed that the proposals won't start being reviewed until mid-February. If the deadline is a little loose and I work at it, I could probably have something submitted in the break between Christmas and New Year's. Thanks for any advice you can give.

19-12-05 *A Small(?) Request* (to A. Radosz)

I have skimmed your student's paper. I'll just reply by telling you a story from my first year of study with Carlton Caves, in 1993. At the time, I had just read a paper by Braunstein and Caves on information-theoretic Bell inequalities. One of the things they note in that paper is that whereas their information-theoretic inequalities have an advantage over CHSH-type inequalities because of the ease and automated nature of their derivations, they have a *dis*advantage in that they are not violated over as large of a range of measurements as the CHSH ones. Well, in my digging up various references in information theory (as I was learning the ropes), I learned about a whole new class of information functions—the Daróczy entropies—and as I played with them, I discovered that they too could be used in a Bell inequality derivation ... just along the same lines as Braunstein and Caves had discovered for Shannon's measure of information. Excited, I ran to Caves, told him about my result—I think it was something like the Daróczy 2.73 entropy was optimal for finding a result, and with it I could find a violation over 12.7 degrees more than the Braunstein and Caves result. Then I asked him if I should publish it?!?! He replied, "Well, I wouldn't." I replied, "But that's you; you've got a hundred papers, and I have none." He replied, "I'm just saying, if it were me, I wouldn't." I got the hint, finally, and never published it. Caves always wanted his students to stay away from incremental work or isolated results. In my case, it helped; but different students have different requirements. It'll be your call.[84]

27-12-05 *Randomness and Quantum* (to D. Overbye)

While up this morning with a little insomnia (it's 4:00 AM as I write this), I skimmed over your *NY Times* article on "Quantum Trickery." I especially enjoyed some lines near the end:

> As a result of the finiteness of information, he explained, the universe is fundamentally unpredictable.
>
> "I suggest that this randomness of the individual event is the strongest indication we have of a reality 'out there' existing independently of us," Dr. Zeilinger wrote in *Nature*.
>
> He added, "Maybe Einstein would have liked this idea after all."

That's an idea I'm very attracted to and have explored in a decent set of my writings. (There's a sampling of some posted at my webpage, link below.) With regard to entertainment value along those lines, you yourself might enjoy Sections 4 and 5 of this 2002 pseudo-paper:

`http://www.arxiv.org/abs/quant-ph/0204146`.

[84] I suppose for the purposes of subtle persuasion I intentionally left off an addendum to the story that I *do usually* include with it. Not fair of me really! Some time in the mid to late 1990s, I was having a walk with Michał Horodecki, and the conversation turned to the Braunstein and Caves inequalities. Michał noted, "Sadly, they cannot be generalized to the Rényi information measures. The derivation just doesn't work." I said, "Oh, that's right; that's because the Rényi entropies are additive, but not subadditive. The same derivation does however go through if you use the Daróczy entropies instead. For, though they are not additive for independent random variables, they are subadditive generally, and that's all you need." Michał became quite excited and asked, "Where have you written this?" I said, "Oh, I never published it. I worked it out a long time ago, but it didn't seem very important." He responded, "That is wrong, you should publish it even now."

Here's a sample paragraph from that connected to the above:

> I would say all our evidence for the reality of the world comes from without us, i.e., not from within us. We do not hold evidence for an independent world by holding some kind of transcendental knowledge. Nor do we hold it from the practical and technological successes of our past and present conceptions of the world's essence. It is just the opposite. We believe in a world external to ourselves precisely because we find ourselves getting unpredictable kicks (from the world) all the time. If we could predict everything to the final T as Laplace had wanted us to, it seems to me, we might as well be living a dream.
>
> To maybe put it in an overly poetic and not completely accurate way, the reality of the world is not in what we capture with our theories, but rather in all the stuff we don't. To make this concrete, take quantum mechanics and consider setting up all the equipment necessary to prepare a system in a state $|\Psi\rangle$ and to measure some noncommuting observable H. (In a sense, all that equipment is just an extension of ourselves and not so very different in character from a prosthetic hand.) Which eigenstate of H we will end up getting as our outcome, we cannot say. We can draw up some subjective probabilities for the occurrence of the various possibilities, but that's as far as we can go. (Or at least that's what quantum mechanics tells us.) Thus, I would say, in such a quantum measurement we touch the reality of the world in the most essential of ways.

27-12-05 *A Little Christmas Randomness* (to A. Zeilinger)

Merry Christmas! I hope you and your family are doing well and are having a relaxing holiday.

Anyway, you are on my mind again this morning because I read Dennis Overbye's article in today's *New York Times* on "quantum trickery," and I was struck once again by the similarities in the essential parts of our views. Overbye wrote this about you:

> As a result of the finiteness of information, he explained, the universe is fundamentally unpredictable.
>
> "I suggest that this randomness of the individual event is the strongest indication we have of a reality 'out there' existing independently of us," Dr. Zeilinger wrote in *Nature*.
>
> He added, "Maybe Einstein would have liked this idea after all."

Compare that with some things I wrote in several places in *Notes on a Paulian Idea*, but particularly in the pseudo-paper `quant-ph/pdf/0204/0204146.pdf` where in Sections 4 and 5, I said:

> I would say all our evidence for the reality of the world comes from without us, i.e., not from within us. We do not hold evidence for an independent world by holding some kind of transcendental knowledge. Nor do we hold it from the practical and technological successes of our past and present conceptions of the world's essence. It is just the opposite. We believe in a world external to ourselves precisely because we find ourselves getting unpredictable kicks (from the world) all the time. If we could predict everything to the final T as Laplace had wanted us to, it seems to me, we might as well be living a dream.

To maybe put it in an overly poetic and not completely accurate way, the reality of the world is not in what we capture with our theories, but rather in all the stuff we don't. To make this concrete, take quantum mechanics and consider setting up all the equipment necessary to prepare a system in a state $|\psi\rangle$ and to measure some noncommuting observable H. (In a sense, all that equipment is just an extension of ourselves and not so very different in character from a prosthetic hand.) Which eigenstate of H we will end up getting as our outcome, we cannot say. We can draw up some subjective probabilities for the occurrence of the various possibilities, but that's as far as we can go. (Or at least that's what quantum mechanics tells us.) Thus, I would say, in such a quantum measurement we touch the reality of the world in the most essential of ways.

AND

So, I myself am left with a view of quantum mechanics for which the main terms in the theory—the quantum states—express nothing more than the gambling commitments I'm willing to make at any moment. When I encounter various other pieces of the world, if I am rational—that is to say, Darwinian-optimal—I should use the stimulations those pieces give me to reevaluate my commitments. This is what quantum state change is about. The REALITY of the world I am dealing with is captured by two things in the present picture:

1. I posit systems with which I find myself having encounters, and
2. I am not able to see in a deterministic fashion the stimulations (call them measurement outcomes, if you like) those systems will give me—something comes into me from the outside that takes me by surprise.

Just two examples, but I think they indicate I'm going to enjoy your paper very much! Could I ask you to send me a copy of it electronically, however? (I can't access it directly from the journal until I am back at Bell Labs, and that won't happen until the holidays are over.) It would give me some holiday cheer.

27-12-05 *Indistinguishable Particles* (to Y. Omar)

Thanks for the warm Christmas wish! I enjoyed it and laughed and laughed.

I will try to take care of the bureaucratic things I owe you within the first week after New Year's Day.

By the way, I think I have discovered a reason why nature allows for indistinguishable particles. It comes from observing my two children play with their new toys on Christmas. After listening all day to *each* say of the *other's* toys, "I wish I had that," I thought, wouldn't it be nice if there were a way for them to not know whose was whose? And, indeed, a way for them not to see any distinguishing qualities for the toys at all, other than the amount of sheer pleasure they give? Then the parents' job would be so simple: Simply give them identical quantities of pleasure to play with at their leisure, and be done with it.

27-12-05 *Regarding the Detached Observer Attached* (to H. C. von Baeyer)

von Baeyerism 10 : *Dear Chris, the detached observer is attached. I thought this footnote might interest you not so much for its text, which is less than limpid, as for its citations of the phrase.*

> In Wolfgang Pauli, *Scientific Correspondence with Bohr, Einstein, Heisenberg, a.o.* (Springer, 1996) Volume IV, Part I: 1950-1952, page 340 there is a three-page editorial comment on letter number [1263], from David Bohm to Pauli. The penultimate paragraph of this comment, in German, begins:
>
>> Pauli, on the other hand, under the influence of his psychological point of view, had progressively moved away from the classical-Cartesian assumption of the detached observer (cf. letters [1313] and [1314])[14] who assumes a *fixed or pre-arranged game* behind the stage *(abgekartetes Spiel)* [letter 1388]: (in English) 'For me, however, it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the observers own body and own brain connected with his own observations. . . .'
>
> {I believe the editor may be mixing up two different notions here, that of the detached observer, and that of the view of nature as a pre-arranged game, but I wanted to give the context of footnote 14. HvB}
>
>> [14]This term first appears in this context in Pauli's letter [1197] of 31 January 1951 to M.-L. von Franz and then again in the letter from M. Fierz [1288] of 10 October 1951. From now on Pauli used it more frequently in his lectures, publications, and letters. In a letter of 5 May 1953 to C. F. von Weizsäcker he mentioned this question in connection with his Kepler study: "As I hinted in my essay, it seems to me that Fludd was much closer to the symbolic formulation of the *unity of existence*, which in turn so paradoxically divides into 'observer' and 'external world' ('the cut'), than was Kepler, with his 'detached observer' of classical physics. The 'archaic' Fludd had the stronger feeling for the proposition that the 'position of the cut' is arbitrary (Heisenberg)." Similarly Pauli wrote on 15 May 1953 to M.-L. von Franz: "Holding on to these assumptions requires one to restrict oneself to *statistical* laws and to 'sacrifice' the individual case. Einstein, on the other hand, would like *both* to 'have his cake and eat it.' He yells 'incomplete', regresses to the detached observer of classical physics, and places 'world formulae' into a blue fog (which does not contain the observer).

Thanks so much for the quote! I've just incorporated it into my "activating observer" compendium (and given you credit for the translation). But now you'll start to see my insatiable appetite! For I'd dearly love to see the FULL letters 1197, 1288, 1313, 1314, 1388, as well as the 5 May 1953 letter to von Weizsäcker and 15 May 1953 letter to von Franz (for some reason there were no numbers listed next to the latter two).

In my existing collection, a search on the year 1951 only gives this quote from a Primas paper,

> Faced with the wholeness of the reality, scientists have been slow to accept the challenge of discussing the premises of Baconian science. They have even been reluctant to

> consider the simplest modification of a mechanistic world view, namely the inclusion of teleological considerations as an essential part of their discipline – a relatively simple problem for which in the framework of modern quantum mechanics all necessary tools are available presently. But Pauli was looking for an incomparably deeper approach which goes far beyond the limits of current quantum theory, and which includes *physis* and *psyche* as complementary aspects of the same reality. A reality containing both rational and irrational elements, and alchemic *conjunctio* of spirit and matter. In psychology as well as in physics, quaternity is taken to be an expression of all concepts of unbroken totality. In a letter Pauli wrote: "Ich bin ja auf Kepler als Trinitarier und Fludd als Quaternarier gestossen – und fühlte bei mir selbst, mit deren Polemik, einen inneren Konflikt mitschwingen. Ich habe gewisse Züge von beiden, sollte aber jetzt in der zweiten Lebenshälfte zur quarternären Einstellung übergehen. Das Problem ist, dass dabei die positiven Werte der trinitarischen Einstellung nicht geopfert werden dürfen." (Pauli to Fierz on October 3, 1951, quoted in Enz, p. 509). Pauli could not solve his dilemma of three and four which plays a great role in alchemy as "the axiom of Maria Prophetissa" ("Out of the Third comes the One as the Fourth"), and this shows that we are at the bare beginning to understand reality. But we again reached a turning point, a way of thinking is developing which is very different from that which has been dominant in the past decades, and which recognizes the repression of the irrational as incongruous.

(of which I don't know what the German part is saying) and this point of interest from a Lindorff paper

> This statement, taken together with the way Pauli associated the radioactive atom with the lapis, moved Jung to question whether 'the archetype and its effects and the effect of the active atom on its environment is not more than a metaphor.'
>
> With these thoughts in mind Jung said he would amplify his essay, 'Synchronicity: an acausal connecting principle'. He presented the material to the Psychological Club in two parts on 20 January and 2 March 1951. The essay, together with Pauli's work on Kepler, was subsequently published in book form.

The only materials I have from 1953 are a few letters from Pauli to Jung; nothing from Pauli or von Weizsäcker or von Franz or Fierz.

At the moment, I'm particularly interested in better understanding the thing Pauli wrote to von Franz in what you sent me: "For me, however, it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the observers' own body and own brain connected with his own observations . . . " That really intrigues me! What exactly does he mean by that?

In that regard, let me send you a little compendium I sent to David Mermin, in fact just today. It's pasted below. As you'll see, I'm starting to lean very heavily on an alchemical analogy for building some imagery of what quantum measurement is all about. (I hinted a little about this to you before, but supplied no details.) And the quote you gave me above seems to touch on that in a way I hadn't seen before. This is something I really would love to explore.

Thanks so much again for all this! You're bringing me back to life in these lazy days between Christmas and the New Year.

I hope you and your wife are having a great holiday yourselves.

Hans's Reply, "Translations," 31-12-05

I am happy to translate this and all the other letters, but in order to avoid an infinite regression, we need to establish some ground rules.

1. My primary source for the next year or two will be the Pauli correspondence, with the [....] numbering. This means that unless it is absolutely essential, I will avoid other sources.
2. I am not good at filing, so you are in charge of keeping order.
3. Both of us need to be vigilant about the existence of translations – no use repeating someone else's work. Of course I will make sure to check any translations I find for accuracy. (The art historian Panofsky, Pief's brother I think, considered some of von Franz's translations from Latin, made for Pauli, to be distorted to the point of gobbledygook.)
4. I had decided to start translating Fierz letters, but will start, instead, with the list you sent – i.e., with the selection criterion "detached observer" instead of "Fierz."
5. There will be three types of footnotes: by the writers themselves, by Karl von Meyenn, and by me. I will endeavor to flag them clearly. I will probably skip some parts, and will say so. Any time you want me to fill in the blanks, I'll be happy to do so. Letters to von Franz, for example, often have appended dreams. If these don't seem relevant to the subject matter of the letter, but are simply entries in an ongoing dream-book, I will skip them, but say so.

27-12-05 *Words of Yours I Liked and Didn't* (to N. D. Mermin)

Happy holidays! I've been missing you a lot lately, particularly as Rüdiger was visiting a couple of weeks back, and once again we picked up on our old thread of trying to develop a paper on "certainty"—you don't know how many times your image came to us in the conversations!

I've been meaning to write you, also, to thank you for having a copy of your book sent my way! It is beautiful. Per my promise, I will read it cover to cover; just give me a little time.

I have at least read the preface as of now, and I think I have discovered why you have been so nice to me all of these years (or at least tolerant of me)! Is it because my heart is in the right place, i.e., wanting to see quantum theory reduced to a statement that can be taught in an ordinary high school? I hadn't realized that you had made such an important point of this with Special Relativity. Wonderful!

Now, why am I writing you today in particular? It's because I had insomnia last night and I stumbled across Dennis Overbye's article in the *NY Times*. When I read this,

> In an essay in 1985, Dr. Mermin said that "if there is spooky action at a distance, then, like other spooks, it is absolutely useless except for its effect, benign or otherwise, on our state of mind."

I thought it was sheer genius, and I was ashamed that I didn't remember having read it before. I so wish I could write with your cleverness! Words I liked.

But when I read this,

> "I would say we have to be careful saying what's real," Dr. Mermin said. "Properties cannot be said to be there until they are revealed by an actual experiment."

I thought, "Tsk. Tsk." What trouble I continue to think that way of expressing things makes for our imaginations. Thinking of quantum measurement outcomes as "revealing properties" (whether they are pre-existent properties or not) is, I think, one of the biggest problems getting in our way of making a decent myth for the quantum mechanical world. Words of yours, this time, I didn't like.

By way of saying Happy New Year again, I'll send you three recent emails I wrote to Bas van Fraassen (all pasted below) where once again I'm groping to find the right words to say what quantum measurement "is" and what the clicks it gives correspond to. A quantum measurement outcome (considered on its own, without relation to other issues) is just a "surprise," full stop; it doesn't reveal anything. It's just a primitive reaction within the agent. Or at least that's the myth I'm groping for! (You'll see in the notes below that even I'm guilty of sometimes using the same words that I said "tsk, tsk" to you about. Once again—as always!—I learn from reading even the tiniest things of yours; somehow you cause me to introspect like few others do.)

Happy, happy, happy New Year!

27-12-05 *White Christmas* (to W. G. Demopoulos)

I've been thinking about you most of the holiday season, but am only now getting around to sending out holiday greetings. Happy holidays! The event that seemed to start it off was watching the old movie "White Christmas" (with Bing Crosby and Danny Kaye) sometime early in December. For some reason that I can't put my finger on, I was struck by how the actor who played General Waverly reminded me of you. His name was Dean Jagger in real life, and I don't know if it was his facial features or his voice, but something about him made me think of you throughout the movie.

But, who knows, maybe I was already thinking about you earlier than that! This morning, when I was reviewing some of my old emails from my last discussions with Rüdiger Schack (for a paper we're writing on Wigner's friend), I came across a note I wrote that had these lines in it:

> Quantum theory has no more power within it to say when (quantum) events happen than classical probability theory has the power within IT to say when its own events happen. Maybe this is why some developments of classical probability theory are careful to say that the h's in a probability assignment $P(h)$ are propositions. Propositions don't "happen", they are simply either true or false. And thus, by fiat, one never has to address the question of when h "happens". But we know that we shouldn't try to make that further move (i.e., viewing the h's as propositions with timeless truth values) in the quantum setting. Quantum events do "happen." But that doesn't mean that quantum theory should have the burden on it of explaining or determining "the moment at which 'all is said and done'."
>
> This is ultimately, I suspect, why our view will be greeted with the same revulsion that William James's theory of truth was by Russell and Moore and the like: Because for James, too, truth is something that is made to happen; it is not something that is just there in a timeless way.
>
> > The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication.* Its validity is the process of its valid-*ation.*

When I read this, I thought of the nice walk and talk we had in Maryland a couple of years ago. What I wrote above seems to be the main issue that separates us at the moment in our views of quantum mechanics, but it may be the only one!

All the best, and I hope you have a great New Year.

27-12-05 *Happy Troubles* (to D. Overbye)

I actually got a lot out of reflecting over your article today. So I wanted to say thanks and that I enjoyed it. Particularly, I got a lot out of thinking about David Mermin's sparse remarks that you recorded and then noting an inconsistency in my own ways. I tried to capture the point in an email I wrote to David a couple hours ago. I'll paste it below, as I feel I owe you a little bit too. (David and I are old friends and have been having a discussion on some of these points for a long time.) [See 27-12-05 note "Words of Yours I Liked and Didn't" to N. D. Mermin.]

And since you're not a full-time physicist, I'll give you a special treat: A cartoon version of the whole story! It is attached as a `.jpg` file. (Quantum theory is about all the stuff to the left side of the sparks; the stuff to the right is 'reality,' the partial source of the sparks.)

28-12-05 *Wheeler and the Pleasures of Life* (to D. Overbye)

I'm up again at this annoying hour, and, bringing my chance meeting of you last night to a closure, I read a little more about you on the web. I enjoyed the interview posted on *Edge*. "What else is there? Sex and physics." We might be kindred spirits ... When I first started calling my own view of quantum mechanics "the sexual interpretation of quantum mechanics," one friend's reaction was that the title was of no real content—it was only a cheap way to draw attention to the view—but another friend's reaction was that I was looking for a way to combine my ONLY two pleasures in life ... (The latter friend doesn't think I'm particularly deep.)

More seriously, I also discovered that you seem to be a little bit of a fan of John Wheeler. John has been a huge influence on my scientific life. Before last night, I had not recalled reading anything of yours regarding him, but tonight, searching in this big document I'm putting together "The Activating Observer: Resource Material for a Paulian-Wheelerish Conception of Nature" (which is a compendium of a lot of quotations) I found two entries in your name. I'll place them below for fun. So, you've been writing about Wheelerish stuff since at least 1981!? And apparently I've been reading you since about 1985 (when I started systematically digging up Wheeler stuff)?! Good things to learn.

If you're interested in some funny Wheeler anecdotes, I've got a few compiled in my samizdat (which will soon be published as a real book by Springer) "Notes on a Paulian Idea." (The version with the most complete index presently is posted on my webpage; it's slightly better to go there than `quant-ph`.) For instance, you might get a kick out of the story on page 149, which relates a little to my second quote of you below.

Anyway, as I say, bringing last night's meeting to a closure,

- D. Overbye, "Messenger at the Gates of Time," Science81 **2**(5), 61–67 (1981). This contains some biographical material on John Wheeler. Also, there are some great quotes of John Wheeler:

  > The best way to find oneself outside the ranks of science is to find oneself inside the ranks of mysticism.

  and

If there's one thing in physics I feel more responsible for than any other, it's this perception of how everything fits together. I like to think of myself as having a sense of judgement. I'm willing to go anywhere, talk to anybody, ask any question that will make headway. I confess to being an optimist about things, especially this question about our hopes of being able to understand how things are put together. So many young people are forced to specialize in one line or another that a younger fellow can't afford to try and cover this waterfront—only an old fogey who can afford to make a fool of himself. If I don't, who will?

- D. Overbye, "What Happened Before the Big Bang?," New York Times, 12 March 2002.

> In their dreams, theorists of both stripes [string theory and loop quantum gravity] hope that they will discover that they have been exploring the Janus faces of a single idea, yet unknown, but which might explain how time, space and everything else can be built out of nothing. A prescription for, as the physicist Dr. John Archibald Wheeler of Princeton puts it, "law without law."
>
> Dr. Wheeler himself, the pre-eminent poet-adventurer in physics, has put forth his own proposal. According to quantum theory's famous uncertainty principle, the properties of a subatomic particle like its momentum or position remain in abeyance, in a sort of fog of possibility until something measures it or hits it.
>
> Likewise he has wondered out loud if the universe bootstraps itself into being by the accumulation of billions upon billions of quantum interactions—the universe stepping on its own feet, microscopically, and bumbling itself awake. It's a notion he once called "genesis by observership," but now calls "it from bit" to emphasize a proposed connection between quantum mechanics and information theory.
>
> One implication of quantum genesis, if it is correct, is that the notion of the creation of the universe as something far away and long ago must go. "The past is theory," Dr. Wheeler once wrote. "It has no existence except in the records of the present. We are all participators, at the microscopic level, in making that past as well as the present and the future."
>
> If the creation of the universe happens outside time, then it must happen all the time. The Big Bang is here and now, the foundation of every moment.
>
> And you are there.

28-12-05 *Wheeler and the Pleasures of Life, 2* (to D. Overbye)

Overbye-ism 1: *I am a huge fan of John Wheeler, actually. i wrote a long profile of him in Science Times three years ago. Also he is featured in my book, Lonely Hearts of the Cosmos, currently a Little, Brown paperback, 1999.*

More entries for my compendium! I'm always hungry for more (and hopefully within the next year or so, I'll try to publish the thing). I'll look those things up as soon as I can.

28-12-05 *Grad Research and Quantum Information* (to C. Ududec)

I apologize for taking so very long to reply to you; I have gotten far, far, far behind on my email, and am only now trying to catch up during the holidays.

Cozminism 1: *I'm currently in my last year in a math/physics honours program at the University of Manitoba in Canada, and I'm interested in going on to graduate school and working on something in quantum mechanics and its foundations. I've read a little about different approaches to the measurement and interpretation problems, and I've started reading your 'Quantum Mechanics as Quantum Information' paper. I'd be very interested in learning more about this kind of approach or working on something similar for my graduate studies.*

I was wondering if you have any suggestions for other things I should read, or if there is a good starting point for this topic. I was also wondering if you have any suggestions for schools I could look at, or other people I should talk to.

I would suggest reading anything posted on my website, and maybe supplementing that with

- `http://www.arxiv.org/abs/quant-ph/0404156`
- `http://www.arxiv.org/abs/quant-ph/0404122`.

As for a good place for you to do graduate studies, if you are really interesting in this approach to quantum mechanics, I would suggest studying with Carl Caves at the University of New Mexico or Rüdiger Schack at Royal Holloway University of London.

Cozminism 2: *Also, when you talk about the consequences of an experimental intervention, are they just consequences for the one experimenter's beliefs about the system and about future interventions?*

That is right.

Cozminism 3: *They aren't consequences for all other possible experimenters as well, right?*

Correct. A mantra that came up during Rüdiger Schack's last visit to NJ was, "A quantum state represents one's personal beliefs about the personal consequences of one's personal interventions."

28-12-05 *And a Cartoon* (to H. C. von Baeyer)

And I shouldn't forget to send you the cartoon that goes with the last note I sent you. It is attached as a `.jpg` file. Quantum theory is about all the stuff to the left side of the sparks—i.e., the actions and transformations of the agent. The stuff to the right of the sparks is the 'external reality,' the partial source of the sparks—it is the 'philosopher's stone,' without which the agent would never get transformed. The word 'catalyst' in the cartoon is a little misleading, as anyone with a little chemical training thinks of a catalyst as something that remains unchanged in the reaction; however, I'm thinking here of it in the broader sense of, say, the *American Heritage Dictionary* where a catalyst is simply "an agent that stimulates or precipitates a reaction, development, or change," full stop.

The reason I'm intrigued by the Pauli quote to von Franz is that he says, "For me ... it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the observer's own body and own brain connected with his own observations ..." That is, he seems to be talking about something going on to the left of the sparks too, not to the right of the sparks—i.e., just the part I claim quantum theory is about.

31-12-05 *If We Make It Through December . . .* (to G. Musser)

I don't guess you know the old Merle Haggard song, "If we make it through December / / Everything's gonna be all right I know . . . " Well, that's where I am at the moment. Thank you though for prodding me.

I apologize for the delay; it all started out innocently enough, before my interruptions snowballed. The innocent part was that I started to worry some about your words:

Musserism 28 : *One yoke your article will have to carry is that talking of the "subjective" elements of quantum theory sets off alarm bells – people think of Fritjof Capra and get suspicious of a metaphysical agenda. So you might want to state outright that interpretations of quantum mechanics have a somewhat checkered history, but that this shouldn't prevent us from plowing ahead and going where the physics leads us.*

So, I took a little hiatus to learn more about Frank Ramsey's presentation of probability theory—the logic being that Ramsey's view of probability is the *same* as Bruno de Finetti's "radically subjectivistic" interpretation and yet, something about the way he presented the view wasn't nearly as incendiary as de Finetti's way. I wanted to incorporate any presentational insight I could find from him.

But then the snowballing of delay came with 1) Thanksgiving, then 2) Rüdiger Schack's visit, then 3) Chris Timpson's visit, then 4) a long case of Montezuma's revenge, then 5) Christmas . . .

Anyway, the new year is about to start and all these damned holidays will be over. I have to go to a meeting in Snowbird, Utah, Jan 3–6; but I am hoping that that won't slow me down too much. I very much hope to give you something the following week. I've got to stop stalling; it's time to write something serious.

Thanks for pointing out the Zeilinger article too. Somehow it didn't quite register when you wrote me about it (at that time, I was in the throes of the Montezuma thing I told you about), but my curiosity got piqued when I read Overbye's article in the *Times* last week. I ended up sending the note below to Zeilinger, to which I got a cordial response and a copy of the article to finally read. [See 27-12-05 note "A Little Christmas Randomness" to A. Zeilinger.] We are definitely on the same wavelength about some things, as you'll see below.

31-12-05 *Wheeler Meeting at Princeton* (to W. G. Unruh and several others)

Marlan Scully and I are organizing a somewhat impromptu, but nonetheless serious, meeting in Princeton to give a little tribute to John Wheeler, law-without-law, it-from-bit, and quantum information. It'll be a one-day meeting with maybe 10 short talks and a little socializing, held mid- to late-February (or possibly early March) on a Friday. Wheeler has said that he will make an appearance, and we would like to invite you too.

Presently our preferred target date is February 17, but we'll also consider February 23 and March 3 if there are too many scheduling conflicts. Please let me know as soon as possible if you would like to come and how your schedule fits with these possibilities. There are likely to be funds available for your travel expenses too, if you need them.

On this first round of invitations, we're checking on the availability of the following colleagues to see if they can participate: Yakir Aharonov, Carroll Alley, Janos Bergou, Michael Berry, Rob Calderbank, Carl Caves, Leon Cohen, John Conway, Ken Ford, Bas van Fraassen, Danny Greenberger, Hans Halvorson, Lucien Hardy, Mark Hillery, Simon Kochen, Seth Lloyd, Edward Nelson, Ben Schumacher, Yanhua Shih, Bill Unruh, Bill Wootters, Anton Zeilinger, Suhail Zubairy, and Wojciech Zurek. More than likely, we've forgotten other colleagues who would also be very appropriate for this meeting; if you see some glaring absences, give us your suggestions, and we may be able to take them into account on a second round of invitations.

All the best, and a very happy New Year to you and your family,

01-01-06 *Quantum Events and Propositions* (to W. G. Demopoulos)

Demopoulosism 2 : *Maybe this is what separates us: I think an event's happening is strictly analogous to a proposition's being true. Just as events happen or fail to happen, propositions are true or false. So I'm not sure what to make of the idea that props are true or false but events just happen, seeing these as different ways of saying the same thing.*

Actually, I guess I fear that I agree with the ending part of this statement, i.e., that "I'm not sure what to make of the idea that props are true or false but events just happen, seeing these as different ways of saying the same thing." But, I think, the force of it goes in the opposite direction for me than it does for you. That is, I think I think [sic] one of the great lessons of quantum mechanics is that it is a call to arms to rethink what is meant by the truth value of a proposition. Here's the way I put it in one of the proposals I once made:

> Quantum Mechanics as a Powerful Hint. In my opinion, the most profound statement yet to come out of quantum theory is the Kochen-Specker theorem. For it licenses the slogan, "Unperformed measurements have no outcomes." This is just a beginning. If one canvasses the philosophic traditions for one that has significantly developed this slogan, one will find the now mostly-forgotten tradition of pragmatism fathered by William James and John Dewey. As a source of ideas for what quantum mechanics can more rigorously justify, no block of literature is more relevant: The connections between the two fields cry out for systematic study. Quantum mechanics holds the promise of drastically changing our worldview on the wide scale. It is time to let that happen.

Demopoulosism 3 : *On another matter, there is an idea in Pauli that your remark about quantum mechanics and classical probability theory reminded me of, but I can't recall it exactly. Doesn't he say somewhere that what QM does is to give a precise formulation of how the probabilities of propositions all stand with respect to one another without, however, specifying what actually occurs?*

(I've quoted this: "Just as in the theory of relativity a group of mathematical transformations connects all possible coordinate systems, so in quantum mechanics a group of mathematical transformations connects all possible experimental arrangements.")

I don't recall Pauli saying something like in your first version, only your second, but he might well have: I'm a horrible thief at times. Here's the way I put something similar in my anti-Växjö pseudo-paper `quant-ph/0204146`:

> In choosing one experiment over another, I choose one context over another. The experiment elicits the world to do something. To say that the world is indeterministic means simply that I cannot predict with certainty what it will do in response to my action. Instead, I say what I can in the form of a probability assignment. My probability assignment comes about from the information available to me (how the system reacted in other contexts, etc., etc.). Similarly for you, even though your information may not

> be the same as mine. The OBJECTIVE content of the probability assignment comes from the fact that *no one* can make *tighter* predictions for the outcomes of experiments than specified by the quantum mechanical laws. Or to say it still another way, it is the very existence of transformation *rules* from one context to another that expresses an objective content for the theory. Those rules apply to me as well as to you, even though our probability assignments *within* each context may be completely different (because they are subjective). But, if one of us follows the proper transformation rules—the quantum rules—for going to one context from another, while the other of us does not, then one of us will be able to take advantage of the other in a gambling match. The one of us that ignores the structure of the world will be bitten by it!

That is, part of the rational part of quantum mechanics is much like de Finettian or F. P. Ramseyan "coherence": If you gamble this way about this, and you gamble that way about that, etc., etc., then you'd better gamble such way about the other, or you're not being coherent with respect to your beliefs of the properties of the world.

Demopoulosism 4: *I'm back in Canada for the rest of the academic year and perhaps for the foreseeable future. Jeff is supposed to visit PI for several months beginning in February and Itamar should be here in May. Any chance you might be through here? Will you be in Maryland this spring? I plan to be there.*

Yes, I plan to be at the Maryland meeting in the Spring. I don't think I'll be dropping by PI though anytime soon. However, another way I might see you in the next year is that I've thought about organizing a symposium on "subjective probabilities and quantum mechanics" at the next PSA meeting in Vancouver. When I wrote a couple of people in the organizing meeting to see if they'd give me a little time post-deadline to get a proposal in, I wrote this:

> I noticed that you're on the PSA 2006 program committee. The reason I was snooping there is because I had a flash that maybe I'd like to organize a session or workshop at the meeting comparing and *contrasting* epistemic views of the quantum state to Bayesian views of probability. It'd be nice if I could get, for instance, Persi Diaconis or Brian Skyrms and Abner Shimony to give talks, along with, say, maybe you and/or Rob Spekkens and/or Huw Price and/or Itamar Pitowsky and/or Bill Demopoulos, etc., etc.—I'm just starting to think about it all.

They've given me an extension. Now the question is what I might do. What do you think of the idea? Would you be willing to participate in the discussion or give a talk? Do you think Brian Skyrms would be interested? I don't really know him well yet.

02-01-06 *Final Installment* (to W. G. Demopoulos)

Now let me tackle this point of yours

Demopoulosism 5: *I also don't see why we should need something as fundamental as KS to sustain the notion that "unperformed measurements don't have outcomes." I'm being a devil's advocate here because I think what you really mean is that without a measurement of whether the cat is alive, the cat is neither alive nor not alive. But would you put it this baldly? If not, why not?*

in a more longwinded way. For that, I'll paste in an anthology of emails I've had recently with David Mermin and Bas van Fraassen. I think they refer to your point explicitly, but as always I'm still groping to try to get the right language. All the emails are connected, and you can read them linearly from top to bottom.

To answer your question in the best way I know how at the moment, I would say: The transformation that quantum mechanics speaks about, the transformation from a 'superposition' to 'aliveness' or 'deadness', is a transformation *within the agent*, and that transformation cannot take place without some interaction with the external physical system labeled by the word 'cat'. What happens to 'cat' itself (described in a way that makes no reference to the agent)? On that, I think quantum mechanics is silent. With a mantra: Quantum mechanics is a theory for ascribing (and intertwining) personal probabilities for the personal consequences of one's personal interactions with the external world.

Does all this (and particularly the stuff below) go some way toward answering your question?

02-01-06 *The Oblique Pauli* (to H. C. von Baeyer)

Look at this little gem I discovered today. I stumbled across it in the Schilpp volume on Einstein as I was researching for the talk I have to give in Utah Thursday: The topic I got roped into is whether I think Bohr gave an adequate reply to EPR. (My talk's title is, "Why I Never Understood Bohr's Reply to EPR, But Still Liked It.") The quote comes from page 683:

> It may appear as if all such considerations were just superfluous learned hairsplitting, which have nothing to do with physics proper. However, it depends precisely upon such considerations in which direction one believes one must look for the future conceptual basis of physics.
>
> I close these expositions, which have grown rather lengthy, concerning the interpretation of quantum theory with the reproduction of a brief conversation which I had with an important theoretical physicist. He: "I am inclined to believe in telepathy." I: "This has probably more to do with physics than with psychology." He: "Yes." —

Who else could that "important theoretical physicist" be but Pauli! They were certainly discussing these sorts of things at length at that time. Einstein wrote his remarks in 1949 (I think), while Pauli had been visiting Einstein in 1948 (recall how Pauli adjudicated the quarrel between Born and Einstein on quantum foundations during that time).

Also compare the similarity between what Einstein says above and these words of Pauli to Fierz, 10 August 1954:

> All of this then led me onto further, somewhat more phantastic [*sic*] paths of thought. It might very well be that we do not treat matter, for example viewed in the sense of *life*, "properly" if we observe it as we do in quantum mechanics, *specifically when doing so in complete ignorance of the inner state of the "observer."*
>
> It appears to me to be the case that the "after-effects" of observation which were ignored would *still* enter into the picture (as atomic bombs, general anxiety, "the Oppenheimer case" e.g. etc.), but in an *unwanted form.* The well-known "incompleteness" of quantum mechanics (Einstein) is certainly an existent fact somehow-somewhere, but certainly cannot be removed by reverting to classical field physics (that is only a "neurotic misunderstanding" of Einstein), it has much more to do with *integral relationships between "inside" and "outside" which the natural science of today does not contain* (but which alchemy had suspected and which can also be detected in the symbolics of my dreams, about which I believe them specifically to be characteristic for a contemporary physicist).

> With these vague courses of thought I have reached the border of that which is recognizable today, and I have even approached "magic." (From this standpoint observation in quantum mechanics might even appear to someone as a "black mass" after which the "ill-treated" matter manipulates its counter-effect against the "observer," thereby "taking its revenge," as a "shot being released from behind"). On this point I realize well that this amounts to the threatening danger of a regression into the most primitive superstition, that this would be much worse than Einstein's regressive remaining tied to classical field physics and that everything is a matter of holding onto the positive results and values of the *ratio*.

So, I think it's just got to be Pauli that Einstein is referring to!

On another matter, let me come back to something you wrote in your last letter:

von Baeyerism 11: *I had decided to start translating Fierz letters, but will start, instead, with the list you sent – i.e., with the selection criterion "detached observer" instead of "Fierz."*

I apologize for causing trouble. And the more even I think about it, a well defined theme for you is probably called for. How else would you be able to turn your work into a book? I fear a little, however, that "detached observer" may be too narrow, as I think Pauli and Fierz must have discussed all kinds of "mystical" things tangentially related to that topic, from the possibility of physical/psychical neutral language to synchronicity to archetypes, etc., etc. And I think all that stuff is worthwhile to get into the public eye. On the other hand, as my getting carried away has already demonstrated, some of what he wrote to von Franz and others is probably quite interesting too. So, where does that leave you? I hope not an infinite task! Maybe outside of little side ventures of gathering a little background material here and there, maybe indeed it is better to stick with your original plan of tracking the Pauli-Fierz conversation.

Tomorrow I leave for Snowbird and may be out of email contact for a little while.

Happy New Year again!

Hans's Reply

> I think we will come across even more bizarre beliefs of Pauli! For example, he and Fierz were apparently REALLY convinced that the "Pauli effect" was real. That's more than telepathy – that's telekinesis!
>
> Do not worry about my plans. I am retiring, and don't ever have to write a book again. The translations are very slow and arduous, and I'm going to just plunge in and let a theme find me, rather than the other way around. The point, for me, is that translation forces me to try to understand these things very carefully.
>
> B.T.W., did you know that the title of my Anchorage lecture is "How I learned to stop worrying about Schrödinger's cat?"
>
> Have fun in Utah! Me, I'm going to the Bahamas for a few days.

03-01-06 *The Oblique Pauli, 2* (to H. C. von Baeyer)

von Baeyerism 12: *B.T.W., did you know that the title of my Anchorage lecture is "How I learned to stop worrying about Schrödinger's cat?"*

Actually, you did tell me already. It is a great title, and more importantly a great transition!

As it turns out, I wrote a small something about Schrödinger's cat yesterday. It was in a preamble to sending Bill Demopoulos the same collection of letters I sent you in the note titled "Regarding the Detached Observer Attached." I'll place it below for your amusement. [See note to W. G. Demopoulos, dated 02-01-06 and titled "Final Installment."]

Enjoy the Bahamas. I wish I were in your shoes rather than mine.

06-01-06 *The Swedish Bayesian Team* (to A. Y. Khrennikov)

PS. I'm just returning from Marlan Scully's annual big meeting at some ski resort in Utah; there were 220 people there. Roy Glauber, this year's Nobel Prize Winner, gave a talk, and in it he showed a picture of Willis Lamb from 1964. What struck me was how much you and Lamb look alike! (Maybe there's a Nobel Prize in your future?)

09-01-06 *Sunday Reading on Monday* (to A. Cho)

Cho-ism 1: *We met a couple of years ago at the Seven Pines symposium.*

Of course I remember you: I tried to contact you last year to drum up some coverage for our meeting "Being Bayesian in a Quantum World" in Konstanz last August, but never heard back from you. (I notice that your email address has changed; maybe that has something to do with it.) Anyway, you missed a very good meeting with some of the most interesting people in the field of quantum information (Nielsen, Briegel, Mermin, Unruh, Milburn, Wootters, Hardy, etc.,) Have a look at

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`;

we would have loved to have you.

Cho-ism 2: *I was hoping you might look at the attached paper (accepted by PRL) and give me your take on its significance and importance in a phone interview.*

In the paper, Francesco De Martini of the University of Rome, "La Sapienza," and colleagues report an experiment in which they've performed a so-called minimal disturbance measurement on one photon by entangling it with a second photon and measuring it. When the two photons are maximally entangled, this effectively results in a von Neumann measurement on the first photon; when the degree of entanglement is lower, the measurement on the second photon produces a smaller disturbance of the first, at the cost of some information about its state. The researchers show that they can trace the optimal trade-off between gaining information about the original photon and disturbing its state, as was calculated in 2001 by Konrad Banaszek.

This strikes me as a very pretty experiment, and I like the fact that it seems to put a quantitative experimental handle on a pretty fundamental theoretical issue. But I'm just a journalist. I'd be most interested in your opinion of the work.

Yeah, I think it is a pretty fundamental theoretical issue; I have ever since Asher Peres and I wrote up the first calculation of this variety in 1995, "Quantum State Disturbance vs. Information Gain: Uncertainty Relations for Quantum Information," `http://www.arxiv.org/abs/quant-ph/9512023` (published as PRA **53**, 2038–2045, 1996). In fact, I have wondered on many occasions whether the effect they explore in this paper may be the single idea upon which all the formal structure of quantum mechanics can be built.

Let me give you some easy Sunday reading to skim before we talk; you may find some of it entertaining, particularly the imaginary conversation with God. First, see the closing section, "The Oyster and the Quantum," of my paper `http://www.arxiv.org/abs/quant-ph/0205039`. Next, look at pages 83–84 and 156 of my book *Notes on a Paulian Idea*, `http://www.arxiv.org/abs/quant-ph/0105039`. It's purely coincidence that those two identifiers differ only by one number. Also maybe look at the letter to Rolf Landauer on pages 190–191. (The book in its present edition is published by Växjö University Press, but a more formal edition will be published this year by Springer.)

Anyway, that's a start for indicating what I think of the importance of the paper you cite. I'm going to be pretty tied up tomorrow, but I should be able to talk Wednesday, any time between 11:00 AM, say, and 5:00 PM. My office phone number is below.

10-01-06 *Quantum Information at Princeton?* (to A. R. Calderbank)

PS. I was intrigued by Richard Jeffrey's remark in his book *Subjective Probability: The Real Thing* that "[Brian Skyrms] is my main Brother in Bayes and source of Bayesian joy. And there are others—as, for example, . . . Ingrid Daubechies, Our Lady of the Wavelets (alas! a closed book to me) but who does help me as a Sister in Bayes . . . ." I bring that up because a large component of my work is devoted to building a Bayesian-like way of viewing quantum information. You or your wife might interested in looking at some of what we accomplished at the meeting I organized last year in Konstanz, "Being Bayesian in a Quantum World"; here's a link:

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`.

Nicely, we had some of the most interesting people in quantum information there (Nielsen, Wootters, Briegel, Milburn, Mermin, Smolin, Hardy, etc.). If you have a look at the picture you'll see those guys and maybe some other familiar faces; I'm the guy in the middle with the coffee cup.

10-01-06 *Stones and Glass Houses?* (to L. Smolin)

Today I happened upon your letter in this month's *Physics Today*, which in turn led me back to your June article which I had missed. I found myself largely in agreement with most everything you said and, in particular, very much liked the original article.

However, I was struck by the conjunction of these words of yours in the June issue,

> Some other countries seem to be better at making room for the independent thinkers. . . . Canada has opened the Perimeter Institute, whose specific mandate is to be a home for independent foundational thinkers, and other such projects are in planning stages around the world.

with these words of yours from this month's reply to letters,

> In fact, young people are contributing important new results and ideas to the foundations of quantum theory, but none are working at US research universities. Let me name a few of them: Chris Fuchs, Lucien Hardy, Rob Spekkens, Antony Valentini, and David Wallace.

All I know from my own experience with Perimeter—to which I had dearly wanted to join—is that it was no more supportive of me than any "US research university."

10-01-06 *Subject and Object* (to D. M. Appleby)

Thanks for the note, which I very much enjoyed reading tonight (while listening to Abbey Lincoln and Hank Jones in the background). It did my soul good, and I'll certainly be reading it again.

Let me, however, give you a quick first reaction to this:

Applebyism 9: *I know we agree that physics isn't any kind of mirror. Well: it seems to me that if you start trying to identify some parts of quantum mechanics as "objective" and other parts as "subjective" then you are going back on that.*

In my talk at Konstanz I mentioned the idea of Galileo and Descartes that properties like "redness" don't faithfully depict properties actually in the object, whereas properties like "cuboidal" do. They thought, in other words, that the property of "redness" is subjective whereas the property of being "cuboidal" is objective. And they thought that if one eliminates all the subjective features of the visual field, one will be left with an accurate reflection of things as they are in reality: a mirror of reality. Quite possibly I have misunderstood you. If I have please correct me. But when you talk about identifying the parts of quantum mechanics having objective content it sounds, to my ears, as though you are thinking in a similar way to Galileo and Descartes. Purifying the reflection. Polishing the mirror.

Might I ask you to go back to Sections 4 and 5 (but particularly Section 5) of my anti-Växjö paper `quant-ph/0204146` and tell me whether you think the words in there seem to alleviate any of my sins? I had been planning to use the "It's a Wonderful Life" allusion to open up my *Scientific American* article, so I'd like to know what you think of it in this context. (If you don't know the movie, find a way to rent it and give it a watch.)

Honestly, I'm getting confused on these issues even myself of late. I have been meaning to read Donald Davidson's essay titled "The Myth of the Subjective" and see if that helped me any, but I haven't had the gumption yet.

You're definitely making me think about these things.

11-01-06 *Quantum Information and KS* (to J. H. Conway)

Peter Winkler, whom I was visiting at Dartmouth recently, told me a little (a very little) about your "marvelous free will theorem," as he called it, with Simon Kochen. He suggested that I might contact you to learn more about it. I have a very deep interest in Kochen-Specker type theorems, and would like to learn if there is something new and interesting here that I and collaborators could use in our Bayesian-style approach to the foundations of quantum information.

Winkler also suggested that I send you a CV and a paper or two to demonstrate that I might be worth talking to. I'll do that in the next email. In fact, I'll also send you a little research proposal I wrote up for Caltech the last time I thought about jumping ship from Bell Labs – it makes it clear that I was hoping to build a whole curriculum around the KS theorem! So, I do take it seriously. (Moreover, I take issues about "free will" seriously; thus my side interest in William James, Charles Peirce, and all of pragmatism, which that document also hints of.) At the moment, whatever you have sounds to be awfully intriguing.

I need to come to Princeton next week, in any case, to dig up some old papers. If you'll be around sometime, I'll try to tune my schedule to yours, and I would very much like to have a visit with you to get something of an explanation of all this.

11-01-06 *For the Record* (to N. D. Mermin)

Well, I'm back at home, safely again at Bell Labs, after having spent some time at a meeting in Snowbird, Utah. I am afraid I angered our friend, Prof. Plotnitsky, with my talk. My original title for the talk had been "Why I Never Understood Bohr's Reply to EPR, But Still Liked It"—but I wrote it on two overlapping transparencies so that, at the appropriate moment, I could strip off the part that said "But Still Liked It." (I hadn't originally intended to do that, but it was the only thing I could do with honesty after rereading Bohr.) And so the talk went. I explained how I didn't see much in Bohr's reply that EPR hadn't anticipated in their second- or third-to-last sentence, "... one would not arrive at our conclusion if one insisted that two or more physical quantities can be regarded as simultaneous elements of reality only when they can be simultaneously measured or predicted." (Which is a conception they pretty much simply dismiss.) Then I showed how the EPR criterion of reality can nevertheless be made to implode through a combination of perfect predictability (through entanglement) and KS noncolorable sets. Then I read some long passages from Einstein's autobiographical notes in the Schilpp volume, and claimed that his own logic was flawless: The conclusion being that a quantum state cannot correspond to a "real factual state of affairs"—when Einstein was right, he was really right! Finally I asked, so what is the uncertainty given by a quantum state about? And concluded with a picture that was meant to capture much of what I put in the emails to you a couple of weeks ago.

There's just not a lot you can do by considering only *two* noncommuting variables, and as far as I can tell, that's all Bohr ever really did.

But that's not why I'm writing you—i.e., to make my own record—but rather to get you to come down on the record. I ask because I don't know how many meetings in the last year or two where I've heard people praise the Ithaca interpretation or "correlation without correlata"—this meeting in Snowbird was one of them (Ivan Deutsch being the most recent admirer). And in all cases, I've said, "I don't think Mermin subscribes to those ideas anymore, or at least not fully." But when asked what your problems are with your earlier ideas, I don't know that I've had adequate answers of your own point of view. So, could I ask you again to what extent you now disavow the II and exactly why? I'd like to get it on the record so I don't screw up when trying to represent you.

On another issue, have you ever looked at the section in Max Jammer's book *The Philosophy of Quantum Mechanics* on "relational conceptions of the quantum state"? It seems like there's probably a lot of material in there that would interest you. I myself don't recall ever having noticed that section before last week (though surely I read it before, as I read the whole book cover to cover in the summer of 1984 or 1985), and I found it quite interesting. I also found his section on "latency theories" at the end, where he reports Margenau's view, very useful: In particular, I've started to wonder if, DISREGARDING the part where Margenau thinks of the quantum state as objective, the rest of his view might correspond quite nicely with where Schack and I stand at the moment. Margenau, apparently, views quantum measurement outcomes as secondary qualities in Locke's hierarchy, not primary ones, much like the idea of blueness (which I think you call qualia). I found myself wondering if, in the end, my own view might not just boil down to that. (This, of course, is not unconnected to some points you were trying to make in your "What Is QM Trying to Tell Us?" paper.)

See, I can write a paper on certainty, but I never can be certain myself!

David's Reply

I wish I ran into people who liked the IIQM. Maybe I should go to more meetings. I probably should write some sort of update before the 10th anniversary of the AJP article, but from my perspective it seems vaguely auto-erotic, since I haven't detected anything like the amount of interest in the paper that you describe.

It seems to me that I touched on much of what bothers me (and bothered me even then) in the final section of the AJP paper, reproduced below for your convenience:

> XII. A FEW FINAL REMARKS
>
> At the risk of losing the interest of those who — like myself — read only the first and last sections before deciding whether the rest is worth perusing, I conclude with some brief comments about loose ends.
>
> As noted at the beginning, what I have been describing is more an attitude toward quantum mechanics than a systematic interpretation. The only proper subject of physics is how some parts of the world relate to other parts. Correlations constitute its entire content. The actual specific values of the correlated quantities in the actual specific world we know, are beyond the powers of physics to articulate. The answer to the question "What has physical reality?" depends on the nature of "what." The answer is "Everything!" if one is asking about correlations among subsystems, but "Nothing!" if one is asking about particular values for the subsystem correlata.
>
> This alters the terms of the traditional debates. Traditionally people have been asking what correlata have physical reality. The many different schools of thought differ by answering with many different versions of "Some" while the IIQM answers "None!" The question of what correlations have physical reality, which the IIQM answers with "All!" has not, to my knowledge, been asked in this context. While I maintain that abandoning the ability of physics to speak of correlata is a small price to pay for the recognition that it can speak simultaneously and consistently of all possible correlations, there remains the question of how to tie this wonderful structure of relationships down to anything particular, if physics admits of nothing particular.
>
> At this stage I am not prepared to offer an answer, beyond noting that this formulates the conceptual problem posed by quantum mechanics in a somewhat different way, and suggesting that there may be something to be learned by thinking about it along these lines. I suspect our unfathomable conscious perceptions will have to enter the picture, as a way of updating the correlations. To acknowledge this is not to acknowledge that "consciousness collapses the wavepacket." But it is to admit that quantum mechanics does not describe a world of eternally developing correlation, described by "the wave-function of the universe"!, but a phenomenology for investigating what kinds of correlations can coexist with each other, and for updating current correlations and extrapolating them into the future. This phenomenology applies to any system that can be well approximated as completely isolated.
>
> A skeptic might object that the problem of how to update correlations is nothing more than the measurement problem, under a new name. Perhaps it is, but at least the problem is posed in a new context: How are we to understand the interplay between correlation as the only objective feature of

> physical reality and the absolute particularity of conscious reality? Is something missing from a description of nature whose purpose is not to disclose the real essence of the phenomena but only to track down relations between the manifold aspects of our experience? Is this a shortcoming of our description of nature or is it a deep problem about the nature of our experience?
>
> ... By acknowledging that in our description of nature the purpose is not to disclose the real essence of the phenomena, we free ourselves to construct from the manifold aspects of our experience formal representations of the systems we want to talk about. We have learned how to express their possible correlations by an appropriate state space, and the evolution of those correlations by an appropriate Hamiltonian. By setting aside "the real essence of the phenomena" we also acquire the ability to replace the befuddling spectre of an endlessly branching state of the universe—as disturbing in the self-styled down-to-earth Bohmian interpretation as it is in the wildest extravagances of the many worlds interpretation—with a quantum mechanics that simply tells us how we can expect some of the manifold aspects of our experience to be correlated with others. While this may sound anthropocentric, it is my expectation that anthropos can be kept out of everything but the initial and final conditions, and often—but not always—even out of those. But this remains to be explored.

What can I add to that, 8 years later?

1. You persuaded me quite soon that "objective probability" was problematic. Until I met you I had never taken the notion of subjective probability seriously, or even know very much about it. While I'm still not convinced (sorry) that you've got it right either, I'm much more aware that one of the pillars of the IIQM is much more fragile than I thought.

2. Not unrelated to 1, the notion of "correlation" is not well defined, beyond my assertion that it means nothing more than "joint distribution". But what does it mean to say that joint distributions are fundamental, while conditional distributions, which can be constructed from joints, have no physical meaning? And what are these joint distributions describing? How are they tied down to experience? Which brings us to

3. Consciousness. Although I say that the problem of consciousness should be set aside, when I went around giving physics colloquia on the IIQM in the late 90's, during questions everything kept coming back to conscious perceptions. It ended up being too big a rug to sweep problems under. This is a point in your favor. If probability is subjective, then there is a subject (with conscious perceptions) built in at the beginning, and consciousness becomes the starting point, rather than a completely mysterious add-on.

4. Quantum computation (which I only started studying after writing the paper) made me much more sympathetic to Copenhagen and a purely instrumentalist (positivist?) view of the subject than I had ever been. (See "How I stopped worrying and learned to love Bohr.")

Doubtless there's more, and perhaps I should think about writing something more careful and considered, but since my views have yet to settle down, that still seems premature.

So if you want something to tell these people who you claim exist, I think it's too strong to say that I disavow the IIQM. But I do regard it as at best incomplete (as I think I made clear in the 1998 paper). I guess I was hoping somebody would take up the cause and complete it.

11-01-06 *MY Anthropocentrism vs. YOUR Anthropocentrism* (to I. H. Deutsch)

I've been meaning to write you ever since arriving back in New Jersey, but first I had to clear out a huge backlog of other things. Anyway, I wanted to say I very much enjoyed the long walk and talk we had together. I felt like I got to know you more in those few hours than I had in the last ten years.

Let me clear up a couple of loose ends. Among other things, I wanted to come back to the issue of whether our Bayesian view is more overtly anthropocentric than it ought to be. In that regard, let me attach one of my pseudo-papers that addresses the issue. Whenever you get the time, have a look at Sections 4 and 5. I think they better capture some of what I was trying to get across the other day, even if the whole story is not yet worked out completely.

In the next email, I'll send you a pseudo-paper more particularly devoted to F-theory. In that one it's Sections 6 and 7 that you might want to look at, as your *leisure* time permits.

12-01-06 *It's All Your Fault* (to H. C. von Baeyer)

von Baeyerism 13: *Back from the Bahamas, I find three books from Princeton U. P. in payment for a proposal I reviewed for them.* [One I requested] *on the strength of your blurb: Omnès's Converging Realities.*

Just a quick question: Should I really read it, or were you just being kind to him (them)?

I'm not tellin'. Your interests and taste will decide the first part of your question within the first couple of chapters.

As for Omnès's ability to stir the soul, here's an example of Omnès at his best:

> Perhaps the best way to see what it is all about is to consider what would happen if a theory were able to offer a detailed mechanism for actualization. This is, after all, what the advocates of hidden variables are asking for. It would mean that everything is deeply determined. The evolution of the universe would be nothing but a long reading of its initial state. Moreover, nothing would distinguish reality from theory, the latter being an exact copy of the former. More properly, nothing would distinguish reality from logos, the time-changing from the timeless. Time itself would be an illusion, just a convenient ordering index in the theory. ... Physics is not a complete explanation of reality, which would be its insane reduction to pure unchanging mathematics. It is a *representation* of reality that does not cross the threshold of actuality. ... It is wonderful how quantum mechanics succeeds in giving such a precise and, as of now, such an encompassing description of reality, while avoiding the risk of an overdeterministic insanity. It does it because it is probabilistic in an essential way. This is not an accident, nor a blemish to be cured, since probability was found to be an intrinsic building block of logic long before reappearing as an expression of ignorance, as empirical probabilities. Moreover, and this is peculiar to quantum mechanics, theory ceases to be identical with reality at their ultimate encounter, precisely when potentiality becomes actuality. This is why one may legitimately consider that the inability of quantum mechanics to account for actuality is not a problem nor a flaw, but the best mark of its unprecedented success.

12-01-06 *Sounds of Silence and CBH, 2* (to G. Brassard)

Good to hear from you. I've been meaning to write to you for a while—thanks for reminding me. What I had wanted to write you about is that, as a climax to the van Fraassen / Halvorson seminar, Chris Timpson gave a talk as an invited speaker on the CBH theorem. It was a very, very good talk, and *sadly* very convincing. Particularly, he argued that CBH really, really (despite what Bub says) *make no use of* the no-bit-commitment axiom after all. In other words, they are really only getting the structure of quantum mechanics from 1) C^*-algebras, 2) no-signaling, and 3) no-broadcasting. No-bit-commitment comes for free. Foundationally, I don't see that that is devastating at all—it just means that "no-bit-commitment" doesn't get the airplay you had wanted of it. But as Timpson points out, it does indicate that the C^*-algebraic starting point is doing much, much more work in the derivation than had been expected. My own opinion is that *starting* with algebras like that (even weaker ones like Segal or JB algebras), is just the wrong way to go—one needs a pre-algebraic starting point.

You can read about all this in Timpson's PhD thesis, which can be found here: `http://www.arxiv.org/abs/quant-ph/0412063`. The relevant section is 9.2, and particularly pages 206 onward might interest you.

So, what is the right way to go? That's what our postdoc would work on! It sure would be good if we could scheme something up here.

Brassardisme 20: *I'm trying to emerge but at this point it is clear that I shall fail to do so before I leave for Paris (QIP06). Will I see you there?*

Unfortunately, no. But I just looked up the schedule, and I found that I was licking my lips over a large fraction of the talks. I'm very envious of you!

13-01-06 *Proxy for Dick Jeffrey?* (to B. Skyrms)

We've had a little email correspondence in the past, but I don't know if you remember me.

The reason I am writing you now is that I am thinking very hard about organizing a symposium for the PSA 2006 meeting in Vancouver, November 2–5. My tentative title for the symposium is "Radical Probabilism WITHIN Quantum Mechanics," and I'm guessing that the title is clear enough for you to surmise what the subject will be. I have run over deadline for getting a proposal in, but have had some correspondence with the program committee chairs, and they think it will still be OK if I get something in ASAP. At the moment, I am shooting to get a proposal in by Monday or Tuesday.

I'm sorry to put pressure on you like this (i.e., to give me a very quick reply), but I would very much like you to give us a talk, at least partially as a proxy for Dick Jeffrey. What I mean by this is that I would like you to give a talk with your thoughts on these two papers by Jeffrey:

- R. C. Jeffrey, "De Finetti's Radical Probabilism," *Probabilità e Induzione—Induction and Probability*, edited by P. Monari and D. Cocchi, (Biblioteca di Statistica, CLUEB, Bologna, 1993), pp. 263–275.

- R. Jeffrey, "Unknown Probabilities: *In Memory of Annemarie Anrod Shimony (1928–1995)*," Erkenntnis **45**, 327–335 (1997).

or any thoughts you have that are an extension to what he writes about there. Despite the titles, the two papers (particularly the first one) have very large parts devoted to quantum mechanics.

My own opinion is that he definitely gets the subjective interpretation of quantum states right, but when it comes to what those quantum states are effectively degrees of beliefs about, he shoots off the mark a bit. Nevertheless, his contribution and historical role in the "quantum Bayesian" project deserves to be fleshed out a bit—and I can't think of a better person to do it than you.

What I mean by the quantum Bayesian project is a locus of several new results that, I think, have started to make this way of viewing quantum states very respectable (in fact a hot topic for research). Let me give you a reference to a couple of my own papers that may help orient you:

- "Unknown Quantum States and Operations, a Bayesian View"
  `http://www.arxiv.org/abs/quant-ph/0404156`
- "Quantum Mechanics as Quantum Information (and only a little more)"
  `http://www.arxiv.org/abs/quant-ph/0205039`

Also, you might look at the website (containing all the abstracts) for our meeting last year titled "Being Bayesian in a Quantum World"

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`.

Finally you might look at Itamar Pitowsky's paper:

- "Quantum Mechanics as a Theory of Probability"
  `http://www.arxiv.org/abs/quant-ph/0510095`

for another variation on the same theme.

I hope you get the feeling (that I think you should!) that this is now a very large area of research with much promise. And it is an area of research that philosophers of probability can give much to. Thus I hope you will be able to give us a talk, and will let me know of your decision as soon as possible.

Beside you and myself, I'm also hoping to attract Pitowsky to give a talk, and it would be nice if Abner Shimony would serve as a counterpoint to all of us.

13-01-06 *Counterpoint to the Proxy for Dick Jeffrey?* (to A. Shimony)

I just wrote the note below to Bryan Skyrms, asking him if he would participate in a symposium for PSA 2006 that I am hoping to organize. [See 13-01-06 note titled "Proxy for Dick Jeffrey?" to B. Skyrms.] Now, I would like to ask you to participate, in the capacity of trying to keep us all honest: i.e., as a counterpoint to Skyrms, Pitowsky, and me. I know that you have thought very deeply about the subject described below—and I know that you disagree with us—so I hope that you will join us and use your eloquence to explain to the audience why they shouldn't agree with us either!

Please read the note below, and you'll get a feeling for what I have in mind. And if I could hear back from you as soon as possible, I might stand a chance of actually getting this thing organized before the program committee loses patience with me.

I hope that all is well with you, and that your new year has gotten off to a great start!

13-01-06 *Proxy for Dick Jeffrey?, 2* (to B. Skyrms)

Thanks for your note. Well, this is what I get for waiting a month past the last minute! Neither you, nor Shimony, nor Pitowsky even, is in a position to be able to attend! So, I'm not quite sure what I'm going to do—maybe I'll have to drop the project.

I can certainly find others to talk as a "quantum Bayesian" but what was attractive about Pitowsky was that he's a certified philosopher—all the rest I know are physicists by trade. I'll have to think.

But who else but you could really fill the shoes of commenting on Jeffrey's radical probabilism papers? Do you have any suggestions?

Anyway, thanks; I much appreciate your quick reply.

And congratulations by the way on your PSA presidentship.

Brian's Reply

> I believe that Dick liked Stephen Leeds on subjective probabilities and QM. But I don't think that Dick's own views ever got completely developed—especially with regard to measurement.

15-01-06 *Things That Are Outside of Time* (to A. Y. Khrennikov)

Khrennikovism 13: *P.S. My best greetings to your wife! (Did she finish the reconstruction of your house?)*

She will never be finished reconstructing! It would be against her nature ...

16-01-06 *Quantum Information Coincidence?* (to K. T. McDonald)

This morning by chance, playing around, I told my daughter Emma, "Come here; let's see if your name has ever appeared on the internet." So we typed "Emma Jane Fuchs" into Google and got the result in 0.10 seconds. There weren't a whole lot of documents to come up, but what was surprising is that one of them is posted at your website: It's a copy of my samizdat *Notes on a Paulian Idea*! What on earth? I see that you have it stored in a folder called "examples," so I was left wondering "example of what?" I'm almost afraid to hear the answer ...

If for whatever reason you'd like a copy of the paperback version (published by Växjö University Press), let me know and give me a proper mailing address: I have dozens to give away.

16-01-06 *Quantum Information Coincidence?, 2* (to K. T. McDonald)

Thanks for allaying my fears! I feared that you had used the thing as an example of nonsense.

McDonaldism 1: *I'd be pleased to receive a copy of "Notes on a Paulian Idea".*

I sent a note to the secretary in Sweden to send you a copy. It'll probably arrive in a couple of weeks.

Your lecture notes for your quantum computation course are very, very impressive! If this is the norm for a course and I ever do wiggle my way into Princeton, clearly I'll have to adopt a new work ethic!!

McDonaldism 2: *I gather that you stray into quantum metaphysics from time to time, so you might or might not be amused by my brief foray into this realm:*

`http://puhep1.princeton.edu/~mcdonald/examples/evasion.pdf.`

On the whole it's better to leave metaphysics alone, but its temptations are hard for many of us to resist.

Concerning your question of "metaphysics", it depends on what you mean by a "hidden-variable theory" and what you mean by "cloning." If the hidden-variable theory is like Goldstein's version of Bohmian mechanics, which (they say) is empirically equivalent to quantum mechanics for all experiments, then the existence of those hidden variables amounts to nothing beyond their inventors' warm, comfortable feelings—the comfort one usually finds in a religion.

On the other hand, I can think of a perfectly good "unhidden variable theory" that still has a no-cloning theorem: It is simply classical Newtonian mechanics where the question is whether Liouville distributions can be cloned. Since Hamiltonian mechanics is phase-space volume preserving, you can't clone overlapping Liouville distributions any more than you can clone quantum states. (I.e., in my view, quantum no-cloning is not a particularly deep theorem. Certainly not one to get metaphysical about. Save that up for the Kochen-Specker theorem.)

16-01-06 *Bushwhacking* (to H. R. Brown & C. G. Timpson)

The title of this email refers *not* to what I think ought to be done with George Bush's policies (though that's what I think ought to be done), but to some of the content below.

I had a chance to read your new paper after all today, and I very much like it. I even think I agree with most of it. Particularly, I very much agree with this point you make:

> [I]t is worth recalling . . . that the dominant viewpoint in the philosophy of space-time physics over the last few decades puts a very different gloss on Minkowski's contribution to SR. Far from being the basis of a mere algorithm for SR, the current orthodoxy seems to be that Minkowskian geometry provides a constructive dimension to SR (though it is not always put in these terms), and thereby significantly enhances its explanatory power. According to this view, it is the structure of the Minkowski space-time in which they are immersed that ultimately explains why rods and clocks in motion contract and dilate respectively.

As far as I know, that is the dominant viewpoint, and in my own opinion, it is rightly viewed as Minkowski's most important contribution.

This is a point I tried to emphasize in my PSA talk last year; I don't know if Chris remembers it. In the talk, I gave my usual spiel about how we won't ever really clear up the muddle at the foundations of quantum mechanics until we can formulate the essential content of the theory in a couple of crisp, clear, physical statements (drawing on a principle-theory formulation of SR as an *example*). But then, for that particular audience, to make it clear that I set myself apart from Jeff, I included a new transparency to make the point that one shouldn't think of the finding of the principles as the end of the line—rather, they're the real beginning, from whence one can hope to make greater progress. The transparency is attached. I pointed out that from the principles, one finally had a clear enough view to make the leap to Minkowski spacetime—which I see as a unification of the principles and a tentative *ontology* underlying SR. Most importantly, it was only after that tentative ontology was in place, that one had a starting point to move physics forward to General Relativity—namely, to incorporate gravitational phenomena, you curve the manifold.

So I see it with quantum mechanics: first, (convincing) principles rather than Hilbert space rubbish; then, tentative ontology (what I use the word "Zing!" as a placeholder for at the moment); and finally, a test that the ontology has some cash value over-and-above the principle-theory phenomenology: can the ontology be used successfully as a starting point for a broader, more powerful theory? Metaphorically, what happens if you curve the Zing!?

Here's the way I tried to express the sentiment in my old paper `quant-ph/0205039`:

> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.
>
> Let me try to give a better way to think about this by making use of Einstein again. What might have been his greatest achievement in building general relativity? I would say it was in his recognizing that the "gravitational field" one feels in an accelerating elevator is a coordinate effect. That is, the "field" in that case is something induced purely with respect to the description of an observer. In this light, the program of trying to develop general relativity boiled down to recognizing all the things within gravitational and motional phenomena that should be viewed as consequences of our coordinate choices. It was in identifying all the things that are "numerically additional" to the observer-free situation—i.e., those things that come about purely by bringing the observer (scientific agent, coordinate system, etc.) back into the picture.
>
> This was a true breakthrough. For in weeding out all the things that can be interpreted as coordinate effects, the fruit left behind finally becomes clear to sight: It is the Riemannian manifold we call spacetime—a mathematical object, the study of which one can hope will tell us something about nature itself, not merely about the observer in nature.
>
> The dream I see for quantum mechanics is just this. Weed out all the terms that have to do with gambling commitments, information, knowledge, and belief, and what is left behind will play the role of Einstein's manifold. That is our goal. When we find it, it may be little more than a minuscule part of quantum theory. But being a clear window into nature, we may start to see sights through it we could hardly imagine before.

So, as I've already said, I think I agree with you much. However, I wouldn't use the words that you used for Einstein's initial formulation of SR—i.e., as a "strategic retreat"—to describe this methodology (at least in the case of quantum mechanics). Rather, I think of it as an effective method for "clearing out the underbrush"—i.e., making the real lay of the land visible. It's not a strategic *retreat* at all, but rather simply good strategy. I have big dreams for where we might take quantum mechanics, but first we've got to clear out the underbrush of extraneous observer-dependent detail in it. And I see no sure-fire and nonspeculative way of doing that but to <u>first</u> reduce the theory to something like a principle formulation.

Anyway, good paper.

16-01-06 *Lovely Word* (to C. G. Timpson)

By the way, what was that lovely word you used in your lecture at Princeton to describe information's relation to reality? As I recall, it struck me because the idea it conveyed was so similar to the Jamesian phrase I often use in my writings, "numerically additional"—there was an example of that phrase in the note I just sent you and Harvey. Here are some other ways I've used it in various pieces:

> Suppose one wants to hold adamantly to the idea that the quantum state is purely subjective. That is, that there is no right and true quantum state for a system—the quantum state is "numerically additional" to the quantum system. It walks through the door when the agent who is interested in the system walks through the door. Can one consistently uphold this point of view at the same time as supposing ...

or

> William James likes to say that all beliefs are "numerically additional" to the reality they take as their target, even "true" beliefs.

or

> Within quantum mechanics, there is an invariant piece which is common to all of us by the very fact of our accepting the theory. That is what we are in search of because in some sense—which need not pertain to a realistic conception of a theory's correspondence to nature—it is the core of the theory. It is the single part that we agree upon, even when we agree upon nothing else. In the direction I am seeking to explore, the quantum state is "numerically additional" to that core. (That is, the quantum state is a compendium of Bayesian "beliefs" or "gambling commitments" and is thus susceptible to the type of analysis James gives in his "Sentiment of Rationality." Our particular choice of a quantum state is something extra that we carry into the world.)

Would that nice word of yours nicely substitute into these paragraphs? If so, I think I might adopt it.

Chris's Reply

> You know what: I think it might be even better fitted for these contexts than for the ends I had in mind. The term was 'adventitious' adj.: of the nature of an addition from without; extrinsically added, not essentially inherent (OED).

16-01-06 *Little Phrases* (to D. M. Appleby)

A subject and an object, from a physicalist perspective, are simply two objects. Suppose the subject has a belief about something to do with an object (even if only a belief about how some interaction with the object will lead to a certain sensation in the subject). To the extent that the belief is a possession of the subject, and the object need not abide by it, the belief is "subjective." I.e., it is a statement of a property of the subject. To the extent that the belief encodes a statement of the subject's history and composition (perhaps its genetic makeup, the culture in which it was raised, the accidental things that happened to it throughout its life, how much alcohol it had to drink just before the belief, etc.), it is "objective." For, it is a settled and inalienable possession of the actual historical record of that small piece of the world.

Does any of this wordplay have anything to do with settling the issues you wrote me about last week?

17-01-06 *Wheeler Meeting at Princeton* (to several at Princeton University)

Dear colleague in the Princeton community,

Prof. Marlan Scully and I are organizing a somewhat impromptu but nonetheless serious meeting in Princeton to give a little tribute to John Archibald Wheeler and his "law without law" and "it from bit," and also to discuss some recent developments in quantum information. It'll be a one-day meeting with maybe 10 short talks and a little socializing, held Friday, February 24. Wheeler has said that he will make an appearance, and we would like to invite you too.

Please let me know as soon as possible if you would like to come and if your schedule will permit it. Also please say if you would like to give a talk: Tentative titles are encouraged, even if some will have to be eliminated in the end (with a lottery) due to time constraints.

On this round of invitations, we're checking on the availability of the following Princeton locals to see if they can participate: Rob Calderbank, John Conway, Bas van Fraassen, Hans Halvorson, Simon Kochen, Kirk McDonald, and Edward Nelson.

Already from further quarters, we have commitments of participation from: Carroll Alley, Ken Ford, Daniel Greenberger, Mark Hillery, Benjamin Schumacher, Yanhua Shih, William Unruh, William Wootters, and Suhail Zubairy.

It looks to be a promising meeting; we hope you can participate!

17-01-06 *Bushwhacking, 2* (to H. R. Brown & C. G. Timpson)

Brownism 1: *In my opinion, after the weeding, you end up with the wavefunction, or the vector in the Hilbert space. I confess I cannot see why that is 'rubbish'. Maybe you think it is too complicated, too messy. Too many theoretical principles? I am reminded of Einstein's saying about how good it is to simplify as much as possible — BUT NO MORE.*

I'll try to get my hands on your book soon; that'll be fun.

As for your other questions, I'll let Chris Timpson be my proxy if he'll take the task. He'd probably do a better job than I, anyway: What you trim, depends on the first count as to what you see as the more likely solution: that the state vector as 'inherent' to the quantum system or rather 'adventitious' (a lovely word I learned from Chris). Anyway, you two are probably closer to a common pub; and I know you enough already, Harvey, to know that your defenses will have to be down before any of this worldview will ever seep into you.

17-01-06 *Wheeler Meeting at Princeton* (to K. T. McDonald)

McDonaldism 3: *Bill [Brinkman] is very nuts and bolts about bits. He is active in encouraging us to get involved with the implementation of quantum information processing, more than addressing* *theory issues. But I'd hate to see quantum information go the way of high-energy physics in which there's too huge a gap between theory and experiment.*

My own view is that quantum information formalism has gotten way out ahead of lab reality, and that the next phase of hard work is more in the lab than not ...

True, but what else can a theorist do but follow his interests. At least the gap in quantum information theory and experiment is not like the gap between string theory and experiment: There's NO speculation in quantum information, no claims that anything must be right because of 'beauty'—the results are either right or wrong, because after all quantum information is just elementary quantum mechanics pushed a few steps further.

17-01-06 *No 'Plunging Ahead' After All* (to S. Hartmann)

Well, all my hopes for a Bayesian-QM PSA symposium got dashed this weekend: 1) Pitowsky couldn't make it because of teaching duties, 2) Skyrms couldn't give a talk because he'll be giving the presidential address, and 3) Shimony had too many commitments too. I could easily find someone to replace Shimony—he wasn't that crucial—(maybe you would have liked to have spoken too, or maybe Elga as Hans had suggested, or maybe Bill Demopoulos), but without the two headliners of Pitowsky and Skyrms—both of which had ingredients that I couldn't figure out how to replace—there didn't seem much reason to go on. Oh well, c'est la vie; maybe two years from now I'll have a more thought-out plan.

I'm sorry to have troubled you about this, to have it all come to naught.

Hope you are enjoying California. (Outside of the professional part, is there anything to enjoy in Irvine?)

17-01-06 *Diptera* (to D. M. Appleby)

I'm not sure what I did to do it, but it looks like I really angered you. All I guess is that you took very seriously my one (flippant?) use of the work "physicalist," which probably has a lot of connotations for you. I'm not completely sure what it means (other than the Webster definition), but I was trying to search for a word that conveyed the idea that there is no true divide between subject and object in a Kantian or Schopenhauerian sense. What do you call that?

Did you read the Davidson essay "The Myth of the Subjective"? What would you call that kind of idea? Maybe not physicalism, but then what?

You write, "You are probably thinking that the physicalist perspective you ask me to adopt ..." But I didn't ask you to adopt anything. I ASKED, "Does any of this wordplay have anything to do with settling the issues you wrote me about last week?" It was my meager attempt to see if that was what was at issue; it was a (lame?) attempt to clarify things to my mind.

18-01-06 *New York Times Quote* (to me)

Betty Friedan in *The Feminine Mystique*:

> Down through the ages man has known that he was set apart from other animals by his mind's power to have an idea, a vision, and shape the future to it ... when he discovers and creates and shapes a future different from his past, he is a man, a human being.

18-01-06 *What I Must Have Meant* (to D. M. Appleby)

I searched my memory on and off though my sleeping/sleepless hours last night to see if I could find a clue to why I had used the word "physicalist" in my note to you.

Well, I found it: It came to me through Rorty. "Nonreductive Physicalism" is what he calls his view and what he calls Davidson's in the "The Myth of the Subjective" and other places. So, I finally read Davidson's essay, and I RE-read Rorty's essay "Non-reductive Physicalism" in his book *Objectivity, Relativism, and Truth*. And I think that view approximates to some extent the direction I'd like to go.

Did you ever read Rorty's introduction to that volume, like I had once suggested to you? (I'll place my old note suggesting that below.) He states quite clearly there what he means by antirepresentationalism—I think it is a view that approximates our own two views (i.e., yours and mine). Yet, later in the book Rorty has this essay "Non-reductive Physicalism," and he certainly doesn't see it at cross-purposes with his introduction. So my question is, what do you think of it? Is it a meaning for the word "physicalism" that you could accept?

18-01-06 *Integers and Real Numbers* (to D. M. Appleby)

Regarding this piece of your note:

Applebyism 10: *I suppose one might argue, with some plausibility, that the integers are a purely logical structure, devoid of physical content. But I have doubts even about that. Not only does the concept of an integer originally arise in the context of counting sets of physical objects. That is the still the way it is defined in axiomatic set theory. And I would say that the concept of an object is a physical concept, which embodies assumptions about the world. Ditto the concept of a set. Furthermore, it seems to me that there is something very classical about the concept of a set. The concepts of object and set clearly have an approximate, everyday phenomenological validity. But it seems to me that they have "human", in fact "primitive human", stamped all over them. I see no reason to assume that objects and sets exist in ultimate reality. Indeed, to my mind quantum mechanics rather suggests the opposite.*

Below is a note that Danny Greenberger once wrote me, which I liked a lot. Maybe you'd be interested in reading it in this connection. (And maybe I'll cc this note to Mermin, as he and I once had a discussion on the integers—I think it was in Capri—and I took the stubborn position that even they don't exist by themselves ... much like you seem to suspect. I'm not sure I ever shared Danny's note with David.) [See 06-12-02 note titled "Enjoyed" to Greenberger.]

18-01-06 *Wheeler Meeting at Princeton* (to E. H. Lieb)

Good to finally meet you; I've been a fan for many years. (For a while, when I was working on my PhD, I thought the key to all the secrets of the universe might be found in one of your papers on operator-Schwarz inequalities.)

Liebism 1: *Marlan suggested a review of strong subbadditivity, but this is well known stuff, or is it? Probably you have more up to date material in mind.*

Well, the meeting is a bit of a hodgepodge of younger and older guys—the only common thread really being John Wheeler's influence on quantum foundational thought in the past. Certainly, strong subadditivity is old stuff to some of the guys in my side of the invitation list—say, Schumacher, Wootters, maybe Unruh, etc. But I doubt Greenberger, Alley, and others (including Marlan!) know anything about it. So, I think it couldn't hurt to give a talk about it. Maybe you could squeeze statements of some of your newer theorems in as asides … but the talks will probably be pretty short if we try to get 10 of them in in one day.

19-01-05 *The Wheeler Meeting* (to D. B. L. Baker)

Well the holidays came and went and I didn't write you a thing. I thought I would, and maybe send you some new pictures, but time just slipped away—it always seems to slip away now.

But I found myself thinking about you again yesterday, particularly that incident in 1984 when John Wheeler called your mom because you hadn't turned in a term paper for his class. John was, I think, 73 then.

He's almost 95 now. I've been involved in organizing a little conference for him at Princeton, and so all kinds of memories have been coming to me. In this connection, I got the nice note (and attachment) below from Ken Ford, John's biographer and former student, that gives some sense of what the man is like now. It was Ken's line about "courtliness" that made me think about John's calling your mother. I hope you have MS Word installed, so that you can read the attachment; if you don't let me know and I'll put the words into a plain text file.

Mostly, Ken's closing lines got to me. It kind of reminded me of that Joan Baez – Kris Kristofferson duet, "Hello in There". I think the song was written by John Prine. Do you know it? If you don't, some approximation to the lyrics can be found here:

`http://www.cowboylyrics.com/lyrics/prine-john/hello-in-there-10843.html.`

I hope you and the family are all doing well.

19-01-06 *Elevator Stories* (to A. Cho)

Let me send you one last little thing to read while it's on my mind. You might enjoy looking at the introduction and conclusions to this paper: `http://www.arxiv.org/abs/quant-ph/0404122` (particularly the elevator stories). Anyway, this has to do with the question you asked near the end of the phone conversation about what I would like to see experimentally explored next—this paper talks about that optimal alphabet I was telling you about.

Good luck with your writing.

22-01-06 *QCMC, Hirota, and Giacometti* (to O. Hirota)

Thank you for the invitation to serve on the advisory committee for QCMC; I graciously accept.

It is a funny coincidence that I open my email to find a note from you this morning. The reason is because, before turning on my computer, I had planned to write you a note myself this morning!! This is really true. Last week, I took my older daughter Emma to the New York Metropolitan Museum of Art to celebrate her 7th birthday. All week, I have been meaning to tell you about how we saw two pieces of Alberto Giacometti's work there! Did you see those pieces when you visited New York? One was a sculpture, called "Three Men Walking" I think. And the other was a painting of his mother, I believe. They were striking!

I very much look forward to QCMC this year. It has now been several years since I have been to Japan; I miss it very much.

I hope you will sometime come to visit us in our new home in New Jersey. It is a big old Victorian home (from 1895) that my wife has been reconstructing wildly. Most recently she painted it orange!! (I will attach two pictures.) It would be wonderful to talk of quantum information and noncausality within these orange walls with you! And, Kiki will cook for you grandly!

22-01-06 *Stranger Still!* (to O. Hirota)

Me again. Reviewing some of my old emails to you, I discovered something stranger than the coincidence I wrote you about this morning! I discovered a whole set of coincidences with you. Note, for instance, what I wrote you on 1 June 2004:

> It was a coincidence that you wrote me today. I had been meaning to write you again about the QCMC award—and I was going to do it today!

or as another example what I wrote you 27 May 2003:

> It was an interesting coincidence that I would get your book today. Just this morning, the secretary in Sweden told me that she mailed off several copies of my samizdat *Notes on a Paulian Idea*. You were in the list of recipients. So it should be arriving soon.

Non-causality!!

25-01-06 *Beyond QM and So On* (to A. Y. Khrennikov)

Khrennikovism 14: *And finally: I got again the proposal from AIP to publish Conf. Proc. Would you like to be one of the Editors? It would be natural (then at the end you should write about people from your team in the preface and make selection of papers related to quantum information).*

As for my being an editor, that will be fine. And yes, I will write an introduction for my team.

By the way, I met Simon Kochen for the first time Monday, and had an extended discussion with him (2.5 hours). The main point I got from him is that he thinks the essence of quantum mechanics is that it is a *contextual probability theory*, and he thinks that he and John Conway have some strong mathematical results that support that claim. I thought of you as he told me all that.

26-01-06 *Some Words to Use, Maybe?* (to R. E. Slusher)

The great advantage of the controlled cold collision technique for optically trapped neutral atoms is the ease and simplicity with which it can generate Raussendorf-Briegel "cluster states" over any number of qubits [1]. These are entangled quantum states with the remarkable property that they are a *universal* resource for quantum computing [2]. If a technique can produce these states deterministically, then the remainder of any quantum computation amounts to simply performing single qubit measurements thereafter. Moreover, the complexity class structure of this type of computation may be somewhat better than the older unitary gate style (or circuit model) of quantum computation because it allows for many of the measurements required of an algorithm to be performed in parallel—this is something the circuit model cannot support [3]. The discovery of the cluster-state method of quantum computation should not be underestimated: It is among the top four theoretical discoveries in the history of the field (up there with the factoring algorithm, database search, and the existence of quantum error correction and fault tolerance).

However, beyond the technology-enabling universality property already discussed, cluster states are also of significant raw physical interest [4]. For instance, among the class of entangled states over N qubits, there is a sense in which cluster states have the most persistent entanglement: A user must perform measurements on at least $N/2$ qubits of the original N before the entanglement in the remainder is completely depleted. This is a property that Bell, GHZ, and error-correcting code states do not have. Also, cluster states have the interesting property of being maximally connected: By performing local measurements, one can teleport between any two sites in the array. This physical phenomenon that has not been demonstrated in an array before. Finally, the cluster-state model of quantum computation provides a test-bed for quantum foundational studies that take quantum measurement as the primary nonclassical feature of quantum mechanics [5], for, among other things, they show that even unitary evolution is ultimately representable in measurement and entanglement terms alone [6].

[1] Recently, there have been a few experiments producing $N = 4$ cluster states (and possibly $N = 5$ in unpublished work)—see, for instance, P. Walther, et al., "Experimental One-Way Quantum Computing," Nature **434**, 169 (2005); P. Walther, et al., "Experimental Violation of a Cluster State Bell Inequality," Phys. Rev. Lett. **95**, 020403 (2005). However the production of these states rely on down-conversion techniques that cannot produce cluster states *deterministically.* Moreover, the experimental results reported are always of the post-selected variety: I.e., the cluster state must be destroyed before it can be said to have *been* there.

[2] R. Raussendorf, D. E. Browne, and H. J. Briegel, "Measurement-based Quantum Computation with Cluster States," Phys. Rev. A **68**, 022312 (2003).

[3] R. Jozsa, "An Introduction to Measurement Based Quantum Computation," `quant-ph/0508124`.

[4] H. J. Briegel and R. Raussendorf, "Persistent Entanglement in Arrays of Interacting Particles," Phys. Rev. Lett. **86**, 910 (2001).

[5] C. A. Fuchs, "Quantum Mechanics as Quantum Information (and only a little more)," `quant-ph/0205039`.

[6] M. A. Nielsen, "Cluster-state Quantum Computation," `quant-ph/0504097`, to appear in Rep. Math. Phys.

27-01-06 *Wheeler Quantum Information Meeting, February 24–25* (to the Wheelerfest participants)

Dear Colleague,

If you are receiving this email, it means that I have heard back from you with a firm or tentative yes that you will be able to participate in the "Wheeler – Quantum Information Meeting" that Marlan Scully and I are organizing at Princeton. The final dates for the meeting have now been set: The meeting will run all day Friday February 24, reconvening Saturday morning February 25 for a further session (until noon). Friday night, we will have a conference dinner.

If you have not done so yet, please send me a tentative title for your talk.

Presently we are either hoping for or expecting talks from: Carroll Alley, Rob Calderbank, Ken Ford, Chris Fuchs, Danny Greenberger, Hans Halvorson, Mark Hillery, Simon Kochen, Elliot Lieb, Stephen Lyon, Kirk McDonald, Edward Nelson, Wolfgang Schleich, Ben Schumacher, Marlan

Scully, Robert Seiringer, Yanhua Shih, Bill Unruh, William Wootters, Suhail Zubairy, Wojciech Zurek, and maybe John Conway and Anton Zeilinger. Also, there will likely be some local students and postdocs giving talks.

Attached are instructions for making your hotel reservation and suggestions for how to get from the local airports to the hotel; we strongly suggest you fly into Newark airport if you are flying. For the Friday of the meeting, all talks and the conference dinner will be held at a conference center near the Hyatt hotel; shuttle service from the hotel to the conference center will be provided by grad students. The Saturday morning session will convene at a lecture hall on Princeton University campus; again shuttle service will be provided.

The present plan is that John Wheeler will be attending the Friday morning part of the meeting. To give some sense of what John is (still) interested in, I'll also a attach a nice article that Ken Ford wrote recently for the Princeton Physics Department Newsletter.

Marlan and I look forward to seeing you all. And I will be communicating with you all again before the date with further details.

27-01-06 *Monday or Tuesday Meeting* (to H. Halvorson)

Halvorsonism 5: *Thanks for coming to Princeton yesterday. I really did appreciate the fairly comprehensive overview of your current perspective. By the way, regarding upcoming meetings, I could also come up your way sometime, or we could try to find a midpoint meeting place. In any case, I would like to try to maximize the chances that we will meet again soon and often.*

I enjoyed it too, and it was nice to learn a little more about the things you're thinking and about our common ties through Davidson. Thanks for your nice offer too; I'll certainly take you up on it sometime.

In the meantime, though, I was thinking about coming to Princeton Monday or Tuesday (if everything continues to go OK with my step-dad): My in-laws are visiting, and I thought my father-in-law might like to explore a little of the town and campus. So to kill two birds with one stone, maybe I could have a meeting with you while he's entertaining himself. Would you have any time Monday or Tuesday?

By the way, I thought of something I should tell you with regard to Hardy's axioms. You said you were particularly interested in Hardy's continuity axiom. But that axiom, at least within the context of the other ones, is particularly weak. In its place, Hardy could have inserted almost any distinction between classical probability theory and quantum mechanics, and it would have still done the trick. For instance, instead of requiring the existence of a continuous transformation between pure states, he could have outlawed broadcasting. That would have been just as effective for narrowing him down to quantum mechanics (as all one needs in that spot is a statement that works in the quantum case but not the classical one). Thus, his axiom structure is not particularly tight.

27-01-06 *Degrees of Freedom / Distinguishable States* (to H. Halvorson)

Also, I spent a little time this morning trying to search my archive of mumbles to see if I could find something that decently expressed my distaste for taking "the number of degrees of freedom" or "the maximum number of distinguishable states" as the foundation for Hilbert-space dimension.

Unfortunately, I wasn't as successful as I had hoped I would be: Maybe it means my memory is failing, and I haven't really yet written a clear statement of what I'm thinking.

So, let me give you the pointers that I could find in the meantime:

- "On the Quantumness of a Hilbert Space," `quant-ph/0404122`
- pages 159–160 of *Quantum States: W.H.A.T.?*

I want Hilbert-space dimension to denote not (a priori at least) number of distinguishable states, but rather something to do with a quantum system's sensitivity to the touch or its creative power. And that's partially what I am trying to get at with ideas like "quantumness".

30-01-06 *Loose Ends and Pedagogy* (to K. T. McDonald)

It took me quite a while, but I finally found it. I'm talking about the John Wheeler passage on $\hbar$ that I mentioned to you. Let me paste it here:

> The quantum, $\hbar$, in whatever correct physics formula it appears, thus serves as a lamp. ...
>
> Giving us its as bits, the quantum presents us with physics as information.
>
> How come a value for the quantum so small as $\hbar = 2.612 \times 10^{-66}$ cm^2? As well as ask why the speed of light is so great as $c = 3 \times 10^{10}$ cm/s! No such constant as the speed of light ever makes an appearance in a truly fundamental account of special relativity or Einstein geometrodynamics, and for a simple reason: Time and space are both tools to measure interval. We only then properly conceive them when we measure them in the same units. The numerical value of the ratio between the second and the centimeter totally lacks teaching power. It is an historical accident. Its occurrence in equations obscured for decades one of nature's great simplicities. Likewise with $\hbar$! Every equation that contains an $\hbar$ floats a banner, "It from bit". The formula displays a piece of physics that we have learned to translate into information-theoretic terms. Tomorrow we will have learned to understand and express *all* of physics in the language of information. At that point we will revalue $\hbar = 2.612 \times 10^{-66}$ cm^2—as we downgrade $c = 3 \times 10^{10}$ cm/s today—from constant of nature to artifact of history, and from foundation of truth to enemy of understanding.

I certainly agree with significant pieces of that. Particularly, through quantum information theory I think we are already seeing strong hints of how to formulate the essence of quantum mechanics without ever making mention of $\hbar$. The idea is to let finite dimensional Hilbert spaces lead the way to telling what is really quantum and what is not.

It probably won't add much to that statement, but let me record that I really believe it by attaching a teaching proposal I wrote for Caltech the last time I thought about jumping ship from Bell Labs (a couple years ago). At the time, there was a small chance of an interdisciplinary position between physics, philosophy, and engineering (through their Information Science and Technology initiative), and the proposal was written with that mix-match in mind. The relevant part for you is the section on "Innovative Undergraduate Quantum Mechanics." It took a little nerve to write that stuff, but I think it needs to be said.

I've been meaning to write you since returning from NIST, but I wanted to find the Wheeler quote first. That meeting really fired me up for this DTO proposal we're writing. Let me ask you this. As you get a chance, have a look at: O. Mandel, *et al.*, "Controlled Collisions for Multi-Particle Entanglement of Optically Trapped Atoms," *Nature* **425**, 937–940 (2003). If we (Lucent) could use our MEMS spatial light modulators to create 50,000 individually movable atom traps for enacting these controlled cold collisions—as we think we can in the near-term future—to set the scene for cluster-state quantum computation, would that count in your mind as "doing physics" within quantum information? I'm just trying to get a feeling for the sorts of distinctions you seemed to be making at lunch the other day.

30-01-06 *Loose Ends and Pedagogy, 2* (to K. T. McDonald)

McDonaldism 4: *Your teaching proposal to Caltech looks like it might find better reception in a philosophy department than in a physics department.*

Well, yes. The potential position was explicitly to be interdepartmental if it materialized (it didn't materialize at the time). I guess you're telling me my proposal looks to have left out the other half ... ... ouch.

McDonaldism 5: *A clarification. I have somehow (without being well informed) come to associate "Bayesian" with the notion that there is no such thing as intrinsic randomness in Nature.*

I hope that this term means something different to you.

McDonaldism 6: *But I am a bit worried by the quotation: "Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterizing how an individual should act in order to avoid certain kinds of undesirable behavioral inconsistencies."*

I interpret this as implying that Bayesian Statistics provides a kind of deprogramming for those misguided souls who take too seriously the claims that intrinsic probability plays a key role in Nature.

I'll consider how to best respond to your other queries over the rest of the day. In the meantime, let me ask if Sections 3, 4, and 5 of [`arXiv:quant-ph/0204146v1`] don't already answer your questions relatively directly? And within that maybe pay particular note to the discussion near the ALL-CAPPED "reality" on page 6 and the last two paragraphs of page 8 for your question on "intrinsic randomness."

30-01-06 *Wheeler Accuracy?* (to J. Preskill)

Marlan Scully and I are organizing a little impromptu meeting for John Wheeler and quantum information at Princeton Feb 24 & 25, and I think I want to open my talk with that story you once told me about the flying equations. It's recorded at the top of page 149 of my *Notes on a Paulian Idea*, which you can get off my webpage below. I wonder if I could ask you to check that write-up for accuracy, and tell me any other tidbits you remember that might be of use for setting the mood of the story. I just want to make sure I get the dramatic effect right.

After that intro I will spend some time talking about our Bayesian view of QM: That the formal structure of quantum mechanics should not be viewed as some kind of mathematical mirror image of the real world, but rather as a layer on top of Bayesian probability theory (molded by the real world). It represents how an agent should tie together his gambles when the subject matter is the agent's "kicking" the external world and waiting for the reaction it produces within him (formerly known as quantum measurement). Then, to end with making a connection back to John, I want claim that the disconnect between the "real world" and "QM as normative theory of gambling" is precisely the sort of space one needs to get the kind of flight John was talking about—no space, no flight.

I'm surely responsible for all the errors in the latter part of the talk, but I hope you can help me to be as historically accurate as possible in the first part of it.

By the way, if you're interested in coming (and/or talking), please do come. I could send you the logistical information if you're interested. John will make an appearance for two or three hours the Friday morning. So far, outside of the Scully crew, I've only been inviting his old students and Princeton locals; but now that I think of it, you might possibly be interested too. I'll paste in the tentative speaker list below. I think everybody in the list will be coming except (probably) Zeilinger, Zurek, and Schleich—I just don't have talk titles yet from most (only previous confirmations).

John's Reply

> Well, the moral of the story is correct. The course was actually called Honors Physics and it was for sophomores intending to major in physics. It started with Goldstein *Classical Mechanics*, but actually we also did E&M, relativity, statistical physics and quantum physics, all from a rather idiosyncratic perspective.
>
> My recollection is that he didn't ask us for "all the equations" but for the ones we thought were most important and fundamental, the ones that succinctly sum up the known laws of physics. Otherwise, though, you have captured the moment pretty well, to the best of my recollection. The phrase "But the universe flies" had just the right tone of wonder, and there was a dramatic pause following, to let it sink in for a few seconds.
>
> The meeting sounds like fun. Sorry I can't be there.

30-01-06 *Locus of Free Will* (to H. Barnum)

In August 1999 you wrote me this:

Barnumism 15: *Ah! I have come to understand (or remember, perhaps) the importance of leaving a place for free will in your views on the foundations of qm. . . .*

Here's a caricature, so feel free to object: Bell's worry about the foundations of QM has been: that we have "measurement" as an "unanalyzed primitive" of the theory. Everett shows us how to get around that. You don't like Everett's resolution because you want *to have an unanalyzed primitive around so it can be the locus of free will.*

and I just thought of your writing it via our correspondence a few minutes ago in conjunction with a note I had written John Preskill a few minutes before that. I'll place the note to Preskill below. [See 30-01-06 note titled "Wheeler Accuracy?".]

Have you ever read the Wheeler story I refer to? Anyway, it struck me that maybe I'm still talking about that same locus!

30-01-06 *Island of Misfit Toys* (to K. T. McDonald)

McDonaldism 7: *Looking over your anti-Växjö note, I infer that I am much less concerned with the relation of human beings to quantum theory than you. So I doubt that I will be of much use to you as a sparring partner.*

Yeah, you're probably right.

McDonaldism 8 : *Instead, I'm led to infer that your view is that physics = psychology.*

I must be a horrible expositor if that is what has come through to you.

McDonaldism 9 : *Probability arises in situations involving people.*

No, it is that probability theory is as much prior to empirical science and the peculiar properties of the objective, physical, man-independent world (which I have no doubt is out there) as number theory and mathematical logic are. That is why we can practice probability theory (just as we can mathematical logic) without knowing anything necessarily of physics.

McDonaldism 10 : *People have lots of subjective impressions.*

They do, and I'm gaining one.

McDonaldism 11 : *Therefore, the notion of probability is probably subjective.*

Non sequitur.

The Bayesian view of probability, as developed for instance in

- E. T. Jaynes, *Probability Theory: The Logic of Science*, edited by G. L. Bretthorst (Cambridge University Press, Cambridge, 2003).
- J. M. Bernardo and A. F. M. Smith, *Bayesian Theory* (Wiley, Chichester, 1994).
- L. J. Savage, *The Foundations of Statistics* (Dover, New York, 1972).

is that one's *initial* probabilities (for anything) are largely beyond one's control. For they depend upon one's particular history, the accidents of one's learning, even the bad lessons one might have received in college. So, in that sense, probabilities are "subjective." Initial probabilities are like the initial conditions of a pendulum in a physics problem; their origins are left unanalyzed as a matter of principle. The best one can do is trace back the initial condition of a pendulum to the initial condition of some larger system (whose origin again goes unanalyzed), and so similarly with probability—the best one can do in analyzing the origin of some prior probability is to trace it back to some further prior (whose own origin then goes unanalyzed).

What is *not* subjective and not dependent on history in Bayesian probability theory, however, is how probabilities *should* fit together. As an example, if you gamble with odds $P(A)$ for some event A, and $P(B)$ for some event B, and gamble with odds $P(A \vee B)$ for the disjunction of A and B, and with $P(A \wedge B)$ for the conjunction of A and B, then it had better be the case that your numbers fit together according to this relation

$$P(A \wedge B) = P(A) + P(B) - P(A \vee B)$$

or you will lay yourself open to a sure loss. (This is called the Dutch book argument.) Thus it is the transformation rules between probabilities that are *objective*—the edict that one should strive to make sure that one's probability assignments meet the constraints of all Dutch book arguments is something that does not depend on the peculiarities of one's actual history. In that sense, the transformation rules are not "subjective," but rather "objective."

McDonaldism 12 : *A clarification. I have somehow (without being well informed) come to associate "Bayesian" with the notion that there is no such thing as intrinsic randomness in Nature.*

I hope that this term means something different to you.

Your association is the common one. A. J. M. Garrett, for instance, expresses it very eloquently,

> The nondeterministic character of quantum measurement can, and should, be taken to imply a deeper 'hidden variable' description of a system, which reproduces quantum theory when the unknown values of the variables are marginalised over. Differences in measurements on identically prepared systems then represent differences in the hidden variables of the systems. Not to seek the hidden variables, as the Copenhagen interpretation of quantum mechanics arbitrarily instructs, is to give up all hope of improvement in advance, and is contrary to the purposes of science.

But Caves, Schack, Appleby, Peres, Leifer, I (and I hope a few others) are renegades from that. If what you mean by "intrinsic randomness" is that quantum measurement outcomes do not preexist the measurement process—that they are made "on demand"—and that there is no fact of the matter in the universe that will determine which way they will go, then Caves, Schack, Appleby, Peres, Leifer, and I all accept that.

But that does not contradict the main Bayesian point, which I attempted to express in the section above: That initial probabilities are *subjective*, while the transformation rules and the way probabilities tie together are not—they are *objective*. Garrett-style hidden variables, regardless of what Garrett might say, are thus not necessary for a Bayesian view of quantum probabilities.

Which leads to this point:

McDonaldism 13: *But I am a bit worried by the quotation: "Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterizing how an individual should act in order to avoid certain kinds of undesirable behavioral inconsistencies."*

I interpret this as implying that Bayesian Statistics provides a kind of deprogramming for those misguided souls who take too seriously the claims that intrinsic probability plays a key role in Nature.

Quantum measurement is a "context of uncertainty"—and it is *because* of "intrinsic randomness," as defined idiosyncratically (but carefully) above. As far as I can tell, that has nothing to do with psychology—that is now a statement about our empirical world. But that has no implication that quantum probabilities have to be any more objective than Bayesian ones—they are still as much dependent on the gambling agent's peculiar history as any agent's probabilities in *any* context (because even the quantum states one ascribes to a system depend on unanalyzed prior probabilities).

What Bernardo and Smith are talking about in that quote is the *objective* content of Bayesian Statistics: It is that one should strive to be invincible to a Dutch book. (Not being invincible is what they mean by "undesirable behavioral inconsistencies.") The point that Caves, Schack, and I are attempting to develop is that the *transformation rules* of quantum mechanics *too* are of just such an *objective* flavor. That is, if a gambling agent bets this way on the outcomes of this measurement, and that way on the outcomes of that measurement, and so on and so on, for a complete or overcomplete set of quantum observables, and yet that gambling agent does not relate his probabilities in the way quantum mechanics prescribes (through the transformations between operators), then the world will be able to smite him down as certainly as any Dutch bookie can.

So, far from saying that physics is psychology, our Bayesian program is a careful attempt to ferret out the objective content of quantum mechanics at the same time as accepting "intrinsic randomness" (in the sense above). Only *part* of quantum mechanics is about the gambler-independent world, and that part still calls for a full identification. This task of separating the wheat from the chaff is the price one must pay if one accepts the idea that quantum *states* are of essence information—luckily it is a task that stands a chance of bearing fruit (and already has born fruit). To say 'every physical system is a quantum computer' adds nothing to existing physics—you would have been better to stick with the tautology that a physical system is a physical system, full stop. Using the terminology you do only anthropomorphizes things that shouldn't have been anthropomorphized in the first place. It erases the very distinction that one wants to make in order to get at an objective statement.

OK, enough.

I have strived to use the word peculiar as many times as possible in this note.

30-01-06 *Not Quite Enough* (to K. T. McDonald)

And I still need to reply to this:

McDonaldism 14: *As I sketch in my blurb, the issue as I see it is that the so-called classical world (with its unexplained long-lived bound states) is largely due to the quantum character of Nature. . . . The challenge is to understand better how the classical view arises out of the quantum world.*

It may be that in your way you are addressing this challenge. If so, I need a little more help from you to be able to follow your thinking on this.

Indeed, I think that is a valid and important question (the part I quoted at least). And, yeah, I do think that what Caves, Schack, the rest and I are doing is an important movement toward that goal. Or, at least, I hope so.

But there's no sense in attempting to say anything about our efforts along these lines unless/until I can first instill a little respect in you for the substructure that supports our approach. I.e., you have to have a little respect for the basics first—and I fear that we may not get there.

30-01-06 *Krugman on Reality* (to me)

From Paul Krugman, "A False Balance," *New York Times*, 30 January 2006:

> "How does one report the facts," asked Rob Corddry on *The Daily Show*, "when the facts themselves are biased?" He explained to Jon Stewart, who played straight man, that "facts in Iraq have an anti-Bush agenda," and therefore can't be reported.
>
> Mr. Corddry's parody of journalists who believe they must be "balanced" even when the truth isn't balanced continues, alas, to ring true. The most recent example is the peculiar determination of some news organizations to cast the scandal surrounding Jack Abramoff as "bipartisan."

30-01-06 *Exponentiation* (to K. T. McDonald)

In the words of Scarlett O'Hara, "Tomorrow is another day." A cooler head always prevails on the day after with me; maybe the process is already starting. It was just—and you should know it—that I found some of your choices of words insulting. I don't mind a skeptic raising doubt—being challenged to make our point of view consistent is what keeps the view moving along and making progress: You can find any number of notes in my archives with polite and patient explanations, and plenty of evidence that I find the challenges I receive to be very useful. But to use phrases like "what little I can reconstruct from your note . . . " and "I'm led to infer that your view is that physics = psychology"—you've got to admit—had the feel of throwing down a gauntlet. What physicist would not find it insulting that his work is seen as trying to say "physics = psychology"?

I'll let the issue rest, and actually thank you for firing me up: I did rather like some of the formulations I used in the longer note I sent you—they will be useful in the future, in a calmer context.

I look forward to seeing you again at the Wheeler meeting.

01-02-06 *Measurement Based Quantum Computation* (to H. Halvorson)

While I'm thinking of it (because of the grant proposal I happen to be working on), let me give you references to my three favorite papers on measurement-based quantum computation:

- Measurement-based quantum computation with cluster states
  Authors: R. Raussendorf, D. E. Browne, H. J. Briegel
  `http://www.arxiv.org/abs/quant-ph/0301052`

- Cluster-state quantum computation
  Author: Michael A. Nielsen
  `http://www.arxiv.org/abs/quant-ph/0504097`

- An introduction to measurement based quantum computation
  Author: Richard Jozsa
  `http://www.arxiv.org/abs/quant-ph/0508124`

That computational model strikes me as a veritable goldmine for exploring how the notion of "measurement" I've been talking to you about (i.e., action on a system, followed by unpredictable reaction in the agent) is given *meaning* in the workaday sense. Maybe a way to put it is: Quantum measurements don't "inform," rather they "enable." And I think the Raussendorf-Briegel computational model starts to give that slogan some precision.

01-02-06 *Enabling Alchemy* (to R. E. Slusher)

Below, let me place three notes for you to look over. The first one contains the slogan I just told you about. The second one (somewhat about alchemy—don't tell anyone) provides some background for why I would say something like that. The part that comes closest to a technical paragraph—and the part that you should take away in your memory—is the fourth paragraph from the bottom of it. Finally, in the third note (to van Fraassen), I expand on how one should *not* think of quantum measurements as informative of anything. [See 01-02-06 note "Measurement Based Quantum Computation" to H. Halvorson, 19-06-05 note "Philosopher's Stone" to G. L. Comer, and 14-11-05 note "Questions, Actions, Answers, & Consequences" to B. C. van Fraassen.]

Let me also attach one illustration of the idea (hand drawn of course).

03-02-06 *Red Spears* (to D. B. L. Baker)

It was the strangest thing to my ear when you called him Red in your note to me the other day. I hadn't heard him called that in so long—it kind of came back to me, "That's right; people used to call him Red Spears."[85]

Red passed away this morning, about an hour ago. It was during his sleep. A pretty good way to go in the end, I suppose.

08-02-06 *The Quotes* (to D. B. L. Baker)

Here's the quote that'll be going in the revised edition of the *American Heritage Dictionary of American Quotations* (though it has been bought by Oxford and will be renamed the *Oxford Dictionary blah blah blah*):

> There is no one way the world is because the world is still in creation, still being hammered out.

It originally appeared in one of my samizdats, then made its way into a *Scientific American* article by George Musser, and I guess this will be its final resting place. Actually, I'm very proud of this little accolade (probably more than any of the other ones in my career); I'm a little surprised I had not already told you about it. The fuller context of the quote is reproduced below.

It was so good seeing you again. Your company is one of the very few reasons I would ever want to come back to Texas—otherwise, the place is a kind of desert to me.

At the moment, I'm finally on a plane bound for Newark and waiting for my two little bottles of red wine. I was originally supposed to be home by 5:30 today; now I'm scheduled to arrive in Newark just at 10:00, where/when I'll have to catch a taxi home rather than be greeted by my family. [...]

OK, well that exhausts all the quotes I could find tonight. And it seems I'm not so good at holding my two little bottles of wine at 32,000 feet anymore. Maybe I'll drift into one of those reveries like old John Wheeler.

15-02-06 *Discussion* (to S. J. van Enk)

Yeah, I still feel awful; I don't know why. I've been in bed most of the day. It's a constant mild nausea, but no other symptom. I suppose it could be a stomach virus.

Anyway, you are right that in a certain sense, the only thing one ever *learns* (in the common sense of the word) from a "quantum measurement" are the consequences of one's actions. Another way to put it is that, strictly speaking, measurement is nothing other than the changing of a prior state to a posterior state—and the only means the quantum formalism gives for enacting such a change is when one takes an action (i.e., a "measurement") on a system.

To say that one learns something in the common sense (i.e., of becoming aware of which of a set of alternatives is already there), one has to give up treating a system (or part of a bipartite system) quantum mechanically. That is effectively what one is doing when one says that a sender "prepares" one state or another. If one had treated everything quantum mechanically, then there would simply be one quantum state in the picture (circumscribing the preparer and the smaller system), as you point out.

[85] My curriculum vitae presently includes this passage in it: "To the present date, I have given over 175 invited lectures and seminars. Beyond traveling through or over most states in the United States, this has allowed me the opportunity to visit Australia, Austria, Belgium, Canada, China, Denmark, England, Finland, France, Germany, Greece, Hungary, Ireland, Israel, Italy, Japan, Mexico, The Netherlands, New Zealand, Northern Ireland, Poland, Portugal, Scotland, South Africa, Spain, Sweden, Switzerland, and Wales. This is my tribute to my stepfather W. T. Spears, who would say, 'Chris, travel is the best form of education.'"

Have another look at the "clipping" of the note to van Fraassen that I sent you a little while ago. [See "Questions, Actions, Answers, and Consequences."]

Does any of this help your worry?

Steven's Preply

> If you're not too sick to read email, let me ask you a question about yesterday's discussion:
>
> We discussed only very briefly the case where someone, Alice, prepares some qubit A in a state ρ in a particular way (say, tossing a coin, and preparing $|0\rangle$ or $|1\rangle$ depending on the result of that coin toss). It seemed you were going to say then that a measurement by you on system A does teach you something, namely about the preparation.
>
> But I don't see how that would be consistent: after all, you might as well consider Alice + the coin + qubit A as one quantum system consisting of $N \gg 1$ qubits, and assigning as state (in oversimplified but obvious $N \gg 1$ form)
>
> $$|0\rangle^N + |1\rangle^N$$
>
> to Alice + coin + qubit A. But then you would say that you learn nothing from a measurement on A ...
>
> Isn't it consistent to say you never ever learn anything??
>
> And in particular, you created this email by reading it!

15-02-06 *Free Will* (to S. J. van Enk)

van Enkism 7: *I also thought of another paradoxical statement about counterfactuals: "If a counterfactual statement could be experimentally tested, it wouldn't be a counterfactual statement, would it?"*

I like that one.

On another subject, could I challenge you to write down in an email what you seemed to like in the discussion yesterday? I'd like to be able to look over it and absorb it better. You characterized how you thought we quantum Bayesians were saying something different than Bohr's position. Could you repeat that?

I also liked the way you said something like we always include the observer in the counterfactual. But I can't quite reconstruct what you really said.

Of myself, I also liked the little characterization I gave of how barring counterfactuals in the way we do is equivalent to the very idea that "measurements" are always generative of their outcomes. But just as I can't reconstruct what you said, I can't seem to reconstruct what I said either! Feel free to jump in if you've got a pithy way to say it.

It would be great if any of this clarifies the Hardy paradox for you. Might it finally be time for another van Enk – Fuchs paper?

Steven's Reply

> [CAF wrote:] *You characterized how you thought we quantum Bayesians were saying something different than Bohr's position. Could you repeat that?*

I thought that Bayesians think they get out of paradoxes related to Bell inequalities etc. by the observation that

> 1) Alice's measurement changes something about *her* description of Bob's state, but nothing for Bob's description

True, that leaves nothing nonlocal but it avoids the "real" paradox which occurs when Alice and Bob get together, compare their measurement results and wonder what the heck they would have found had they chosen different measurements. And

> 2) Both Bohr and Bayesians say: but quantum mechanics gives you all the probabilities you need for actually performed measurements, and you should not even ask the question what would have happened if something else had been measured.

So, again, what I like about yesterday's discussion is that now you *do* say something very clear about what would have happened if you had measured something else: If you assign ρ to some system, and measure E_a, with result a_0, what would you have found had you measured F_b instead? Answer: You ignore the result a_0 and assign probabilities $p(b) = \mathrm{tr}(\rho F_b)$.

So, I guess that disproves my impression about what Bayesians say, but I still would guess that Bohr, if he were still alive [yep, another one!] would still insist on not giving an answer to counterfactual questions.

> [CAF wrote:] *I also liked the way you said something like we always include the observer in the counterfactual. But I can't quite reconstruct what you really said.*

Darn, I tried many different ways, but I can't say clearly what I meant! I'll think about it more.

> [CAF wrote:] *Of myself, I also liked the little characterization I gave of how barring counterfactuals in the way we do is equivalent to the very idea that "measurements" are always generative of their outcomes. But just as I can't reconstruct what you said, I can't seem to reconstruct what I said either! Feel free to jump in if you've got a pithy way to say it.*

I agree you said that, and I think I agree with the content. Isn't it this: Suppose a measurement does reveal some preexisting property of a system. Then my counterfactual statements about that system would be affected by that knowledge. In particular, had I done a very similar measurement on the system, then I would have found a very similar measurement result. And conversely, if a measurement generates the outcome, then if I had done a slightly different measurement, the outcome could have been very different, since it is as if you're doing a new measurement which will generate a new outcome from scratch. Anyway, obviously that's not the way you said it yesterday but it's how I understand it now.

> [CAF wrote:] *It would be great if any of this clarifies the Hardy paradox for you.*

I still think it does! The paradox arises there only if you consider 2 counterfactuals: if both Alice and Bob had done a different measurement then they would have found

1. something that will never occur in an experiment, if you use the "standard" rule for counterfactuals
2. exactly what will occur in an experiment if you use the new great rule for counterfactuals.

And again, the only change I need to get rid of the paradox is change how I answer a counterfactual question!

> [CAF wrote:] *Might it finally be time for another van Enk – Fuchs paper?*

Yes! But only if it does not fall in the same category as "Entanglement is super ... but not superluminal!"

16-02-06 *Back from the Silence* (to H. C. von Baeyer)

Thanks for the notes of 2/1 and 2/6. I'm sorry to take so long to write back to you: My stepdad became quite ill in late January and finally passed away February 3. Between trips to Texas and all else, I got quite behind.

First off, I'm glad to hear that your AAPT talk went well! I wish I could have seen it. And I like the idea of this B3 diagram—it's good to have a trademark; it focuses people's attention.

Second off, thanks for showing me your first installment in the translation project! I read it on the plane back from Texas the other day; so I should have just written you then, but I guess I was too exhausted. I like your system for the endnotes. One thing: I detected a couple of typos. How should we handle things like this in the future? Would you like me to write a short note indicating them, or would you like me to simply insert the corrections directly into your Word file and send it back? I could flag them by writing them in boldface or red or something.

Finally, let me apologize to you for a bad oversight on my part. Marlan Scully and I have organized a little meeting at Princeton next Friday and Saturday in honor of John Wheeler. In my invitations, I only approached old (quantum-oriented) students of John and Princeton locals for giving talks. But I should have thought of you too, at least for participation! It's probably too late for you now, but if you're interested, you're certainly welcome to come. I'll attach the meeting program so you can get a feel for what it'll be like. John himself, too, will make an appearance — either for the first two or three hours Friday morning, or for the dinner Friday evening, we don't know which yet. If I'm not mistaken, he'll soon turn 95. John is very frail now, and almost completely deaf, but Ken Ford has said that he will be able to understand that the meeting is in honor of him, and he will recognize some faces. If you're interested, let me know and I'll send you all the logistical information.

Within my "Pauli Project" folder on my computer, I have now started a subfolder titled "von Baeyer" to hold your translations and the like. I can imagine my hard drive, if it could talk, saying, "Now that's some comfortable real estate in my landscape."

Hans's Preply, "AAPT Conference Talk," 01-02-06

> Here is an unanticipated fallout from my Anchorage lecture. I am heartened to see the Bayesian view taking hold in Europe and Australia (is New Zealand part of that continent?), and also in industry and the elementary physics lab.
>
> The "B3 triad" is a triangle with the words psi, information, and probability at its vertices, and the names Bayes, Born, and Bohr, respectively, along their opposite sides.

My talk was well received by many, including my old friend Stuewer, who mentioned you.

Hans's Preply, "First Installment," 06-02-06

Here is the first instalment of a translation. You can regard it as a finger exercise of the kind piano students play. I don't know yet what will develop.

Endnotes are labeled E, A, and T, for editor, author, and translator, respectively. Letters are numbered in square brackets [. . .]. References to the book's extensive bibliography are in curly brackets {...}.

[1286] Pauli to Fierz.

Zurich, 3 October 1951

Dear Mr. Fierz!

I have now read your essay[86] and thank you also for the written addendum. Of course the psychology of the unitary versus dualist theories of electricity were very interesting to me, representing, as it does, a special case of the psychology of the development of scientific theories (since the empirical facts to be explained were the same for both theories.) Furthermore I was particularly interested in your written addendum about trinitarian and quaternarian scholars (the latter are sometimes non-thinkers), where you count Spinoza and Leibniz among the former, Voltaire and Kant among the latter. (Your positive attitude toward Voltaire is very congenial to me.)

I came upon Kepler as trinitarian, and upon *Fludd* as quaternarian – and felt inside myself, with respect to their polemics, the resonance of an inner conflict. I have certain traits from both, but now, in the second half of my life, I ought to change over to the quaternarian position. The problem is that the positive values of the trinitarian position must not be sacrificed in the process. (Mr. Panofsky in Princeton, who loves wordplay and bad puns, once wrote to me so amusingly: "Today you can no longer pour out the Kepler with the Fluctibus[87]!")[88]

By the way, I would like to remark that once upon a time (in Hamburg) my route to the exclusion principle was concerned with the difficult transition from 3 to 4: namely with the necessity to ascribe to the electron instead of *three* translations another *fourth* degree of freedom (which soon thereafter was explained as "spin".) To wrestle my way to the realization that, contrary to the naive "view", the fourth quantum number is also a property of one and the same electron (just like the known *three* quantum numbers now called n, l, and m) — this was really the *principal effort.* (I had to fight so hard against the then current theories that had ascribed the fourth quantum number to the *rest of the atom* or core).

When I began to work on Kepler's trinitarian point of view, I did not yet know anything about Fludd's polemic, and even less that the quaternity hat

[86]E1. Fierz had sent Pauli his essay *The development of the science of electricity as an example of physical theory making*, which had been published as a brochure by the University of Basel {1951b}.

[87]T1. Latin for Flood.

[88]E2. Pauli's remark in letter [1278] to Panofsky.

such an essential symbolic meaning for Fludd (the relevant longer passage is repeated and translated in my essay) — I only knew that for Kepler the Pythagorean tetraktys, which he knew well, had *no* symbolic meaning.[89] In this way, by following a psychological line, I happened *again* upon the problem of the transition from 3 to 4. In both cases Mr. C. G. Jung had certainly not suggested this to me, nor did I have the intention from the start to wrestle with the problem 3 and 4, of all things.

Therefore I am pretty sure that *objectively* an important psychological problem, and perhaps one of natural philosophy, is related to these numbers.

In general I am completely in agreement with everything you said. The question is, however, whether one should *add* several things — perhaps on another occasion when you want to publish this matter. (I have the feeling that in that case the work should be longer.)

For example, one should note and explain that Dirac's hole theory is a not entirely successful new attempt to interpret the relativistic quantum mechanics of the electron in the sense of a unitarian theory of electricity.

Then on to the problem of opposites: in physics there are *compensatory pairs of opposites* (represented by positive and negative quantities like the two electrical charges) and *complementary pairs of opposites* (represented by non-commuting quantities like p and q).[90] This is a very important distinction, because it my personal conviction that it has a counterpart in the realm of psychology: A compensatory pair of opposites seems to me – as it does to you – to be good and evil, a complementary pair, on the other hand, conscious – unconscious[91] (in Chinese the corresponding pair is Yang and Yin.) In my opinion analytical psychology is hitherto suffering severely from the absence of this distinction (which is related to the insufficient mathematical-scientific training of its representatives.) (See below.) I would be very glad if, in your essay, you insisted pedantically on this conceptual differentiation.

This matter brings me to another subject that is also treated in your essay: The fundamental difficulty of the "field" concept. On page 14 you very beautifully describe the *assumption* of the reality of the field: "But Faraday thought that the field had to be there, whether we prove it or not, just as we believe that the moon is there, whether we look at it or not." One might add: "just as we assume that the motion of the moon is the same whether we look at it or not" (which goes far beyond its mere existence.)[92] This is of course the cloven hoof itself, both with respect to physics (*quantum* field theory) and to the psychological analogy.

I have a slightly different opinion from you in that I do not ascribe the same significance to the impossibility of empty space in quantum field theory as you do. In my opinion the cloven hoof remains *exactly the same* in quan-

[89]E3. In his later letters [1383] and writings Pauli often worked on the special role of the tetraktys or quaternity which the Pythagoreans had already emphasized. This interest had been awakened by Jung, who pointed in his 1937 *Terry Lectures on Psychology and Religion* {1940, chapter 2} to the psychological meaning of the number four, or quaternity, which appeared also in Pauli's dreams.

[90]E4. Cf. the preceding commentary to letter [1286].

[91]T2. I use the word "unconscious" rather than "subconscious" to preserve the German distinction between "Unbewusstsein" and "Unterbewusstsein".

[92]A1. I might call this "the classicistic idea of objective reality in the universe."

tum field theory as in the unquantised classical theory: A field without the test bodies required for its measurement should not even be *thinkable* according to mathematics and logic. Actually, however, in today's theory things stand as follows: if you put $e = 0$, and thus describe light fields, then these are, regardless of whether they are classical fields or photons, mathematically possible *without* charges; if one puts $e \neq 0$ and describes electrons, positrons, or photons (Schwinger), then these are mathematically possible *without* the heavy masses in the measurement devices that are necessary in order to measure fields or charge densities in small spaces (of order $h/2\pi mc$, $m =$ electron mass). As for the true complementarity relation between the possibility of regarding *the same* physical objects as fields or test bodies (measuring devices) (the former when *other* objects function as measuring devices), this choice is *not* expressed in today's formalism. (N.B. What good is it to me that *no* empty space is possible?)

Now to switch to the psychological analogy of the physical field concept, it seems to me to reside in the concept of the *unconscious.* The latter cropped up, and was applied practically, approximately simultaneously with the former. The "unconscious" also "assumes" a reality — in particular, just as in the physical field, an *invisible reality* (in the sense of everyday life) *which mediates a connection between spatially (and perhaps also temporally) distant events.* For this reason it seems to me that there is here a more profound similarity than a mere analogy. Consciousness corresponds to the test bodies. And this conceptual correspondence between field and unconscious extends further to the fact that both exhibit the corresponding cloven hoof. The concept of the unconscious was initially also used in analytical psychology as though one could observe it without changing it. And although its representatives occasionally admit the opposite, many of their assertions about the unconscious are still much too close to what I called above "the classicistic idea of objective reality in the universe." This seems to me to be especially the case when C. G. Jung tries to find law-like assertions about the sequence of "archetypes" (he calls this the "dynamics of the self")[93] — which are supposed to be valid regardless of the intervention of human consciousness. These statements remind me in their type too much of Maxwell's classical equations. An analogy to the quantum mechanical-complementary description does not exist yet, which seems to correspond to the absence of a satisfactory quantum field theory.

This last paragraph is my personal opinion and probably not yet sufficiently demonstrable to be publishable. But the correspondences between psychological and quantum mechanical concepts in general (I mention similarity, acausality, experimental set-up, correspondence, pairs of opposites, and holism – these concepts are used in both disciplines, although they were formulated independently of each other) — this correspondence is too striking not to believe in a more profound meaning of their *simultaneous* appearance in the history of ideas.

And that is exactly what your essay wanted to show, using different ex-

[93]E5. In the chapter XIV of his new book *Aion*, which Pauli had mentioned in his preceding letter [1285] to M.-L. von Franz, Jung dealt with the *Structure and Dynamics of the Self.*

amples from different times.

I will be very interested in hearing your opinion of my ideas!

Once more, many thanks for last Sunday at noon, and greetings to you and your wife.[94]

Editorial comment on letter [1286] from Pauli to Fierz, 3 October 1951

The significance for the history of ideas of the problems of opposites, such as light-dark, warm-cold, one-many [1209, 1212], male-female [1391], good-evil [1291, 1364, 1373], idealism-materialism [1395], physical-psychic [1291, attachment], matter-form [1363, 1391], chance and necessity [1497], causal and acausal [1388], as well as their central role in Jung's psychology, henceforth begin to occupy Pauli more and more.[95] Jung considered the *Self*, which included the conscious as well as the unconscious psyche, as the center of the spirit (*das seelische Zentrum*). It was made manifest by a unification of opposites (*coniunctio*) which "joins the temporal with the eternal and the specific with the most general." [96]

The parallel appearance of such pairs of opposites in modern physics was for Pauli another hint of the profound relationship which he assumed to exist between physics and psychology [1179, 1180]. As Pauli said in his radio lecture on the occasion of the centenary celebration of Columbia University in 1953,[97] the biggest impression upon him was made precisely by the fact that "in physics there are real pairs of opposites, such as particle v. wave, location v. momentum, energy v. time, whose contradiction can only be resolved in a symmetrical way." The special property of these novel pairs of opposites was that "one partner is never eliminated in favor of the other, but both are taken over into a new kind of physical law, which expresses the complementary character of the pair in an appropriate way."[98]

His interest as physicist was directed primarily toward the psychology of the creation of scientific concepts.[99] He paid particular attention to the development of the concept of matter, which was created in analogy with the doctrine of *privatio boni* in Platonism, which Pauli had aptly called the *Hole Theory of Evil* [1029,1278], after Dirac's hole theory. Since Evil is defined here by the absence of the Good, matter, which is governed by a dark principle, is supposed to be explained by a deficiency of the spiritual [1283].[100] A more positive expression was given to the concept of matter by Aristotle and his successors by calling matter, or *hyle*, something which actually existed because

[94] E6. Pauli had evidently been invited to lunch by Fierz.

[95] E1. The problem of pairs of opposites, and their resolution (by unification or conjunction), for which Pauli mentioned quantum mechanics as physical paradigm, also plays a central role in Pauli's later letters to Jung. Cf., for example, Pauli's letter to Jung dated 27 February 1953 [1526].

[96] E2. Jung, *Alchemie und Psychologie*, 1972, p. 34.

[97] T1. Actually, the year was 1954, and the celebration was the bicentennial.

[98] E3. Cf. Pauli {1961/84, p. 8}.

[99] E4. Cf. Pauli's *Bemerkungen zur Psychologie der naturwissenschaftlichen Begriffsbildung* in the biographical collection PLC Bi 67 in his literary estate. Cf. also von Meyenn {1994, pp. 18–23}.

[100] T2. In this 1951 letter to Panofsky, Pauli writes: "The Christian theology of the '*summum bonum*' and the '*privatio boni*' is both absurd and historically unique. To call the entire *material* world 'evil' is consistent; but to define the material as an 'absence of the spiritual' would be absurd."

it was possible, and thus accessible to conceptual thinking.[101]

According to Pauli's opinion, the negative assessment of everything material, by its commingling with the ethical pair of opposites good-evil[102], thus entered into the European history of ideas, and through Fludd and the Platonism of the Renaissance permeates all Western thought.[103]

However, compared to the doctrine of the *privatio boni*, Pauli was more impressed by the more symmetrical 'conceptions of the whole' of the gnostics [1270] and by the ideas of Plotinus about the 'problem of the one v. the many' [1236, 1278]. These ideas anticipated certain aspects of Bohr's notion of complementarity.

In the letter to Fierz below [1286] Pauli seeks to clarify even further the difference between the two conceptions, by differentiating between *compensatory* (or polar) and *complementary* pairs of opposites.[104] As paradigms of complementary opposites he considers of course the non-commuting quantities p and q of quantum mechanics, which have already been mentioned. Compensatory opposites, on the other hand, are quantities that cancel each other when they meet. Pauli points out good-evil and conscious-unconscious as their psychological counterparts.

But what Pauli was striving for in these thoughts about pairs of opposites was the far more significant effort at a *comprehensive coniunctio (umfassende Coniunctio)*, as he put it in a commentary on a dream he had had on 20 December 1952 on the occasion of his visit to Bombay.[105] "I want to try," he explained there, "to say somewhat more clearly what I really wanted to say in the last part of my Kepler essay[106]: A firm grip on the *tail*, i.e. physics, provides me with unexpected means which may perhaps be useful in the larger enterprise, *to grasp the head*. For it seems to me that the *complementarity in physics*, with its solution of *wave-particle duality*, is a kind of *model or paradigm for this other, more comprehensive coniunctio*. The smaller *coniunctio* in terms of physics, the quantum or wave mechanics constructed by physicists, shows, quite without the intent of its inventors, certain characteristics which might turn out to be useful in the resolution of other pairs of opposites."

To this, Pauli further added the following epistemological explanations: "By admitting events and making use of possibilities which can no longer be considered pre-determined and existing independently of the observer, the quantum mechanical manner of explaining nature comes into conflict with the old ontology, which was able to state simply: *Physics is the description of reality* (words of *Einstein*), in contrast to, say, *the description of that which one merely imagines* (words of *Einstein*). *Being* and *Non-being* are not unique characterizations of properties which can only be checked by statistical series of experiments with different set-ups, which may, in certain cases, exclude

[101]E5. The Aristotelian concept of *hyle* was later translated into the Latin *materia*. Cf. also Happ {1971}.

[102]E6. Cf. remarks in the letters [1354, 1357, 1362–1364, 1366, 1373, 1376, 1391, and 1396].

[103]E7. Cf. the attachment to letter [1328]. Pauli also presented his ideas on the development of the concept of matter in his "*sermon*" to the *International Conference of Scholars* in Mainz, March 1955. Cf. Pauli {1961/84}.

[104]E8. Pauli had already mentioned this subject in 1948 in his *Background Physics*. Cf. Meier {1992, p. 182}.

[105]E9. Cf. also the comment on Letter [1489].

[106]E10. Pauli {1952, p. 155}.

each other."

Hans's Reply, 16-02-06

Thanks for the nice email and your kind invitation. Your attachment is ominously labeled "newsletter blurb... doc.suspect" and my computer haughtily rejects it. But no matter — I cannot get away on this short notice. I wish you and your friends, including John Wheeler, a wonderful weekend. I happened to sit next to Ken Ford at the Anchorage banquet.

The comment and Pauli letter are background for Fierz's reply, which is so important that it comes in multiple drafts – my next instalment. I am grateful for all corrections, and would prefer a little email note about them, so I can make sure we are on the same page. I am now in correspondence with Atmansbacher, and will probably visit him in the summer. Furthermore, Roger Stuewer told me that one of his students had written a good thesis about Pauli's mysticism (my phrase), but has not emailed about it yet. I must remind him.

Hans's Reply, "Trivium," 08-03-06

I have been sick in bed and unable to translate, but things are better now. I am reading a good thesis by John Gustafson, Roger Stuewer's student, claiming that contrary to conventional scholarship, Pauli became "Jungian" well before he met Jung. Here is a bit I thought you might enjoy:

> There would have been many good reasons for Pauli's father to leave Prague, to seek his fortune in nearby Vienna, to leave his family religion, and to embrace the Roman Catholic faith. The Pascheles home at No. 7 on Prague's Old Town Square (Altstädter Ring), had been a Paulan convent earlier, which may have been the reason he changed the family name to Pauli.

Did you learn anything, or teach anything, at the Wheelerfest?

16-02-06 *Synchronicity* (to H. C. von Baeyer)

von Baeyerism 14: *Cheers from sunny Williamsburg, where the total number of snowflakes during the recent storm was 17.*

Interesting number, since it turns out that throughout my yard I fairly evenly measured 17 inches. First Fierz and Pauli, and now the two of us!

16-02-06 *Many Worlds Does Not Explain Much* (to B. C. van Fraassen)

Thanks for sending your notes. I enjoyed reading them (twice actually), and I think I get your point. I don't know that I have much to add at the moment by way of endorsement or, alternatively, criticism, but your ideas are tumbling around in my head—maybe something will eventually emerge.

Let me only point out a couple of connections that you evoked in me. First, I liked the way you put this:

> In retort it will perhaps be suspected that I yearn for a classical understanding of the world, if I'm not willing to count the 'many worlds' answer as explanatory. Quite the contrary, I would say: as I see it, quantum theory does leave unexplained why the actual outcome of a measurement is this rather than that one among the possible outcomes. Yearning for a classical understanding means yearning for an explanation of all that this theory leaves – and legitimately leaves – unexplained.

Its tone reminded me mildly of something David Mermin said in his lecture notes for his quantum computing course:

> There are nevertheless some who believe that all the amplitudes α_x have acquired the status of objective physical quantities, inaccessible though those quantities may be. Such people then wonder how that vast number of high-precision calculations (10^{30} different amplitudes if you have 100 Qbits) could all have been physically implemented. Those who ask such questions like to provide sensational but fundamentally silly answers involving vast numbers of parallel universes, invoking a point of view known as the many worlds interpretation of quantum mechanics. My own opinion is that, imaginative as this vision may appear, it is symptomatic of a lack of a much more subtle kind of imagination, which can grasp the exquisite distinction between quantum states and objective physical properties that quantum physics has forced upon us.

[Actually the source of this quote is `quant-ph/0207118`.]

The other thing you evoked in me was a memory of an article by Markus Fierz that I reprinted in my *Notes on a Paulian Idea* with the title: "Does a physical theory comprehend an 'objective, real, single process'?" I read it again, between my two readings of your note. I think you too will enjoy the piece, and particularly near the end of it, see some similarity to what you have written in your paragraph above. Tell me if I am on the money? For your convenience, I'll paste in the whole article below; I hope the remnants of LaTeX code in it won't bother you too much.

Finally let me remark on these words of yours:

> A beautiful program for the interpretation of quantum mechanics has lately received new life as new results, new techniques, and other approaches have been mined to aid in its elaboration. The excitement in such work is its own reward.

My own feeling is that all their purported progress is illusory. It is a lot of technical-looking huffing and puffing (which builds a shield of seeming protection), but in the end doesn't amount to much. At a crucial point they make the identification "weight = probability" for no other reason than that they know that's what they have to do to get the answer they want. Here's the way my friend Howard Barnum put it recently:

> The basic idea is that the Wallace argument is just some version of a Laplace's symmetry principle argument in another guise ... and that I think all these arguments have things backwards: our belief that there is a "physical symmetry" just about IS the invariance of our preferences under the relevant transformation ...

Basically, I say beware of giving the New Everettians too much credit!

16-02-06 *Wheelerfest Program, User-Friendly Version* (to the Wheelerfest participants)

Dear Wheelerfesters,

Many of you have had trouble opening the MS Word document I sent out previously with the program for our upcoming meeting. Thus, I have cut and paste the information directly into the present email. The formatting is not quite as nice as it was before, but at least you can (without doubt) read the information this way.

Please note that there have been some changes to a couple of titles, and the final session has been shortened by one talk.

Wheelerfest: Princeton University

Friday, February 24, 2006

8:45-9:00 C. A. Fuchs and M. O. Scully
Welcoming Remarks

Delayed Choice and Quantum Eraser

09:00-09:30 C. O. Alley
"Time For Choice Without Further Delay: 'Deputy-General Relativity' (Professor Wheeler's name for the Yilmaz Theory) Must Now Take Command"

09:30-10:00 M. S. Zubairy
"Time and Quantum: Quantum Eraser"

10:00-10:30 Y. Shih
"A Random Delayed Choice Quantum Eraser"

10:30-11:00 Coffee

Foundations of Quantum Mechanics, I

11:00-11:30 L. Hardy
"Quantum Foundations and Quantum Gravity"

11:30-12:00 S. Kochen
"It from Bit: Reconstructing the Quantum Formalism from Qubits"

12:00-13:30 Lunch Break

Entanglement and Quantum Mechanics

13:30-14:00 W. P. Schleich
"Entanglement and the Riemann Zeta Function"

14:00-14:30 M. Hillery
"Programmable Quantum Circuits"

14:30-15:00 W. K. Wootters
"How Come Phase Space?"

15:00-15:30 Coffee

Foundations of Quantum Mechanics, II

15:30-16:00 B. Schumacher
"The Physics of Impossible Things"

16:00-16:30 J. Conway
"The Free Will Theorem"

16:30-17:00 E. Nelson
"Two Defects of Stochastic Mechanics"

Saturday, February 25, 2006

EPR-Bell

09:00-09:30 M. O. Scully
"Do EPR-Bell Correlations Require a Nonlocal Interpretation of Quantum Mechanics?"

09:30-10:00 W. G. Unruh
"Bell Inequalities and Nonlocality"

10:00-10:30 D. M. Greenberger
"A Bell Theorem for Two Particles - No Inequalities, Inefficient Detectors"

10:30-11:00 Coffee

Understanding Quantum Mechanics

11:00-11:30 C. A. Fuchs
"The Equations of Quantum Mechanics Already Do Fly"

11:30-12:00 H. Halvorson
"Deriving Quantum Mechanics from Information Theory: The CBH Theorem"

12:00-13:30 Lunch Break

Quantum Information

13:30-14:00 R. Seiringer
"Some Refinements of Strong Subadditivity of Quantum Entropy and Their Applications to Statistical Mechanics"

14:00-14:30 S. A. Lyon
"Low-Decoherence Electron Spin Systems for Quantum Computing"

14:30-15:00 K. T. McDonald
"Reflections of a (Skeptical) Experimental High-Energy Physicist after Teaching a Course on Quantum Computation"

Posters: [Posters will be displayed throughout Feb. 24, 2006 outside the conference hall.]

- T. Di: "Quantum teleportation of an arbitrary superposition of atomic Dicke states"

- N. Erez: "Bohm Trajectories: Realistic or Surrealistic?"
- A. Jordan: "Fluctuations in Bose-Einstein Condensation, a path integral approach"
- M. Kim: "Thermodynamic quantities in BEC"
- A. Muthukrishnan: "Precision phase measurement as a global quantum Fourier search"
- R. Ooi: "An intense source of large non-classical two-photon correlation"
- A. Patnaik: "A new method to measure coherence dephasing via Raman photon correlation"
- Y. Rostovtsev: "Bunching and anti-bunching of photons in coherently prepared media."
- A. Svidzinsky: "Fluctuations in Bose-Einstein condensate"
- H. Xiong: "From correlated spontaneous emission laser to an entanglement amplifier"
- L. Zhou: "Generation of two-mode squeeze state and coherent-squeeze state in cavity QED system"

Banquet will be held in the evening of Feb. 24, 2006 at the Conference Center at 6:00 pm.

17-02-06 *Free Will Again* (to S. Kochen)

Now that I've got a few bureaucratic burdens off me, I'm hoping to finally get a chance to skim this free-will manuscript you gave me over the weekend. Free will at last! Maybe we can talk about this a bit Monday.

I'm not sure you got this impression from me when we were talking a few weeks back, but I think this theorem may be deeply important for the Bayesian tack on quantum probability that Caves, Schack and I want to develop. And your partial Boolean algebra view may not be so unrelated to our trains of thought either. The paragraphs below taken from my pseudo-paper `http://www.arxiv.org/abs/quant-ph/0204146`, I think, give some decent indication of how seriously I take this "measurement context dependence" view. Also, I'll excerpt from a recent email to Kirk McDonald on this subject.

Let me give you a couple of other pointers to our work. I think these are the best things we've put together yet on our quantum foundational ideas:

- "Quantum Mechanics as Quantum Information (and only a little more)"
  `http://www.arxiv.org/abs/quant-ph/0205039`
- "Unknown Quantum States and Operations, a Bayesian View"
  `http://www.arxiv.org/abs/quant-ph/0404156`

See you Monday. Quotes below; maybe this will give us a little more to talk about.

17-02-06 *Incompletely Knowable vs 'Truth in the Making'* (to W. G. Demopoulos)

Now I have to apologize to you again for a long silence: Soon after the new year my stepfather became very ill, finally passing away a couple of weeks ago. It has been very tough on the family, and I am only now catching myself back up.

However, don't think that throughout all that, your ideas have not been on my mind. Indeed I enjoyed another re-reading of your 'incompletely knowable domain' paper—it lifted me on my sad flight back from Texas—and I tried to think hard about whether there really is a substantial distinction between us or not.

Let me try to consider a situation and 1) try to imagine what you would say of it (but probably in my idiosyncratic language), followed by 2) what I think I would say of it ... and then see if there is a substantial distinction.

Start with a finite dimensional Hilbert space, say of dimension 3, and imagine it indicative of some real physical system within an observer's concern. From that Hilbert space, let us form all possible sets of three mutually orthogonal one-dimensional projection operators. That is, let us consider all possible sets of the form $\{P_1, P_2, P_3\}$.

What is it that you would say of those sets? If I understand you correctly, it is this. Each such set $\{P_1, P_2, P_3\}$ corresponds to a set of mutually exclusive properties that the system can possess. At any given time, one of those projectors will have a truth value 1 and the other two will have values 0. Now consider a potentially different such set $\{Q_1, Q_2, Q_3\}$; again, at any given time, one of those projectors will have a truth value 1 and the other two will values 0. What is interesting in your conception, if I understand it, is that even if two elements happen to be identified between those two sets—for instance, if $P_1 = Q_3$—there is *no requirement* that P_1 and Q_3 need have the same truth value; P_1 might have the truth value 0, whereas Q_3 might have the truth value 1. Another way to say this is that the truth-value assignments depend upon the whole set and not simply the individual projection operators. For you, all the identification $P_1 = Q_3$ amounts to is that the *probability* for the truth value of P_1 within the set $\{P_1, P_2, P_3\}$ is the *same* as the *probability* for the truth value of Q_3 within the set $\{Q_1, Q_2, Q_3\}$. (If you were a Bayesian about probabilities—though I don't think you are—you would say, "Well P_1 has whatever truth value it does, and Q_3 has whatever truth value it does (each within their appropriate set of mutually exclusive triples), but my degree of belief about the truth value of P_1 is the same as my degree of belief about the truth value of Q_3. That is the rule I am going to live by.") Then it follows from Gleason's theorem that there exist no probability assignments for the complete (i.e., continuously infinite) set of triples that are not of the quantum mechanical form. In particular, one can never sharpen one's knowledge to a delta function assignment for *each* triple. This is how you cash out the idea of an 'incompletely knowable domain.'

That is a novel idea, and if I understand it correctly, I like it.

However, now let me contrast my characterization of you with what I think has been my working conception. I prefer not to think of the triples $\{P_1, P_2, P_3\}$ as sets of mutually exclusive *properties* inherent within the system all by itself, but rather *actions* that can be taken upon the system by an external *agent*. Each *set* of such projectors corresponds to a distinct action; what the individual elements within each set represent are the (generally unpredictable) *consequences* of that action. What are the consequences in operational terms? Distinct sensations within the agent. The reason I insist on calling them consequences, rather than "sensations" full stop, is because I want to make it clear that the domain of what we are talking about is sensations that come about through the action of an agent *upon* the external world.

The essential idea of the sexual interpretation of quantum mechanics is that no element of a set $\{P_1, P_2, P_3\}$ has a truth value before the action of the agent. Rather the truth value—if you want to call it that (maybe it is not the best terminology)—is generated (or given birth to) in the process. At the point, one of the P_i stands in autonomous existence (within the agent), whereas the other two fall.

I hope I have characterized both of us accurately!

Here is the question that has been troubling me. Is there any real distinction (one that makes an pragmatic difference) between our views? You say the truth value is there and revealed by the measurement, and I say it's made by the measurement and wasn't there beforehand. So what?

If there is a pragmatic distinction, Steven van Enk and I through discussions this week have come to believe that it may show up most clearly in how you and I would treat counterfactuals with regard to measurement. Let us take a situation where an agent ascribes a quantum state ρ to the system; contemplating the measurement $\{P_1, P_2, P_3\}$, we know that he will ascribe probabilities according to the Born rule $\mathrm{tr}(\rho P_i)$ for the various outcomes. Suppose he now performs that measurement and actually gets value P_2.

What does getting that outcome teach him about the quantum system? I think you would say it reveals which of the three mutually exclusive properties the system actually had. On the other hand, I would say it teaches him nothing about the system per se; the outcome P_2 is just the consequence of his action. What is the implication of this on counterfactuals? Here's at least one.

Suppose after you get your outcome, you contemplate magically having performed a distinct measurement $\{Q_1, Q_2, Q_3\}$ instead. I think you're careful to point out in your paper that the knowledge of P_2 carries no implication for what you would have found with this other imaginary measurement. But what happens if you conceptually transform this measurement $\{Q_1, Q_2, Q_3\}$ to one closer and closer to the original, i.e., to $\{P_1, P_2, P_3\}$? In the *limit* when the two are identical again, I think you would say that knowledge of the outcome P_2 in the original case implies that P_2 will also be the outcome in the limiting counterfactual case. But what would I say? From my conception, there is no reason at all to believe that the limiting counterfactual case will give rise to the same outcome P_2. The best one can do, either in the original case or the counterfactual case, is to say that an outcome i will arise with probability $\mathrm{tr}(\rho P_i)$. In fact, a counterfactual analysis with this kind of result may be the very meaning of the idea that quantum measurements are generative of their outcomes.

At least that is a potential distinction Steven and I see at the moment. We are toying with the idea that this may have some implications on the analysis of Hardy-type paradoxes, etc., where counterfactuals abound, and if anything comes of that, we'll let you know. In the meantime, would you say that we have given you a fair characterization?

17-02-06 *Dutch Book Presentation!* (to R. Schack)

In that talk, I'm going to try to say some of those things I told Kirk McDonald when I got pissed off. I think our project when we get together in Sweden ought to be to try to make some of this business rigorous: What is this extra "coherence" that quantum mechanics forces upon us significatory of? The more I think about it, that has to be the content of the Born rule—that our probability assignments for seemingly distinct measurements are linearly related.

Before that of course, we have to finish the certainty paper …

Let me hope I survive this week and next. (Monday I give the Princeton applied mathematics colloquium; Friday and Saturday are the Wheelerfest; and in the middle there is certainty, certainty, certainty.)

18-02-06 *Two Questions* (to W. G. Demopoulos)

Here are two questions that came up in the discussions with Steven. I probably won't have a chance to think about your answers too deeply until the week after next (I've got to give the Applied Maths Colloquium at Princeton and then run the Wheelerfest there Thursday and Friday ... and I've got a million things to do for both). But let me throw the questions on the table anyway.

1) Almost by definition, what you are proposing is a contextual hidden variable theory. But what is its status with regard to locality? At different times (while driving, taking a shower, etc.), I've been able to convince myself that a constraint of *locality* can be placed upon the truth values, but then I get confused. What can you say on the matter?

2) Take two triads of one dimensional projectors, $\{P_1, P_2, P_3\}$ and $\{Q_1, Q_2, Q_3\}$, as in my last note to you. And as before, suppose $P_1 = Q_3$. However this time, let us be careful to assume that P_1 and Q_3 differ in truth value in their respective sets. What happens now when we consider nonelementary propositions of the form $\{P_1, \neg P_1\}$ and $\{\neg Q_3, Q_3\}$ where by $\neg P_1$ I mean the orthocomplement of P_1, etc. Presumably you still want to view these sets as representative of mutually exclusive properties inherent within the quantum system. However, by construction the sets are identical: $\{P_1, \neg P_1\} = \{\neg Q_3, Q_3\}$. How does one decide on a truth value assignment here, given the previous truth value assignments for $\{P_1, P_2, P_3\}$ and $\{Q_1, Q_2, Q_3\}$?

21-02-06 *London Overnighter* (to R. Schack)

I had a pathetic long talk with John Conway at Princeton about Dutch book yesterday (about three hours). He buys none of it. Can you believe this is the man who wrote the Atlas of Finite Groups (and actually has all that in his head ... as far as I can tell and as is rumored there). Yet, a simple argument like Dutch book can't pierce his preconceptions.

John Nash was also at my talk: I think he never blinked once, and he looked a million miles away. I couldn't help but remember the passage in *A Beautiful Mind* where he says that it was thinking about quantum foundations that pushed him over the edge.

21-02-06 *Poetry* (to S. Kochen & J. H. Conway)

I enjoyed the sparring yesterday, and I think just before we parted from the lounge Si had a good summary of a hefty aspect of what I'm shooting for. So, thanks for the understanding, if not the agreement. (Some of that stuff, Si, in case you haven't read it, is in the last note I sent you "Free Will Again"—hopefully I'm a little clearer there than I was in person.)

While it's on my mind, let me come back to the part of the discussion where we had three different ways of talking about "measurement", going from left to right written on the board. Recall John changed the word "action" to "interaction." At about that time, I said something that made you both cringe.

With your indulgence, I want to say it again using the words I used at my Caltech summer school lectures last summer. [See 17-06-04 note "Preamble" to H. Mabuchi.] I'm sure it'll still make you cringe, but maybe you'll enjoy the pentameter of this version. Like always when I talk, it emphasizes the distinction between ontology ("the quantum world")—i.e., the exciting part—and epistemology ("*part* of quantum theory")—i.e., the dull but necessary part. Luckily, as I see it, in an oblique way, the epistemology is teaching us something about the ontology. Maybe that's enough to leave us a little room for further discussion.

21-02-06 *Heaping the Poetry* (to J. H. Conway)

Conwayism 1: *Well, it ended before it began. Yeah, yeah. JHC*

It's a general problem; that's why I keep at it year after year.

I won't have the time to reply to you in depth until I get all the planning for this Wheelerfest done, but don't you worry, eventually I'll be back in full force: I, of course, think I have a reply to your last note that will run you in a circle. But I want to spend some time writing it delicately.

In the meantime, let me give you a little something related to your present note that I happen to have in my archives. (Menand's book was my very favorite of 2001, and I've recommended it to a lot of people.) You should see the relevance to our discussion very quickly into the quote. For me, a probability is like a beak: That is all I am saying. To that extent, probabilities are real in nature, but only to that extent. Free decisions—as you call them—may be part of the fundamental furniture of nature (I'm willing to bet that)—that is, they are of a more respectable status than beaks—but it is going to be hard to convince me that probabilities are.

Still, I am a somewhat logical being, and as such I will give your arguments a hard go.

As I say, I'll be back eventually (sooner, rather than later).

From L. Menand, *The Metaphysical Club*, (HarperCollins, London, 2001):

> The world is filled with unique things. In order to deal with the world, though, we have to make generalizations. On what should we base our generalizations? One answer, and it seems the obvious answer, is that we should base them on the characteristics things have in common. No individual horse is completely identical to any other horse; no poem is identical to any other poem. But all things we call horses, and all things we call poems, share certain properties, and if we make those properties the basis for generalizations, we have one way of "doing things" with horses or poems—of distinguishing a horse from a zebra, for example, or of judging whether a particular poem is a good poem or a bad poem. These common properties can be visible features or they can be invisible qualities; in either case, we create an idea of a "horse" or a "poem," or of "horseness" or "poetry," by retaining the characteristics found in all horses or poems and ignoring characteristics that make one horse or poem different from another. We even out, or bracket, the variations among individuals for the sake of constructing a general type.
>
> Darwin's fundamental insight as a biologist was that among groups of sexually reproducing organisms, the variations are much more important than the similarities. "Natural selection," his name for the mechanism of evolutionary development that he codiscovered with Alfred Russel Wallace, is the process by which individual characteristics that are more favorable to reproductive success are "chosen," because they are passed on from one generation to the next, over characteristics that are less favorable. Darwin regretted that the word "selection" suggested an intention: natural selection is a blind process, because the conditions to which the organism must adapt in order to survive are never the same. In periods of drought, when seeds are hard to find, finches that happen to have long narrow beaks, good for foraging, will be favored over finches with broad powerful beaks: more of their offspring will survive and reproduce. In periods of abundance, when seeds are large and their shells are hard, the broad-beaked finches will hold the adaptive advantage. "Finchness" is a variable, not a constant.
>
> Darwin thought that variations do not arise because organisms need them (which is essentially what Lamarck had argued). He thought that variations occur by chance, and that chance determines their adaptive utility. In all seasons it happens that some finches

are born with marginally longer and narrower beaks than others, just as children of the same parents are not all exactly the same height. In certain environmental conditions, a narrower beak may have positive or negative survival value, but in other conditions—for example, when seeds are plentiful and finches are few—it may make no difference. The "selection" of favorable characteristics is therefore neither designed nor progressive. No intelligence, divine or otherwise, determines in advance the relative value of individual variations, and there is no ideal type of "finch," or essence of "finchness," toward which adaptive changes are leading.

Natural selection is a law that explains *why* changes occur in nature—because, as Darwin and Wallace both realized after reading, independently, Thomas Malthus's *Essay on the Principle of Population* (1798), if all members of a group of sexually reproducing organisms were equally well adapted, the population of the group would quickly outgrow the resources available to sustain it. Since some members of the group must die, the individuals whose slight differences give them an adaptive edge are more likely to survive. Evolution is simply the incidental by-product of material struggle, not its goal. Organisms don't struggle because they must evolve; they evolve because they must struggle. Natural selection also explains *how* changes occur in nature—by the relative reproductive success of the marginally better adapted. But natural selection does not dictate *what* those changes shall be. It is a process without mind.

A way of thinking that regards individual differences as inessential departures from a general type is therefore not well suited for dealing with the natural world. A general type is fixed, determinate, and uniform; the world Darwin described is characterized by chance, change, and difference—all the attributes general types are designed to leave out. In emphasizing the particularity of individual organisms, Darwin did not conclude that species do not exist. He only concluded that species are what they appear to be: ideas, which are provisionally useful for naming groups of interacting individuals. "I look at the term species," he wrote, "as one arbitrarily given for the sake of convenience to a set of individuals closely resembling each other .... [I]t does not essentially differ from the term variety, which is given to less distinct and more fluctuating forms. The term variety, again, in comparison with mere individual differences, is also applied arbitrarily, and for mere convenience sake." Difference goes all the way down.

21-02-06 *Rényi Distinguishability Measures* (to E. H. Lieb)

It was good meeting you yesterday. I only wish I had had a chance to extend the discussion: I've got loads of mathematical problems I'd love to get some expert input on!

Here's the reference where you can read a little about the quantum Rényi overlaps I was telling you about: `http://www.arxiv.org/abs/quant-ph/9601020`. The place to look is pages 60–64. (Also pages 16–19 explain why this kind of quantity even comes up in the classical context.) I had plenty more inequalities than that related to the quantity, but I never published them and they burned up in the Los Alamos fire. In any case, none of them were probably very interesting, as they weren't tight and the thing I really wanted was the infimum.

22-02-06 *Hollow Eyes* (to S. J. van Enk)

Oh, let me tell you a funny little story, and then I should go. During my Princeton talk, there was one of the oddest looking fellows in the audience. His eyes just looked so hollow—I don't know how to describe it. He hardly blinked the whole talk, and his face was completely expressionless. After it was all over, I kept thinking, "He looks familiar; who is that?" Well the next morning in the shower, I got a hunch and checked it out on Google: It was John Nash! After discovering that, I amused myself by remembering how he claimed that it was his thinking about quantum foundations that pushed him over the edge to schizophrenia! And I hoped I didn't push him over the edge again!

22-02-06 *Tutorial* (to J. E. Sipe & R. W. Spekkens)

T3 Current Interpretations of Quantum Mechanics
Organizer: Rob Spekkens, Perimeter Institute for Theoretical Physics,
Room 302, Baltimore Convention Center ...
Instructor: Professor John Sipe, University of Toronto

Too bad the pseudo-Bayesian view of Caves, Schack, Appleby, Fuchs and few others isn't a current interpretation of quantum mechanics ...

... ☺

P.S. Below is a nasty note I wrote a while ago. I kind of like the way I put some of the things in it (the explanatory things that is, not the nasty things)—I think they represent a little more mature way of putting things than I may have expressed to you two before. Maybe you'll enjoy it for the content. [See 30-01-06 note "Island of Misfit Toys" to K. T. McDonald.]

22-02-06 *Probably More Than I Should Send* (to J. E. Sipe)

Sipesm 2: *Anyway, I haven't yet decided what (or where in the presentation) to say something about your approach. So this is a good chance for me to ask a couple of questions.*

I hope you didn't take me too seriously; your tutorial title just looked like a good set-up to abuse you two, and I couldn't resist.

Sipesm 3: *As the saying goes, I didn't have time to write you a short letter so I wrote you a long one ... apologies for the verbiage!*

You ought to know that *I* am more guilty of this than *anyone*! I practically make a living of it.

If you can wait 'till Sunday or so, I'll write you some more delicate and specific answers to all your questions—at the moment, I'm struggling to get everything finished up for the Wheelerfest this weekend (including my own talk).

But let me send you two or three older emails in the meantime that I think fairly directly answer a couple of your questions. Rob calls the ontology being spoken of (more accurately, dreamed of) here "F-theory," where the F can be interpreted a couple of ways.

I'll come back to the issue of why I'm not very open-minded on hidden variables theories in the next note after the Wheelerfest, and I'll (probably) resculpt the partial answers that I'm sending you at the moment to be more in the shape of your present questions while I'm at it.

BTW, you and Rob have great gifts: you both write very, very clearly, even in your hastily composed emails. Your letter was a joy.

See notes below on "F-theory" and (what I don't view as) "instrumentalism." [See 21-02-06 note "Poetry" to S. Kochen & J. H. Conway, 19-06-05 note "Philosopher's Stone" to G. L. Comer, 14-11-05 note "Questions, Actions, Answers, & Consequences" to B. C. van Fraassen, and 17-02-06 note "Incompletely Knowable vs 'Truth in the Making'" to W. G. Demopoulos.]

John's Preply

Well, I'm still writing the tutorial. It's supposed to be four hours long. As you can imagine, this is not an easy task. In some ways (counting the slides that have to be made, for example!), four hours is dreadfully long. But in another way it is too short to do much justice to anything. My main goal in this is to "first, do no harm" in presenting the different views. We'll see how well I do ...

Anyway, I haven't yet decided what (or where in the presentation) to say something about your approach. So this is a good chance for me to ask a couple of questions. First, I am glad to see statements like:

> To say that 'everything is information,' as Zeilinger does, or to say 'every physical system is a quantum computer,' as you and Lloyd do, adds nothing to existing physics—you would have been better to stick with the tautology that a physical system is a physical system, full stop.

I certainly feel that statements like Zeilinger's likely convey nothing, but only propagate buzz-words and probably, by their implicit over-hyping, do science no good in the long run. I also have a lot of sympathy for statements like:

> Only *part* of quantum mechanics is about the gambler-independent world, and that part still calls for a full identification. This task of separating the wheat from the chaff is the price one must pay if one accepts the idea that quantum *states* are of essence information—luckily it is a task that stands a chance of bearing fruit (and already has born fruit).

I gather from this that you see the ultimate task to be the identification of just what is the part of quantum mechanics that is "about the gambler-independent world." The Bayesian analysis is a strategy, as I understand it, to identify that task and undertake it. It is a means, not an end.

But if I understand this correctly I am puzzled to see statements like:

> If what you mean by "intrinsic randomness" is that quantum measurement outcomes do not pre-exist the measurement process—that they are made "on demand"—and that there is no fact of the matter in the universe that will determine which way they will go, then Caves, Schack, Appleby, Peres, Leifer, and I all accept that.

Why could it not be that there is a "fact of the matter in the universe" that will determine the outcome of (maybe just some) measurements. You say that

> Garrett-style hidden variables, regardless of what Garrett might say, are thus not necessary for a Bayesian view of quantum probabilities.

Fair enough. But might it not be that the "gambler-independent world" is in fact described by a hidden-variable theory of some sort? For example, why would you (immediately!) rule out a Bohm - de Broglie universe (or a modified one, with the usual distribution over configuration space being a kind of 'equilibrium distribution' that we might someday be able to overcome, such as Valentini suggests) simply because of your Bayesian strategy in trying to identify just what that "gambler-independent world" is? Could it not be that Bohm and de Broglie have really identified the "fact of the matter in the universe," or that some such approach would? At least, must not this possibility be seriously considered? How does a Bayesian strategy immediately rule it out? By rejecting even the possibility that some quantum measurement outcomes may pre-exist the measurement process, do you not prejudge the outcome of the larger quest, for which the Bayesian viewpoint is not a goal but a strategy?

Perhaps I simply have read too much between the lines of what you say, or perhaps I truly misunderstand your views. In any case, (and to make sure in this tutorial I don't confuse somebody about what you really do hold!) could I get you to see if the way I describe your approach below is (a) basically right, (b) partly right but with major flaws, or (c) totally screwed up? Below I am going to phrase things in the kind of language I'm going to use in the tutorial. Please bear with me; I realize this is not the way you might want to frame the issues, and I really am not wedded to this as necessarily the 'best way' to phrase the issues. But for better or worse it's the one I've adopted for the tutorial, and so it's the one in which I have to somehow (and hopefully without too much damage) fit the thoughts of Chris Fuchs.

Here we go.

I use the term "interpretation" of a modern physical theory to mean an identification of just what it is to which the abstract elements in the theory purportedly refer. A "type of interpretation" identifies the type of things to which the abstract elements in the theory refer.

The abstract elements in quantum mechanics are density operators, POVMs, etc., etc. A "realist" type of interpretation identifies the abstract elements with stuff that purportedly really exists in the world, the "furniture of the universe." An "operationalist" type of interpretation identifies the abstract elements with tasks in the laboratory, such as preparations, transformations, and measurements. And so on. An "instrumentalist" type of interpretation says that abstract elements don't refer to anything at all, except perhaps in the way a hammer refers to a nail. That is, theories are just (only) tools for coping in the world.

Admittedly these categories are not as hard-and-fast as the language implies, but let's take them as a rough guide to thinking about these things. Now you and your colleagues hold

> "the idea that quantum *states* are of essence information"

and the obvious question a beginning student would ask—and undoubtedly someone will ask if I say something like this in the tutorial—is "information about what?" Perhaps one could argue that this question has no meaning, but I don't see how that view can be sustained. In any case, from the talks I have heard you give I came away with the impression that, at least in the short term and from the point of view of our current work-a-day physics, you take this to be information, broadly speaking, about the impact and results of future tasks in the laboratory, such as measurements. On the basis of this it seems to me you are in the "operationalist" camp.

Now within this camp there are various tribes. Some feel that any talk about underlying "furniture of the universe" is meaningless, or at least not worthwhile. They are "hard core operationalists." Gadgets and tasks for them are ultimate primitives in their theorizing, and for them an operationalist physics is all there can ever be, at least if it is to be what they would consider a proper physics.

I take you to be in a different tribe, whose members feel that ultimately we want a physics that does indeed talk about the furniture of the universe, the "gambler-independent world." Indeed, you refer to that as the "wheat," as opposed to the "chaff." But that "gambler-independent world" has not yet been identified. In the interim you argue that the abstract elements of quantum mechanics should be understood operationally, and the predictions and statements of quantum mechanics subjected to a kind of "Bayesian rack" to clarify exactly what beyond the objective rules of probability actually exists in them. This analysis then provides a firm footing for the search for the actual "furniture of the universe," whatever it is. Ultimately you want a theory that does describe this furniture; I would guess you are uncertain as to whether quantum mechanics (sufficiently purified by Bayesian analysis) would provide that, or whether a successor theory would be required. But in any case in your heart of hearts you hope someday for a realist science, or at least hope that that hope makes sense.

OK, that's my take. How close to your thoughts is it? It may be a while before I can respond to anything you say, because I am at a conference in Germany, but I really would appreciate some comments!

P.S.: As the saying goes, I didn't have time to write you a short letter so I wrote you a long one ... apologies for the verbiage!

22-02-06 *Wheelerfest* (to D. Overbye)

Here is something I should have told you about a good while back, but—I have to apologize—I didn't think about you until this morning. Since you're such a fan of John Wheeler, you might be interested.

Marlan Scully and I have organized a little meeting in Princeton in honor of John Wheeler to discuss quantum foundations and quantum information. It'll be held Friday Feb 24 and Saturday Feb 25; on the Friday it'll be at a certain conference center in Princeton, and on the Saturday it'll be on campus. I'm sorry for the very, very short notice.

I'll place the meeting schedule below, so that you'll have some indication of what it's all about. [See 16-02-06 note titled "Wheelerfest Program, User-Friendly Version".] If you're interested in coming let me know.

John himself will join us Feb 24, the Friday, between 5:00 PM and 6:00 PM (and maybe a little longer, depending upon his stamina) to be greeted by former students, etc. There'll be a conference dinner Friday evening at the same conference center (starting at 6:00), but we don't think John will stay all the way through that. However, you're welcome to come—choice of prime rib or salmon.

22-02-06 *Wheelerfest, 2* (to D. Overbye)

Overbye-ism 2: *I am a huge fan of Wheeler's and would love to have been at this meeting. Unfortunately I'm already committed to going to Pennsylvania for my mother-in-law's 80th birthday. I am especially intrigued by at least the title of Conway's talk about free will. Is that John Conway, the inventor of Life? Sorry I can't be there but the weekend has long been taken.*

Too bad! I'm so sorry I didn't think about contacting you before today. Yeah, that's John Conway—the inventor of "life" and a much more famous mathematician than that for his *Atlas of Finite Groups* and many other things. He's a very lively character. The theorem he'll be talking about is one by Simon Kochen and him, that is a stronger, newer version of the sort of thing first proven by Kochen and Specker. I would say it this way: That quantum mechanical measurement outcomes do not pre-exist the process of measurement; nature makes a choice when forced to the point. Or, another way I would put it is, that a quantum measurement forces a little act of creation (or birth). They claim it's the strongest hidden-variable no-go theorem yet. It's hasn't been published yet, but there has been plenty of press coverage due to Conway's talks.

23-02-06 *John Wheeler's Visit* (to L. Wheeler Ufford)

Thank you for your greeting. It has been a pleasure to organize this. John has long been one of my heroes, ever since I took a first-year college course with him in 1984. Almost every paper I have written in physics has concerned one or more of the problems he brought to my attention way back then. So, this is really a very small gesture in return.

I look forward to meeting you, your husband, Jackie and, of course, John tomorrow. (It will be the first time I have seen him since 1994.)

Letitia's Preply

> I am John Wheeler's older daughter and with Jackie Fuschini, his secretary, I and my husband will be bringing him to your Fest, hoping to arrive there a little before 5 p.m. I look forward to meeting you and thanking you for arranging such a happy and encouraging event for him.

27-02-06 *Wheelerfest, Thanks* (to M. O. Scully)

I wanted to write you a note to try to express my heartfelt thanks for supplying the resources that made this meeting happen. There is nothing that tickles me more than seeing our understanding of quantum mechanics advance, and meetings like this one are crucial in that regard. In fact, I think on one of the things John Wheeler wrote in that letter to Carroll Alley that I read at the beginning of the meeting:

> Expecting something great when two great minds meet who have different outlooks, all of us in this Princeton community expected something great to come out from Bohr and Einstein arguing the great question day after day—the central purpose of Bohr's four-month, spring 1939 visit to Princeton—I, now, looking back on those days, have a terrible conscience because the day-after-day arguing of Bohr was not with Einstein about the quantum but with me about the fission of uranium. How recover, I ask myself over and over, the pent up promise of those long-past days?

I hope we fulfilled a little of John's dream: Though the time was short, we did have a very impressive array of "great minds with different outlooks" arguing the great question.

Particularly, for me, I got a lot out of the interaction. My favorite talks were yours and Unruh's (with which I heartily agreed) and Conway's and Kochen's (which stretched me to a new level). Furthermore, Schumacher took my "Dutch book" challenge seriously, and, I think, may have already provided a very nice solution to it. Finally, Hardy stayed at my house over the rest of the weekend and we got a good bit of conceptual work done with regard to his "causaloid" framework. So, this conference turned out to be nothing but pleasure for me!

28-02-06 *Foundational Cost Cutting?* (to A. Wilce)

Are you planning to go to the "New Directions in the Foundations of Physics" meeting, April 28–30? If so, would you be interested in having a roommate to cut down on costs? My travel budget is tiny this year, and I'm trying to do everything I can to stretch it.

Let me know what you think, or if you have any other suggestions.

01-03-06 *Foundational Cost Cutting?, 2* (to A. Wilce)

Too bad. OK, either I'll try to look for another roommate, or I might even drop the plan of going myself. I'm wishy-washy anyway: Something deep inside me has been telling me that I've been going to too many foundational conferences (the payoff doesn't seem so big anymore).

06-03-06 *Be Forgiving* (to G. L. Comer)

Comerism 11: *I'm very much convinced that the historical approach to teaching special relativity is very much a disservice to the subject. It gives precisely the wrong foundational point-of-view.*

I think you ought to be a little forgiving to something like a historical process for thinking about the foundation of the theory—or, at least admit that it may have its place. It may not be the best pedagogy for students, but it may have its place elsewhere.

What I'm thinking of here particularly is how one might incorporate quantum mechanical ideas into the scene. If one starts the analysis off—crucial words, "starts the analysis off"—with spacetime before rods and clocks (or clocks and maybe some stand-in for light rays), then the only natural progression one can take may end up being to try to "quantize gravity" in the usual sorts of ways (for instance, wave functions over three-geometries and variations of that program).

On the other hand, a more operationalistic approach—one that doesn't assume spacetimes or wave functions of spacetimes at the outset—may be just what is needed to free up the conceptual playground enough to make good progress. For instance, if a quantum measurement event helps set the very notion of causal structure in the first place, then maybe it is best to rethink where along the lines of earlier thinking one could make the conceptual leap to "metric is fundamental." In the quantum world, maybe one cannot make that conceptual leap—and thus maybe that is not a good starting point.

On a different subject:

Comerism 12: *I mean not discussing the metric as fundamental is like presenting quantum mechanics without mentioning the wave function.*

You see, but that is what I would ultimately like to do! For, for me, wave functions are always about *someone's* degrees of belief. And what we (quantum Bayesians) would like to get at is a gambling-agent-independent account of what is up in the quantum world. As long as one invokes a wave function, one is always *explicitly* talking about an observer. See transparency #12 in my talk "Being Bayesian in a Quantum World" posted at the bottom of my webpage. It talks to that point directly!

08-02-06 *Counterfactual* (to S. J. van Enk)

I'd like to soon get back to the Demopoulos issue. Particularly, I would like to record better the change you pointed out in how we quantum Bayesians must be treating counterfactuals. I wish there were a whole paper in that idea. I guess I came back to the issue today as I was reading Thomas Marlow's paper, "Relationalism vs. Bayesianism" `gr-qc/0603015`. I couldn't really understand it, but he did drive one point home that you've been driving at with me for a while:

> It is often stated that EPR and Bell-like theorems are avoided (not disproved but just avoided) by using Bayesian probability theory (this is often the major reason people invoke Bayesian reasoning in quantum theory [6]), but rarely is it explicitly stated how

> the invocation of Bayesian probabilities overcomes arguments for causal nonlocalities. We shall attempt to argue why this is the case and why arguments that claim the opposite are lacking.

Ref. [6] refers to me. Unfortunately, as I've already hinted, I didn't get his argument. But his sentiment is good (and seems to second yours). So, if by recording the conversation you and I have been having, we can provide a little service to the community, it might be worthwhile.

14-02-06 *Notwithstanding* (to D. Bacon)

Steven van Enk pointed out to me the award you recently bestowed upon us on your blog. [See `http://dabacon.org/pontiff/?p=1189`.] Well, thanks—I'm honored. (If I could only figure out a way to put it in my CV.)

Not completely unrelated (but mostly so), here's something that might tickle your fancy. Once upon a time, Steven told me a story that he had heard from Klaus Mølmer. It went that Bohr needed to write a paper for a conference proceedings and insisted on starting it off with the word "notwithstanding." Thereafter, for a day or two, Bohr was absolutely stumped and didn't write a word until he could compose his first sentence!

Well Steven and I, wanting to learn from the master, decided that we too would compose a paper around the word "notwithstanding." The result was a little paper titled, "Entanglement is Super … but not Superluminal!," that appeared in a book of collected papers on spooky action-at-a-distance. We were so embarrassed with the paper that we never posted it on `quant-ph`! But for you, since you've earned it with your kind accolades to us, here is its opening sentence:

> Notwithstanding its wonderful potential for enhancing and extending our capabilities within the realm of communication technology—through applications like quantum cryptography, quantum superdense coding, and quantum teleportation—quantum entanglement as a physical resource falls far short of being the feast the hungry seekers of superluminal communication would hope it to be.

22-02-06 *The Way the Cookie Crumbles* (to H. C. von Baeyer)

Thanks for the note. As I just wrote to Marcus Appleby, "At the moment, I am scrambling to get something together for the Wheelerfest meeting I organized at Princeton this weekend. I gave my talk the title 'The Equations of Quantum Mechanics Already Do Fly,' but honestly I don't yet have a clue what I'm going to say. I hope I get some inspiration before tomorrow morning, so that I'll have time enough to put it together before Friday morning!" But I want to get caught up on some emails too!

Just a couple of comments on your note:

von Baeyerism 15: *Translating Fierz's sentence: "As is often the case, when you don't understand anything, you think of all sorts of things that you feel are connected, but whose connection you cannot express clearly" inspires me to speculate as follows.*

How did the Greeks discover atoms? By reasoning from the observation of steam that water could not possibly retain its characteristics under arbitrary dilution unless those characteristics were stored in tiny, finite lumps.

How did Schrödinger invent molecular genetics? By reasoning that living organisms could not possibly retain their characteristics over millions of years unless those characteristics were stored in tiny, material, finite lumps.

In both cases the argument was that the cookie would crumble into chaos unless its parts were quantized.

It seems to me that this is the basis of Zeilinger's principle. Information would dissolve into ignorance if it were not quantized into bits – even though it is carried by qubits. Conversely, if you add analyticity to bits, you get qubits (Hardy). (This does not imply that information is material.)

With regard to the part about Hardy (which reminds me that I had wanted to invite him to my house this weekend—thanks), see the note below that I wrote to Hans Halvorson a few weeks ago. [See 27-01-06 note "Monday or Tuesday Meeting" to H. Halvorson.]

von Baeyerism 16: *So if Spekkens had a toy model in which the ontic content was continuous, and the epistemic content countable, he would have quantum mechanics.*

OK, as far as I know he doesn't have that. But he and Terry Rudolph and Steve Bartlett do have one in which the ontic content is continuous. See their talk at the APS March meeting; here is their abstract. (Or at least I think that's the same toy model he told me about before, and as far as I understood, there was a continuous set of epistemic states allowed.) [Though, now see `http://arxiv.org/abs/1111.5057`.]

> **3:30PM D40.00004**
>
> Liouville mechanics with an epistemic restriction and Bohr's response to EPR
>
> TERRY RUDOLPH, Imperial College, STEPHEN BARTLETT, University of Sydney, ROBERT SPEKKENS, Perimeter Institute
>
> We introduce a toy theory that reproduces a wide variety of qualitative features of quantum theory for degrees of freedom that are continuous. Specifically, we consider classical mechanics supplemented by a constraint on the amount of information an observer may have about the motional state (i.e. point in phase space) of a collection of classical particles – Liouville mechanics with an epistemic restriction (This may well be how Heisenberg initially understood the Uncertainty Principle). We develop the formalism of the theory by deriving the consequences of this "classical uncertainty principle" on state preparations, measurements, and dynamics. The result is a theory of hidden variables, although it is not a hidden variable model of quantum theory because of its locality and noncontextuality. Despite admitting a simple classical interpretation, the theory also exhibits the operational features of Bohr's notion of complementarity. Indeed, it includes all of the features of quantum mechanics to which Bohr appeals in his response to EPR. This theory demonstrates, therefore, that Bohr's arguments fail as a defense of the completeness of quantum mechanics.

In addition though, I'm pretty sure I don't agree with what you're hoping for here. That is because, toy models, at least as conceived along the original Spekkens line, have the property of noncontexuality (which gives rise to the fact that in these models, the measurement 'outcomes' are already alive before the measurement actually takes place—i.e., they pre-exist). In that sense, these are still detached-observer models. One paper that I don't think I have recommended to you before, but that you might enjoy is this:

> The Bell-Kochen-Specker Theorem
> Authors: D. M. Appleby
> Comments: 22 pages
> Journal-ref: Stud. Hist. Philos. Mod. Phys. **36** (2005) 1
> `http://www.arxiv.org/abs/quant-ph/0308114`

von Baeyerism 17: *In any case, I feel (wish?) that we are very close to understanding the quantum. Maybe somebody will figure it out in Wheeler's lifetime.*

I can't disagree with that!

27-02-06 *Wheeler* (to W. P. Schleich)

No, thank YOU very much for coming. I was very much impressed by the humanity I saw in you; I had not got to know you so well before as I did this weekend.

John continues to be an inspiration for me — as I hope my talk conveyed.

I hope that one day we'll get a chance to sit down and talk about this representation of (finite dimensional) quantum states in terms of true-blue probability distributions, which I mentioned briefly in my talk. My own understanding of it is long in development, and I suspect there are some insights I could glean from Mr. Phase Space's view on it. Schumacher, by the way, provided a nice "Dutch book" argument over the weekend, fulfilling the challenge I put at the end of the talk. So, on the technical end even (i.e., even outside of the opportunity to see John honored), this meeting was quite useful to me. It was a real pleasure to see it come together.

27-02-06 *Convex Sets / Algebras Tutorial?* (to H. Halvorson)

I very much enjoyed your talk at the Wheelerfest, the parts of it I understood at least—I'm terribly primitive when it comes to mathematics.

To continue our mutual quest, I wonder if the logical next step might be for you to help bring me up to speed on the connection between convex sets and algebras if you would? That is definitely an important connection which I need to understand. It would be great if I could pull a tutorial from you that a) concerned itself only with the finite dimensional case, and particularly b) used only the language of the finite dimensions in its presentation. My mind gets unusually distracted when I hear terms like Hausdorff and compactification, and if you could help me get to the truly essential points (which I view always as the finite dimensional issues), I think it would help both of us exponentially (you from the teaching, me from the learning).

Might you have some time early next week?

27-02-06 *Dutch Book* (to D. M. Greenberger)

I'm sorry I never answered your question about Dutch book after my talk. Maybe the simplest solution is for me to just point you to the nice notes Carl Caves put together on it:

> `http://info.phys.unm.edu/~caves/reports/dutchbook.pdf`.

Another place to look is the appendix in our paper:

> "Conditions for Compatibility of Quantum State Assignments"
> Authors: Carlton M. Caves, Christopher A. Fuchs, Rüdiger Schack
> `http://www.arxiv.org/abs/quant-ph/0206110`

It was good seeing you at the meeting; you had me laughing and laughing with your quips.

27-02-06 *Wheelerfest* (to K. W. Ford)

Thanks for the note. I'm glad you enjoyed the meeting, and I'm particularly glad that you think John enjoyed it. Seeing him at this late stage of life was just as inspiring for me as seeing him, and thinking about his thoughts, at every other stage. He is an amazing man.

You're right about the gender balance issue, and your note helped promote it a little further to the top of my consciousness (and conscience). I'll try to think harder next time when I work on an invitation list.

I hope you got something out of my talk. I'm a little disappointed with it myself: I tried to squeeze both too much history and too much technicality to get any live points across effectively. (I was pleased though that Schumacher took my "Dutch book" challenge seriously over the weekend; I think he has provided a very nice solution to it!) Your question at the end was a very good one. It's one of the crucial issues—and I wish I had a much better answer for you by this stage. As best I can put it at the moment, I think the issue is not consciousness at all; but the distinction between an "inside description" and an "outside description"—analogous to one of the points Conway brought up in his talk about the "free will theorem" (which unfortunately you missed). Maybe a way to put it is this: What we call quantum theory has sadly been misidentified all these years as a "description from the outside," when in fact it is almost completely a "description from the inside." There is a hint of the outside in it—through these very powerful theorems of Kochen and Specker (and now Conway and Kochen)—and that is our best handle for getting at our next big step in physics, but the first thing we've really got to disabuse ourselves of is confusing the inside with the outside. Quantum states, and their changes, live only on the inside; the "acts of creation," the "clicks," that trigger those changes live on the interface between the inside and outside—thus they should be viewed as distinct from the quantum-state changes themselves. When we really get that cleared up (as I hope our Bayesian program is doing), then we'll be ready for some serious progress.

Let me paste in couple of notes below that go a little further along these lines. [See 19-06-05 note "Philosopher's Stone" to G. L. Comer and 17-06-04 note "Preamble" to H. Mabuchi.] But you can see I've got a long way to go in my understanding before I'll be able to drop the poetry and put in its place a little clarity.

If you weren't able to recover the manuscript I gave you from the conference center, let me know and I'll send you another copy if you'd like one.

28-02-06 *Our Special Sauce* (to L. Hardy)

I'm just back from a mandatory "Bell Labs town meeting." It seems that the over-riding purpose of the meeting was to remind all the researchers here that they and their thoughts are just part of the food chain. Here were some of the words and phrases that caught my attention:

> "competition makes you stronger," "value proposition," "territory," "what do we have to do to win," "our special sauce," "where do we want to compete to win," "ways to attack certain verticals to win," "attack," "kickin' some butt," "organic ways to win," "where the puck is moving," "we drive value," "new competitive landscape," "we need to develop stickiness," "or else we don't eat," "monetize the core product strategy," "early alignment around our value proposition."

This note seemed like a good opportunity to record them all. Coming back to our conversation on the drive to the airport, I think it is this culture that is the main thing getting me down.

Your visit was a breath of fresh air. Thanks for coming. I've had the early Einstein arguments on my mind ever since, and I think you're on to something.

Concerning the Boston meeting, as I work on my schedule this afternoon, I think I probably ought to bow out: I really can't afford it, both because of my travel budget and because of my time. I leave for Lisbon March 27, and this would put me away from home immediately beforehand for still more days. You go there, and remind them that decoherence, many worlds, and nonlocality really aren't all that interesting or creative.

Attached is a picture of Cap'n Hardy and ye maties.

28-02-06 *Quantum Causal Structure* (to L. Hardy)

I'm back again: I had forgotten that when Rob Spekkens was here last, I put together a little collection of some of my wilder speculations on the connections between quantum and GR. Let me share that file with you too. I think it made little effect on Rob, but—given all the stuff you told me the other night—you might find more of use in it. (Well, probably nothing of use, but maybe some food for thought.)

Particularly, for me, one of the phrases you used the other night has gotten me excited that it may be possible to give some true-blue substance to those old thoughts of Mead near the end of the document. I.e., that taking account of "this" event may change the "causal structure" of all the rest:

> [T]he greatest lesson quantum theory holds for us is that when two pieces of the world come together, they give birth. They give birth to FACTS in a way not so unlike the romantic notion of parenthood: that a child is more than the sum total of her parents, an entity unto herself with untold potential for reshaping the world. Add a new piece to a puzzle—not to its beginning or end or edges, but somewhere deep in its middle—and all the extant pieces must be rejiggled or recut to make a new, but different, whole. That is the great lesson.

I'm very attracted to that, and to some extent I took the imagery from Mead and his musings on time.

I will be studying your paper thoroughly, and rethinking old Einstein again.

28-02-06 *Stupider Chris* (to T. Rudolph)

Rudolphism 9: *I really like that second paragraph you quoted,*

> Is the entirety of existence ... built upon billions upon billions of elementary quantum phenomena, those elementary acts of "observer-participancy," those most ethereal of all the entities that have been forced upon us by the progress of science? — John Wheeler

although to be honest I'm somewhat skeptical of the overriding sentiment (I suspect things are ultimately a little more mundane, the history of science is littered with the corpses of the Anthropocentricians).

Yeah, that's too bad: Unfortunately, I agree with you. However, I continue to wonder if one could de-anthropocentrify John's idea and still make something of it: I.e., that the big bang is here, rather than way back there. That part of it very much intrigues me.

Below is a daisy-chain of notes demonstrating my latest flailing for the right imagery. [See 27-02-06 note "Wheelerfest" to K. W. Ford.]

07-03-06 *Invitation to Grinnell and Talk Titles* (to W. B. Case)

Thanks for the invitation; I am flattered. What is a Squire Lecturer? It sounds like an honor (and even Bell Labs people think that $800 sounds like good money). I would very much like to come, but April is pretty much already filled up for me. Would it be possible to come in May? How long of a visit were you thinking?

Titles? Maybe for the undergraduate talk:

> "Drawing a Qubit in a Tetrahedron"

And how about this for the general talk:

> "Quantum Information and the Malleable World"

They're just ideas. If you don't like them, send me back to the drawing boards.

07-03-06 *April* (to N. C. Menicucci)

Menicuccism 1: *I would really like to talk to you about Bayesianism in QM. Howard Wiseman from Griffith University gave a talk recently where he called the Bayesian approach 'solipsism' – i.e., that there is no external world, only one's own mind. We eventually convinced him that that was crap and not true of the interpretation, but he never really seemed to "get it": How can there be an external world if all quantum states are states of my knowledge? Well, I told him, they're states of your knowledge about the external world. Somehow that didn't make sense. This really got my goat because I think this interpretation might be one of the only ways of correctly looking at quantum theory, since it embraces the paradigm shift the theory inspires (although being a paradigm shift, it's hard for me to pin down in words), but Howard* **completely** *missed the point and made it sound like all Bayesians are looney. I tried to defend the position, but I was at a loss for words.*

What you tell me is very sad. A solipsist, it seems to me, would have no reason whatsoever to do physics, much less try to understand quantum mechanics.

I append a little story below from my "Quantum States: What the Hell Are They?" about solipsism. [See 21-07-01 note "The Reality of Wives" to A. J. Landahl & J. Preskill.] (If you give me a mailing address, and you want a copy, I'll have a copy of the VUP edition of *Notes on a Paulian Idea* sent to you.)

07-03-06 *Some Reading* (to N. C. Menicucci)

Going back to solipsism, I could point you to a lot of things I've written on this annoying issue, but let me start with this: `http://www.arxiv.org/abs/quant-ph/0204146`. Do Sections 4 and 5 help any already?

08-03-06 *A Little Wheelerfest Report* (to H. C. von Baeyer)

von Baeyerism 18: *Did you learn anything, or teach anything, at the Wheelerfest?*

Indeed I got a lot from the meeting. And had many ridiculous fights with John Conway, who is afraid of the Bayesian idea of probability. Such a great mind; such a great prejudice!

Particularly useful for me in preparing my own talk was the *strong* realization that the Born rule is not at all about setting probabilities but transforming them! Its role really is that of a transformation rule. There are hints of that in my writings before, but finally I feel it with all my heart and soul—that's what I meant by the "strong realization." Anyway it indicates that maybe one can get at the content of the Born rule through some Dutch-book-like arguments. Thus I posed that as a question at the end of my talk. Very nicely, Schumacher took the challenge and has started to make progress in that direction.

I sort of like the talk I put together for this—even though it goes out on the limb more than usual (even for me). I will try to scan in a copy soon (and send to you), and I will probably give an expanded variation on it in Sweden.

09-03-06 *Carl Again, a Few More Words* (to R. E. Slusher)

In the last three years, Carl has posted 13 papers to the quant-ph archive (with his students and postdocs).

The 5 papers I personally found the most interesting are:

- `quant-ph/0304083` [abs, ps, pdf, other] :
  Title: Physical-resource demands for scalable quantum computation
  Authors: Carlton M. Caves, Ivan H. Deutsch, Robin Blume-Kohout

- `quant-ph/0306179` [abs, ps, pdf, other] :
  Title: Gleason-Type Derivations of the Quantum Probability Rule for Generalized Measurements
  Authors: Carlton M. Caves, Christopher A. Fuchs, Kiran Manne, Joseph M. Renes
  Journal-ref: Found. Phys. 34, 193 (2004)

- `quant-ph/0310075` [abs, ps, pdf, other] :
  Title: Symmetric Informationally Complete Quantum Measurements
  Authors: Joseph M. Renes, Robin Blume-Kohout, A. J. Scott, Carlton M. Caves
  Journal-ref: J. Math. Phys. 45, 2171 (2004)

- `quant-ph/0404137` [abs, ps, pdf, other] :
  Title: Minimal Informationally Complete Measurements for Pure States
  Authors: Steven T. Flammia, Andrew Silberfarb, Carlton M. Caves

- `quant-ph/0409144` [abs, ps, pdf, other] :
  Title: Properties of the frequency operator do not imply the quantum probability postulate
  Authors: Carlton M. Caves, Ruediger Schack
  Journal-ref: C.M. Caves and R. Schack, Ann. Phys. 315, 123–146 (2005)

All of the papers are cut from the same cloth: They are devoted to developing a technical apparatus for getting at what is quantum about quantum information. Particularly, Carl's worldwide impact has been in his efforts to develop a view of quantum information along Bayesian lines—in fact he is the very father of that field. To get some indication of the number of serious quantum information theorists willing to take this line of thought seriously, one can look at the participation list for the meeting "Being Bayesian in a Quantum World" for which Caves was a co-organizer:

`http://web.archive.org/web/20090511060206/http://www.uni-konstanz.de/ppm/events/bbqw2005/`

You'll see Hans Briegel, Lucien Hardy, David Mermin, Gerard Milburn, Michael Nielsen, John Smolin, Bill Wootters, and many others there.

Caves is a behind-the-scenes worker, a kind of theorist's theorist. The press may not know him so much (because he's not a weird, journalist-attracting recluse like David Deutsch) nor may the funding agencies (because he's not a flamboyant stage actor like Seth Lloyd), but anyone in this field who is worth his salt knows of Caves's intellect and influence.

10-03-06 *Opening of My APS Talk* (to R. E. Slusher)

Your story of how you opened your MIT talk inspired me for the opening of my own talk at the APS meeting Monday.

I'm going to start off by reading this quote of William James:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself.

Then I'm going to show a big $|\psi\rangle$ and say we finally know its *name*: It is "expectation"—raw, unmitigated expectation with no further need for justification. If we take that name and that idea seriously, we will "possess the universe itself."

I'm going to Princeton this morning to work with Halvorson a bit. Depending upon when that finishes, I may or may not come in to Bell Labs. If you really need me around, drop me a note.

10-03-06 *Probably More Than I Should Send, 2* (to J. E. Sipe)

My post-Wheelerfest days have passed away, and it has just been one thing after another. Now the APS meeting is upon us! Time, time, time; what has become of me!

Still, I wouldn't mind writing you something today: Never pass up an opportunity to expose a colleague to Bayesian ways of thought! But I have to go to Princeton this morning to work with Halvorson. May I ask, what is the last time you'll be checking email before leaving for Baltimore (if you haven't already left for Baltimore)? You know, if there's a carrot in front of the horse, he'll pull the cart!

I'll plan my afternoon according to you.

10-03-06 *Isms* (to J. E. Sipe)

Sipesm 4: *I'll be checking email until about 5PM today (Friday); I fly to Baltimore tonight, and hope to be checking email there, but I'm not sure that'll be possible until the conference starts, and my tutorial is Sunday morning!*

Ouch, I've run out of time for all those delicate and carefully constructed answers I wanted to give you! My fault—you deserve much better than me. It's now almost 3PM, and I'm just finished talking to Hans. Now I'm in a Panera Bread in Princeton, having a cup of coffee before my drive home. Let me send you a very little note answering some of your questions, and maybe we can talk more next week.

I certainly liked the way you posed your questions.

Sipesm 5: *I gather from this that you see the ultimate task to be the identification of just what is the part of quantum mechanics that is "about the gambler-independent world." The Bayesian analysis is a strategy, as I understand it, to identify that task and undertake it. It is a means, not an end.*

But if I understand this correctly I am puzzled to see statements like: ... [blah blah blah measurements create outcomes]

Fair enough. But might it not be that the "gambler-independent world" is in fact described by a hidden-variable theory of some sort? For example, why would you (immediately!) rule out a Bohm - de Broglie universe (or a modified one, with the usual distribution over configuration space being a kind of 'equilibrium distribution' that we might someday be able to overcome, such as Valentini suggests) simply because of your Bayesian strategy in trying to identify just what that "gambler-independent world" is? Could it not be that Bohm and de Broglie have really identified the "fact of the matter in the universe," or that some such approach would? At least, must not this possibility be seriously considered? How does a Bayesian strategy immediately rule it out? By rejecting even the possibility that some quantum measurement outcomes may pre-exist the measurement process, do you not prejudge the outcome of the larger quest, for which the Bayesian viewpoint is not a goal but a strategy?

Here is the way I once answered Jeff Bub when he asked a similar question. Jeff asked:

> You want to take quantum mechanics as a theory of the way information is represented and the limitations on the communication of information, and not a description of the behavior of particles, as in classical mechanics. Granted, the way the world is hard-wired might impose limitations on the gathering of information and the exchange of information – limitations expressed precisely by the 'limited sort of privacy' we have (i.e., secure kd, but no secure bc), hence by science necessarily taking the form of quantum mechanics. But how does it follow from this that we must interpret quantum mechanics as a theory of information, and not as a descriptive theory in the sense of classical mechanics?

My reply was:

> It doesn't. You're completely on track there. Do you remember my slide where I listed the axioms of quantum mechanics in Montréal? In my presentation I said how I'm always struck by the stark contrast between that list of axioms and the ones we take for our other cornerstone theory of the world (referring to special relativity): (1) the speed of light is constant, and (2) physics is the same in all frames. The debate over the foundations of quantum mechanics will not end until we can reduce the theory to such a set of crisp physical statements—I believe that with all my heart. However, just as special relativity will always be interpretable in Lorentz's way, quantum mechanics will likely always be interpretable in Bohm's way. There's nothing we can do about that.
>
> What I'm really searching for is just a polite way to say, "Ahh, blow it out your butt. You can believe that Lorentzian way of looking at things if you want to, but why when have this absolutely simple alternative conceptual structure?"

> What I'm looking for is just a couple of crisp physical statements—contradictory appearing even, just as Einstein's—that can characterize what quantum mechanics is all about. Something like (but more precise than):
>
> 1. The effects of our interventions into the world are nondiminishable. And,
> 2. But still we have science; all the world is not simply a dream of our own concoction.
>
> Once we get that cleared up, we'll finally be ready to move to the next stage of physics, much like Einstein was ready to move on to general relativity once he had reduced the Lorentz contractions to the two statements above.

That's the best answer I think I can give you at the moment. Even a Bayesian approach to quantum mechanics cannot DISPROVE nonlocal, noncontextual hidden variables theories. It just emphasizes that they don't smell right.

The "proof" of any approach will ultimately be in what new physics it gives rise to.

Now for your most important question:

Sipesm 6: *In any case, (and to make sure in this tutorial I don't confuse somebody about what you really do hold!) could I get you to see if the way I describe your approach below is (a) basically right, (b) partly right but with major flaws, or (c) totally screwed up? ...*

I use the term "interpretation" of a modern physical theory to mean an identification of just what it is to which the abstract elements in the theory purportedly refer. A "type of interpretation" identifies the type of things to which the abstract elements in the theory refer.

The abstract elements in quantum mechanics are density operators, POVMs, etc., etc. A "realist" type of interpretation identifies the abstract elements with stuff that purportedly really exists in the world, the "furniture of the universe." An "operationalist" type of interpretation identifies the abstract elements with tasks in the laboratory, such as preparations, transformations, and measurements. And so on. An "instrumentalist" type of interpretation says that abstract elements don't refer to anything at all, except perhaps in the way a hammer refers to a nail. That is, theories are just (only) tools for coping in the world. ...

Now you and your colleagues hold "the idea that quantum STATES are of essence information" and the obvious question a beginning student would ask – and undoubtedly someone will ask if I say something like this in the tutorial – is "information about what?" Perhaps one could argue that this question has no meaning, but I don't see how that view can be sustained.

In any case, from the talks I have heard you give I came away with the impression that, at least in the short term and from the point of view of our current work-a-day physics, you take this to be information, broadly speaking, about the impact and results of future tasks in the laboratory, such as measurements. On the basis of this it seems to me you are in the "operationalist" camp.

Now within this camp there are various tribes. Some feel that any talk about underlying "furniture of the universe" is meaningless, or at least not worthwhile. They are "hard core operationalists." Gadgets and tasks for them are ultimate primitives in their theorizing, and for them an operationalist physics is all there can ever be, at least if it is to be what they would consider a proper physics.

I take you to be in a different tribe, whose members feel that ultimately we want a physics that does indeed talk about the furniture of the universe, the "gambler-independent world." Indeed, you refer to that as the "wheat," as opposed to the "chaff." But that "gambler-independent world" has not yet been identified. In the interim you argue that the abstract elements of quantum mechanics should be understood operationally, and the predictions and statements of quantum mechanics subjected to a kind of "Bayesian rack" to clarify exactly what beyond the objective rules of probability actually exists in them. This analysis then provides a firm footing for the search for the actual *"furniture of the universe," whatever it is. Ultimately you want a theory that does describe this furniture; I would guess you are uncertain as to whether quantum mechanics (sufficiently purified by Bayesian analysis) would provide that, or whether a successor theory would be required. But in any case in your heart of hearts you hope someday for a realist science, or at least hope that that hope makes sense.*

I think my way of looking at the terms *within* quantum mechanics runs the whole gamut from instrumentalism to realism, and there's no way to put it into one single category.

1. When I posit a physical system about which I will speak (by assigning it a Hilbert space, etc.), I am doing that in an almost naïve realistic way. I.e., the $\mathcal{H}$ I write down, represents a piece of the world that is out there independently of me.

2. When I assign a dimension d to that Hilbert space, I am hypothesizing an inherent *property* of that system. I might be right, or I might be wrong, but d is something I hypothesize of it, even if only provisionally. Realism again.

3. When I draw a quantum state out of the space of operators defined by $\mathcal{H}$, however, I am expressing a bundle of my expectations. These are not properties inherent in the system. They are subjective expectations that I bring into the picture (presumably because they have served me well in the past, or at least done me no harm). If I were to conceptually delete myself from the picture, these expectations would disappear with me. To that extent, the view might sound a little like—or at least be confused with—idealism (but that's only if one—as Howard Wiseman often does—forgets elements 1 and 2 above and one of the further elements below.)

4. The subject matter of those expectations, i.e., what they are about, refers both to me and the system I posit. They are MY expectations for the consequences (for ME) of MY interactions with the system. That, you might think is a kind of operationalism: For if I were to conceptually delete myself from the picture, those interactions would disappear too.

5. On the other hand, the reason we use the formal structure of quantum mechanics to bundle our expectations, to manipulate and update them, to do all that we do with them, is to better cope with the world. It is a means to help our species to survive and propagate. That is a kind of instrumentalism. That part of quantum mechanics is a tool like a hammer; it can be used to fix a lot of things, or simply as an aid to help defend ourselves.

6. Still one can never forget the ultimately uncontrollable nature of each quantum measurement outcome—and through Kochen-Specker, at least the way I view it, the non-pre-existence of those "outcomes". That smacks of realism in the oldest, most time-honored way. The world surprises us and is not a creation of our whims and fancies. Back to realism ... but the twist is quantum mechanics, as used by each individual user, only refers to the outcomes HE helps generate. (That smacks of alchemy ... dangerous to say so, but I call it like it is.)

7. Nevertheless, having learned a little from Copernicus, it seems we should ultimately try to abstract away from these personal encounters with the world (having learned what we could from the formalism concerned with gambling on them). If a tiny little system and I create something new in the world when we get together—i.e., we give rise to a birth or new fact—so must it be likely, it seems to me, that any two things give rise to a birth when they get together. That is "F-theory" or the "sexual interpretation of quantum mechanics" ... but you'll have to wait for the movie if you question me any further on that ...

I hope that answers you a little. It took me a cup and a half of coffee, and I think I just barely made deadline.

Have a safe trip. It'll be fun talking to you next week.

10-03-06 *Isms, 1* (to H. Halvorson)

I didn't explain in the last note why I didn't leave Princeton until 4:00. What happened was that I owed John Sipe a note before his leaving for the APS meeting to give a four-hour tutorial on quantum foundations Sunday—he wanted a little explanation of our Bayesian point of view so that he might incorporate it into the tutorial. Well, after leaving Marx Hall, I started thinking, "I don't quite know when he'll be leaving, so I'd better check my email." Thus I popped in to Panera's Bread, and sure enough I found out that he'd soon be checking his email for the last time before his flight. So, I quickly composed the note below. [See 10-03-06 note "Isms" to J. E. Sipe.]

The part that might interest you is the list at the end explaining the various isms that my view of the quantum mechanics seems to cross. The list is incomplete: I think I hit even more isms than that, but maybe this is a good starting point for categorization.

Another note on relevant isms coming after I get a chance to search the web.

Still a silent house!

10-03-06 *Isms, 2* (to H. Halvorson)

The other thing I wanted to write you about isms is this. Here is the paper by Huw Price that I was telling you about:

> "Naturalism without Representation"
> `http://www.usyd.edu.au/time/price/preprints/naturalism-final.pdf`

The abstract for the paper is below. I recall liking the paper very much. And to the extent that I understand the term, I believe I can classify myself (presently) as what Huw calls a "subject naturalist."

I wonder how you would characterize a) yourself, and b) your other colleagues in your department, in these terms.

> Abstract: I begin with a distinction between two ways of taking science to be relevant to philosophy. The first ("object naturalism") is a ontological thesis – it holds that what exists, what we should be realists about, is the world as revealed by science. The second ("subject naturalism") is a prescription for philosophy, based on the belief that we humans (and in particular, our thought and talk) are part of the natural world. What is the relationship between these two kinds of naturalism? Contemporary naturalists are apt to think that the latter view is a mere corollary of the former. I argue that there is an important sense in which the priority is the other way around: object naturalism depends on "validation" from a subject naturalist perspective – in particular, on confirmation of certain "representationalist" assumptions about the functions of human language. Moreover, I maintain, there are good reasons for doubting whether object naturalism deserves to be validated, in this sense. Thus, an adequate naturalistic philosophy threatens to undermine what most contemporary philosophers have in mind, when they call themselves philosophical naturalists.

10-03-06 *Singularities and Evolutionary Laws* (to H. Halvorson)

Below are the three quotes I have by Poincaré concerning the subject above. The quotes seem horribly inadequate now (with respect to what I remember of the niceness of the article); I wish I had more extensive quotes in the computer. Always too much to do ...

From: H. Poincaré, "The Evolution of Laws," in his book *Mathematics and Science: Last Essays (Dernières Pensées)*, translated by J. W. Bolduc, (Dover, New York, 1963), pp. 1–14.

> Mr. Boutroux, in his writings on the contingency of the laws of Nature, queried, whether natural laws are not susceptible to change and if the world evolves continuously, whether the laws themselves which govern this evolution are alone exempt from all variation. ... I should like to consider a few of the aspects which the problem can assume.

and

> In summary, we can know nothing of the past unless we admit that the laws have not changed; if we do admit this, the question of the evolution of the laws is meaningless; if we do not admit this condition, the question is impossible of solution, just as with all questions which relate to the past. ...
>
> But, it may be asked, is it not possible that the application of the process just described may lead to a contradiction, or, if we wish, that our differential equations admit of no solution? Since the hypothesis of the immutability of the laws, posited at the beginning of our argument would lead to an absurd consequence, we would have demonstrated *per absurdum* that laws have changed, while at the same time we would be forever unable to know in what sense.
>
> Since this process is reversible, what we have just said applies to the future as well, and there would seem to be cases in which we would be able to state that before a particular date the world would have to come to an end or change its laws; if, for example, our calculations indicate that on that date one of the quantities which we have to consider is due to become infinite or to assume a value which is physically impossible. To perish or to change its laws is just about the same thing; a world which would no longer have the same laws as ours would no longer be our world but another one.

and

> No doubt many readers will be dismayed to note that I seem constantly to substitute for the world a system of simple symbols. This is not due simply to a professional habit of a mathematician; the nature of my subject made this approach absolutely necessary. The Bergsonian world has no laws; what can have laws is simply the more or less distorted image which the scientists make of it. When we say that nature is governed by laws, it is understood that this portrait is still rather lifelike. It is therefore according to this description and this description only that we must reason, or else we run the risk of losing the very idea of law which was the object of our study.

12-03-06 *The Spirit of Stevie Ray* (to G. L. Comer)

Boy you sure send a lot of pictures of your guitar! Handsome picture of the singer though—you're right, you do look authentic.

I'm just off to the APS March meeting in Baltimore … where our little efforts in physics has finally been recognized with a topical group all of its own. Sessions all week long. I chair the one on entanglement tomorrow morning, and then give my talk in the session on quantum foundations in the afternoon.

Just made a transparency of this William James quote:

> Metaphysics has usually followed a very primitive kind of quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, or the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's *principle*, and to possess it is after a fashion to possess the universe itself.

I'm gonna read that to the audience, and then put up a transparency with a big $|\psi\rangle$ and nothing else on it. Then I'll say that since the beginning of quantum mechanics, the debate has been over what this thing actually is. But now we know its name: it is "expectation." Raw expectation. (A better word than "information" I think.) And now that we finally possess its name, we will after a fashion possess the universe itself.

Then I'll bombard them with too many equations … as one is expected to do in a physics talk.

20-03-06 *The Rest of the Story?* (to J. E. Sipe)

Sipesm 7: *Regardless of the strategy, the quantum world of this interpretation is a fixed, static thing. It is a frozen, changeless place. Dynamics refers not to the quantum world, but only to our actions, our experiences, and our beliefs as agents. Or, more poetically (à la Chris), life does not arise from our interventions; it is our interventions.*

Just like in our conversation, I think I'm pretty sympathetic with your description … until we get to here.

It is the substrate (and catalyst) that is changeless, but it is being written upon like a writing pad with the construction of a story. And that part of the process seems to me to be as important as anything else. In fact, I personally consider it a crucial part of the ontology. I want to better understand your de-emphasis of it.

Let me give you a little material below to react to. The first three passages represent what I have been calling the "Paulian Idea"—they are almost literal quotes from Pauli himself, but not quite, as I had to compress them somewhat to fit on the transparencies I was using for my presentation (at the Wheelerfest). I am not in complete agreement with these—for instance, sometimes he forgets his own emphasis that the measurement device should be considered as a prolongation of the agent—but maybe they can help get your gears turning, before I turn to more radical stuff.

Following Pauli, I'll put one version of Wheeler's 20-question story that was influential on me.

Finally I'll end with one excerpt from my *Notes on a Paulian Idea* and a couple of excerpts from my "Anti-Växjö Interpretation of Quantum Mechanics." These pieces emphasize, why I think the unpredictable outcome of a quantum measurement should be included in the ontology.

Do you see these things as consistent with your description above, or do you see them as contradictory? I would like to get that straight, because as I say, I like much in your description previous to that. (I'll come back to some more minor technical points in it later.) And who knows, maybe I've been misunderstanding my own view! It, like the world, is malleable after all.

Paulian Idea, 1

[Einstein and I] often discussed these questions, and I invariably profited very greatly even when I could not agree with Einstein's views. "Physics is after all the description of reality," he said to me, continuing, with a sarcastic glance in my direction, "or should I perhaps say physics is the description of what one merely imagines?" This question clearly shows Einstein's concern that the objective character of physics might be lost through a theory of the type of quantum mechanics, in that as a consequence of its wider conception of objectivity of an explanation of nature the difference between physical reality and dream or hallucination become blurred.

The objectivity of physics is however fully ensured in quantum mechanics in the following sense. Although in principle, according to the theory, it is in general only the statistics of series of experiments that is determined by laws, the observer is unable, even in the unpredictable single case, to influence the result of his observation—as for example the response of a counter at a particular instant of time. Further, personal qualities of the observer do not come into the theory in any way—the observation can be made by objective registering apparatus, the results of which are objectively available for anyone's inspection. Just as in the theory of relativity a group of mathematical transformations connects all possible coordinate systems, so in quantum mechanics a group of mathematical transformations connects the possible experimental arrangements.

Einstein however advocated a narrower form of the reality concept ...

Paulian Idea, 2

It seems to me appropriate to call the conceptual description of nature in classical physics, which Einstein so wishes to retain, "the ideal of the detached observer". To put it drastically, the observer has according to this ideal to disappear entirely in a discrete manner as hidden spectator, never as actor, nature being left alone in a predetermined course of events, independent of the way in which the phenomena are observed. "Like the moon has a definite position" Einstein said to me last winter, "whether or not we look at the moon, the same must also hold for the atomic objects, as there is no sharp distinction possible between these and macroscopic objects. Observation cannot *create* an element of reality like a position, there must be something contained in the complete description of physical reality which corresponds to the *possibility* of observing a position, already before the observation has been actually made." It is this kind of postulate which I call the ideal of the detached observer.

In quantum mechanics, on the contrary, an observation changes in general the "state" of the observed system in a way not contained in the mathematically formulated *laws*, which only apply to the time dependence of the state of a *closed* system. I think here on the passage to a new phenomenon by observation which is taken into account by the so called "reduction of the wave packets." As it is allowed to consider the instruments of observation as a kind of prolongation of the sense organs of the observer, I consider the impredictable change of the state by a single observation to be *an abandonment of the idea of the isolation (detachment) of the observer from the course of physical events outside himself.*

Paulian Idea, 3

Like an ultimate fact without any cause, the *individual* outcome of a measurement is, however, in general not comprehended by laws. This must necessarily be the case, if quantum mechanics is interpreted as a natural generalization of classical physics, which takes into account the finiteness of the quantum of action. . . .

The significance of this development is to give us insight into the logical possibility of a new and wider pattern of thought. This takes into account the observer, including the apparatus used by him, differently from the way it was done in classical physics. In the new pattern of thought we do not assume any longer the *detached observer*, occurring in the idealizations of this classical type of theory, but an observer who by his indeterminable effects creates a new situation, theoretically described as a new state of the observed system. In this way every observation is a singling out of a particular factual result, here and now, from the theoretical possibilities, thereby making obvious the discontinuous aspect of the physical phenomena.

Nevertheless, there remains still in the new kind of theory an *objective reality*, inasmuch as these theories deny any possibility for the observer to influence the results of a measurement, once the experimental arrangement is chosen. Therefore particular qualities of an individual observer do not enter the conceptual framework of the theory.

Wheelerish Idea, 1

But if I could have asked Bohr, how did he think the Universe came into being, and what is its substance, what would he have said?

It is too late to ask. The plan is up to us to find.

The Universe can't be Laplacean. It may be higgledy-piggledy. But have hope. Surely someday we will see the necessity of the quantum in its construction. Would you like a little story along this line?

Of course! About what?

About the game of twenty questions. You recall how it goes—one of the after-dinner party sent out of the living room, the others agreeing on a word, the one fated to be a questioner returning and starting his questions. "Is it a living object?" "No." "Is it here on earth?" "Yes." So the questions go from respondent to respondent around the room until at length the word emerges: victory if in twenty tries or less; otherwise, defeat.

Then comes the moment when we are fourth to be sent from the room. We are locked out unbelievably long. On finally being readmitted, we find a smile on everyone's face, sign of a joke or a plot. We innocently start our questions. At first the answers come quickly. Then each question begins to take longer in the answering—strange, when the answer itself is only a simple "yes" or "no." At length, feeling hot on the trail, we ask, "Is the word 'cloud'?" "Yes," comes the reply, and everyone bursts out laughing. When we were out of the room, they explain, they had agreed not to agree in advance on any word at all. Each one around the circle could respond "yes" or "no" as he pleased to whatever question we put to him. But however he replied he had to have a word in mind compatible with his own reply—and with all the replies that went before. No wonder some of those decisions between "yes" and "no" proved so hard!

And the point of your story?

Compare the game in its two versions with physics in its two formulations, classical and quantum. First, we thought the word already existed "out there" as physics once thought that the position and momentum of the electron existed "out there," independent of any act of observation. Second, in actuality the information about the word was brought into being step by step through the questions we raised, as the information about the electron is brought into being, step by step, by the experiments that the observer chooses to make. Third, if we had chosen to ask different questions we would have ended up with a different word—as the experimenter would have ended up with a different story for the doings of the electron if he had measured different quantities or the same quantities in a different order. Fourth, whatever power we had in bringing the particular word "cloud" into being was partial only. A major part of the selection—unknowing selection—lay in the "yes" or "no" replies of the colleagues around the room. Similarly, the experimenter has some substantial influence on what will happen to the electron by the choice of experiments he will do on it; but he knows there is much impredictability about what any given one of his measurements will disclose. Fifth, there was a "rule of the game" that required of every participator that his choice of yes or no should be compatible with *some* word. Similarly, there is a consistency about the observations made in physics. One person must be able to tell another in plain language what he finds and the second person must be able to verify the observation.

Flying Equations Story

Good holidays to you. This morning, as I was driving to work, it dawned on me that roughly this day 10 years ago, I was conferred my degrees at the University of Texas. Time does fly.

It made me think of a little anecdote about John Wheeler that I heard from John Preskill a few days ago. In 1972 he had Wheeler for his freshman classical mechanics course at Princeton. One day Wheeler had each student write all the equations of physics s/he knew on a single sheet of paper. He gathered the papers up and placed them all side-by-side on the stage at the front of the classroom. Finally, he looked out at the students and said, "These pages likely contain all the fundamental equations we know of physics. They encapsulate all that's known of the world." Then he looked at the papers and said, "Now fly!" Nothing happened. He looked out at the audience, then at the papers, raised his hands high, and commanded, "Fly!" Everyone was silent, thinking this guy had gone off his rocker. Wheeler said, "You see, these equations can't fly. But our universe flies. We're still missing the single, simple ingredient that makes it all fly."

Merry Christmas.

Växjination, 1

The situation of quantum mechanics—I become ever more convinced—illustrates this immersion of the scientific agent in the world more clearly than any physical theory contemplated to date. That is because it tells you you have to strain really hard and strip away most of the theory's operational content, most of its workaday usefulness, to make sense of it as a reflection of "what is" (independent of the agent) and—importantly—you insist on doing that for all the terms in the theory. . . .

So, I myself am left with a view of quantum mechanics for which the main terms in the theory—the quantum states—express nothing more than the gambling commitments I'm willing to make at any moment. When I encounter various other pieces of the world, if I am rational—that is to say, Darwinian-optimal—I should use the stimulations those pieces give me to reevaluate my commitments. This is what quantum state change is about. The REALITY of the world I am dealing with is captured by two things in the present picture:

1. I posit systems with which I find myself having encounters, and
2. I am not able to see in a deterministic fashion the stimulations (call them measurement outcomes, if you like) those systems will give me—something comes into me from the outside that takes me by surprise.

Växjination, 2

Yes there is certainly a kind of realism working in the back of my mind, if what you mean by "realism" is that one can imagine a world which never gives rise to man or sentience of any kind. This, from my view, would be a world without science, for there would be no scientific agents theorizing within it. This is what I mean by realism: That man is not a priori the be-all and end-all of the world. (The qualification "a priori" is important and I'll come back to it later.)

A quick consequence of this view is that I believe I eschew all forms of idealism. Instead, I would say all our evidence for the reality of the world comes from without us, i.e., not from within us. We do not hold evidence for an independent world by holding some kind of transcendental knowledge. Nor do we hold it from the practical and technological successes of our past and present conceptions of the world's essence. It is just the opposite. We believe in a world external to ourselves precisely because we find ourselves getting unpredictable kicks (from the world) all the time. If we could predict everything to the final T as Laplace had wanted us to, it seems to me, we might as well be living a dream.

To maybe put it in an overly poetic and not completely accurate way, the reality of the world is not in what we capture with our theories, but rather in all the stuff we don't. To make this concrete, take quantum mechanics and consider setting up all the equipment necessary to prepare a system in a state Π and to measure some noncommuting observable H. (In a sense, all that equipment is just an extension of ourselves and not so very different in character from a prosthetic hand.) Which eigenstate of H we will end up getting as our outcome, we cannot say. We can draw up some subjective probabilities for the occurrence of the various possibilities, but that's as far as we can go. (Or at least that's what quantum mechanics tells us.) Thus, I would say, in such a quantum measurement we touch the reality of the world in the most essential of ways.

20-03-06 *Replies to Sipe* (to R. Schack)

The history of the conversation with John Sipe goes like this. After reading the abstract for his tutorial at the APS meeting, I sent he and Rob Spekkens this little quip (and asked Matt Leifer if I embarrassed him by using his name in some of the attached material):

> T3 Current Interpretations of Quantum Mechanics
> Organizer: Rob Spekkens, Perimeter Institute for Theoretical Physics
> Room 302, Baltimore Convention Center
> ...
> Instructor: Professor John Sipe, University of Toronto
>
> Too bad the pseudo-Bayesian view of Caves, Schack, Appleby, Fuchs and few others isn't a current interpretation of quantum mechanics ...

That got John to asking questions, but also Matt replied:

> I think that in Rob and John's terminology the Bayesian approach is not a "current interpretation" but an idea for an interpretation. They are careful to distinguish the two in their book. A full blown interpretation is required to account for all of nonrelativistic quantum mechanics and to make precise what the ontology of the interpretation is. I don't think we have all agreed upon a consistent answer to the latter question yet.

I tend to take Matt's point seriously, and I worry about losing the ground we've gained with Sipe. It was for this reason that I really tried to shy away from the issue of Hamiltonians when Carl and I were talking to him the other day ... though Carl really wanted to put it front and center.

Similarly I might say about a possible corrective to his description of "measurements as [objectively described] tasks [via the formal assignment of a POVM]." Which is what I think you were getting at with your words:

Schackcosm 89: *One point that surprises me is that he got the impression that there is a fundamental difference in the way we look at preparations and measurements. How on earth did that happen?*

Partially it happened by my choosing to be silent on the issue.

Below I'll place the part's of John's description that I myself consider problematic, ... either because it doesn't convey properly our current unified thinking, or because it may pinpoint an area our current disagreement.

How to reply to him further, *if at all*, I am not yet clear on.

I've got to do a bunch of stupid company things today, but I really, really, really hope to start writing you two some detailed notes on the paper tomorrow.

Oh, also concerning Sipe, did you already get from Carl the one thing I did send him in reply a few days ago? I'm somewhat timid to send it over the airwaves again, as Carl has already pointed out its exorbitant length ...

— — — — — — — — — — — — — — — — —

Things from Sipe's note and small comments.

> Thus, while the abstract elements in the theory associated with measurements are identified with tasks in the laboratory, as in operationalism, the abstract elements in the theory associated with preparations are identified with beliefs of the agent, signaling a kind of empiricist perspective.

I understand Rüdiger's worry, but there is some small truth in this and I think it worth nurturing at this stage in his thinking (without overwhelming him). Yes, from our view, writing down a quantum state and writing down a set of effects is, in both cases, the writing down of beliefs. But they are beliefs of different things. And John carries the hint of that in his description. A POVM represents the agent's belief about the action he will take on the external world; a quantum state represents his belief about which of the consequences will come about from his action.

> In contrast, a usual Stern-Gerlach device oriented along the z direction does not, in this interpretation of quantum mechanics, reveal the z-component of angular momentum, or for that matter anything else. The particular outcome of one experimental run is simply a consequence of performing the experiment.

Agreed.

> Nonetheless, repeated experimentation does reveal that the electron associated with the atom passing through the device should be taken as a spin-1/2 particle. Here the attribute under consideration is taken to be internal angular momentum, and the instance – the irreducible representation appropriate to the particle of interest – spin-1/2.

This passage highlights the difference in ways that Carl and I talk. Carl would say that a system's attributes of this nature—say the system's Hamiltonian—are set intrinsically. The Hamiltonian (again, for instance) gives the system identity across time. Whereas, I would say that such identity is set by a subjective judgment.

It is my judgment that I should write down a constant Hamiltonian through time for such and such system: Measurement cannot reveal the TRUE Hamiltonian. To the extent that it can, one is making a judgment analogous to exchangeability. Because of the quantum de Finetti theorem, we know there is only the illusion that measurement can be used to infer the one true quantum state. Similarly I would say of the one true Hamiltonian.

On the other hand, I appreciate Carl's point that measurements (and their outcomes) have to be given meaning by some means. (This is what John was trying to get after with his phrase "anchors of belief.") But I lean much more on the agent's initial involvement in that term than Carl. The bureau of standards measurement, however abstract, does the trick for me. Its intention is to make clear that as long as an agent ascribes internal meaning to the outcomes of one measurement, then the meanings of all the rest are set too. But it's no deeper than choosing a standard meter stick. We have no right to say that the meter stick is the one true meter in nature's eyes, and similarly for the timeless quantum attribute. We set it, and thereafter, the practice of quantum mechanics is in trying to being consistent (coherent) with that initial call.

Which leads us to this:

> The role of an "instance of an attribute" in this interpretation is not to specify one of a number of possible expressions of existence, as it is in realist classical mechanics, but rather to specify one class of possible beliefs – the one that the theory recommends – about the consequences of future interventions of a particular type.

which I can again accept, with emphasis on "class of beliefs," before disagreeing with this for the reasons above:

> The point of physics is to identify these nondynamical variables. Repeated interventions by experimentalists, and the careful noting of the range of consequences that those interventions elicit, is how these fixed instances are discovered.

Finally, I've already told John and Carl know my strong dislike of this (and maybe it's been forwarded to Rüdiger):

> Regardless of the strategy, the quantum world of this interpretation is a fixed, static thing. It is a frozen, changeless place. Dynamics refers not to the quantum world, but only to our actions, our experiences, and our beliefs as agents.

but as I reread it, maybe I'm willing to find a grain of truth in its emphasis too. John puts the formal structure on one side of a dividing line, and the extra-formal stuff (like the indices on a POVM) on the other side. What we may be disagreeing over may only be whether the latter stuff is allowed to be called part of quantum mechanics.

— —

In all, after writing all this, I find myself wondering even more about how much of all this should be brought to John's attention. Maybe it is best to leave John's temporary happiness as it is (he'll eventually come out of it anyway) and leave our internal arguments as a family matter for the moment.

21-03-06 *The Unchanged Changer* (to R. Schack)

Schackcosm 90:

***Sipesm 8*:** *But in your interpretation some of the abstract elements of the theory do describe something that "actually exists"; it is these "frozen" elements, your Bureau of Standards measurement results, which might be called the scaffold of our experiences.*

Am I right that he has misunderstood something?

I'm not sure at what level you're asking this question. Is you question concerning the part about "in your interpretation some of the abstract elements of the theory do describe something that 'actually exists' "? Or rather the part about "your Bureau of Standards measurement results, which might be called the scaffold of our experiences"?

Since you know that I'm pragmatist about theories (and only willing to talk about "objective with respect to a theory"), let me guess that your concern is with the second issue. If that's the case, then let me rather focus on this passage of his:

Sipesm 9: *The results of your Bureau of Standards measurements, existing in some Platonic heaven at some time in the future, are "once and for all." Carl would say that the irreducible representation of a spin-1/2 particle is the same today as it always was and always will be.*

He's got the bit about the attributes being frozen in time right, for both Carl and me (or, at least, as I understand Carl). That is one of the reasons I call the quantum system a "catalyst" in the attached diagram (which John would never look at in our conversation the other day)—it is the unchanged changer (at least at this level of our understanding).

Where he flubs things is where he attributes to me the idea that, "The results of your Bureau of Standards measurements, existing in some Platonic heaven at some time in the future, are 'once and for all.' " I don't at all imagine the results as existing at some time in the future. For no measurement results pre-exist an actual measurement process; or to say it better, without the actual action, there is no actual reaction. Instead, to the extent that an agent judges that he is always thinking of the same system, he will fix a single B of S measurement with which to make reference (which in turn fixes an irreducible representation, as in Carl's version). It is only that that is timeless in the way John is thinking, not any putative outcome for the Platonic measurement.

More generally about the note, he seems to have the flavor of the "phenomenalism" wrong that he attributes to Bohr and Pauli (those two for sure), but also the one he attributes to me. There is something about his description that seems to leave it disconnected from the quantum formalism. Whereas I would say that I have come to this point precisely because of Kochen-Specker and Gleason, which surely rely on the quantum formalism. The categories and their motivations just can't be separated so cleanly as he would like.

22-03-06 *Web Page, Second Draft* (to R. E. Slusher)

1.5.6 Fundamentally New Science and Technology

Quantum Computing, Communication, and Cryptography at Bell Labs

Quantum mechanics, as a branch of physics, has been with us since the late 1920s, and it can be safely said that without it our modern technological society would not exist. Quantum effects power almost all that we know, from transistors, to lasers, to medical imaging, to nuclear weapons and much, much more. However, only in the last 10-15 years has it been realized that the most arcane features of quantum mechanics—ones not presently used in any technology—can be harnessed for computing and communication tasks unimaginable within classical physics. This is not so much because quantum devices can be made smaller and faster, but because the language of quantum mechanics allows the writing of new algorithms that simply cannot be written in classical terms. The most famous algorithms in this respect are Peter Shor's prime factorization algorithm (which, if implemented, would make RSA public key cryptography insecure) and Lov Grover's quantum search algorithm, both of which were discovered here at Bell Labs. Finding ever new uses for these arcane quantum effects and implementing them in real technologies is an active and lively field, known as Quantum Information.

Present work at Bell Labs ranges the field: On the theoretical side, we specialize in algorithms, topological quantum computation, fundamental aspects of quantum information, quantum error-correcting codes, and supporting technologies calculations. On the experimental side, we are participating in a large effort to implement quantum computation in scalable micro-ion-traps and scalable optical-lattices for cold trapped atoms. As well, we have a strong effort to implement multi-channel gigahertz-rate quantum cryptography. All our experimental efforts make crucial use of Lucent's expertise in MEMS device technologies, with fabrication of ion traps and spatial light modulators through the New Jersey Nanotechnology Consortium.

23-03-06 *A Review of General Covariance* (to G. L. Comer)

Waking up very slowly. Silly, after an almost sleepless night. All night long I tossed and turned obsessing over the same thought/question:

> An 'action' is defined by the set of its 'consequences.' However the only consequences I can see in a quantum measurement are the refinement of one's expectations. But some actions lead to identical sets of refined expectations with differing probabilities. So they must be different actions after all. But that contradicts the first part. Repeat. Repeat. Repeat. What's going on? Repeat. Repeat. The only consequences of quantum measurement should be refined expectations. What's going on?

Somewhere around 6:00, I said, "Aha, it's 'likelihoods' that are the consequences." I still don't really understand that, but somehow it was enough to let me finally fall asleep. But then it was almost time to get up.

24-03-06 *On Certainty, Quantum Outcomes, Subjectivity, Objectivity, and Expanding Universes, Part 1* (to R. Schack & C. M. Caves)

Amazing, how hard it is to write this paper and even discuss these things internally between the three of us. When we started this debate on certainty in August 2001, I had no grey hair; now

I've got lots of it. ... OK, maybe the real problem has been Lucent all along (which, as of this morning, may soon be Alcatel), but still it's been a tough paper to write. Luckily—and despite all that—I do think Rüdiger's outline/draft is pretty good. Major chunks of it, to my mind, are certainly very usable. It is a valiant attempt and certainly something I haven't been able to do.

But, there are some things in the present draft and in the latest discussion between you two that, I think, go in the wrong direction, or at least diverge from all the progress I thought we had made in confronting the Wigner's friend issue in December. Maybe it was illusory, but I don't want to think so.

So, let me try to get this on paper before having a phone conversation about it. (I know you two like to talk rather than write all this email; you find it more efficient. But I don't most of the time. I can pain over my sentences and pause as much as I want without getting ridiculed or sidetracked or rushed along, and that is pretty important for me.)

But even now, where to start?

Maybe a good place is these two sentences from Rüdiger (even if one or both are outdated by now ... and I think at least one of them is):

Schackcosm 91: *There is a* quantum *reason for certainty being subjective, which does not apply in the classical case.*

and

Schackcosm 92: *I agree, reluctantly. The category distinction doesn't buy as much as I hoped it would.*

I don't believe I had ever thought of it that way. I.e., that the category distinction between the elements of the event space and the probability function over the event space would ever 1) "buy us" something new—I only saw it as something that had to be emphasized to patch up a perceived inconsistency in our Bayesian position—, or 2) be such that there could be additional, specifically quantum reasons for invoking it.

The problem is, without a *philosophical* category distinction of some sort between the x and the $P()$ in $P(x)$—and not just the mathematical one—the Bayesian view of probability runs the risk of collapsing into a kind of solipsism or mysticism (as Howard Wiseman and Wayne Myrvold indeed, and sometimes Bill Unruh, seem to think it does). That risk is already there without quantum mechanics.

One of our main contributions, I thought, was in emphasizing (like no one else ever had) that this category distinction *must* be maintained even in the limiting case of $P(x) = 1$. Then, after doing so, we would leverage that to say the Penrose argument carries no weight after all. And partially this has been carried out in the draft.

But here's where I think things have gotten off track. I'm seeing language of this order traveling between the two of you: "certainty never implies a fact," "knowledge of a fact h can, however, compel the assignment $P(h) = 1$," "some probabilities are objective after all." More colloquially, at some far off meeting, I think I even heard someone say this, "this 'category distinction' business is a red herring." These may not be precise quotes, but I think they capture the sorts of phrases that are going around. And I think that is very dangerous business.

We had a clean point (or at least we were groping toward one); let's not lose it.

Part of the trouble, I think, is coming from derailed uses of the terms 'subjective' and 'objective' creeping back into this draft, and then starting a snowball effect. Let me try to expand on that. When I finish, I will come back to give specific replies to:

1) this thing that Carl said:

Cavesism 72 : *my concern is that the category distinction loses some of its force. The original idea, I believe, was to say that facts never determine beliefs (i.e., probabilities), either classically or in qm. We can still say that, but the important distinction between realism and qm will be different: in a realistic world, knowledge of facts can dictate delta probabilities, but in qm knowledge of facts cannot dictate quantum states. What do you think?*

2) this thing that Rüdiger said:

Schackcosm 93 : *And you should make the point in this way when you talk to Chris ... Let's say X is "up" in a spin measurement. Agents A and B agree about everything concerning the setup and the precise conditions under which you would call X true. The measurement is performed, and they both agree that X is true. Then $P(X) = 1$ for both of them. This P is objective.*

and 3) this thing Carl said:

Cavesism 73 : *I am now thinking that it would be a mistake to get into distinguishing "facts" from "knowledge of facts," since that is not the distinction we want to make. Rather we want to say that "knowledge of facts" can never compel a pure-state assignment, whereas it would seem that we are saying that "knowledge of facts" can compel a delta probability assignment.*

In all three cases I have a bone to pick. In 1), it is in hinging too much on the nonoperational term 'knowledge,' which I would like to banish in any case. In 2), it is in a definition of 'objective' that I do not like anymore, and I think runs into trouble or at least becomes confusing in the places where we need it most. And ultimately in 1) and 3), I think this is just the wrong distinction, and, in any case, it is not true.

— — — — — —

I'll go ahead and send you this much now, to show you that I wasn't lying when I said that I've been thinking of these things, but before going on, I've got to go home to satisfy an urge to try to catch some squirrels.

Recommended reading: Item #7, "Epistemology Probabilized," by Richard Jeffrey, at `http://www.princeton.edu/~bayesway/`.

27-03-06 *Block of Ice-Nine* (to C. M. Caves)

Cavesism 74 : *We should never have let Sipe get away with the ice-nine characterization of the Bayesian interpretation. He is right that we posit a changeless world of attributes that physics is trying to get at, but this is less than half the story. The rest of the story is the evolving universe in which we participate. He would say that the only dynamics that occurs is the dynamics of our beliefs (or the descriptions based on our beliefs), but there is a world out there, which we are trying to describe as best we can, and the evolution of our beliefs is our best attempt to capture what's happening in the world.*

Cavesism 75 : *It's a Vonnegut novel, in which all the water in the world follows a tiny seed into a solid thermodynamic state called ice-nine.*

Thanks for the definition.

I hate it when I can't pin down all my rantings. I remember one day going off on someone (maybe it was Howard Barnum or maybe Jeremy Butterfield, but probably someone completely different) about how the many-worlds conception is of a universe that's a big block of ice. And I became so tickled with that phrase that I just kept saying it over and over.

Rüdiger, if you remember, come to my rescue. I remember us talking about this sort of thing when we were talking about that guy's book, *The Paradox of Cause*.

Nothing more distasteful than a block-universe view.

Looking through my emails, I'm surprised by how little I've used the phrase "block universe"! And even more shocked that I couldn't find the phrase "block of ice." I was going to send you some examples ... but I see I've now got to go catch my plane.

Ice-nine, I like it.

> I go up and I go down when it comes to speaking the words gravity and quantum in the same sentence. At times I find myself thinking that general relativity and quantum mechanics express two absolutely incompatible worldviews. The general relativistic universe is a "block universe" in William James's sense: It's just there. One can talk about foliations and dynamics, etc., *within* the 4-manifold, but in the largest view—the view from nowhere—the world and all its history is just there. It is a universe without life (in the creative sense). In contrast, the quantum world strikes me as a malleable world—one that is still in formation, and in particular, one for which it is impossible to get such a "view from nowhere" (as Nagel would call it).

30-03-06 *The Sqrt Operation* (to W. K. Wootters)

What are you doing up so early?! I'm in Lisbon at the moment; so at least that's my excuse for reading your email at this time!

Woottersism 14: *You probably know the answer to this question. Consider any POVM. Depending on how one implements the POVM, the system being measured could end up in many different states, even for a specific outcome of the POVM. (I'm assuming that the system survives the operation.) One very easy case to consider, at least mathematically easy, is the case in which the final state of the system is given simply by $\sqrt{\Pi_j}$ applied to the original state, where Π_j is the POVM element. Is there any precise sense in which this particular implementation of a POVM is the* least disturbing *to the system's state? (I suppose there will have to be some sort of averaging over all the possible initial states. Some will be disturbed more than others.)*

Yeah, you've got it exactly right. If the input is promised to be a random pure state (wrt unitarily invariant measure), and the measure of disturbance is 1-fidelity between input and output, then the operation you describe for a fixed measurement is the least disturbing on average. This issue came up around 1996 and the proof was harder than one might think. Howard Barnum has a large bit about it in his PhD thesis. I think he finally published it here: `http://www.arxiv.org/abs/quant-ph/0205155`.

04-04-06 *Title* (to S. J. van Enk)

"Had We Found a Better Title, We Would Have Chosen It" [See 09-05-08 note "Change of Plan" to S. J. van Enk.]

19-04-06 *Church of the Smaller Hilbert Space* (to M. S. Leifer)

I love it!

Almost semi-seriously, it'd be nice if you'd hone that and present it in Växjö.

Matt's Beautiful Preply: "The Church of the Smaller Hilbert Space"

I can't believe I wasted a whole hour writing this thing, and I can't even post it on my office door because some god-fearing member of the public might see it.

TEN COMMANDMENTS OF THE CHURCH OF THE SMALLER HILBERT SPACE

With apologies to Charlie Bennett and, of course, God.

1. I am ρ, your state, who brought you out of wavefunction realism, the place of orthodox dogmatism.
2. Do not have any other states except Me. Do not represent states by false purifications, conceived as ontological states of the Platonic forms above, of reality below, or of the space-time foam underlying reality. Do not bow down to such states or worship them. I am ρ your state, a state that demands exclusive belief.
3. Do not announce your state ρ in vain. ρ will not allow the one who announces it in vain to go unpunished by a Dutch bookie.
4. Remember the CP-map $\mathcal{E}$ to keep dynamics meaningful. You can work things out using six different Kraus decompositions or Steinspring dilations and do all your tasks. But the CP-map $\mathcal{E}$ is an equivalence class to ρ your state. Do not do anything that attaches meaning to the arbitrary tools you choose to work with. This includes your Hilbert Space basis $\{|j\rangle\}$, your purifications $|\Psi\rangle$, your Kraus decompositions $\{E_j\}$, $\mathcal{E}(\rho) = \sum_j E_j \rho E_j^\dagger$, your Steinspring dilations U, $U^\dagger U = UU^\dagger = I$, your Naimark extensions P_j, $P_j P_k = \delta_{jk} P_j$, your path integral decompositions and your virtual particles. It is for perturbation theory calculations in QED that ρ is supplemented by the Feynman diagrams, the path integrals, the Greens functions, and all that is in them, but the true evolution of ρ rests on the CP-map. ρ therefore blessed the CP-map E and made it meaningful.
5. Honor your forefathers by using the Hilbert space algorithm they handed down to you to calculate your expected utilities. You will then live long on the land that ρ your state describes your beliefs about.
6. Do not commit murder, since there is no other "branch of the wavefunction" in which your victim will survive.
7. Do not adulterate the Schrödinger equation by adding nonlinear terms designed to cause collapse.
8. Do not steal from classical physics by insisting that particle position or field configuration variables must evolve deterministically.
9. Do not testify as a false witness to the existence of histories of events that do not appear in the empirical records.
10. Do not be envious of your neighbor's state σ. Do not be envious of your neighbor's dynamical CP-map $\mathcal{F}$, his POVM elements $\{N_j\}$, his update CP-maps $\mathcal{F}_j$, his Kraus operators F_{jk}, his donkey, or anything else that is your neighbor's, for they only describe his beliefs (except for the donkey), which naturally differ from yours.

20-04-06 *Always Quantumness!* (to M. Sasaki)

Thank you too for your other kind offer. With friends like you, I am spiritually wealthy! But do not break your back trying to realize an invitation for me if your budget is tight—certainly take care of important things first!

I hope things are going well for you scientifically. A couple months ago, I talked about our quantumness again, for the first time in quite a while. It was for the colloquium of the Applied Mathematics Dept. at Princeton University. It was quite well received, in fact, as generating some interesting questions in mathematics. In the audience there were Rob Calderbank (a coding theorist), Ingrid Daubechies (inventor of wavelets), Simon Kochen (a famous logician, particularly for the Kochen-Specker theorem in quantum mechanics), ... and even the super-famous John Conway ("Atlas of Finite Groups" and the cellular automaton "Life") and John Nash (Nobel Prize in economics). I'll place the abstract of the talk for you below.

Anyway, there are many things still to be done with the concept, and I hope that one day we will get a chance to work on it together again. If the world of quantum information picks up here at Bell Labs in the next year (after the merger), I hope that I can reciprocate on your kind invitation.

> Title: Math Problems from the Far Side of Quantum Information
>
> Abstract: The field of Quantum Information has recently rightly attracted great interest for the technological fruits it may bear. But there is a sect of its practitioners who think it stands a chance to bring us much more than that—namely, that its theoretical tools will give us a means for exploring what quantum mechanics is really all about and for settling some of the deepest problems in physics. The roots of this optimism come from a very old thought: that a quantum state has more to do with representing its user's information, than any inherent physical property of the system to which it is ascribed. What is new and nice is that quantum information teaches us how to formulate this idea precisely and even check its consistency. Nicer still for the mathematics community is the number of juicy mathematical problems the consistency-checking process poses. In this talk, I will review some of the history of this and then quickly settle on a sample problem that has been annoying me a lot lately: the question of the existence of symmetric informationally complete positive-operator-valued measures for finite dimensional Hilbert spaces. I'm not alone—it turns out to be equivalent to a 30-year-old problem in coding theory—but I will say some things about it that you may not have heard before.

20-04-06 *Church of the Big-Enough Hilbert Space, 1* (to M. S. Leifer & D. Bacon)

In contemplating the genius of Matt's commandments (and being especially motivated from watching the old Charlton Heston movie with my seven-year-old this weekend), I took a trip down memory lane tonight. I found the passage below in my *Notes on a Paulian Idea*:

> I had a very nice conversation with Charlie on the drive back to Wendell from your house, that, I think, has allowed me to sharpen what I think about things in quantum mechanics. In particular, I would like to work on a point of view that substitutes in place of the "Church of the Larger Hilbert Space," something along the lines of "Church of the Big-Enough Hilbert Space." However, it'll require some writing for me to get it down coherently.

It led to the question is big-enough bigger than smaller—maybe, maybe not: probably depends upon who you're kneeling with. Anyway, I hope it's bigger than empty.

On another subject, I've also been contemplating dumping another 500 pages onto `quant-ph` May 10 (for the fifth year anniversary of my last big dump). Experienced bloggers that you both are, what do you think of the idea? Do you think it goes too far, given my earlier foray?

20-04-06 *Church of the Big-Enough Hilbert Space, 2* (to D. Bacon)

Baconism 1: *Whoever first framed the words "Church of the Larger Hilbert Space" was a genius.*

Yes, John Smolin is a genius, and for many reasons.

Baconism 2: *I mean, it makes other interpretations just seem silly!*

But this point worries me. You do realize that Matt's manifesto, despite the humor is semi-serious? It captures many of the essential points of what he, Carl Caves, Rüdiger Schack, I and a few others, are trying to get at with our Bayesian view of quantum probabilities. Particularly take head of his second commandment:

> Do not have any other states except Me. Do not represent states by false purifications, conceived as ontological states of the Platonic forms above, of reality below, or of the space-time foam underlying reality. Do not bow down to such states or worship them. I am ρ your state, a state that demands exclusive belief.

He means it. And that statement is the antithesis of what Charlie Bennett and John Smolin are thinking when they invoke their church.

Thanks for endorsing another dump. I'll take your vote into account as I make this painful decision ... (One day I'll have to tell you about the advice William James once wrote to his brother Henry concerning the latter's hygienic habits.)

25-04-06 *My Title and Abstract* (to A. Y. Khrennikov)

I'm not sure what title I sent you before, but let us now go with the one below. It is a talk I gave at Princeton for their Applied Math Dept colloquium one week ago—I hope you don't mind if I recycle it, but I've run out of time for something new.

> Title: Math Problems from the Far Side of Quantum Information
>
> Abstract: The field of Quantum Information has recently rightly attracted great interest for the technological fruits it may bear. But there is a sect of its practitioners who think it stands a chance to bring us much more than that—namely, that its theoretical tools will give us a means for exploring what quantum mechanics is really all about and for settling some of the deepest problems in physics. The roots of this optimism come from a very old thought: that a quantum state has more to do with representing its user's information, than any inherent physical property of the system to which it is ascribed. What is new and nice is that quantum information teaches us how to formulate this idea precisely and even check its consistency. Nicer still for the mathematics community is the number of juicy mathematical problems the consistency-checking process poses. In this talk, I will review some of the history of this and then quickly settle on

a sample problem that has been annoying me a lot lately: the question of the existence of symmetric informationally complete positive-operator-valued measures for finite dimensional Hilbert spaces. I'm not alone—it turns out to be equivalent to a 30-year-old problem in coding theory—but I will say some things about it that you may not have heard before.

01-05-06 *Conversations with God* (to W. B. Case)

Attached are the two excerpts from my samizdat "Notes on a Paulian Idea" that I told you about last night.

From a 23 January 2000 note to Gilles Brassard, "I See Why Bit Commitment"

Let me just say that I had a bit of an epiphany in the shuttle bus at Dulles Airport: for the first time I have understood why you want to take *both* the EXISTENCE of secure key distribution and the NONEXISTENCE of bit commitment as pillars in your sought-after derivation of QM. You have been thinking more deeply than me since the beginning!

Let me place at the end of this note a little piece from my samizdat. [See letter to Greg Comer, 22 April 1999, titled "Fuchsian Genesis."] It sort of presents what I've been trying to get at in a dramatic way: it may be my best presentation of the idea of why quantum key distribution has something to do with the foundations of quantum mechanics. But more to the present point, let me tell you about a second way I use to get the point across. I've used this slide in a few talks. It consists of five frames with the following little story.

> In the first frame God starts to speak to Adam at a time just before Genesis, "Adam, I am going to build you a world. Do you have any suggestions?"
>
> **Adam**: Mostly I don't want to be alone. I want to have friends ... and enemies to spice things up ... and generally just plenty of people to talk to.
>
> **God**: Done. I'll give you a world populated with loads of other people. But you ask for a bit of an engineering feat when you ask to be able to talk to them. If you want to communicate, the world can't be too rigid; it has to be a sort of malleable thing. It has to have enough looseness so that you can write the messages of your choice into its properties. It will make the world a little more unpredictable than it might have been for me—I may not be able to warn you about impending dangers like droughts and hurricanes anymore—but I can do that if you want.
>
> **Adam**: Also God, I would like there to be at least one special someone—someone I can share all my innermost thoughts with, the ones I'd like to keep secret from the rest of the world.
>
> **God**: Now you ask for a tall order! You want to be able to communicate with one person, and make sure that no one else is listening? How could I possibly do that without having you two bifurcate into a world of your own, one with no contact whatsoever with the original? How about we cut a compromise? Since I'm already making the world malleable so that you can write

your messages into it, I'll also make it sensitive to unwanted eavesdropping. I'll give you a means for checking whether someone is listening in on your conversations: whenever information is gathered from your communication carriers, there'll be a reciprocal loss in what you could have said about them otherwise. There'll be a disturbance. Good enough? You should be able to do something clever enough with that to get by.

> **Adam**: Good enough!

> **God**: Then now I'll put you in a deep sleep, and when you awake you'll have your world.

> **Adam**: Wait, wait! I overlooked something! I don't want an unmanageable world, one that I'll never be able to get a scientific theory of. If whenever I gather information about some piece of the world, my colleagues lose some of their information about it, how will we ever come to agreement about what we see? Maybe we'll never be able to see eye to eye on anything. What is science if it's not seeing eye to eye after a sufficient amount of effort? Have I doomed myself to a world that is little more than chaos as far as my description of it goes?

> **God**: No, actually you haven't. I can do this for you: I'll turn the information-disturbance tradeoff knob just to the point where you'll still be able to do science. What could be better? You have both privacy and science.

> So Adam fell into a deep sleep, and God set about making a world consistent with his desires. And, poof(!), there was QUANTUM MECHANICS.

That's the tale. But now I see the crucial spot of outlawing bit commitment within it. God could have supplied Adam with a set of impenetrable boxes (and keys to open them) where he could place his information whenever he wanted some secrecy. A bit commitment protocol could certainly be used in that secondary fashion. But God chose to make all information open for all the world to see: he just left the possibility of an imprint whenever someone has a look.

From a 22 April 1999 note to Greg Comer, "Fuchsian Genesis"

> In the beginning God created the heaven and the earth. And the earth was without form, and void; and darkness was upon the face of the deep. And the Spirit of God moved upon the face of the waters. And God said, Let there be light: and there was light. And God saw the light, that it was good; and God divided the light from the darkness. And God called the light Day and the darkness he called Night. And the evening and the morning were the first day. ... [Day 2], [Day 3], [Day 4], [Day 5] ... And God saw everything that he had made, and behold, it was very good. And there was evening and there was morning, a sixth day. Thus the heavens and the earth were finished, and all the host of them.

But in all the host of them, there was no science. The scientific world could not help but STILL be without form, and void. For science is a creation of man, a project not yet finished (and perhaps never finishable)—it is the expression of man's attempt to be less surprised by this God-given world with each succeeding day.

So, upon creation, the society of man set out to discover and form physical laws. Eventually an undeniable fact came to light: information gathering about the world is not without a cost. Our experimentation on the world is not without consequence. When *I* learn something about an object, *you* are forced to revise (toward the direction of more ignorance) what you could have said of it. It is a world so "sensitive to the touch" that—with that knowledge—one might have been tempted to turn the tables, to suspect a priori that there could be no science at all. Yet undeniably, distilled from the process of our comparing our notes with those of the larger community—each expressing a give and take of someone's information gain and someone else's consequent loss—we have been able to construct a scientific theory of much that we see. The world is volatile to our information gathering, but not so volatile that we have not been able to construct a successful theory of it. How else could we, "Be fruitful, and multiply, and replenish the earth, and subdue it?" The most basic, low-level piece of that understanding is quantum theory.

The *speculation* is that quantum theory is the unique expression of this happy circumstance: it is the best we can say in a world where *my* information gathering and *your* information loss go hand in hand. It is an expression of the "laws of thought" best molded to our lot in life. What we cannot do anymore is suppose a physical theory that is a direct reflection of the mechanism underneath it all: that mechanism is hidden to the point of our not even being able to speculate about it (in a scientific way). We must instead find comfort in a physical theory that gives us the means for describing what we can *know* and how that *knowledge* can change (quantum states and unitary evolution). The task of physics has changed from aspiring to be a static portrait of "what is" to being "the ability to win a bet."[107]

This speculation defines the large part of my present research program.

09-05-06 *The Bayesian Big Bang* (to H. Barnum)

Wow, that's some report! Thanks for sending me all that. It sounds like I really did miss a thought-provoking time.

I wouldn't know what to suggest to put in a note to Albert.

Barnumism 16: *Incidentally, the discussion with Albert ended with him saying "so you don't believe parts of the early universe do work on other parts even though there is no subject who is using its knowledge to extract work. . . " I was rather tired, so called a pause there and will deal with that later. I think it can be dealt with, the main point being that WE understand parts of the early universe as doing work on other parts [. . .], perhaps, because OUR KNOWLEDGE of those parts is the same kind of knowledge (canonical ensemble at some temperature, blah-de-blah) that would allow us, given large enough apparatus etc. . . , to extract that much work. . . because some of the processes that actually go on in the early universe are (I guess. . .) similar to various kinds of thermodynamic processes of putting stuff in pistons and expanding, etc. [. . .] [N]evertheless I believe this is basically right. . . our description of early-universe processes in thermodynamic terms is because we have the same kind of knowledge about it that we have about gases and liquids being boiled and compressed etc. . . "The radiation background was at XX degrees Kelvin when it decoupled from matter" is a statement of what we think we know about it, not a property of its microstate at the time.*

[107]The nice phrase "physics is the ability to win a bet" is due to J. R. Buck (a grad student at Caltech) circa 19 February 1999.

I've told you my wacky idea that for all Bayesians there must be a big bang, haven't I? In case not, the idea is this. Consider my beliefs about yesterday's events. If we were to lump all of them into a big joint probability distribution, it would have some entropy. Now consider my beliefs about the events the day before that. They too could be lumped into a distribution. However, since I'm probably less sure about the things that happened the day before last than I am about the things that happened yesterday, the latter distribution should have more entropy. And so on we could go further back in time. Assuming I lose effectively all predictability as I conceptually reach back to some finite time (based on the size of my brain and my processing capability), I should end up with a distribution of effectively infinite entropy. Associating (somehow) this distribution with one of canonical form—that's one of the hard steps—I find an effectively infinite temperature in the finite past.

Just another way of saying the universe came into existence (at least with regard to my ability to extract work from it).

Homework Problem: Given reasonable models of a typical brain's inferential powers and the resolution of human senses, estimate the number of years since the big bang.

17-05-06 *Beautiful Passage* (to G. L. Comer)

Most of my work tonight though has been on going through your files—I'm up to December 2002 at the moment, so there's still a long way to go. But it's been very enlightening to read our correspondence between each other again. And so nice to read your poems again. I feel like there's so much I want to tell you—about all the clarifications that have come to me about the transformation rules in quantum mechanics in the last year, about the disanalogy I start to see with relativity (relativity connects disparate observers, the transforms in quantum mechanics connect the gambles one single observer is willing to make, much like Dutch-book consistency), and about how so much of these thoughts probably find their origins in the things you were bringing to my attention in 2002. But all of that is going to require new notes, not old! So you have to wait old friend.

19-05-06 *Spring Is in the Air . . . MUBs Are in the Air* (to W. E. Lawrence)

It dawned on me today that it was about this time last year that you visited us and gave us a talk about MUBs and phase space. There must be something about the time of year! As it turns out Bill Wootters will visit us next week, and give the talk below. What is it about the Spring?

Hope all is well with you.

> RESEARCH TALK –
>
> Phase Space for Qubits
> Prof. William K. Wootters
> Williams College
>
> Wednesday – May 24, 2006
> 11:00 AM
> MH 1D-224
>
> Abstract:
> For a single particle moving in one dimension, the particle's quantum state can be rep-

resented as a real function of position and momentum, the Wigner function, which has certain properties that make it useful for state reconstruction. In this talk I show how a system of binary quantum objects (qubits) can be represented by a closely analogous function, a discrete Wigner function, on a discrete phase space. The two axis-variables of the discrete phase space, that is, the analogs of position and momentum, are associated with two "conjugate" bases of the state space, and are labeled by the elements of a finite field (in the algebraic sense of "field"). The use of a finite field is by no means arbitrary: one finds that the structure of the field accords remarkably well with certain features of the complex-vector-space structure of quantum mechanics.

01-06-06 *Cirac Has Been Deservedly Awarded* (to M. Pérez-Suárez)

Thanks for the letter. I'm sorry to hear that staying in the US put you in a funk, though. Ignacio is a great guy; it is great to hear he has won such an award. I talked to him for almost three hours at the APS March meeting, mostly about de Finetti theorems and SIC-POVMs, and it was such an exhilarating experience. He is lightning fast and simply understands everything almost immediately.

Congratulations too on your book. Thanks so much for sending me a copy. I'll put it with pride on my shelf (even if I cannot read it).

We will miss you in Sweden. I hope it is a productive meeting and actually expect it will be. I will talk on recent things Bill Wootters and I have discovered on SIC-POVMs—that they are not all unitarily or anti-unitarily equivalent—also, I will frame better what I mean by saying that the Born rule for calculating quantum probabilities is really a kind of transformation rule (along the lines of a de Finettian coherence principle). It would have been nice to have your critical input at the talk.

Send me your thesis as it comes together.

01-06-06 *Critical Paper* (to J. Bub)

Thanks for sending the paper! It looks interesting. I've just printed it out and will be taking it with me to Sweden tomorrow. I'll send you my comments hopefully early in the week.

I didn't think there was any real difference between the Ramseyan and de Finettian views of probability (other than perhaps emphasis). There may, however, be a difference between how you and I think of "truth"—particularly in the quantum context. I'll flesh out my thoughts to you just as soon as I can and give you feedback on your paper.

BTW, I've also printed out your most recent paper on quantum computation from quant-ph to take with me. That looks very exciting.

02-06-06 *Enjoyable Paper* (to J. Bub)

As I wrote you earlier, I'm on my way to Sweden today (though I didn't tell you about how roundabout of a way I'm taking). Anyway, I'm in Chicago at the moment and will board the next plane soon.

But I wanted to tell you that I used the first flight to read, take notes on, and contemplate your paper. It was quite enjoyable. And certainly I'm glad to see you making a movement in the Bayesian direction!

If I had known about your thinking along these lines, I would have certainly invited you to this meeting. With the funding I had, I put together what I've been calling the "Swedish Bayesian Team" (Schack, Leifer, Appleby, Pitowsky, and Caticha), so that hopefully we'll have a very focused discussion on some of the technical aspects of this subject (as opposed to the impasse that I mostly viewed the Konstanz meeting as). You would have been a great contributor.

Anyway, as a proxy for not having you physically present, I'll be writing you this week. I have plenty of comments and queries about your paper.

13-06-06 *Little Chocorua* (to L. Simon)

I am a research physicist at Bell Labs doing quantum information theory and quantum foundations for a living, but have a great side interest in William James and pragmatism in general. I even pride myself—probably incorrectly—on having the largest personal library on pragmatism in New Jersey! (About 420 volumes.)

Anyway, I write to you because it dawned on me that you may be able to help me in a silly little quest. My wife is constructing a playhouse for our daughters from 12 doors she recovered from an 1896 house being demolished near our home. (Our own house is an 1896 Victorian, and the coincidence of the years gave her this clever idea.) Of course it's one more door than James' home in Chocorua had, but nonetheless we've decided to call the playhouse "Little Chocorua"—the kids seem to like the name and we've run with it.

This is where you come in: Do you know of any good sources of pictures of the original Chocorua home? Or do you perhaps have any of your own that you have scanned into your computer? We're hoping to get ideas on how to give the playhouse some final touches so it fits its name even better.

The only picture of the Chocorua home that I have ever seen comes from your book. (Wonderful book, by the way; I very much enjoyed it.) But I also know from an interview with you on the web that I read a few months back, that you have physically visited the home. Perhaps you took some pictures then.

Sorry to bother you with such a strange request … but I thought it might be novel enough that you would be interested in helping us out.

P.S. If you are interested in seeing how some of James's idea strike me as having an intimate connection with quantum foundations issues, download the big document posted at the bottom of my webpage titled "Cerro Grande II". Then simply do a search on "William James" within it, and maybe you'll find an interesting idea or two. There is a link to my webpage below.

Linda's Reply

> Dear Christopher Fuchs, your playhouse sounds wonderful, but I don't have photos. I was able to see the house because it was on the market at the time (Janice Hamel was the realtor) and I think it has since gone on the market again. Through the years, owners made lots of updates. It may be that the historical society, which I think is housed in the Chocorua Public Library, might be able to come up with some historical photos. Or maybe the realtor has some from when she handled it.
>
> Thank you so much for your interest in my book and, of course, in James. I DO think you have a record number of books on pragmatism.
>
> Good luck with the playhouse!

19-06-06 *Report* (to Fellowships)

This is an excellent proposal and I wholeheartedly recommend that it be funded. Recent advances in quantum information theory have made it quite respectable within the physics community to view quantum states as representing agent-centered information about quantum systems, rather than as intrinsic physical properties for those systems. Concomitant with these advances has been a reawakening in the philosophy-of-physics community for the need to study and develop philosophical systems that can support such an idea. Pragmatism in one guise or another (Jamesian, Deweyian, Putnamian) seems to fit the bill, and there has been a flurry of activity in this direction. Particularly nice so far has been the work of Prof. Michel Bitbol. I see the present proposal [of a younger researcher] as a continuation down this line. Novel to this approach is a melding of pragmatic and Kantian (or transcendental) lines of thought—this sort of work is long overdue, and I give it my highest recommendation. It will have direct impact on the philosophical discussion of quantum foundations and direct impact on theoretical methods in quantum information science.

10-06-06 *Title and Abstract—Special Treat* (to G. M. D'Ariano)

I apologize for not getting the title and abstract to you yesterday as I had promised: From my hotel in Copenhagen, I could not get a connection to the internet (something to do with my wireless "location profiles list" being full, and I could not figure out how to fix it). Now, though, I am in the Chicago airport, and can send you mail by my usual routes.

I know it was a further delay, but at least it allowed me the time to come up with something of a new abstract for you. The result is below. If you feel that your name ought properly to be in the list with the other guys, feel free to take out the part "(maybe D'Ariano?)" and replace it with your name in the earlier list right after Caves (alphabetical order).

I'm looking forward to much better understanding your axiom system. It looks indeed very promising—I am quite excited. This is going to be a great trip.

I will take care of the whatever needs to be done in the other emails you wrote me, right after the weekend.

> **Title:** Where Is the Reality in a Bayesian View of Quantum Mechanics?
>
> **Abstract:** In the neo-Bayesian view of quantum mechanics that Appleby, Caves, Pitowsky, Schack, the author and others (maybe D'Ariano?) are developing, quantum states are taken to be compendia of partial beliefs about potential measurement outcomes, rather than objective properties of quantum systems. Different observers may validly have different quantum states for a single system, and the ultimate origin of each individual state assignment is taken to be unanalyzable within physical theory—its origin, instead, ultimately comes from probability assignments made at stages of physical investigation or laboratory practice previous to quantum theory. The objective content of quantum mechanics (i.e., the part making no reference to observers) thus resides somewhere else than in the quantum state, and various ideas for where that "somewhere else" is are presently under debate—there are adherents to the idea that it is purely in the "measurement clicks," there are adherents to the idea that it is in intrinsic, observer-independent Hamiltonians, there are adherents to the idea that it is in the normative rules quantum theory supplies for updating quantum states, and so on. This part of the program is an active area of investigation; what is overwhelmingly agreed upon is only the opening statement of this abstract—that quantum states are

compendia of beliefs. Still, quantum states are not simply Bayesian probability assignments themselves, and different representations of the theory (in terms of state vectors or Wigner functions or C^*-algebras and the like) can take one further from or closer to a Bayesian point of view. It is thus worthwhile spending some time thinking about which representation might be the most propitious for the point of view and might, in turn, carry us the most quickly toward solutions of some of the open problems. In this talk, I will explore various issues to do with the above and explain why I prefer a representation of quantum mechanics that makes crucial use of a single probability simplex.

12-06-06 *Grover's Alg and Temporal Bell Inequalities* (to L. K. Grover)

By the way, I met a really interesting young man in Sweden, who gave an excellent talk on some ideas surrounding your algorithm. Here is the paper he presented:

Title:	Information-theoretic temporal Bell inequality and quantum computation
Author:	Fumiaki Morikoshi
Journal-ref:	Phys. Rev. A **73**, 052308 (2006)

It's on the archive at: `http://www.arxiv.org/abs/quant-ph/0602011`.

The basic idea is that he shows that your algorithm violates a certain kind of temporal Bell inequality. Further he speculates that something like this might be at the heart of the speedup in all quantum algorithms. It was certainly a thought provoking thought.

Anyway, I think he is very interesting and amiable, and he was very articulate in English too for a Japanese. Also, he was very keen to talk and work with you. If you want to see his full list of publications, go here: `http://arxiv.org/find/quant-ph/1/au:+Morikoshi/0/1/0/all/0/1`.

If you have any money left for visitors, I think this guy could be a winner for you. His excitement was so high for visiting you and Bell Labs, that I suspect he would jump at the chance, even if you could only give him partial support.

21-06-06 *Hoan Dang* (to A. P. Ramirez)

Thanks. At the moment, I'm planning to have Hoan work on one of these Wigner-function type representation of quantum mechanics, to see where the power of quantum computing comes from. There have recently been some encouraging results along these lines. See for instance,

- `http://www.arxiv.org/abs/quant-ph/0506222`
- `http://www.arxiv.org/abs/quant-ph/0405070`.

Also, I'd like to see if we can translate those results into the particular q-function-style representation that I've been working on the last year. There, the key issue wouldn't be negativity, but something else.

It all goes back to a mostly forgotten point made in Feynman's original paper on quantum computation—I think there's a lot of life in the idea and everyone has gotten carried away talking about entanglement and multiple universes.

Anyway, some meaty mathematical problems, and I'll probably try to use his programming skills to simulate a lot of things. (Mabuchi at Caltech wrote me some very good things about his abilities.)

22-06-06 *Triangles* (to H. C. von Baeyer)

I was just taking care of some administrative things to do with the Växjö conference, and it dawned on me that I haven't yet written you since the conference's end. I was surprised to find you gone already by the Wednesday. I hope you didn't become ill or something. It was also too bad to see you gone especially as I think you missed three of the most interesting talks of the meeting: Bengtsson, Leifer, and D'Ariano. For your reference, here are the two papers associated with Leifer's and D'Ariano's talks:

- `quant-ph/0606022`
  Title: Quantum Dynamics as an Analog of Conditional Probability
  Author: M. S. Leifer

- `quant-ph/0603011`
  Title: How to Derive the Hilbert-Space Formulation of Quantum Mechanics From Purely Operational Axioms
  Author: Giacomo Mauro D'Ariano

The D'Ariano construction, to me, really starts to smell right from the Paulian perspective—so, I'm quite pleased with it. It still could use a good bit of Bayesianization, but the mathematics, I think, is starting to get in place. Saturday, I'm off to Italy for a little meeting that D'Ariano put together; it'll be another good opportunity to delve into these things.

Below, I'll paste in the fragment of the note I started to write you in March. One of these days, I should finish that up.

I hope all is well with you, and that you got something out of your Växjö experience.

— — — — — — — — — — — — — — — —

> Dear Hans,
>
> I hope you're better by now. I'm between meetings once again: Last Friday I got back from the APS March meeting in Baltimore, and this coming Monday I zip off to Portugal.
>
> While in Baltimore at dinner one evening, as we were waiting for our crabs, somehow your name came up, and I ended up drawing your B3 diagram:
>
> **von Baeyerism 19:** *The "B3 triad" is a triangle with the words psi, information, and probability at its vertices, and the names Bayes, Born, and Bohr, respectively, along their opposite sides.*
>
> (though I accidentally inverted the vertices and sides).
>
> I was trying to explain why I thought the B3 idea was pretty catchy. However, as we—that is, Rob Spekkens, Matt Leifer, Carl Caves, Hideo Mabuchi, and I—discussed it, I found myself slowly morphing the content of your diagram to better fit my predilections. Here was the result, which was left on a sheet of butcher paper in Obrycki's Crab House:
>
> > A triangle with the names Bayes, Pauli, and Gleason at the vertices, and $p(x)$, "contextuality", and $|\psi\rangle$ along the opposite sides.
>
> It won't be as catchy and memorable, I know (what small fraction of physicists have ever heard of Gleason, and what even smaller fraction have ever heard of "contexuality"?). But let me explain the reasons.
>
> At first it was just that I was dissatisfied with ...

24-06-06 *Notes on "What are Quantum Probabilities"* (to J. Bub)

ABSTRACT: Jeff says, "Chris Fuchs presents a different analysis of the status of the projection postulate as Bayesian updating, associated with a very different account of quantum probabilities as degrees of belief than the view I want to argue for here. . . . But this rather complicated analysis misses the essential point." Chris says, "No. It may not be perfect, but it captures the essential point it was meant to capture. It just so happens that that essential point is different from the one Jeff wants to make."

Abstract

Bubism 5: *I distinguish between the measurement problem of quantum mechanics, which is a problem about truth, and a problem about probabilities.*

I like that strategy and think it is an important step forward. It has been my strategy since 1995, though I didn't know it in these terms then. In July 2001, I learned a little about James, Dewey, and pragmatist theories of truth, and realized that that's what I had been on about with respect to quantum measurement. But I started to take the Bayesian step toward quantum probabilities long before that.

At this stage, I doubt very much that you'd be happy to think that you might ultimately be led to a pragmatist notion of truth (in the quantum measurement setting at least), but the distinction you make is—in my eyes—the first step toward it. In any case, there is certainly nothing to be lost by starting to make a distinction between the "issue of quantum measurement" and the "problem of quantum-state collapse." I will certainly admit that the former is not refined or understood well enough for my own tastes at this stage, but I think worrying about the latter anymore is just a waste of time—I think the evidence is simply overwhelming.

Here's the way I put the distinction of the concepts to Oliver Cohen in December 2003. I will quote the passage at decent length because it helps set the stage for the particular points I want to make with regard to your own paper.

> I'm writing you because I've been reading your paper "Classical Teleportation of Quantum States" this week. It's a nice paper, and I very much like the simplicity of your scheme and the point you make with it. I am in complete agreement.
>
> In fact it took me a little down memory lane. You see, Asher Peres and I had used teleportation as an example in our March 2000 *Physics Today* article, "Quantum Theory Needs No 'Interpretation'," precisely to illustrate the sensibility of the conception of a quantum state as a "state of knowledge, rather than a state of nature". When the paragraph peaked in clarity (i.e., before the editor's knife), it went like this:
>
>> The peculiar nature of a quantum state as representing information is strikingly illustrated by the quantum teleportation process. In order to teleport a quantum state from one photon to another, the sender (Alice) and the receiver (Bob) need a pair of photons in a standard entangled state. The experiment starts when Alice receives another photon whose polarization state is unknown to her, though known to some preparer in the background. She performs a measurement on her two photons, and then sends Bob a classical message of only two bits, instructing him how to reproduce the unknown state on his photon. This economy of transmission appears remarkable because to completely specify the state of a photon, namely one point in the Poincaré

> sphere, we need an infinity of bits. However, the disparity is merely apparent. The two bits of classical information serve only to transfer the preparer's information, i.e., his *state*, to be from describing the original photon to describing the one in Bob's possession. This can happen precisely because of the previously established correlation between Alice and Bob.

… The conclusion you draw, I think, is particularly important: the phenomenon of quantum teleportation only looks surprising and remarkable if one takes an ontic view of the quantum state. In fact, in the past, I have accused some of my friends (some of whom were authors on the original teleportation paper) of sticking with an ontic interpretation of the quantum state precisely because it is the only way to keep the phenomenon surprising and newsworthy. …

If you are interested in seeing the struggle Asher and I had in constructing the paragraph above see … Maybe the main lesson in those discussions is how difficult it is to give up an objectivist language when using quantum states, even for a recalcitrant positivist like Asher, and even in an example intended to be illustrative of why quantum states should be viewed as states of knowledge, rather than states of nature. (The philosophy being: if quantum mechanics looks too very mysterious, then you're probably being wrong-headed about it. Case in point: if teleportation looks mysterious, then you're probably being wrong-headed about it too.)

The sense I get from your paper is that you are much more neutral about the lesson than I am. You say simply: "[O]ur classical version of teleportation is just as impressive as the original protocol, if we think of quantum states as representing states of knowledge. … If, on the other hand, we think of a quantum state as having ontological content, …, then our classical version of teleportation is not equivalent to the quantum case," and leave it at that. However, there is a spate of evidence starting to come out that a significant fraction of some of the most 'remarkable' phenomena in quantum information theory can be mocked up with classical toy models just as your own. The only requirement for seeing it is that one must focus on the epistemic states (i.e., the states of knowledge) in such models rather than the ontic states (like the actual H or T in your own model). For instance, Rob Spekkens has a toy model which he has presented in several conferences and which he is writing up presently as a paper, "In Defense of the Epistemic View of Quantum States: A Toy Theory," in which he can reproduce the following quantum mechanical and quantum information-theoretic type phenomena in a pretty NON-REMARKABLE way: the noncommutativity of measurements, interference, a no-cloning theorem, … and many others. (In particular, he gets teleportation too, just like you do.) As Rob puts it in his abstract:

> Because the theory is, by construction, local and non-contextual, it does not reproduce quantum theory. Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. … The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

So, given that your paper is an independent and particularly notable link in that, and as opposed to his paper, your result is not buried within over 70 pages (and counting) of text, I very much endorse it. I think the lesson is this: A good lot of quantum information theory is simply regular probability theory and information theory applied in ways that had not been deemed interesting before. *What is interesting and unique to the quantum itself, thus, must be something else.*

In my paper `quant-ph/0205039`, "Quantum Mechanics as Quantum Information (and only a little more)," I tried to give the community to a call to arms by saying this:

> This, I see as the line of attack we should pursue with relentless consistency: The quantum system represents something real and independent of us; the quantum state represents a collection of subjective degrees of belief about *something* to do with that system (even if only in connection with our experimental kicks to it). The structure called quantum mechanics is about the interplay of these two things—the subjective and the objective. The task before us is to separate the wheat from the chaff. If the quantum state represents subjective information, then how much of its mathematical support structure might be of that same character? Some of it, maybe most of it, but surely not all of it.
>
> Our foremost task should be to go to each and every axiom of quantum theory and give it an information theoretic justification if we can. Only when we are finished picking off all the terms (or combinations of terms) that can be interpreted as subjective information will we be in a position to make real progress in quantum foundations. The raw distillate left behind—minuscule though it may be with respect to the full-blown theory—will be our first glimpse of what quantum mechanics is trying to tell us about nature itself.

What your work and Spekkens' work does, from my perspective, is give the best illumination yet of what I was hoping for when I was speaking of "combinations of terms" in that passage. Teleportation—being a certain combination of uses of the axioms of quantum mechanics—is nevertheless a purely probabilistic or information-theoretic effect. As such, it tells us very little about the ontology behind quantum mechanics.

My own view—and the thrust of my research program presently—is that these examples help us to realize that what is unique in quantum mechanics is not the probabilities (i.e., the quantum states) but what the probabilities are applied to. There, I think, lies the essence of quantum mechanics: It is localized in the Kochen-Specker theorem. "Unperformed measurements have no outcomes," as Asher Peres likes to say. That is to say, where quantum mechanics gets its uniqueness is from *breaking* with the old idea that a probability (as a subjective state of knowledge) must be knowledge about a pre-existent reality. Instead, probabilities can just as fruitfully be applied to capturing one's knowledge of "what will come about due to one's actions." The predominant issue becomes how to formalize the difference between probability theory as applied to pre-existent facts and probability theory as applied to "creatables" (for want of a better word). ...

Bubism 6: *I show that the projection postulate can be interpreted as a probability updating rule, and I argue for a subjective Bayesian interpretation of quantum probabilities as rational degrees of belief, in the sense of Ramsey rather than de Finetti.*

I would certainly call it a probability update rule too—so, if I have missed an "essential point" it is certainly not that. On the other hand, I think you're hoping that there is an essential distinction between Ramsey and de Finetti, where as far as I can tell there is none. However I'll have much more to say about both of these subjects later.

Introduction

From Classical to Quantum Mechanics

Bubism 7: *Faced with the conceptual puzzles of quantum mechanics, there is a temptation to begin with a blank slate. It seems, then, that if one were only careful enough with implicit assumptions about physical theory and measurement, the characteristic features of the theory could have been foreseen before classical mechanics.*

I think this view is entirely mistaken.

I am in agreement with your last sentence. Attempts like the former effectively erase the empirical content (or contingency) of quantum mechanics, and I just don't see that. When I myself invoke the slogan "Quantum Mechanics is a Law of Thought," I only do it partially tongue in cheek, quickly correcting myself. For if quantum mechanics were *only* law of thought, it would be like one of Kant's a priori categories of the understanding. On the other hand, I come dangerously close to viewing "probability theory" (i.e., the theory of coherent gambling) in such a Kantian kind of way. And, indeed, it is partially because of this that I would not want to view quantum mechanics as a *generalized* probability theory.

Bubism 8: *The transition from classical to quantum mechanics involves replacing the representation of properties as a Boolean lattice, i.e., as the subsets of a set, with the representation of properties as a certain sort of non-Boolean lattice.*

The? I would rather say *one possible way of looking at* the transition from classical to quantum mechanics involves blah, blah, blah. And, you partially recover from this a few paragraphs later where you write:

Bubism 9: *Of course, other ways of associating propositions with features of a Hilbert space are possible, and other ways of assigning truth values, including multi-valued truth value assignments and contextual truth value assignments. Ultimately, the issue here concerns what we take as the salient structural change involved in the transition from classical to quantum mechanics, and this depends on identifying quantum propositions that take the same probabilities for all quantum states.*

But let me hang on this point for a moment despite your partial recovery. For when you say things like, "Fuchs misses the essential point," you should realize that that judgement (at most) comes from within a context very different from the one I am working in.

I would, for instance, never say "the representation of properties in quantum mechanics involves as a certain sort of non-Boolean lattice." That is just not the context I'm working in. Similarly, I would not say, as you say in the next section, "Somehow, a measurement process enables an indeterminate property, that is neither instantiated nor not instantiated by a system in a given quantum state, to either instantiate itself or not with a certain probability."—i.e., I would not say that a measurement process instantiates any *property* at all for a quantum system.

Instead, the setting for our quantum Bayesian program (i.e., the particular one of Caves, Schack, and me), is one where all the *properties* <u>intrinsic</u> to a quantum system are timeless and have no dynamical character whatsoever—moreover, those properties have nothing to do with particular quantum state assignments or particular quantum measurement outcomes. In that way, the idea of a non-Boolean lattice simply doesn't apply to them.

John Sipe recently made a nice write-up of our view for his book that, I think, brings this one difference between you and me into pretty stark relief. Maybe it's worthwhile to quote it at length, as it may lay the groundwork for a good bit of our later discussion:

> This interpretation shares some features with operationalism. *Measurements*, for example, are understood in a manner close to that adopted by an operationalist. They are characterized by POVMs, and those abstract elements are associated with tasks in the laboratory undertaken with gadgets that are part of the primitives of the theory. The result of any such measurement is simply one of a possible number of outcomes, and there is no talk of these measurements "revealing" the value of any variable, in the sense that an arbitrarily precise position measurement in classical mechanics is often described as revealing the position of a particle. Yet, compared to the operationalist's quiet, unassuming terminology of "tasks" and "outcomes," advocates of this interpretation adopt a more active manner of speaking, referring to "actions" (or even "interventions") undertaken by an agent, and the "consequences" that those actions elicit.
>
> This indicates a role for the observer (or agent) in this interpretation that is more significant than the role played by such a person in operational quantum mechanics. The significance of that role becomes clear when we consider the reference of density operators in this interpretation. Density operators do not refer to sets of tasks that define *preparations*, as they do in operational quantum mechanics. Rather, a density operator is taken to encode the beliefs of an agent concerning the probabilities of different consequences of possible future actions. While these beliefs may be *informed* by knowledge of the tasks involved in setting up the particular gadgetry associated with a preparation, they are not *determined* by it. Hence there is not a unique, "correct" density operator necessarily associated with each preparation procedure, as there is in operational quantum mechanics. In the present view two different researchers, one more skilled in quantum mechanics than the other, could adopt different density operators after being identically informed of the details of a particular preparation procedure. One density operator might be more successful than the other in predicting the possible consequences of future actions, but each would be the correct density operator *for that agent* insofar as it correctly encoded that agent's beliefs.
>
> Thus, while the abstract elements in the theory associated with measurements are identified with tasks in the laboratory, as in operationalism, the abstract elements in the theory associated with preparations are identified with beliefs of the agent, signaling a kind of empiricist perspective.
>
> So in contrast to operational quantum mechanics, where density operators are necessarily updated following a measurement — since the combination of the previous preparation and the measurement constitutes a new preparation, and an operationalist associates the new density operator with *that* — in this view there is no necessary updating of a density operator in the light of measurement outcomes, since there is no *necessary* connection between the consequences of an agent's action (more prosaically, "measurement outcomes") and his or her beliefs. After all, foolish researchers, like foolish men and women more generally, could choose not to modify their beliefs concerning the consequences of future actions despite their knowledge of the consequences of recent ones. And note that even wise researchers will not update their beliefs concerning future actions until they *know* the consequences of recent ones; hence a wise researcher's

"personal density operator" (the only kind of density operator there is in this view!) will not change until that researcher is actually aware of a measurement outcome.

Other abstract elements in the theory, such as the dimension of the Hilbert space, and the dimensions of various factor spaces, are actually associated with instances of attributes of physical objects. Hence with respect to the reference of *these* abstract elements this interpretation is realist. The manner in which this works can best be seen by first reviewing the role measurement outcomes play in revealing aspects of the universe in realist classical mechanics, and then comparing that with the role such outcomes play in this interpretation of quantum mechanics.

An arbitrarily precise position measurement of a bead moving along a wire, in realist classical mechanics, reveals the position of the particle, the instance (say, $x = 10$ cm) of a particular attribute (bead position) of a physical object (bead) that actually exists in Nature. In contrast, a usual Stern-Gerlach device oriented along the z direction *does not*, in this interpretation of quantum mechanics, reveal the z-component of angular momentum, or for that matter anything else. The particular outcome of one experimental run is simply a consequence of performing the experiment. Nonetheless, repeated experimentation *does* reveal that the electron associated with the atom passing through the device should be taken as a spin-1/2 particle. Here the attribute under consideration is taken to be *internal angular momentum*, and the instance – the irreducible representation appropriate to the particle of interest – *spin-1/2*. The role of an "instance of an attribute" in this interpretation is not to specify one of a number of possible expressions of existence, as it is in realist classical mechanics, but rather to specify one class of possible beliefs – the one that the theory recommends – about the consequences of future interventions of a particular type.

Note that, at least within nonrelativistic physics, the instances of the attributes in this interpretation are fixed. A spin-1/2 particle remains a spin-1/2 particle. Thus there are no dynamical variables in this theory, only nondynamical variables analogous to the mass of a particle in nonrelativistic classical mechanics. The point of physics is to identify these nondynamical variables. Repeated interventions by experimentalists, and the careful noting of the range of consequences that those interventions elicit, is how these fixed instances are discovered.

In this interpretation of quantum mechanics, with its mix of operationalist, empiricist, and realist identification of abstract elements in the theory, these fixed instances specify the [[agent independent features]] of the "quantum world," and it is the business of physics to figure them out. This is done by experimentation, and the theoretical linking of basis vectors in the appropriate Hilbert space with various measurements, providing an "anchor" for those basis kets to our experience, the consequences of our actions. Particularly significant is the Hamiltonian operator and its basis kets [[in Caves' particular version of all this]]. As time evolves during what is colloquially described as "unitary evolution," we have the option to modify our beliefs or to modify the anchors of those beliefs; the first strategy corresponds to the usual Schrödinger picture, the second to the Heisenberg picture.

Regardless of the strategy, the [[properties intrinsic to the]] quantum world of this interpretation [[are]] a fixed, static thing. [[This aspect of the quantum world]] is a frozen, changeless place. Dynamics refers not to the quantum world, but only to our actions, our experiences, and our beliefs as agents. Or, more poetically (à la Chris), life does not arise from our interventions; it is our interventions.

John doesn't represent us correctly in every detail of this presentation—for the purpose at hand, it only seemed essential to modify him in a few instances, which I have have marked with double brackets [[·]]—but I would say he is roughly on track, and he certainly gets it that we are not concerned with the usual way of ascribing properties to quantum systems via the values of measurement outcomes or probability-1 predictions (i.e., the eigenvector-eigenvalue link).

Which brings me back again to your paper:

Bubism 10: *For a quantum state, the properties represented by Hilbert space subspaces are not partitioned into two such mutually exclusive and collectively exhaustive sets: some propositions are assigned no truth value. Only propositions represented by subspaces that contain the state are assigned the value 'true,' and only propositions represented by subspaces orthogonal to the state are assigned the value 'false.' This means that propositions represented by subspaces that are at some non-zero or non-orthogonal angle to the ray representing the quantum state are not assigned any truth value in the state, and the corresponding properties must be regarded as indeterminate or indefinite: according to the theory, there can be no fact of the matter about whether these properties are instantiated or not.*

You see, my way of looking at things wouldn't even allow me to say what you say here. It is just a very different world that I am working in.

To try to make this point, let me quote a couple of emails I wrote to Bas van Fraassen a few months ago. It started with my saying this:

> The way I view quantum measurement now is this. When one performs a "measurement" on a system, all one is really doing is taking an ACTION on that system. From this view, time evolutions or unitary operations etc., are not actions that one can take on a system; only "measurements" are. Thus the word measurement is really a misnomer—it is only an action. In contradistinction to the old idea that a measurement is a query of nature, or a way of gathering information or knowledge about nature, from this view it is just an action on something external—it is a kick of sorts. The "measurement device" should be thought of as being like a prosthetic hand for the agent—it is merely an extension of him; in this context, it should not be thought of as an independent entity beyond the agent. What quantum theory tells us is that the formal structure of all our possible actions (perhaps via the help of these prosthetic hands) is captured by the idea of a Positive-Operator-Valued Measure (or POVM, or so-called "generalized measurement"). We take our actions upon a system, and in return, the system gives rise to a reaction—in older terms, that is the "measurement outcome"—but the reaction is in the agent himself. The role of the quantum system is thus more like that of the philosopher's stone; it is the catalyst that brings about a transformation (or transmutation) of the agent.
>
> Reciprocally, there [[may]] be a transmutation of the system external to the agent. But the great trouble in quantum interpretation—I now think—is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory actually talks about, the "measurement outcome") takes place. It [[may]] be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. [[Quantum mechanics]] is only a hint of that more interesting transmutation. [[Instead, the main part of quantum mechanics is about how]] the agent and the system [[together bring about]] a little act of creation that ultimately has an autonomy of its own—that's the sexual interpretation of quantum mechanics.

which led to the following dialogue:

> **van Fraassenism 8 :** *Writers on the subject have emphasized that the main form of measurement in quantum mechanics has as result the value of the observable at the end of the measurement – and that this observable may not even have had a definite value, let alone the same one, before.*
>
> Your phrase "MAY NOT even have a definite value" floated to my attention. I guess this floated to my attention because I had recently read the following in one of the Brukner/Zeilinger papers,
>
> > Only in the exceptional case of the qubit in an eigenstate of the measurement apparatus the bit value observed reveals a property already carried by the qubit. Yet in general the value obtained by the measurement has an element of irreducible randomness and therefore cannot be assumed to reveal the bit value or even a hidden property of the system existing before the measurement is performed.
>
> I wondered if your "may not" referred to effectively the same thing as their disclaimer at the beginning of this quote. Maybe it doesn't. Anyway, the Brukner/Zeilinger disclaimer is a point that Caves, Schack, and I now definitely reject: From our view all measurements are generative of a NON-preexisting property regardless of the quantum state. I.e., measurements never reveal "a property already carried by the qubit." For this, of course, we have to adopt a Richard Jeffrey-like analysis of the notion of "certainty"—i.e., that it too, like any probability assignment, is a state of mind—or one along (my reading of) Wittgenstein's—i.e., that "certainty is a tone of voice"—to make it all make sense, but so be it.

and

> **van Fraassenism 9 :** *Suppose that an observer assigns eigenstate $|a\rangle$ of A to a system on the basis of a measurement, then predicts with certainty that an immediate further measurement of A will yield value a, and then makes that second measurement and finds a. Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had? He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?*
>
> It is not going to be easy, because this in fact is what Schack and I are actually writing a whole paper about at the moment—this point has been the most controversial thing (with the Mermin, Unruh, Wootters, Spekkens, etc., crowd) that we've said in a while, and it seems that it's going to require a whole paper to do the point justice. But I'll still try to give you the skinny of it:
>
> - Q: He does not need to change his subjective probabilities at all in response to the 2nd measurement outcome, does he?
> - A: No he doesn't.
> - Q: Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had?
> - A: No I don't.
>
> The problem is one of the very consistency of the subjective point of view of quantum states. The task we set before ourselves is to completely sever any supposed connections

between quantum states and the actual, existent physical properties of the quantum system. It is only from this—if it can be done, and of course we try to argue it can be done—that we get any "interpretive traction" (as Chris Timpson likes to say) for the various problems that plague QM. [[...]]

This may boil down to a difference between the Rovellian and the Bayesian/Paulian approach; I'm not clear on that yet. [[...]] Rovelli relativizes the states to the observer, even the pure states, and with that—through the eigenstate-eigenvalue link—the values of the observables. I'm not completely sure what that means in Rovelli-world yet, however.

I, on the other hand, do know that I would say that a measurement intervention is always generative of a new fact in the world, whatever the measurer's quantum state for the system. If the measurer's state for the system HAPPENS to be an eigenstate of the Hermitian operator describing the measurement intervention, then the measurer will be confident, CERTAIN even, of the consequence of the measurement intervention he is about to perform. But that CERTAINTY is in the sense of Jeffrey and Wittgenstein above—it is a "tone of voice" of utter confidence. The world could still, as a point of principle, smite the measurer down by giving him a consequence that he predicted to be impossible. In a traditional development—with ties to a correspondence theory of truth—we would then say, "Well, that proves the measurer was wrong with his quantum state assignment. He was wrong before he ever went through the motions of the measurement." But as you've gathered, I'm not about traditional developments. Instead I would say, "Even from my view there is a sense in which the measurer's quantum state is WRONG. But it is MADE WRONG by the ACTUAL consequence of the intervention—it is made wrong on the fly; its wrongness was not determined beforehand." And that seems to be the main point of contention.

Particularly this is going to be a key point when I finally come to the analysis in Section 7 of your paper.

The Probability Problem

Bubism 11: *The orthodox answer is that the probability assigned to a property of a system by a quantum state is to be understood as the probability of finding the property in a measurement process designed to ascertain whether or not that property is instantiated. I will defend this proposal later in the paper, but a little thought will reveal that it is rather problematic. When the system is represented by a quantum state that assigns a certain property the probability 1/2, say, this property is indeterminate. Physicists would say that ascribing the property to the system in that state is 'meaningless.' But somehow it makes sense to design an experiment to ascertain whether or not the property is instantiated by the system. And in such a measurement, the probability is asserted to be 1/2 that the experiment will yield the answer 'yes,' and 1/2 that the experiment will yield the answer 'no.' Clearly, a measurement process in quantum mechanics is not simply a procedure for ascertaining whether or not a property is instantiated in any straightforward sense. Somehow, a measurement process enables an indeterminate property, that is neither instantiated nor not instantiated by a system in a given quantum state, to either instantiate itself or not with a certain probability; or equivalently, a proposition that is neither true nor false can become true or false with a certain probability in a suitable measurement process.*

I found this paragraph interesting. Particularly as I could both agree and disagree with the last sentence! The difference comes at the semi-colon. That is, I disagree with this: "Somehow, a measurement process enables an indeterminate property, that is neither instantiated nor not instantiated by a system in a given quantum state, to either instantiate itself or not with a certain probability." But I agree with "A proposition that is neither true nor false can become true or false with a certain probability in a suitable measurement process." The trouble comes in at the connective "equivalently." I don't believe those separate thoughts are equivalent at all. The saving grace of the second of the two clauses for me is that you don't explicitly mention what the proposition is about. In the first clause, on the other hand, you are talking about properties of the system.

The Measurement Problem

Bubism 12: *In classical theories, we measure to find out what we don't know, but in principle a measurement does not change what is (and even if it does change what is, this is simply a change or disturbance from one state of being to another that can be calculated on the basis of the classical theory itself). In quantum mechanics, measurements apparently bring into being something that was indeterminate, not merely unknown, before, i.e., a proposition that was neither true nor false becomes true in a measurement process, and the way in which this happens according to the theory is puzzling.*

Here again, I can agree. You nicely did not say anything about what the proposition refers to. For instance, with your wording here—as long as I am careful to take the paragraph out of context!—I am free to think that the proposition that gains a truth value with the process of measurement refers to MY sensations, not a property of the system at all.

Indeed, I rather like the very next sentence:

Bubism 13: *The standard measurement problem of quantum mechanics is fundamentally* a problem about truth *. . . , distinct from the probability problem.*

as long as I am not forced to refill the ellipses with your parenthetical "(or the instantiation of properties)." I think this is the fundamental problem—it is about *truth*—and I am very happy that you're saying that much. When you say things in the philosophy-of-science community people listen. It is quite important to make a careful distinction between the problem of probability and the problem of truth, and not many people are doing that presently.

My own conviction that the measurement problem is fundamentally a problem about truth explains my fascination with James, Dewey, Schiller, Rorty, and Putnam, and the whole pragmatist framework for truth. Here is something I wrote Carl Caves in 2001:

> Today I focused on rounding up some more William James, John Dewey, Percy Bridgman material. I think James is taking me over like a new lover. I had read a little bit of him before, but I think I was more impressed with his writing style than anything. But I was drawn back to him by accident, after reading Martin Gardner's *Whys of a Philosophical Scrivener*. Gardner devoted a lot of time knocking down James' theory of truth, because it is just so much easier to accept an underlying reality that signifies whether a proposition is true or false, rather than saying that the knowing agent is involved in eliciting the very proposition itself (along with its truth value). And something clicked! I could see that what James was talking about might as well have been a debate about quantum mechanics. He was saying everything in just the right way. (Let me translate that: he was saying things in a way similar to the way I did in my NATO "appassionata.") And things have only gotten better since.

My recommendation—tongue in cheek—I like the direction your paper starts to move in, but I think you could take a good dose of James!

Here's a little dialogue I had with Bill Demopoulos earlier this year:

> **Demopoulosism 6:** *I also don't see why we should need something as fundamental as KS to sustain the notion that "unperformed measurements don't have outcomes." I'm being a devil's advocate here because I think what you really mean is that without a measurement of whether the cat is alive, the cat is neither alive nor not alive. But would you put it this baldly? If not, why not?*
>
> To answer your question in the best way I know how at the moment, I would say: The transformation that quantum mechanics speaks about, the transformation from a 'superposition' to 'aliveness' or 'deadness', is a transformation *within the agent*, and that transformation cannot take place without some interaction with the external physical system labeled by the word 'cat'. What happens to 'cat' itself (described in a way that makes no reference to the agent)? On that, I think quantum mechanics is silent. With a mantra: Quantum mechanics is a theory for ascribing (and intertwining) personal probabilities for the personal consequences of one's personal interactions with the external world.

Bubism 14: *The most sophisticated formulation of Everett's interpretation is probably the Saunders-Wallace version (Saunders, 1998; Wallace, 2003). Here the preferred basis is selected by decoherence, and probabilities are introduced as rational degrees of belief in the Bayesian sense via a decision-theoretic argument originally due to Deutsch (Deutsch, 1999).*

Sophisticated in the sense of being complicated, yes. Sophisticated in the sense of hiding things in layer upon layer. See our original criticism of Deutsch—Barnum *et al.*, "Quantum Probability from Decision Theory?," Proc. Royal Society London A **456**, 1175–1182 (2000), and `quant-ph/9907024`—which still stands, as well as some new nice stuff by Huw Price, `quant-ph/0604191`. Or talk to Jos Uffink, who's got some very good replies.

Bubism 15: *Of course, quantum mechanics could be false, but it seems wildly implausible that a modification of quantum mechanics whose sole motivation is to solve the measurement problem will survive fundamental advances in physics driven by other theoretical or experimental questions.*

I like this point very much.

Solving the Probability Problem

Bubism 16: *[T]he physical world is* nonlocal*, in that spacelike separated systems can occupy entangled states that persist as the systems separate.*

This language hints of an ultimately ontic view of quantum states.

Bubism 17: *Solving the probability problem* without reducing the problem to a solution of the measurement problem (the truth problem)*, amounts to treating quantum mechanics as a theory of information, in which no measurement outcomes are certified as determinate* by the theory*. Rather, measuring instruments are sources of classical information in Shannon's sense, where the individual occurrence of a particular distinguishable event produced stochastically by the information source lies outside the theory. In this sense, a measuring instrument, insofar as it functions as a* *classical information source, is ultimately a 'black box' in the theory. So a quantum description will have to introduce a 'cut' between what we take to be the ultimate measuring instrument in a given measurement process and the quantum phenomenon revealed by the instrument. The 'cut' is just a reflection of the fact that quantum mechanics is a* theory about the representation and manipulation of information *constrained by the possibilities and impossibilities of information-transfer in our world, rather than a theory about the ways in which nonclassical waves and particles move.*

I like much of this of course, if not not literally all.

Bubism 18: *If we set aside the measurement problem, the Gleason probabilities cannot be intelligibly interpreted as the objective chances or relative frequencies that dynamical variables take determinate values, and the only viable option is a subjective Bayesian interpretation of the quantum probabilities as rational degrees of belief.*

Why? Of course, I like the sentiment of this, but it'd be nice to see in your words an extended discussion of this point. As it is written right now, I think this represents a weak point of the paper.

On the other hand, I don't agree with any of this:

Bubism 19: *To show that quantum mechanics can 'stand on its own feet' as a theory of probability, i.e., as theory of information, we need to take account of the phenomenon of decoherence: an extremely fast process that occurs in the spontaneous interaction between a macrosystem and its environment that leads to the virtually instantaneous suppression of quantum interference. What happens, roughly, is that a macrosystem like a measuring instrument or Schrödinger's cat typically becomes correlated with the environment—an enormous number of stray dust particles, air molecules, photons, background radiation, etc.—in an entangled state that takes a certain form with respect to a preferred set of basis states, which remain stable as the interaction develops and includes more and more particles. It is as if the environment is 'monitoring' the macrosystem via a measurement of properties associated with the preferred states, in such a way that information about these properties is stored redundantly in the environment. This stability, or robustness, of the preferred basis, and the redundancy of the information in the environment, allows one to identify certain emergent structures in the overall pattern of correlations—such as macroscopic pointers and cats and information-gatherers in general—as classical-like: the correlational information required to reveal quantum interference for these structures is effectively lost in the environment. So it appears that the information theoretic constraints are consistent with both (i) the conditions for the existence of measuring instruments as sources of classical information, and (ii) the existence of information-gatherers with the ability to use measuring instruments to apply and test quantum mechanics, given a characterization of part of the overall system as the environment. That is, by selecting a preferred basis, decoherence provides an explanation for the emergence of classical information in a quantum correlational structure.*

This is just Zurekian obfuscation. You have already invoked the black-box concept for measurement, explaining the need for this if quantum mechanics is to be viewed as a theory of information. You did that nicely. Why back off and re-ontologize all the ideas to try to *explain* the existence of classical observers? WHO is writing down these decohering wavefunctions? In an information theoretic approach to quantum mechanics (by that I mean a non-ontic approach to wave functions), it is not fair to give in to temptation at the last moment and invoke God in the quad.

Bubism 20: *If something like the above account of decoherence is acceptable, then the probability problem reduces to showing that the probabilities assigned to measurement outcomes by these information-gatherers, in the subjective Bayesian sense, are just the Gleason probabilities.*

Again, why? It seems you're being very short on the key points you want to make here (i.e., this point and the last point above where I said, "Why?").

The Projection Postulate as Bayesian Updating

Bubism 21: *An analysis of quantum probabilities as measures of ignorance in the Bayesian sense, i.e., as degrees of belief measured by rational betting behaviour, has been developed by Schack et al. (2001), and Caves et al. (2002). In Pitowsky's formulation (Pitowsky, 2002, 2005), the structure of 'quantum gambles' encoded in the subspace structure of Hilbert space imposes nonclassical probabilistic constraints that define a logic of partial belief in the sense of Ramsey.*

What is this explicit distinction between Ramsey and de Finetti that you keep invoking? Can you articulate it?

Here is a quote by Keynes on Ramsey. How do you react to it?

> The application of these ideas [regarding formal logic] to the logic of probability is very fruitful. Ramsey argues, as against the view which I had put forward, that probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief, and he succeeds in showing that the calculus of probabilities simply amounts to a set of rules for ensuring that the system of degrees of belief which we hold shall be a *consistent* system. Thus the calculus of probabilities belongs to formal logic. But the basis of our degrees of belief—or the *a priori*, as they used to be called—is part of our human outfit, perhaps given us merely by natural selection, analogous to our perceptions and our memories rather than to formal logic.

And here is a long quote by Sandy Zabell, "Ramsey, Truth, and Probability." How do you react to it, along with my parenthetical comment?

> The key point is that previous attempts to explain induction had attempted to model the process by a unique description of prior beliefs [[references]], or by a very narrow range of possibilities [[references]]. De Finetti realized that because probability is a logic of consistency, one can never—*at a given instance of time*—uniquely dictate the partial beliefs of an individual; at most one can demand consistency. The essence of inductive behavior, in contrast, lies not in the specific beliefs that an individual entertains at any given point in time, but the manner in which those beliefs evolve over time. [[In this way it is exactly like classical logic: One is not judged as irrational for starting with the incorrect truth value for some proposition in one's considerations; one is judged irrational only if one makes an incorrect inference in the proof process.—CAF]] Let us pause briefly over this point.
>
> I change my mind slowly; you do so with rapidity; you think I am pigheaded, I think you are rash. But neither of us is of necessity irrational. Disagreement is possible even if we share the same information; we may simply be viewing it in a different light. This is what happens every time the members of a jury disagree on a verdict. Of course it can be argued that the members of the jury do not share the same body of facts: each brings to the trial the sum total of his life experiences, and one juror tries to persuade another in part by drawing upon those experiences and thus enlarging the background information of their fellow jurors. It is the credibilist view of probability that if you knew what I knew, and I knew what you knew, then you and I would—or at least should—agree.

> Such a metaphysical stance may well be, as I. J. Good says, “mentally healthy”. But it is an article of faith of no real practical importance. None of us can fully grasp the totality of our own past history, experience, and information, let alone anyone else’s. The goal is impossible: our information cannot be so encapsulated.

Bubism 22: *Here I want to show that the quantum rule is in fact just a noncommutative version of the classical rule.*

The next three pages give a very nice treatment! (And I’m serious about that.) Why would I—Bayesian to the core—ever want to find to find a different analogy between quantum collapse and Bayesian conditionalization than the one you present here? In a quick word, the idea of a POVM (even a standard von Neumann measurement) as an analog of an indicator function goes in the wrong direction for what I want to squeeze out as the essential structure of quantum mechanics. Indicator functions carry the baggage of thinking about quantum measurement as a process of revealing pre-existent values. But I’ll come back to this in a longwinded way, as you can guess.

Bubism 23: *The prior probability assignment of an observer about to make a measurement on a quantum system S is given by an initial density operator $\rho_S \in \mathcal{H}_S$. What should an observer take this density operator to be?*

Here we go again ... You sure have some dangerous remaining objectivist tendencies when it comes to probabilities and quantum states.

Bubism 24: *Suppose the universe $U = S + E$ is in an initial pure state $|\psi\rangle \in \mathcal{H}_R$.*

Whose pure state? Is the state information (in the sense of Bayesian probability theory), or is it not?

Quantum Probabilities as Rational Degrees of Belief

Bubism 25: *Now, Fuchs points out (Fuchs, 2002b, p. 34) that the state change following a quantum measurement of a POVM $\{E_d\}$ can be presented as a 2-state process:*

I think you meant “2-stage” here.

Bubism 26: *In the case of a projective measurement $\{E_d\} = \{E_d = |d\rangle\langle d|\}$, where the E_d are projection operators, the state change on measurement with outcome d is a collapse corresponding to a readjustment by the unitary operator $U_d = |d\rangle|\psi\rangle$.*

There’s a typo at the end of the sentence here; an operator is not a state. Here is the way I said it in the final version of the paper (after I had fixed my own typo at this very spot!):

> In particular, when the POVM is an orthogonal set of projectors $\{\Pi_i = |i\rangle\langle i|\}$ and the state-change mechanism is the von Neumann collapse postulate, this simply corresponds to a readjustment according to unitary operators U_i whose action on the subspace spanned by $|\psi\rangle$ is
> $$|i\rangle\langle\psi| \; . \tag{46}$$

Finally, we come to the real point of contention:

Bubism 27: *Fuchs concludes from this analysis that quantum collapse can be regarded as a noncommutative version of Bayes's rule. But this rather complicated analysis misses the essential point. It is precisely the 'violent' collapse transition (17), where measurement is only disturbance, that has to be explained as a noncommutative variant of Bayes' rule and not merely the 'gentle' selection from an initial density operator of a term corresponding to the outcome of a measurement.*

"When a distinction in concepts can be made, a distinction in concepts should be made." I thought that methodology was the working bread and butter of the philosopher? Let me try to better explain what I did, and why I did it. I made a distinction; I followed through with the implications; I tried to learn a lesson.

The distinction, when it was found, was made because of this: I am working in a quantum foundational context that is trying very hard to dispel that idea that quantum "measurement" has anything *a priori* to do with information gathering about things intrinsic to the quantum system. I tried to give you ample evidence and explanation of that above. Still, though, let me give one last point of reference in that regard. It comes from the same email discussion with van Fraassen that I referenced before. Then I'll return to giving an account of the "distinction" I've alluded to.

> **van Fraassenism 10:** *I thought I would after all not follow you in replacing the term "measurement", despite all the bad effects old connotations have had in various discussions. We need to bracket the old connotations such as that a measurement result reveals a pre-existing value for the measured observable. But I think we can do that because: ...*
>
> *[T]here is a certain kind of retrodictive inference possible also on the basis of qm measurements. For a long time the paradigm was a source preparing a stream of particles in a certain state – measurements on samples taken from the stream give a good basis for conclusions about just what state the source was preparing, and these conclusions can then be used to predict the outcomes of further measurements made on later samples of the stream.*
>
> This is what we in quantum information call quantum-state tomography. One can indeed think of a quantum measurement outcome as *giving information* in the old standard sense in that case and not simply being the "unpredictable consequence of one's action." But then "giving information" is quantified by Shannon's "mutual information," $I(X,Y)$ and not simply by his entropy function $H(Y)$. That is, one has two random variables in the game—one treated classically, namely the "unknown preparation" X, and the other one purely quantum mechanical, the result Y of the measurement interaction. Those two variables have quite different roles, and one indeed would not want to think of X as the "consequence of one's interaction." On the other hand, without making explicit mention of X one has no means for thinking of the elicitation of Y as giving information about anything at all. Before seeing the value of Y, one can expect to be *surprised* to the extent quantified by $H(Y)$, but that's where the story stops.
>
> [For a more detailed Bayesian-like development of this point, you might have a look at our paper "Unknown Quantum States and Operations, a Bayesian View," `quant-ph/0404156` and some of the references therein. Particularly the Introduction and Concluding section might be of some interest to you with regard to the present discussion.]
>
> The only point I want to make to you with regard to your remark above is that, for these reasons, I would say it has no bearing on the issue at debate: I.e., the debate

of whether it is better to think of a "quantum measurement" as simply an action with an unforeseeable consequence, or rather as a kind of "question-asking" or "information-gathering." It is tangential.

So, what is this distinction that I was trying to capture in my formalism that it seems your way of viewing quantum conditionalization does not take into account? Take a single quantum system, for which some quantum-state assignment ρ has been made. Now imagine performing a quantum "measurement" on the system and updating the quantum state to some new value ρ_d, consequent to the measurement's outcome. One can write that update in the usual Kraus way or in my idiosyncratic way. Let's even take the special case where the update is just the Lüders rule—noting that it too can be viewed in both ways.

You have a state change; I have a state change. In the end, they're identical of course. But whereas you have a change that looks *solely* like an application of Bayesian conditionalization, I don't. I have a handle for making a distinction—and in fact it is a distinction I want to make. You write, "It is precisely the 'violent' collapse transition (17), where measurement is only disturbance, that has to be explained as a noncommutative variant of Bayes' rule and not merely the 'gentle' selection from an initial density operator of a term corresponding to the outcome of a measurement."

No. It doesn't need to be, and I don't want it to be. I want it to remain a distant, maybe unrecognizable addition to Bayes, not a variant at all. It quantifies the extent to which my previous opinion changes radically (as compared to the change it would have made solely through Bayes) upon the receipt of the data. It gives me a handle for exploring a new issue. If a quantum "measurement" is not merely the receipt of data with regard something pre-existent, why should it look like a learning process at all? Well, in truth, the consequence of the measurement may allow me a refinement in predictions for the next round of measurement. But to some extent it is simply new input into my beliefs that my previous opinion could not take into account.

In the end, where I think I want to go with this kind of thing is this. Sandy Zabell puts it nicely in his essay on Ramsey.

> Ramsey did consider the question of the dynamic evolution of belief. Conditional probability is defined in terms of conditional bets; it states the odds that someone "would now bet on p, the bet only to be valid if q is true." This approach has since been adopted as the basis of the commonly accepted subjectivist definition (see, e.g., de Finetti, 1972, p. 193); but of course it does not address the relation that conditional probabilities—thus defined—may have to the actual degrees of belief one holds after the observation of an event. And here we run up against an apparent inconsistency in Ramsey's views. Initially, Ramsey notes there is no reason to automatically identify the two quantities:
>
> > the degree of belief in p given q is not the same as the degree to which a person would believe p, if he believed q for certain; for knowledge of q might for psychological reasons profoundly alter his whole system of beliefs.
>
> But further on in the last section of his essay Ramsey writes: ...
>
> Clearly the emphasized portion of the quotation completely ignores the profound insight of the preceding quotation; perhaps this second passage represents a portion of the text written at an earlier stage.

(OK, I'm too lazy to copy the second passage of Ramsey, which has no relevance to my argument.) Anyway, this idea of Ramsey's has been developed at great length in the work of van Fraassen, Skyrms, and Richard Jeffrey—in fact it is the standard fare of what Richard Jeffrey calls "radical probabilism." Standard conditioning in the probabilistic setting is NOT always the way to go. (The precise statement is that diachronic Dutch book arguments *always* need supplementary assumptions that the synchronic ones don't need.)

I want to fit quantum conditioning into that framework, if possible, and this distinction I draw—i.e., splitting the quantum state change into two components—gives me some handle on that. I regret some of the phrases in my original description of this now—all this business about "gut-wrenching violence"—(remember, that was 4 years ago!), and I am sorry it may have caused confusion about my motivations, but the technical result is still exactly where I want to go.

Bubism 28 : *The above analysis simply illustrates the way the projection postulate works in quantum mechanics. It does not explain it, in the sense of showing how quantum collapse can be understood as a noncommutative version of Bayes' rule for updating states of belief. What Fuchs' analysis in terms of POVM's shows is that the relevant features of my example are quite general. But, again, this in itself does not demystify quantum collapse as a noncommutative version of Bayes' rule.*

Well, I didn't think I had claimed to demystify the particular form of the quantum collapse rule so much as quantify it in a new way (one more in line with the point of view I am trying to build). If my bad writing style confused you, again I apologize.

Bubism 29 : *Fuchs' Bayesian interpretation of quantum probabilities follows de Finetti rather than Ramsey and reflects de Finetti's instrumentalism.*

When I get back home, I'll send you a reading list on Ramsey. The divergence is not as drastic as you think. The only difference I have ever made note of is that Ramsey was willing to call "intersubjective agreement" on a probability assignment "objective chance." But mixing the logical and empirical realms, Ramsey did not do or advocate. I'll bet money on that.

Bubism 30 : *For de Finetti, science is just an extension of common sense and cannot inform the 'logical' aspect of probability formalized in the probability calculus through the notion of coherence. So it would not make sense to regard quantum mechanics as a nonclassical probability theory or theory of information, where the formal features of this new theory encode objective structural relations about the physical world. Physics can only be relevant to the extra-logical and context-dependent evaluation of probabilities.*

That is pretty much correct. And it strikes me as exactly what one would want if one were a *realist*! There is our reasoning about the world, and then there is the world itself. When one starts to mix those two ingredients, that's when one is becoming an instrumentalist!

Yes, I definitely want to see the essence of quantum mechanics being born out as an extra-logical and context-dependent (let me say empirical) addition to probability theory. It is a layer on top of probability (for instance the restrictive region that I always draw on the simplex), and in that way quantum reality (whatever that is) keeps its autonomy from my thought.

Bubism 31 : *What Fuchs takes himself as establishing is that quantum states represent subjective degrees of belief . . .*

Yes, my paper was meant to be a foray in that direction. But, the remainder of your sentence

Bubism 32 : *. . . and that quantum collapse is Bayesian conditionalization* in the standard sense.

not at all. Besides this blatantly contradicts the remaining sentences in your paragraph:

Bubism 33: *What the* physics *tells us is summed up in his statement ...: 'The world is sensitive to our touch.' That is, there is an irreducible nonclassical disturbance that occurs whenever we probe the world. But this enters into the* readjustment *of the observer's probabilistic beliefs* after *the application of Bayesian updating, which is a straightforward refinement of prior degrees of belief in the usual sense.*

And finally,

Bubism 34: *The difference, ultimately, is between an instrumentalist approach to quantum mechanics with the application of a strictly classical Bayesian theory of probability, and an interpretation of quantum mechanics as a nonclassical theory of information, in which the structure of quantum gambles, considered as an objective feature of reality, informs the correlational structure of quantum probabilities.*

No, no, a million times, no! My point of view is in no form an instrumentalism. With each of our theories we are making direct or indirect statements of what we believe of the objective, external world. The quantum Bayesian view of Caves, Schack, Appleby, me, and whoever else to quantum mechanics is no less so. On the other hand, imagining melding empirical statements into the very structure of the laws of thought—as you and Itamar seem to do—represents a real danger with regard to the realist-instrumentalist divide.

Maybe more later, but I'll stop now and print this out.

06-07-06 *Try This* (to R. E. Slusher)

Supercomputers of the Future

These quantum computers, for some problems, would be unimaginably faster. For, increasing a problem in size by a factor of 2, can often make it 4 times as hard to solve on a regular computer. But not so with a quantum computer—there the problem only becomes 2 times as hard to solve.

Quantum Computers Vs. Current Computers

Instead of classical bits, a quantum computer would work with qubits. A qubit is a single atom, or a smaller subatomic particle, whose spin works like an oracle for a bit. The oracle doesn't make up its mind until it is asked a question.

Depending on how it is *found* spinning, a qubit signifies a "1" or a "0." It does not make up its mind before the process of observation.

Why Quantum Computers Could Be So Powerful

Pairs of qubits can be much more powerful as computational components than pairs of bits because of a strange quantum feature called entanglement. In a regular computer, if one were to stop it in its tracks and learn the values of some of its bit, this often is of only limited value for guessing the remaining bits. But in a quantum computer, knowing the answers of some oracle calls can give a seemingly unnatural amount of information about other oracle calls. This makes the logical operations of a quantum computer more tightly connected than in a classical computer, effectively allowing a quantum computer to skip computational steps that a regular computer has to meticulously strain to go through one by one.

06-07-06 *College Keys!* (to G. M. D'Ariano)

D'Ariano-ism 3 : *You didn't give back the keys of the college room at S. Caterina.*

I'm so sorry about doing that! When I read your note, I looked into my backpack and there they were! In any case, I have mailed them straight off; I hope it won't take the keys too long to get there.

I have been meaning to write you for several days now, but ever since returning to the US I have had constant things to do because of Bell Labs (and this large nanotech meeting I am organizing for next week). Anyway, I want to thank you again for inviting me; I very much enjoyed my time there and got a lot out of the interaction. My discussions with you in particular will certainly make me rethink terminology, but more than that. I'm not lying when I say I like the smell of your axiom system—it starts to seem right in a way that the others have not. Ultimately, though, my desire is to get out of pure operationalism—i.e., to take a good look at the quantum formalism and distill from it some statement about the world that makes no direct appeal to the notion of an experiment taking place in the world. I view your axiom system (what I understand of it) as a crucial step in that trek.

07-07-06 *Markus Fierz, RIP* (to H. C. von Baeyer)

von Baeyerism 20 : *Today I asked Klaus Hepp (ETH Zurich), who happens to be a distant cousin, whether Fierz is still alive, and he emailed back that he died one week ago at 94.*

I'm always annoyed to hear news like this. I think, "Why on earth did I not seek out a way to talk to the person when I had a chance." I've had an interest in Fierz for 11 years. If I had sought him out in 1995, when I was already traveling the world, I might have caught him at a fresh, relatively young, 83, and might have learned so many things.

von Baeyerism 21 : *I looked at D'Ariano's paper, but found it pretty hard to understand. However, if some of his postulates about "informationally complete observables" and "symmetric faithful states" can be rendered into English, they might indeed be promising. Is the limitation of information that nature allows us to collect about a quantum system explicit or implied in his approach?*

I go up and I go down about the work, particularly after all the fights I had with him last week about terminology. Might sound silly, but I think it reveals some serious underlying philosophical differences. Certainly, I see "purely operational axioms" as a stopgap measure (though a necessary first step), whereas he may (probably) view it as the end of the line. Still, I think leaning heavily on informationally complete measurements (and their properties) in an axiomatization is the way to go.

To answer your question, yes it has to be. It is somehow already buried in the idea of an informationally complete measurement. But ferreting the direct connection out may be a big problem.

In any case, this has been a very exciting summer for me foundationwise. Itamar Pitowsky presented a very nice result at the Pavia meeting that is worth serious study, to do with a kind of pseudo-Dutch-book argument for the quantum probability rule. (I say pseudo-Dutch-book because it had empirical elements in it—to do with Kochen-Specker things—that a straight-up Dutch book argument cannot have.) He told us that the lecture is posted at his homepage, but I could not find it. When I learn the exact coordinates, I will send them your way.

Attached are some notes I put together while in Pavia to comment on a draft of a paper that Jeff Bub is writing. [See 24-06-06 note to Bub titled "Notes on 'What are Quantum Probabilities'".] The notes are certainly more about my point of view than what Jeff is actually writing about. But I felt I had to do that, since part of his paper was strictly intended to be contra-me. Still, for my discussions with you—on Pauli, Fierz, and alchemy—even though you've already seen various pieces of what I tell Jeff, it may be useful for you to see these various pieces of the description all tied together in one place.

08-07-06 *Cerro Grande II* (to H. C. von Baeyer)

von Baeyerism 22: *In passing I noted that "Hans" accused you of the view that the world does not exist. (If, surprisingly, I happen to be the only Hans in this context, then I am that Hans. If there's another, my comment is moot.) The trouble is, I don't remember accusing you of such a drastic view. I always thought of information as the go-between that mediates between the thing and the mind, and that we have no direct evidence of the material world. Which doesn't mean it isn't there.*

If I really said those things, fine. Even if I didn't, I don't mind helping you out as a straw-man. But did I say them? Only one person comes to mind who really did say them: the editor Hervé Poirier in Science et Vie*. I recall that I was uncomfortable when he wrote that, but didn't object because I didn't know what your real views were.*

Anyway, I am not in the least offended. I'm just following your lead in trying to be as accurate as I can – which is difficult with a lousy memory.

Indeed it wasn't you. But you should know that people accuse me of that "drastic view" all the time. Literally all the time. Here's a partial list that comes to mind: Wayne Myrvold, Michael Nielsen, Andrew Landahl, Abner Shimony, Philippe Grangier, Matthew Donald, Hans Halvorson of course (though he doesn't any more), Simon Saunders, Todd Brun, Bob Griffiths, Wojciech Zurek, and untold numbers of people in the audiences of so many of these meetings I've gone to over the years.

Apparently it is a very thin line we are walking—thin at least with respect to the coarse-grained perceptions and views of the people who pipe up with said accusation.

12-07-06 *Notation for Inspiration* (to H. C. von Baeyer)

von Baeyerism 23: *And what of the future? What notational revolutions can we expect, and what new understanding will they bring about? We have to await a future Leibniz, Dalton, or Feynman to tell us – but I can suggest a couple of areas in which a notational innovation would be welcome. [...]*

Quantum mechanics itself presents another opportunity for better notation. The original version of the theory developed in 1925/26 includes two different ways in which atomic systems develop in time – a smooth, predictable evolution according to well established rules, interspersed with abrupt random changes called quantum jumps or quantum leaps. The two processes might be represented respectively by the steps and risers of a staircase, and both are well understood. To describe an atomic process, physicists usually have to deal with two or three steps up and down the stairs. In recent years, much effort has gone into trying to understand the puzzling interface between the quantum world of the atom, and our own, macroscopic experience. [...] Eventually I hope that an evocative notation will suggest a better way to understand how the world as we experience it, and as it is described by histories, arises from its quantum mechanical substrate.

Thanks for sending me your article on notation. I enjoyed it very much. Do you know I've had Yates' book[108] on my shelf for several years now—ever since picking it up at a charity flea market—but have never read it.

The idea is simple and good. Particularly, you are right in that we need a better notation for making sense of what quantum measurement is all about.

My own favorite image of what quantum measurement is all about is the one attached, which you have seen many times now. But how to turn that image into an effective notation? And notation to do what? I don't really know.

Here's maybe an actual notational innovation in our field of quantum computation: `quant-ph/0504097`. If you haven't read about the Raussendorf-Briegel model of quantum computation yet, this may be a good place to start in any case. Quantum computation enacted solely by quantum measurement and no unitary time evolution at all. That thrills me to no end. For myself, it makes me take the idea of "measurement" as "action" even more seriously.

It's all about philosopher's stones. But how to put that into notation? Your article did get me thinking.

16-07-06 *Edward Lear?* (to A. Shimony)

NOTE: For an amusing story of one of the unintended consequences of this note, see the Abner Shimony story in the introduction, "How to Stuff a Wild Samizdat," of my Cambridge University Press book *Coming of Age with Quantum Information*.

I wonder if you're not in transit to Waterloo yet?

I am looking forward to your celebration and have been gearing up mentally for it in several ways. One is I have just read Brent's biography of C. S. Peirce, so that I might get to know the man a little better. (You know that my talk is titled "Peirce, James, and the Quantum Bayesians"?) What a tough thing to do, to read that book! And actually what a depressing thing to do: I couldn't help but note some of the parallels between his life and mine. Though I lack his genius, I have certainly brought some of the same troubles upon myself.

Particularly now though, I'm writing you because I have just spent a frustrating hour on the web trying to hunt down the words of the poem you recited a little in Vienna last year. The key line as I recall was "going to sea in a sieve". It wasn't Edward Lear's "The Jumblies", was it? What you read seemed so much more serious. Anyway, I'd like to use the lines you actually did recite in my own talk in Waterloo. (I'll use them in the obvious way … to make fun of myself.) If you're out there in email-land still, could you let me know? If I don't hear from you, I'll use some of the lines from Edward Lear—they're certainly appropriate!

Can't wait to talk with you again.

Abner's Reply

> It was very good hearing from you and hearing that you would be at the conference in Waterloo. Also that you are reading Peirce and about him, though your dark statements about parallels between his life and yours were disturbing.
>
> The poem you enquired about is by Lear, and the first line is "They all went to sea in a sieve, they did / In a sieve they went to sea." I don't know whether the poem is called "The Jumblies", though that name occurs in the poem. Most poetry books have indices not only by title but by first line.

[108] Frances A. Yates, *The Art of Memory*, (U. of Chicago, 1966).

18-07-06 *Story about Peirce and Habits* (to J. Christian & L. Hardy)

Here's the story about Peirce and linear time from Louis Menand's *The Metaphysical Club*:

> One of the first things Peirce did after he arrived at Hopkins in the fall of 1879 was to start a Metaphysical Club. It was open to faculty and graduate students from any department, and it met once a month to discuss papers presented, usually, by the members themselves. ...
>
> At one meeting, presided over by Morris, Dewey heard Peirce read a paper called "Design and Chance," and joined in the discussion afterward. The paper is the germ of Peirce's later cosmology, and it sums up in a few pages what was probably the substance of the yearlong class Dewey had chosen not to take. Peirce's subject was the laws of nature—the laws that Newtonian physicists believed explained the behavior of matter and that physiological psychologists believed explained the behavior of minds—and he began with a simple question: Does the principle that everything can be explained have an explanation? Or, as he also put it: Does the law of causality (which is another name for the principle that everything can be explained) have a cause? ...
>
> Summarizing Peirce's Metaphysical Club paper on "Design and Chance" is ... not quite the same thing as paraphrasing it. The argument begins with the point James Clerk Maxwell had made with his imaginary demon: that a scientific law is only a prediction of what will happen most of the time. Even "the axioms of geometry" said Peirce, "are mere empirical laws whose perfect exactitude we have no reason whatever to feel confident of." The decision to treat a particular law as absolute is a pragmatic one: sometimes we feel that questioning it will only lead to confusion, and sometimes we feel that questioning it is necessary in order to try out a new hypothesis. A law, in Peirce's pragmatic view (derived, of course, from Wright), is essentially a path of inquiry. It helps us find things out—as the law of gravitation, for example, helped us discover Neptune—and Peirce's first rule as a philosopher of science was that the path of inquiry should never be blocked, not even by a hypothesis that has worked for us in the past.
>
> Maxwell's view was that laws are fundamentally uncertain because there is always a chance that the next time around things will behave in an improbable (though not an impossible) way—a chance that all the fast molecules will congregate on one side of the container. Peirce's point was that a chance occurrence like this can change the conditions of the universe. His illustration was drawn from classic probability theory: in a game with fair dice, a player's wins and losses will balance out in the long run; but if one die is shaped so that there is an infinitesimally greater chance that after a winning throw the next throw will be a losing throw, in the long run the player will be ruined. A minute variation in what seemed a stable and predictable system can have cosmic consequences. In the natural world, Peirce said, such minute variations are happening all the time. Their occurrence is always a matter of chance—"chance is the one essential agency upon which the whole process depends"—and, according to probability theory, "everything that can happen by chance, sometime or other will happen by chance. Chance will sometime bring about a change in every condition." Peirce thought that even the terrible second law of thermodynamics—the law of the dissipation of energy—was subject to reversal by such means.

As Peirce acknowledged, this was a Darwinian argument: "my opinion is only Darwinism analyzed, generalized, and brought into the realm of Ontology," he said. What he meant was that since nature evolves by chance variation, then the laws of nature must evolve by chance variation as well. Variations that are compatible with survival are reproduced; variations that are incompatible are weeded out. A tiny deviation from the norm in the outcome of a physical process can, over the long run, produce a new physical law. Laws are adaptive.

Pragmatically defined, variations are habits. They constitute a behavioral tendency —for if they had no behavioral consequences, they would have no evolutionary significance. Bigness in beak size is whatever big beaks do for you (if you are a finch), just as (to use an example from "How to Make Our Ideas Clear") "hardness" is just the sum total of what all hard things do. What Peirce proposed in "Design and Chance" was that natural laws are also habits. This was not a new thought for him. There is a story, attributed to William James, about a meeting of the original Metaphysical Club in Cambridge, in which the members waited patiently for Peirce to arrive and deliver a promised paper.

> They assembled. Peirce did not come; they waited and waited; finally a two-horse carriage came along and Peirce got out with a dark cloak over him; he came in and began to read his paper. What was it about? He set forth ... how the different moments of time got in the habit of coming one after another.[109]

It sounds like a joke, but the story is probably true. Peirce's paper must have been an extrapolation from the nebular hypothesis—the theory that the universe evolves from a condition of relative homogeneity, in which virtually no order exists, not even temporal order, to a condition of relative heterogeneity, in which, among other things, time has become linear. How did time get straightened out in this way? By developing good habits. In "Design and Chance," Peirce put it this way:

> Systems or compounds which have bad habits are quickly destroyed, those which have no habits follow the same course; only those which have good habits tend to survive.
>
> Why ... do the heavenly bodies tend to attract one another? Because in the long run bodies that repel or do not attract will get thrown out of the region of space leaving only the mutually attracting bodies.

If you are a heavenly body, in other words, gravitational attraction is a good habit to have, in the same way that if you are a proto-giraffe, a long neck is a good attribute to have. It keeps you in the system. When gravitational attraction becomes the habit of *all* heavenly bodies, then we can speak of "the law of gravity," just as when all surviving proto-giraffes have long necks, we can speak of a giraffe species, and (presumably) when all moments of time have the habit of following one another, we can speak of past, present, and future. But the law of gravity did not preexist the formation of the universe, any more than the idea of a giraffe did. It evolved into its present state while the universe was evolving into *its* present state. Gravity was a chance variation that got selected. Objects that didn't have the gravitational habit didn't survive.

[109] See M. H. Fisch, "Was There a Metaphysical Club in Cambridge?," in *Studies in the Philosophy of Charles Sanders Peirce, Second Series*, edited by E. C. Moore and R. S. Robin (U. Massachusetts Press, Amherst, MA, 1964), p. 11. The words are attributed to Dickinson Miller. [Fuchs: Compare this to John Wheeler's story of the Pecan Street Cafe grafitto.]

18-07-06 *The Real Peres Number* (to J.-Å Larsson)

For the Peres 33-ray example, you once told me how many rays and bases it would take if the jumble were to be completed into full sets of bases (i.e., no basis elements missing). Could you tell me those numbers again? (I guess the number of bases is 16—if I have that right—but what is the total number of rays when everything is counted properly?)

As before, I'm asking because I'm guessing you have these numbers at the top of your head. If you don't, don't kill yourself—I just thought it might be nice to mention them in passing when I mention the KS theorem (in passing) in this week's talk. (Actually, I'm going to put a lot of different material than usual in this one; wish you were here at the Abnerfest to discuss these things.)

Jan-Åke's Reply

> The 33 vectors form 16 triads. But to do the proof for these you need to modify the "two ones and a zero" to "maximum one zero in a pair", sometimes you go via a rotation from one triad to a pair of vectors and then to another triad. Completing to triads only I counted to a total of 57 vectors, forming 40 triads interconnected by 96 rotations.
>
> A table for different KS sets is in `quant-ph/0006134`.

19-07-06 *My Draft* (to C. M. Caves & R. Schack)

OK, I have read it all again, and now at least I agree with everything that is actually said in the paper. At this stage, in preparation for printing it out tomorrow morning to hand off to Mermin, the only changes I have made to Carl's latest draft are [. . .]

As I said, now I at least agree with everything that is actually said in the draft. That is progress. That is not to say, however, I still wouldn't like to make some small additions. There are the ones already marked for me to say something—and, I'm sorry again, but I haven't taken care of that yet. But also,

1) I am a little worried that we have not defined the word "objective" all that well. "Subjective" is OK, but "objective" is left a little dangling. I worry about how to do that in a way we will all agree with and so as not to stir up a new hornet's nest.

2) The present paragraph on the "fundamental conceptual difficulty" in the Principal Principle in the coin-toss case is good, but we really didn't give any argument against it in the quantum case. We simply said "the Bayesian contends . . . " I don't think that is adequate, as much of the point of our paper is premised on the idea that "objective probability" is worthless. The Lewisian says, "Why worry about this category distinction business when both terms—the probability function and its argument—are simply objective in the quantum case? It is a different thing than the classical Bayesian case. There is the objective event and its objective propensity; no category distinction called for."

3) I think it could be useful to end on a more rounded out note on the meaning of the Born rule. What is the meaning of the Born rule if it is not specifying a probability by fundamental physical law? Why do we all use the darned thing? We give a negative answer to the first question—i.e., that despite appearances, quantum probabilities are not specified by law itself—but we never give a positive answer. My own contention is that the sum total meaning of the Born rule is that it is a rule of transformation. It tells us how to take our probability assignment for the outcomes of *this* informationally complete measurement (or set of measurements) and transform it

into a probability assignment for the outcomes of *that* measurement. Thus it is the transformation rule that is specified by "fundamental physical law" (a kind of empirical addition to Dutch-book coherence), not the probability assignment itself. [. . .]

5) An equation or two might be called for at the discussion "The quantum operation depends, at least partly, on an agent's beliefs about the device that executes . . . " For instance, something like what Rüdiger and I put in our tomography-volume paper:

> For any trace preserving completely positive map Φ on a system, one can always imagine an ancillary system A, a quantum state σ for that ancillary system, and unitary interaction U between the system and the ancilla, such that
>
> $$\Phi(\rho) = \mathrm{tr}_A\Big(U(\rho\otimes\sigma)U^\dagger\Big) ,$$
>
> where tr_A represents a partial trace over the ancilla's Hilbert space. The Bayesian should ask, "Whose state of belief is σ?"

6) This one may not be for here—i.e., another can of worms thing that we probably don't want to get into for the present paper—but I want to record that I'm also a little worried about our dual use of "facts" and "propositions". Appleby has already called me on it during his reading in Pavia. It could be a serious point, and it has come up in our conversation before. For instance, the present dual use probably contradicts the usage I proposed in several of those old notes to you and Mermin. See for instance, 04-09-2001 "Note on Terminology," in my letter collection.

Appleby's particular point is that our category distinction is really a logical distinction, and one should therefore stray away from any presentation that doesn't make it seem as such. (But I'd probably have to work more than I have the time for now, and quote Wittgenstein and such, to make this mean much to you. So, I won't go into it any further than this.)

If you tell me which, if any of these, you agree with, and you yourself think is important, I will try to oblige. Realistically though (even without any of the changes above) I don't see how we can post anything before Carl returns from vacation. I can promise to do my very best to stop my sinning of trashing deadlines, and see what I can work up to have waiting on your desks when you return August 6.

19-07-06 *Quick Early Reaction* (to C. M. Caves & R. Schack)

Cavesism 76: *God save us from Wittgenstein. We're writing this for physicists. I do think we want to be addressing things other than propositions, because most physicists would think physics has to do with things other than propositions.*

Believe it or not, I am not making this up. (I may be vindictive, but I am not *that* vindictive.) I just had lunch with Mermin and the first thing he said was that he could not get past our first page. He got hung up on the words "proposition," "data," and "fact"—how they were potentially meant to be the same thing, or how they might possibly be different. He didn't know which. The word he was the most comfortable with was "proposition." When I told him that "acquiring data" was meant to be the becoming aware of the truth value of a proposition, he was OK with that too and happier. However, with the word "fact" we made no great progress. He said he might be happier if we didn't have to use the word in our presentation. At the very least, we should be more careful about the distinctions and identities.

. . . just quoting him to the best of my ability. I hope he'll move on to page 2 now.

I know there have been plenty of times when Carl has said that I am not a physicist. (Though I certainly can't blame him: Clearly it's an opinion shared by all the physics departments I've interviewed with.) But do you really want to say that David Mermin is not a physicist?

I think we're going to need some more precision. At least Appleby and Mermin are a sympathetic readership; what's it going to be like when we hit someone who is nasty?

20-07-06 *Capacity for Creation* (to J. E. Sipe)

That's the phrase I should have used. That is what I would like to think of as permanent and unchanging for a quantum system. (As far as I understand, it is not crucial for my ontology at this stage, but it is an idea I am testing out and intrigued by.)

The essay collection "Delirium Quantum" is attached. [See `arXiv:0906.1968v1`.]

For the bit I read you tonight about how

> Our quantum measurements in the present never tell us about the past. They only tell us about the consequences of the past for any of our other measurements in the present.

you can go to the present (incomplete) compilation of the new samizdat. [See 23-09-03 note "The Trivial Nontrivial" to S. Savitt.]

Thanks for the conversation tonight! It was very useful to me.

26-07-06 *Another Question* (to J. E. Sipe)

Thanks for the note. I think it really goes to the heart of the matter (though, I recognize it is what you already brought up in our till-midnight conversation the other day). In fact, it goes to the heart of the matter so much so that I had better search my soul before writing you more. Your question is a really important one, and—I cannot lie to you—I am learning a lot by your forcing me on this point.

At the moment, I am inclined to say that I lean toward being a "dualist Bayesian," though I don't think that definition (i.e., falling within a mere trichotomy) captures the right sense of distinction that should be made. There is a whole spectrum of things that I think I would allow, and "classical Bayesians" and "consequence Bayesians" are just two points in that space (not necessarily extreme points).

Certainly I am sometimes in the habit of thinking of Hilbert-space dimension as akin to a classical property (something that one hypothesizes of a system). Thus if one were to write down a probability distribution for the dimensionality of a system, one would be committing a type of "classical Bayesianism," as opposed to "consequence Bayesianism." What else could I mean by it? On the other hand, I sometimes think of the distinction between dimensionality and a particular state vector as a *logical* distinction (one of the *level* of subjectivity).

See for instance the note "04-07-03 Solid Ground, Maybe? (to G. L. Comer)" starting on page 209 of Cerro Grande II. Particularly the remark on page 301 about levels of subjectivity. Also have a look at the note "25-07-03 Relative Onticity (to R. Schack)" starting on page 314. Finally—though this may confuse things more than help things (because it is spiraling outward from your particular point)—if you have some extra time, *maybe* look at "28-07-03 Your Newest Turn (to R. Schack)" starting on page 316.

Anyway, as I say, you deserve a thorough answer to this one, and it will be a good exercise to me to figure out my own opinion!

Let me now, if you don't mind, turn this note to a different kind of soul searching that I've been going through, and I hope you will give me an honest, straight-up opinion in reaction … no matter what the reaction is. One of my friends at the PI meeting, whose opinion I respect very, very much, absolutely hated my talk—so much so that he took me aside on a private walk to let me know. The kinds of words that came out as a description of the talk were, e.g.: "too vague," "not focused," "irrelevant cartoons," "doing a disservice to this otherwise interesting research program," [i.e., the quantum Bayesian program], overhearing people say that I'm a nut, things like that. It was pretty devastating, and maybe I needed to hear it. On the other hand, from another group of people at the conference, I thought—but maybe it was just wishful thinking—I got the opposite impression. If I'm not mistaken Wayne Myrvold, for instance, said it was the clearest talk he had ever heard from me, and I remember some similar (though maybe not as extreme of) reactions from some of the other philosophers (and Bernstein, Greenberger, and Zeilinger). When I encounter things like this, my head spins and I feel like a lost little sheep; in some ways I am very fragile. All I care to do is get these ideas conveyed, and I have had a tendency to keep simplifying and simplifying to try to get this (what I view as a) rather simple message across: In going from classical to quantum, the notion of probability and its import stays the same, but what changes is the notion of event ("consequence Bayesian"). At the same time, one does not "renounce the enterprise of physics" (as Geoffrey Hellman said explicitly in his talk). Our program, in fact, is exactly what is needed to get back on track in physics. That's the message. But maybe my talks are of absolutely the wrong strategy, and I am doing the program a great disservice after all.

Here's where I'd like your straight-up opinion. Please be brutally honest. I've been devastated once; it is bound to be much easier to be devastated a second time around, and I am prepared for it. Do you think my talks on this subject would benefit by going heavier on technical details—quantum de Finetti theorems, senses of compatibility of quantum state assignments, POVM Gleason theorems, that kind of stuff? I.e., that my method of speaking is way off key, even for a foundational meeting like the Abnerfest? Taking Paul Busch's very clear talk—which I myself enjoyed immensely—as an example, do you think speaking more along the lines of that style would be of greater benefit to the quantum Bayesian program?

John's Preply

> Let me try to me a bit more articulate about one of the questions I was trying to ask in our discussion at Perimeter.
>
> Bayesians, as I understand them, take probability statements to be statements of beliefs. The obvious follow-up is the question "beliefs about what?" As I understand your view, in quantum mechanics you take this to be an agent's belief about the consequences of future actions, and you have shown how the ket can be understood as encapsulating those beliefs through, for example, your "bureau of standards" approach.
>
> Now suppose a Bayesian is not doing quantum mechanics, but rather paleontology, and is discussing the dinosaurs. If probabilities entered in his or her paleontology — and presumably they would because of the kind of central significance that Bayesians give to probabilities — those probabilities would again be taken to be statements of belief. Again one can ask "beliefs about what?" The man-in-the-street Bayesian (if such a person exists, but by the term I mean one not terribly familiar with quantum mechanics) might take those beliefs to be about when particular dinosaurs lived, what their physiology was like, what they ate, what caused their extinction, etc., etc. That is, the "x" in $P(x)$ would be the usual kind of proposition about "the external world" — in this case, "the world of dinosaurs" — that is common in, say, usual readings of

classical mechanics.

This would be different than the "x" in $P(x)$ that appears in your quantum mechanics, for there "x" is a proposition concerning the consequences of your actions, and not concerning "the quantum world."

In contrast to this, one could also imagine a Bayesian who insisted on using the same kind of "x" in paleontology as in quantum mechanics. That is, the "x" would refer to consequences of actions, such (as what we would colloquially describe) as "seeing a fossil of such-and-such a type upon digging down one more layer at such-and-such a site." Any talk about dinosaurs existing, fighting, eating, etc., would be taken just to be heuristic devices for helping one think about the actual consequences of doing such fieldwork in paleontology, and the work of the discipline itself would understood to be properly concerned not with someone's heuristic thoughts of 'dinosaurs' but rather with the fieldwork.

This leads to the point that, even if one is a fully-committed Bayesian, it seems to me that there are a number of types of which one might be a representative. Here are three:

- A thorough "consequence Bayesian," for whom the "x" in *any* $P(x)$ refers to the consequence of an agent's action.
- A "dualist Bayesian," for whom the "x" in $P(x)$ *sometimes* refers to statements about the external world of the type common in the usual readings of classical mechanics, and *sometimes* refers to the consequence of an agent's action.
- A "classical Bayesian," for whom the "x" in $P(x)$ *always* refers to statements about the external world of the type common in the usual readings of classical mechanics.

That is, simply committing to a "belief understanding" of probabilities does not in itself commit you to a particular view of to what kind of entity the "x" in $P(x)$ refers.

You would help me a lot if you would answer the question: In your view, what is the nature of the x's in *all* the $P(x)$ that arise in science? Do they *all* just identify consequences of actions of the agent? Or can some of them identify propositions about the external world? And if some of them identify propositions about the external world, what is the rule for establishing when that is possible? And if none of them identify propositions about the external world, paleontology is not about dinosaurs!

27-07-06 *A Probability Calculus for density Matrices* **(to M. Warmuth)**

Thank you for drawing my attention to your paper with Kuzmin. To the moment, I have only given it a cursory look, but it looks very interesting!

In fact, I hope that you will post both this paper and the other one on the `quant-ph` archive, `http://www.arxiv.org/archive/quant-ph`. It is effectively the main "journal of quantum information and computing" with most of us in the field looking to see what's new there daily. It would be good for your results to be posted there for discussion and debate and building upon—this is certainly a community interested in such things.

Anyway, as I say, I look forward to understanding your papers. There are indeed now several distinct things that one might be tempted to call a "quantum Bayes rule." Exploring the realm of possibilities is good exercise and will lead to a better understanding of all that quantum mechanics has to offer.

My own preferred direction *at the moment* is actually *not* to think of density operators as "generalized probabilities" as you do in your papers, but rather as *single* (normal, usual, classical, whatever you want to call them) probability distributions for the outcomes of a fixed, fiducial measurement (an informationally complete POVM). Then, the probability distributions for the outcomes of all other measurements can be thought of as coming about through a kind of coherence condition (analogous to Dutch book, but empirical in nature). That is to say, the import of the Gleason theorem is that it tells us how to interrelate (or transform) probability distributions, not how to set them.

Caves, Schack, and I will make some remarks on this in our newest paper, which we hope to post soon. However, let me give you a couple of existing pointers that at least broach the subject:

- "Quantum Mechanics as Quantum Information (and only a little more)"
  `http://www.arxiv.org/abs/quant-ph/0205039`
  (See particularly, Subsection 4.2 and Section 6.)

- "Unknown Quantum States and Operations, a Bayesian View"
  `http://www.arxiv.org/abs/quant-ph/0404156`

By the way, I already like the prettiness of this matrix operation you define in your Eq. (1). (Though I was certainly aware of a similar operation used by Cerf and Adami—maybe though they used it in a more restrictive way—once upon a time, you helped bring it from the recesses of my memory.) For your enjoyment (and contrast!), you might have a look at the monstrosity I once constructed to draw out some perceived similarity between quantum conditioning and Bayes' rule. (The idea at the time was that the initial (pre-measurement) quantum state ρ fulfills both the role of $P(h)$ and $P(d)$ in an application of Bayes' rule $P(h|d) = P(h)P(d|h)/P(d)$.) You can find the thing at page 90 of `http://www.arxiv.org/abs/quant-ph/0105039` in the subsection titled "Where Did Bayes Go?"

27-07-06 *Course Adjustment?* (to W. L. Harper)

It was good to finally meet you last week! I greatly enjoyed our discussions, and I hope we'll pick them up again sometime.

For the moment though, I wonder if I might ask you a (probably difficult) question. I hope you will give me your honest, straight-up opinion ... no matter what the opinion might be. The reason I'm picking on you in particular is because it seems you got something out of my talk (I'm basing this on a couple of comments you made at the end) and because I don't think you've ever heard me speak before. Thus perhaps you're a little more blank-slate for the question at hand than someone else I might ask.

OK, here goes.

One of my friends at the PI meeting, whose opinion I respect very, very much, absolutely hated the talk—so much so that he took me aside afterward to let me know. The kinds of words that came out in his description of the talk were, e.g.: "too vague," "not focused," "irrelevant cartoons," "doing a disservice to this otherwise interesting research program," [i.e., the quantum Bayesian program], overhearing people say that I'm a nut, things like that. It was pretty devastating, and maybe I needed to hear it. On the other hand, from another group of people, I thought—but maybe it was just wishful thinking—I got the opposite impression. For instance, if I'm not mistaken Wayne Myrvold said it was the clearest talk he had ever heard from me (though maybe he was just being facetious). Anyway, I certainly got some positive reactions from a few other people.

When I encounter opposite impressions like this, my head spins and I feel like a little lost sheep. All I generally care to do in these quantum foundational talks is get the main "quantum Bayesian" message across, and I have had a tendency to keep simplifying and simplifying to try to get that done. But maybe my talks are of absolutely the wrong strategy, and I am doing the program a great disservice after all, as my friend thought.

Here's where I'd like your straight-up opinion. Please be brutally honest. I've been devastated once; it is bound to be much easier to be devastated a second time around. Do you think my talks on this subject would benefit by my going heavier on technical details—quantum de Finetti theorems, senses of compatibility of quantum state assignments, POVM Gleason theorems, that kind of stuff? I.e., that my method of speaking is way off key, even for a foundational meeting like the Abnerfest? Taking Paul Busch's very clear talk as an example, for instance, do you think my speaking more along the lines of that style would be of greater benefit to the quantum Bayesian program? Or do you think it would be just the opposite, at least for someone's (like your own) first encounter with me?

I am truly perplexed, and if you could give me another data point to work with, that would be most useful for how I adjust my course. Maybe I'll cc this note to Wayne, so he'll know what kinds of things I'm saying to his colleague (particularly if I'm misquoting him!).

28-07-06 *Bad Reality Creation* (to G. L. Comer)

I don't know if you read Paul Krugman in the *NY Times*, but I wanted to have a recorded of the closing lines of today's column. So, I'm going to send the whole column to you if you don't mind (in case you wanted to understand the context). This is a very perplexing situation indeed—I've been scratching my head over it for six years!

From Paul Krugman, "Reign of Error," *New York Times*, July 28, 2006:

> The climate of media intimidation that prevailed for several years after 9/11, which made news organizations very cautious about reporting facts that put the administration in a bad light, has abated. But it's not entirely gone. Just a few months ago major news organizations were under fierce attack from the right over their supposed failure to report the "good news" from Iraq — and my sense is that this attack did lead to a temporary softening of news coverage, until the extent of the carnage became undeniable. And the conventions of he-said-she-said reporting, under which lies and truth get equal billing, continue to work in the administration's favor.
>
> Whatever the reason, the fact is that the Bush administration continues to be remarkably successful at rewriting history. For example, Mr. Bush has repeatedly suggested that the United States had to invade Iraq because Saddam wouldn't let U.N. inspectors in. His most recent statement to that effect was only a few weeks ago. And he gets away with it. If there have been reports by major news organizations pointing out that that's not at all what happened, I've missed them.
>
> It's all very Orwellian, of course. But when Orwell wrote of "a nightmare world in which the Leader, or some ruling clique, controls not only the future but the past," he was thinking of totalitarian states. Who would have imagined that history would prove so easy to rewrite in a democratic nation with a free press?

31-07-06 *To the Memory of Walter Philipp* (to A. Y. Khrennikov)

That is very sad news. Do you know any details of how Walter passed away? Was it a hiking accident or something? Amazing how a person can be with you one day and gone the next. There should be an object lesson in that which stays in the backs of all our minds.

31-07-06 *Hi* (to W. E. Lawrence)

Lawrencesm 3: *The Conference* [“Quantum Reality, Relativistic Causality, and Closing the Epistemic Circle: An International Conference in Honour of Abner Shimony,” July 18–21, 2006, Perimeter Institute, Waterloo, Canada] *looked as if it would be wonderful. What's your impression of it? — actually, before you answer — I've got a mind to call you up and ask “in person,” as it were. But if you still want to answer …*

Good to hear from you. It was a fun conference. I got quite a bit from the talks, and the “dialog” between Lee Smolin and Abner Shimony one night in the Black Hole Bistro was particularly fun.

For myself, I gave a sadly polarizing talk: At the end of it, one of the philosophers came up to me and said it was the best talk he had ever heard from me. On the other hand, one of the physicists (one whose opinion I respect very, very much) came up and said it was the worst talk I had ever given. Unfortunately, I suspect the disparity in those comments indicates something deep! Something I'd really rather live in denial about.

Anyway, you shouldn't feel that you completely missed the meeting. You can still view the talks by going to this website: `http://pirsa.org/`. At the very least, you should enjoy the “Bistro Banter” that I mentioned above.

02-08-06 *Collaboration?* (to L. K. Grover)

Seeing your note to Bill coming by reminded me that I'd like to eventually discuss a possible collaboration if you're interested. I was intrigued by some of your remarks at the July 11 meeting in the auditorium here, about how the $\pi/3$-phase-shift problem had a more understandable picture in terms of the evolution of probabilities rather than amplitudes. That made me wonder how the Grover algorithm and all its variants would look if written in terms of this representation of quantum mechanics I've been developing — where state vectors are replaced by probabilities for the outcomes of a certain informationally complete measurement.

Would you be interested in seeing if we could translate your algorithms into those terms?

I'm working rather steadily with my student Hoan on this representation until he leaves Sept 1. But after Sept 1, I should be free to start another big project like the one above. My hope is that it could give us some real insight into another way of looking at your algorithm.

06-08-06 *The Story of Little Chocorua* (to J. B. Lentz & S. J. Lentz)

> [[Flashback to three months before the present note:]] Kiki's gone freakier than usual on me. A couple of weeks ago, she found a hundred year old Victorian that's going to be torn down, and she convinced the owner to let her have all the doors. So, I've got 16 doors (solid, mind you, and historical), but still 16 doors, stacked up in my garage. Final destination: She's going to make the walls for the girls' playhouse from them.

> Too much time on her hands and certainly too much imagination for her good (or at least for the good of our relationship). I told her she's got a month to get them out of my garage.

I guess I have to admit that Kiki is a true blue carpenter now, and that I'm quite proud of her product. The playhouse is fantastic. And she's a pretty good salesman too, calling it Little Chocorua (after William James's weekend home in Chocorua, NH). Did she tell you the story? James was so proud of the fact that there were 11 doors leading outside at his home; he could walk out and look in any direction on his land. Well, the count of doors on Little Chocorua is 12. I tell everyone we've done William James one better.

By the way, here was another ploy of Kiki's for softening me up. The big green patch on the right front door is chalkboard paint; I don't know if you knew. Kiki says to the kids, "You should think about how much you'd charge Dad for renting out Little Chocorua as a conference center. He could bring his colleagues over and they could have small seminars around the chalkboard, if you'd rent your space out." That certainly did get me to start thinking, "Hmm. This might really be a good place for a small quantum information conference." She's a professional I tell you!

You two made quite a daughter.

06-08-06 *Kiki's Wacky House* (to M. D. Sanders & D. B. L. Baker)

Let me give you a follow-up on the hair-brained scheme of Kiki's that I told you about a couple months ago.

I wrote the note below to Kiki's parents today since it's Kiki's birthday—it tells a little more of the story of the house—and included some pictures. [See 06-08-06 note "The Story of Little Chocorua" to J. B. Lentz & S. J. Lentz.] Before stripping the pictures out of Outlook (so the `.pst` file doesn't get too big), I thought I might send them to you too. In the end, I'm pretty proud of the damned thing, and Kiki too. She built every inch of the place all by herself—from the foundation to the frame to the roof—not one ounce of help from me or anyone else. And even with the old ship's porthole window and the fire pole on the inside, the total cost was less than $350.

Anyway, admitting that maybe she's not so crazy after all (to all the people I had told she was quite crazy) is part of my birthday present to her.

15-08-06 *Drink the Kool-Aid* (to C. H. Bennett)

I have to apologize: After our horse play the other day, I got carried away with some bureaucratic duties, and then simply *forgot* to come back to your note. Pathetic. Then Kiki, the kids, and I took a long weekend of sailing and amusement parks, etc.

I hope I'm not writing you back so late that the reply will be irrelevant.

Bennettism 27: *I thought first of Jonestown, but according to Wikipedia the Jonestowners drank a less well-known brand of grape drink. "Drink the Kool-Aid" is said to have originated earlier, in the 1960's, from Ken Kesey's use of it as a vehicle for LSD. Thus I can be said to have drunk the Kool-Aid of Many Worlds. I thought that Lucent, like IBM, had drunk the Kool-Aid of DTO funding.*

But what about basing QM on Yes QKD and No BC? I am trying to write a piece on conceptual foundations of quantum crypto. As I recall there was some difficulty with this program, and at one *point it had evolved to no-broadcasting and no superluminal communication, without mentioning bit commitment.*

Yeah, the Bub, Clifton, Halvorson stuff wasn't too very convincing after my initial excitement wore off. First, by starting off in a framework of C^*-algebras, they were effectively invoking quantum mechanics to begin with—or at least I always felt that way. Then, it was pointed out that "No BC" wasn't doing any work for them at all. And the last report I heard from Halvorson was that "no superluminal" was mostly redundant too. A good explanation/critique comes in Chris Timpson's PhD thesis: `http://philsci-archive.pitt.edu/archive/00002344/`. See pages 196–225; it's easy reading.

In the end, I don't think those early ideas of Gilles' and mine go very far toward getting at the essence of quantum mechanics. The best counterexample, I think, is in the form of Rob Spekkens' toy model: `quant-ph/0401052`. It appears to have "Yes QKD" and "No BC", and much more (like a no-broadcasting principle and correlation monogamy) but it sure ain't quantum mechanics. Instead it relies "merely" on an information limitation with regard to some hidden variables (i.e. pre-existing values).

I find that work of Spekkens tremendously exciting because I learned so much from it. It didn't lessen my conviction that the essence of quantum mechanics has to do with the intricate connection between observer and observed in the quantum world—in fact I think it is the best demonstration yet that quantum states are expressions of information, rather than stand-alone properties of the systems (or the universe) themselves—but it did help me to understand how much more interesting the story has to be. Spekkens' toy model is based on the very pre-existence of measurement values (for that is what drives his combinatorics), but in the quantum world (because of Bell-Kochen-Specker), measurement values don't pre-exist the measurement process. I now think it is that that is primary in quantum mechanics, and things like NO BC, YES QKD, no-broadcasting, teleportation, etc., are all secondary.

If you have any more questions, I promise to be more attentive this time.

Charlie's Reply

> We miss you too. Why don't you come up and visit us more often, so we can pretend to throw your computer away like in the old days? Or we'll come down and visit you. I thought of you when I was visiting my late mother's best friend Cynthia, 89 years old, the other day, along with her daughter Sally, my sister's best childhood friend, and her grandson Will (in his 20's). Sally said "quantum mechanics – that must be hard to understand". Her mother said, "We'll I'm taking a course in it for seniors, emphasizing the history and concepts, without the math". I said, "Good for you. The history is rather lively. For example one of the greats, Schrödinger, was a notorious womanizer, and historians are trying to figure out which mistress he was with when he came up with the Schrödinger equation". Sally said, "for a while they even thought of calling it condom mechanics."

18-08-06 *Aqualung* (to C. M. Caves & R. Schack)

> Feeling like a dead duck –
> Spitting out pieces of his broken luck.
> Sun streaking cold –
> An old man wandering lonely.
> Taking time
> The only way he knows.
>
> — Jethro Tull, "Aqualung"

Attached is a draft where I made most of the changes I had suggested before. Mostly I niggled a lot with some of the phrases to do with "facts" and "the world", in an attempt to be a little more careful, and so as not to give an impression that we do not think the agent is part of the world. If you would like to get a better feeling for all the places I made changes (even trivially small ones), I could fax you my marked up manuscript.

The main thing that I regret not having touched very much is the discussion of objective chance and the weakness of the Principal Principle and such. That really is the weakest point of the paper, and I wish we could do something better. I suspect it is because of the weakness of that section that Carl's students came out swinging. (Will they like this version better? I did try to keep them in mind, particularly in the closing statements.) And I also suspect that it is the weakness of the chance section that is going to lead to our most trouble in the future (particularly with the philosophical set, which is an annoying, but undeniable, fraction of our audience—I hate to say it, but they're the ones who at least take us more seriously than anyone else).

23-08-06 *My Revision* (to C. M. Caves & R. Schack)

Cavesism 77: *I think I understand why "ascertaining" was replaced by "ascertaining or prompting" in the second paragraph of the Intro: "Gathering new data—i.e., ascertaining or prompting the truth values of various propositions—allows the agent to update his probability assignments, generally by using Bayes's rule." It is probably meant to deal with the impression that ascertaining gives of finding out something that pre-exists. I'm sympathetic to that, but suspect that the prompting will puzzle many readers. Still, I don't want to change it.*

Yes, that was the motivation. And, yes, you are probably right that a reader will be puzzled. And finally, I'm happy that you don't want to change it. But, I don't see that a small footnote with one sentence of explanation could hurt. Might that be an effective analgesic?[110]

I think we're good to go on all else. I'll certainly accept Rüdiger's kind offer of making all submissions. With all my computer problems, I barely have a functional office (still can't pin the problems down).

Cavesism 78: *In other words, here independent does not modify the verb demands, which would call for an adverb, but is a predicate adjective in a clause that has been abbreviated.*

[110] Afterward, Carl added this footnote to the paper: "We introduce 'prompting' here because 'ascertaining' has the connotation of determining a preëxisting property, which is in conflict with the central point of our paper. Having made the point, however, we banish this connotation from the use of 'ascertaining' and use it for the remainder of the paper." Later, he further changed 'prompting' to 'eliciting'; see my 16-10-06 note to Caves below.

In my professional career, I have always felt intellectually second-class whenever I'm in the presence of people who invoke terms like "algebraic geometry" or "diffeomorphism invariance" when speaking of a problem. (Howard Barnum still frightens me to no end when I compare notes with him on any subject that I've been thinking about for some time.) But ever since meeting Charles Bennett and Carlton Caves, I have realized that my greatest inadequacy is actually English grammar.

Cavesism 79: *It's always a privilege to write a paper with Chris (frustrating, but a privilege), partly because the references make one look so much more learned than one really is.*

The intent of the "one" can be read in at least two ways. It's probably psychologically useful to me to believe that it is the more innocent of the two.

23-08-06 *NATO Workshop Gdansk 10-13.09.2006* (to M. Żukowski)

Well, I've had no luck finding a speaker from the U.S. who could carry the torch of "quantum states are information, not physical properties." But I have pinpointed a colleague from the U.K. who would do the point some serious justice, and in a technically important way.

It is Prof. Rüdiger Schack at U. London, Royal Holloway. He would talk about recent work of he and Carlton Caves, that is some of the most important I've seen in our quantum-Bayesian effort in a while: It is an analysis of "quantum random number generators" from our perspective. The work ultimately connects to the precise meaning of how quantum cryptography can be more secure than classical cryptography even in this Bayesian view of quantum probabilities. The differences from the naive view—which amounts to little more than blithely saying, "it's secure because of physical law," which is an almost empty statement—are pretty striking.

So, I hope that piques your interest. He would be a very good representative of our effort, and I saw the talk earlier this summer and know that it's excellent.

I know that he is from the U.K., but he would be talking about collaborations with his U.S. colleagues (Caves and me). Do you think you can do it? Would NATO allow it?

I apologize again for not being able to attend myself. It looks like I am going to miss a really good and really important meeting.

31-08-06 *My Two Cents* (to J. Finkelstein)

Finkelsteinism 7: *It seems to me that to say that a detector has clicked is to make a statement about the state of that detector (or about the state of the device which records the clicks, which comes to the same thing). Since you take seriously the fact that experimental devices are necessarily quantum systems, I would have expected you to say that a statement about the state of a detector is a statement of subjective belief, not of objective fact.*

To put it somewhat differently, in your paper you argue that since a preparation device is itself a quantum system, objective facts about that device do not determine the prepared quantum state. But since the preparation device is a quantum system, how, in your view, could there be any objective facts about it?

The way I would put it is to say that the cut is not three-way as you outline—that is, between the quantum system, the detector, and the agent—but rather two-way: In our scenario there is only the agent and the external world. The way one should think of the measurement device is the same way one should think of a prosthetic hand—it is an extension of the agent.

"Detector click" is shorthand for *sensation* in the agent, and in my personal view, that is all it is. The value of the "detector click" is beyond the control of the agent; it is not a function of his beliefs or his desires. His interaction with the external world gives the class of potential sensations, but it does not set the value.

Thus, the value of the detector click—call it d—is objective for the agent. (There is one sentence in the paper where adopted almost that phraseology "an event is a fact for the agent," and it might have been wise to adopt it more consistently.) What is subjective from our view—i.e., a function of the agent and not the data or external world alone—is the POVM associated with those potential values. That is, the set of operators E_d.

I expand on this in the essay "Me, Me, Me," [in this collection], which I still think is worthwhile reading. Let me also attach a file I wrote a couple of months back in response to some issues of Jeff Bub's (it's maybe a little more up to date). [See 24-06-06 note "Notes on 'What are Quantum Probabilities'" to J. Bub.] Particularly relevant to the present discussion are the points I make right after Bubisms 9, 10, and 13. I'll also attach my favorite way of illustration of the point (in the form of a `.jpg` file.) There is an agent, there is an external world, there is an interaction between the two—the agent acts on the external world, it causes a reaction back in him (sensation). As far as he is concerned that reaction is not subjective; it's as real as anything he's ever seen.

To make it clear: I'm not speaking for Carl or Rüdiger with these paragraphs. They may disagree with one or another nuance of what I've said. We tried to write the paper in a neutral way that didn't do a disservice to any of our particular views (though I think they are mostly aligned, except for particular choices of words).

11-09-06 *My Swerves and Yours* (to M. S. Leifer)

Leiferism 2: *For now, I'll just leave you with one comment. I think a better name for "Zing" would be "the swerve", in deference to Epicurus. That's the earliest reference I can think of to any similar sort of notion.*

I've thought about "swerve" before (in the context of the old atomists and within our own), but I don't think the word really captures whatever it is I'm trying to get at. For instance, somewhere in my first samizdat, you'll find this sentence:

> The quantum mechanical indeterminism doesn't come about from an indiscriminate swerve in the path of an atom; it comes from the point of contact between the theory and the world—the measurement.

In my quantumness of a Hilbert space paper, I also remember writing this:

> Associated with each quantum system is a Hilbert space. In the case of finite dimensional ones, it is commonly said that the dimension corresponds to the number of distinguishable states a system can "have." But what are these distinguishable states? Are they potential properties a system can possess in and of itself, much like a cat's possessing the binary value of whether it is alive or dead? If the Bell-Kochen-Specker theorem has taught us anything, it has taught us that these distinguishable states should not be thought of in that way.
>
> In this paper, I present some results that take their *motivation* (though not necessarily their interpretation) in a different point of view about the meaning of a system's dimensionality. From this view, dimensionality may be the raw, irreducible concept—the single *property* of a quantum system—from which other consequences are derived

> (for instance, the maximum number of distinguishable preparations which can be imparted to a system in a communication setting). The best I can put my finger on it is that dimensionality should have something to do with a quantum system's "sensitivity to the touch," its ability to be modified with respect to the external world due to the interventions of that world upon its natural course. Thus, for instance, in quantum computing each little push or computational step has the chance of counting for more than in the classical world.

Zing is meant to capture something intrinsic to the system. The bit of indeterminism that comes about in a quantum measurement I don't think of as intrinsic to any one thing. Rather, it is more along the lines of the relational stuff you PI people always talk about—it is a function of two objects, not one.

18-09-06 *Seeking Advice* (to L. Hardy, R. W. Spekkens, G. M. D'Ariano, R. Schack, D. M. Appleby, and M. S. Leifer)

Dear Friends and Acquaintances of the epistemic quantum state,

I wonder if I can ask your advice on the following subject—I'd like to get your opinions before making a decision myself. I let the distribution list in sight, so you could see who else I was consulting, but please write back to me privately—I will keep your opinion in confidence.

Marlan Scully has given me the opportunity to have a plenary talk at his annual "Physics of Quantum Electronics" Conference in Snowbird, Utah (January 2–6), and more importantly, associated with that, the chance to organize a session around the subject with 4 or so other invited talks. The thing I'm wondering is whether it would be worthwhile for our community for me to follow through with this? If I were to organize a session it would be to highlight recent work (and potential applications) of epistemic/operational/Bayesian views of the quantum state. Thus, this is why I'm writing to you.

It is decent sized meeting; I think there were over 300 people there last year, including a couple Nobel prize winners (Glauber, and I can't remember the name of the other one). You can go to this website if you want to get a feel for what the line-ups the last few years have looked like: `http://www.pqeconference.com/`. Old programs since 2001 are posted there.

Anyway, the good I can see coming from this is that we would have a captive audience from a set of people who normally have far more to do with the applied side of physics than the ones we are usually around. Maybe that would be useful for raising recognition of the promise of recent research in quantum foundations and its uses for the wider physics community. The downside is that this is a very expensive conference, and there is no financial aid for it: Registration is $350 USD, hotel comes to $180/night (though that could be halved by doubling up in rooms for anyone who cares to), meals are *not* included, and the only restaurants available are the ones of this resort hotel. Not cheap. They say the skiing is great (and the conference shuts down every afternoon for it), but for me personally, that is of very little consequence.

I would like to know your opinion of the value of such a get together before following through with this or not following through with this. Also, would you yourself come?

18-09-06 *Big Expensive Conferences* (to J. E. Sipe)

Sorry, I still owe you that note on the extent of my "consequence Bayesianism." These are heady waters and I took a couple months off from foundational chit-chat to do some hard calculating while

I had a good summer student. Plus, after the Shimony meeting, I think my system needed a good purge. However, I will come back, and probably soon (as we're starting to get referee reports on `quant-ph/0608190`, and some of their points concern the one you're also getting at). But I don't feel too terribly guilty—you yourself never called me, after bringing up the possibility twice.

18-09-06 *Another Topic* (to M. S. Leifer)

Leiferism 3: *On a completely different topic, I wanted to ask you a question in your role as chief expert on distinguishability measures in quantum theory. Do you know if anyone has done any work on axiomatic approaches to distinguishability measures, along the lines of the axiomatic approach to entanglement measures? Specifically, I have in mind that a distinguishability measure would be defined as a functional of two states that is monotonically decreasing under CP maps, with perhaps a few other regularity conditions, and then that two such functionals could be found that provide upper and lower bounds on all such measures.*

You can look at Jozsa's *J. Mod. Optics* article for a first pass. But the real powerhouses in this regard are Uhlmann and Petz. They have plenty written on the axiomatics of the subject. Particularly, monotonicity has always been an essential ingredient. A good place to get a start in the literature is probably the reference list in Ohya and Petz's book; or maybe flipping through Petz's web page: `http://www.math.bme.hu/~petz/`.

18-09-06 *New Course: Einstein's Universe* (to G. L. Comer)

Word of warning on other matters: Never ever get involved with 30 year old mathematical problems if you can help it. They'll destroy your career just as surely as not doing any research at all will. It's "performance review" time for us at Bell Labs this week, and I've got nothing at all to show for this last year. God pity my mortgage.

Comerism 13: *I think I have you beat: I'm working on dissipation in general relativity, which has now about a 100 year history. Maybe more physics than math, but still unresolved.*

Ah, but do you have something to show for your last year of work? I'll bet I've got less than you! (Steven van Enk once told me about a competition he saw where the contestants were each trying to see who could blow a noodle from his nose the farthest.)

18-09-06 *Nonbayesian Musings* (to R. D. Gill)

Gillism 1: *I've made a first rough write up of some zen/frequentist quantum philosophy thoughts:*

`http://www.math.leidenuniv.nl/∼gill/waveparticle.html`
`http://www.math.leidenuniv.nl/∼gill/fifthposition.html`
`http://www.math.leidenuniv.nl/∼gill/BetterBell.pdf` *(Gdansk talk)*

Thanks for the coordinates (all of them). Apparently though, I already had your new email address in my book—don't know how it got there. Congratulations on your move to Leiden; I hope you fill Lorentz's shoes well!

Taking a look at your Gdansk talk, I noticed isn't there a contradiction in these two sentences in your closing transparency:

1) "I think that only detector clicks in the past are real."

2) "The probabilities are for real, the past is real, the wave function is objective ..."

Specifically, if "*only* detector clicks in the past are real" how can there be room for the probabilities to be real TOO? I'll say (tongue in cheek): Make up your mind!

19-09-06 *Werner and Misrepresentation* (to R. D. Gill)

I read this line in your Gdansk talk: "Recall the quantum information open problems site of Reinhard Werner (and let's wish him a speedy recovery too)." What has happened to Reinhard?

And I read this line in one of your other pages:

Gillism 2 : *Contrary to the currently popular Bayesian interpretations of quantum physics, there is an objective reality (again: the past).*

That is simply a gross misrepresentation. You grant reality to detector clicks; we grant reality to detector clicks. And I certainly grant reality to the past. I go further: I grant reality to quantum *systems* (even when they're not in interaction with detectors) and their associated Hilbert-space dimensionality. And Carl Caves among us, for instance, grants reality to the "intrinsic" Hamiltonians associated with quantum systems. There are lots of things within the theoretical structure of quantum theory that we quantum Bayesians are willing to grant reality to.

The main things we *are not* willing to grant an intrinsic reality to (i.e., a reality outside a gambling agent that makes use of them) are *probabilities* and *quantum states.* But that is a far cry from a position that "there is no objective reality," as you effectively attribute to us above.

It's almost insulting that you cannot seem to get this simple, little point.

But you're not all bad, Richard. I've always liked you. And in the present instance, I very much like this sentence of yours: "As the present moves relentlessly forward the 'past' crystallizes out of it, randomly." I am very much attracted to that idea, and have spent some time thinking about it myself. If I understand your sentence correctly, the ideas are along the lines of those of George Herbert Mead's at the turn of the last century. I'll paste in a little review of his ideas below. [See quote in 23-09-03 note to Steve Savitt, titled "The Trivial Nontrivial".]

22-09-06 *Chutes and Ladders* (to D. M. Appleby & H. B. Dang)

Thanks for the sympathetic note.

I spent a tossy-turny night having all kinds of wacky thoughts ... and trying to suppress them so that I might get some sleep. The main thing I kept coming back to is: Might we find some way of combining the first phase of this summer's work with the second? Could we get any traction from that?

What I mean by the first phase is our general study of type-E matrices.[111] Particularly our reduction (under the assumption of Appleby form) to finding a set of d^2 phases so that a certain $d \times d$ matrix is rank-1 (the matrix given by Eqs. (11) and (12) in SIC-Notes C, 10 August). Phase

[111]Let us call a $d^2 \times d^2$ matrix "type-E" if it is Hermitian and has $1/d$ on the diagonal and $1/d\sqrt{d+1}$ times various $e^{i\phi}$s for all the off-diagonal elements. There is no restriction that a type-E matrix be positive semi-definite. SIC Gram matrices (up to a scale factor) are special cases of the type-E matrices—they are simply type-E matrices that are rank-d projectors. For all type-E matrices E it automatically holds that $\mathrm{tr}E = \mathrm{tr}E^2 = d$. Thus if E is a type-E matrix and $\mathrm{tr}E^3 = \mathrm{tr}E^4 = d$, then E is a "SIC-projector".

2 was when we started thinking about minimizing the frame potential and/or explicitly solving the equations for a SIC-generating fiducial vector.

How to combine these two phases?

Here's one, almost surely useless idea. (But it gives an indication of the sorts of things my mind was getting hung up on.) Start with a Weyl-Heisenberg Gram matrix generated from some arbitrary fiducial state. Next, find the nearest (in matrix norm) type-E matrix. It won't be positive semi-definite or rank-1, but it will probably be closer to being such than an arbitrary choice. So, next, find the nearest WH Gram matrix to this type-E matrix. It generally won't be the original WH Gram matrix; it'll be a different one ... presumably being nearer to type-E form than the original. And on we go iteratively. A far-fetched question is: Might one prove that this process generically converges to a sought-for SIC Gram matrix? I.e., we just climb a ladder to get to it.

Sounds tough to me; the idea's probably a chute, rather than a ladder. But mostly I'm interested in how we might combine our two separate trains of thought.

Here was another question that crossed my mind. Might anything shake loose if we stopped thinking about the Gram matrices themselves (in the WH case), but the difference between the Gram matrices and our target. Might this give us the tools to start considering small deviations from the target ... possibly getting a set of quadratic equations (rather than quartic)?

This is off the beaten track, but I remember Holevo's technique for proving his bound on accessible information. He didn't try to prove the bound directly, but rather he started with an arbitrary ensemble of states and considered a one-parameter deviation from them. Then he found the second derivative of the information with respect to that parameter. The second derivative was relatively easily bounded, and then he just integrated back up. I think you can read about the technique in my old paper "Mathematical Techniques for Quantum Communication Theory" (`quant-ph/9604001`).

Just thinking out loud ...

26-09-06 *Something Significant* (to D. M. Appleby & H. B. Dang)

I just wrote you a very quick and dirty note. The result is below. I have no idea why these simplifications seem work, and have not made any attempt to prove them analytically yet. But I am very, very pleased that they appear to be true. You will see below.

I hope all is well with you. I haven't had a chance to read your note from yesterday. I've been sick in bed most of the day with a stomach virus. I'll look at your note soon after this.

Recall that the frame potential in the Weyl-Heisenberg case is given by

$$F = d^3 \sum_{jn} \left| \sum_s a_s^* a_{s+j} a_{s-n} a_{s-n+j}^* \right|^2 \tag{47}$$

and the quantity will achieve its lower bound of $2d^3/(d+1)$ if and only if the following d^2 equations are satisfied:

$$\sum_s a_s^* a_{s+j} a_{s-n} a_{s-n+j}^* = \frac{1}{d+1}(\delta_{n0} + \delta_{j0}) \,. \tag{48}$$

Defining the (unnormalized) vectors $|\psi_j\rangle$ by

$$|\psi_j\rangle = \sum_s a_s^* a_{s+j} |s\rangle \tag{49}$$

another way to write Eq. (48) is as

$$\langle\psi_j|X^n|\psi_j\rangle = \frac{1}{d+1}(\delta_{n0}+\delta_{j0}) \; . \tag{50}$$

(I'm only going to the trouble of introducing this notation so that one can identify the pieces of the little Mathematica routine I'm about to record. It's not essential for what is about to be said.)

Reducing the Number of Equations Drastically

OK, with this set-up, here's the exciting thing. Numerical work—at least from $d=4$ to 9 (I've only checked this little bit because I'm hoping Marcus will see this note before he goes to sleep, and so I'm rushed for time)—seems to indicate that we can effectively get rid of the n-index in the equations all together, being left with a little over $3d$ equations.

That is, it seems we can trim the full set of equations to (at least) only the following. 1) $n=j=0$,

$$\sum_s |a_s|^4 = \frac{2}{d+1} \; . \tag{51}$$

2) $n=0$, $j\neq 0$, (and similarly the case $j=0$, $n\neq 0$),

$$\sum_s |a_s|^2|a_{s+j}|^2 = \frac{1}{d+1} \; . \tag{52}$$

3) $n=j$, $j\neq 0$,

$$\sum_s (a_s^*)^2 a_{s+j}a_{s-j} = 0 \; . \tag{53}$$

And 4) $n=j+1$, $j\neq 0$

$$\sum_s a_s^* a_{s+j}a_{s-1-j}a_{s-1}^* = 0 \; . \tag{54}$$

There may be further simplification possible, but I haven't had a chance to look into it yet. My first instinct was to hope that condition 3) could be taken away too—the roots of this were in one of Hoan's early hopes (one equation per diagonal in the outer product matrix, remember Hoan?) but at the moment this doesn't appear to be the case. (Or it may simply be that Mathematica's numerics are just having trouble settling down when I throw that constraint away. I'm just not sure at the moment.)

Anyway, you should probably check this independently as you have a chance. Below is the Mathematica instruction set I used:

```
a[i_] := b[i]+ I c[i]

NN = Sum[Abs[a[Mod[s,d]]]^2,{s,0,d-1}]

NN2 = Sum[Abs[a[Mod[s,d]]]^4,{s,0,d-1}]

CC[j_,n_] := Abs[Sum[Conjugate[a[Mod[s,d]]]*a[Mod[s+j,d]]*
             a[Mod[s-n,d]]*Conjugate[a[Mod[s-n+j,d]]],{s,0,d-1}]]

EC := Abs[CC[0,0]-2/(d+1)] + Sum[Abs[CC[j,0]-1/(d+1)],{j,1,d-1}] +
```

```
    Sum[Abs[CC[0,n]-1/(d+1)],{n,1,d-1}] +
    Sum[CC[j,j],{j,1,d-1}] + Sum[CC[j,j+1],{j,1,d-2}]

d = 7

nn = NMinimize[{EC, NN == 1, NN2 == 2/(d+1)},
     Join[Table[b[i], {i, 0, d-1}],Table[c[i], {i, 0, d-1}]],
     Method -> {"DifferentialEvolution", "RandomSeed" -> 3}]

bb = Table[b[i] /. nn[[2, i + 1]], {i, 0, d-1}]

cc = Table[c[i] /. nn[[2, i + d + 1]], {i, 0, d-1}]

aa = bb + I cc

psi[j_] := Conjugate[aa] RotateRight[aa, j]

Table[Conjugate[psi[j]].RotateRight[psi[j], n], {j,1,d},{n, 1, d}]

MatrixForm[%]
```

To look at other dimensions, simply change the value of d in there. Also you have to jiggle a bit with the random seeds in each case, to get a correct answer. Here are the random seeds that worked best for me (at least on my machine):

dimension	random seed
4	2
5	3
6	0
7	3
8	5
9	11

The last line of the code above generates a view of how Eq. (50) is satisfied. *EC* is minimized to zero as it should be with the constraints advertised above. But what is nice is then that *all of* Eq. (50) is satisfied automatically.

I am very excited by this.

28-09-06 *Simple, Getting Simpler* (to D. M. Appleby & H. B. Dang)

Notation

Recall that the frame potential in the Weyl-Heisenberg case is given by

$$F = d^3 \sum_{jn} \left| \sum_s a_s^* a_{s+j} a_{s-n} a_{s-n+j}^* \right|^2 \tag{55}$$

and the quantity will achieve its lower bound of $2d^3/(d+1)$ if and only if the following d^2 equations are satisfied:

$$G_{jn} \equiv \sum_s a_s^* a_{s+j} a_{s+n} a_{s+n+j}^* = \frac{1}{d+1}(\delta_{n0} + \delta_{j0}) \ . \tag{56}$$

Defining the unnormalized vectors $|\psi_j\rangle$ by

$$|\psi_j\rangle = \sum_s a_s^* a_{s+j} |s\rangle \tag{57}$$

we can also express the matrix elements G_{jn} by

$$G_{jn} = \langle\psi_j|X^n|\psi_j\rangle \ . \tag{58}$$

Understanding the Problem Better

What it seems we were finding yesterday is this: That the full set of d^2 equations

$$G_{jn} = \frac{1}{d+1}(\delta_{n0} + \delta_{j0}) \tag{59}$$

is actually implied by the smaller set of $3(d-1)$ equations

$$G_{00} = \frac{2}{d+1} \tag{60}$$

$$G_{j0} = \frac{1}{d+1} \qquad j = 1, ..., d-1 \tag{61}$$

$$G_{jj} = 0 \qquad j = 1, ..., d-1 \tag{62}$$

$$G_{j,j+1} = 0 \qquad j = 1, ..., d-2 \tag{63}$$

I still don't know how to prove this implication, but it is starting to seem much less surprising than when I first stumbled across it a couple days ago. It might well be semi-trivial to prove it, if we just think about the problem in the right way.

The reason has to do with the thing Marcus pointed out about how the frame function has roughly a factor of 8 redundancy in it. He worked with a more symmetric matrix H_{jk} in the case of odd d than the matrix G_{jn} here, but the conclusion is surely almost exactly the same in both even and odd d and can be seen directly in terms of the G_{jn}. I think it must simply come from careful index play/redefinition in the definition (56) above—i.e., I haven't checked it directly yet, but I have loads of faith. For, one can clearly see this kind of structure by taking a random $|\psi\rangle$ and looking at the magnitudes $|G_{jn}|$ numerically. They have a pretty and simple layout even without any special conditions on $|\psi\rangle$. For instance, in $d = 8$ one obtains a structure that looks like this

```
Z | Y X W V W X Y
-   - - - - - - -
Y | A B C D C B A
X | B E F G F E B
W | C F H I H F C
V | D G I J I G D
W | C F H I H F C
X | B E F G F E B
Y | A B C D C B A
```

And in $d = 9$, this

```
Z | Y X W V V W X Y
-   - - - - - - - -
Y | A B C D D C B A
X | B E F G G F E B
W | C F H I I H F C
V | D G I J J I G D
V | D G I J J I G D
W | C F H I I H F C
X | B E F G G F E B
Y | A B C D D C B A
```

Anyway, in general one must have:

$$|G_{j,n}| = |G_{-j,n}| = |G_{j,-n}| = |G_{-j,-n}| = |G_{n,j}| = |G_{n,-j}| = |G_{-n,j}| = |G_{-n,-j}| \ .$$

Whenever $j = -j$ or $n = -n$ or $n = j$, etc., some of the distinctions collapse.

What can be seen from this is that specifying the diagonal term to zero for $|G_{jn}|$, and similarly for the first off-diagonal, wipes out a whole lot of the terms! For instance, in the examples above, only C, D, and G remain *ostensibly* free. But of course, we know they they are not completely free. And it really shouldn't be that hard to see that they must actually vanish.

But how to show it, I don't know. I tried several sorts of things with expansions of some vectors in terms of others, and with Schwarz inequalities, but so far I've failed.

In any case, looking at these structures promotes an even simpler looking conjecture. And I propose this conjecture get the bigger proportion of study anyway.

A Prettier Conjecture

One can note that if one specifies the first three columns of these matrices, one is specifying just as many variables as if one specified according to the earlier scheme. Thus, it seems that an even better conjecture would be: That the following $3(d-1)$ equations imply the full set of d^2

$$G_{00} = \langle\psi_0|X^0|\psi_0\rangle \;=\; \frac{2}{d+1} \tag{64}$$

$$G_{j0} = \langle\psi_j|X^0|\psi_j\rangle \;=\; \frac{1}{d+1} \qquad j = 1, ..., d-1 \tag{65}$$

$$G_{j1} = \langle\psi_j|X^1|\psi_j\rangle \;=\; 0 \qquad j = 1, ..., d-1 \tag{66}$$

$$G_{j2} = \langle\psi_j|X^2|\psi_j\rangle \;=\; 0 \qquad j = 1, ..., d-1 \tag{67}$$

(In fact, by looking at the general structure of $|G_{jn}|$, one would really only need to specify about $3(d-1)/2$ equations.)

This is starting to strike me as looking extremely clean. And I think it is as likely to be true as the previous conjecture. I have checked it numerically for various dimensions here and there up to $d = 13$—not as thoroughly and for as many seeds as previously, but enough to give me a sense that it must be true.

Supposing that it is relatively easy to show the equivalence: Can it really be so hard to satisfy the remaining equations? Surely it is not even remotely on the same playing field with Fermat's last theorem or the Poincaré conjecture.

28-09-06 *Simple, Getting Simpler, 2* (to H. B. Dang & D. M. Appleby)

Dangism 1: *By the way, I'm curious how you found out about that set of* $3(d-1)$ *equations. It's so amazing that it's true numerically! And do you know that if you don't have all those* $3(d-1)$ *equations then it won't work, i.e. can we further eliminate some of them?*

Well, I can definitely see that they can be reduced to (roughly, i.e., plus or minus 1 or 2) a total of $3(d-1)/2$ equations. That comes from the automatic symmetry of the G_{jn} matrix. But can they be reduced still further? I don't know. Possibly. If you have a concrete proposal, I can test it out with my newfound Mathematica skills. I had tried simply the equations for the main diagonal and the edges and that didn't work. And I think I've tried other variations on this, but no great success.

How did I find the first set? Well, I was enamored with the $n = j$ equations and had played with them for a day, trying to take their Fourier transform and such. (I was hoping the magnitude equations—the ones on the edge—supplemented by a one-parameter set of equations that said something about the phases would do the trick.) Then, once I got up and running numerically, I thought I would see if they (along with the edge equations) implied anything numerically. The thing that surprised me was that they came pretty close to specifying the full set. So, I thought, maybe if I take one of the off diagonals too, that would add enough. And nicely, it did. That's the basic story.

Now that you've got me thinking about this, I think I was able to successfully drop the $(0,0)$ equation in one of my earlier trials. Let me see if I can still do that. I don't know why I didn't think of this again this morning.

... [time goes by]

Wow! I quickly checked $d = 7$ and $d = 8$, and it seems indeed that that normalization equation—the $(0,0)$ one—can be dropped. So, we're down to $3(d-1) - 1$ equations! (Or half that, as your taste may take you.) That is a little more symmetric and prettier set.

Dangism 2: *My feeling is that it is some inequality (probably no longer the Schwarz) that would do the proof, but I still can't prove anything yet.*

Yes, it would be good to use those known zeroes as upper bounds on the remaining terms. That's why I was trying to use Schwarz yesterday, but no luck. Maybe something to do with concavity? Jensen's inequality? Arithmetic – geometric mean inequality? It sure would be nice to get some sleep tonight!

Let me shuttle this note off to you and Marcus before checking further on dimensions.

28-09-06 *Simpler, Simpler, Simplest?* (to H. B. Dang & D. M. Appleby)

Furthering on what I wrote below. I have now further checked that the conjecture is numerically so in $d = 9$, 10, 11, and 12. So, I think we're good to go on the new conjecture of $3d - 4$ equations. I also checked that some further deletions would not help. For instance, keeping the $(0,0)$ normalization, but getting rid of the 0-column equations otherwise, is no good. Nor does it help to reinstate one of the 0-column terms.

This may indeed be the minimal set. Or if you really want to be picky in the accounting (eliminating all known redundancies):

If d is odd. The unadorned total comes to:

$(d-1)/2$	from the 0-column
$(d-1)/2$	from the 1-column
$(d-1)/2-1$	from the 2-column

for a grand total of $3(d-1)/2-1$.

If d is even. The unadorned total comes to:

$d/2$	from the 0-column
$d/2$	from the 1-column
$(d-2)/2$	from the 2-column

for a grand total of $3d/2-1$.

If you have Mathematica 5.0 (Marcus I know you don't), and you want to do your own experimentation, here is the code I used:

```
a[i_] := b[i]+ I c[i]

NN := Sum[Abs[a[Mod[s,d]]]^2,{s,0,d-1}]

CC[j_,n_] := Abs[Sum[Conjugate[a[Mod[s,d]]]*a[Mod[s+j,d]]*
             a[Mod[s-n,d]]*Conjugate[a[Mod[s-n+j,d]]],{s,0,d-1}]]

EC := Abs[NN-1] + Sum[Abs[CC[j,0]-1/(d+1)],{j,1,d-1}] +
      Sum[CC[j,1],{j,1,d-1}] + Sum[CC[j,2],{j,1,d-1}]

d = 7

Do[Print[k] && Print[NMinimize[EC,
   Join[Table[b[i], {i, 0, d-1}],Table[c[i], {i, 0, d-1}]],
   Method -> {"DifferentialEvolution", "RandomSeed" -> k}]],{k,0,24}]

nn = NMinimize[EC,
     Join[Table[b[i], {i, 0, d-1}],Table[c[i], {i, 0, d-1}]],
     Method -> {"DifferentialEvolution", "RandomSeed" -> 3}]

bb = Table[b[i] /. nn[[2, i + 1]], {i, 0, d-1}]

cc = Table[c[i] /. nn[[2, i + d + 1]], {i, 0, d-1}]

aa = bb + I cc

psi[j_] := Conjugate[aa] RotateRight[aa, j]

Table[Conjugate[psi[j]].RotateRight[psi[j], n], {j,1,d},{n, 1, d}]

MatrixForm[%]
```

The "DifferentialEvolution" method works pretty well up to about $d = 11$, then it's better to use "RandomSearch" as the method. Do the preliminary part first to find an adequate random seed, then do the rest if you want to check that you're getting a full set of equations. Or if you want to test properties of the $|\psi_j\rangle$, etc.

05-10-06 *So Whiskey Tango Foxtrot?* (to M. D. Sanders)

Sandersism 1:

`http://web.archive.org/web/20061005192320/`
`http://www.cnn.com/2006/TECH/science/10/04/teleportation.reut/index.html`

Is this really going on? This is kind of freaking me out.......but it is way cool.....

Yes, it's real. But it's not nearly as exciting as the newspaper makes it sound. "Teleportation" in the quantum information sense isn't so very much like the Star Trek version as the press always wants to portray it. It's not about getting things from here to there without going in between, but about making my information stop being about this and start being about that without being about anything else in between.

Let me tell you a funny story about Polzik. He's a guy who sought asylum in the US after escaping from Russia during the Cold War. In 1998 we were roommates at a conference at Northwestern University (Evanston, IL). We stayed in a dorm room. The last night of the meeting around bed time—I had an early flight the next morning—Polzik said, "I'm gonna go have a swim in the lake," while putting on his swim suit. "Do you want to come with me?" I told him about my early flight and declined. He grabbed a bottle of vodka and went out the door. I got in bed, turned out the lights, etc. About a half hour later, Polzik was back in the room pulling pants over his still dry swimming suit and muttering, "No policeman is going to tell me I can't have a swim." I said, "What's up?" He said, "I'm gonna get to the lake one way or other." I have no idea what he meant by that or what his plan was. But I do know the next morning I found a wet swimsuit in the bathroom and the room reeked of vodka.

That was the same year we had the teleportation paper at Caltech, teleporting from light field to light field; Polzik was one of the authors with us. (He had leant us a student who did much of the actual work.)

05-10-06 *How I Sold Us* (to D. M. Appleby & H. B. Dang)

FYI, here are the words I used to try to sell our project with my superiors. Here's the section on the subject I wrote for this year's performance report. ... I deposit it here, just in case you too would like to know why you're working on this blasted problem!

> The main portion of my research this year has been on using new techniques to tackle a 33-year-old open mathematical problem, and collaborators and I have made significant headway toward its solution—so much so that we think we are far ahead of the game with respect to anyone else thinking about it. Most importantly, by way of this work, I believe I have gained my deepest understanding yet of the structure of quantum information. The final goal is still a little out of reach (and we have felt compelled to hold back on publication of intermediate results), but it is a very exciting time.

The motivation for this particular problem is to find a useful way of formulating finite-dimensional quantum mechanics so that density operators are at the center of its considerations, rather than state vectors. A technical reason for this is for efficiency in quantum information calculations, where density operators always play a prominent role in noisy, real-world situations; a fundamental reason for it has to do with its easier connection to the idea that quantum states represent information, rather than intrinsic properties of quantum systems—a prime component in my research for many years now. For a d-level system (with a d-dimensional Hilbert space, a so-called *qudit*), its density operators live in a d^2-dimensional vector space of Hermitian operators. In fact, they live in the positive cone of that vector space, i.e., the set of operators with non-negative eigenvalues. An example of what I mean by bringing density operators to the center of considerations, is, for instance, that one might like to know how to formulate a superposition principle using elements solely within the cone. For such a project, one should have an analog of an orthonormal basis of state vectors within the cone.

This summer, with my intern Hoan Dang, we were able to formulate this question in a more interesting way than had been done previously, and the answer was surprising. Since there cannot be an orthonormal basis of d^2 operators in the cone, one might ask for a set of operators that, on average, are as close to being orthonormal as possible. We found a rigorous bound on the average orthonormality and also the necessary and sufficient conditions for achieving that bound. To my great pleasure, the conditions were ones I had seen before, but in fairly disparate work—particularly, my work on optimal alphabets of quantum states for sensitivity to eavesdropping in quantum cryptography. This connection is something I had suspected, or more accurately hoped for, for some time, but I did not know how to formulate it very well or make it rigorous. (In fact, I mentioned a preliminary result along these lines in last year's Form 1, but it was significantly weaker than the present one. In the older version, the symmetry was built in by hand, rather than a result of the calculation.) In the usual formulation of the superposition principle, the one at the state-vector level, basis vectors represent the *most classical* properties—ones that can, for instance, be cloned or eavesdropped upon without disturbance. But in the density-operator formulation of a superposition principle, we are led to using bases of states that are as *quantum* as they can be—the ones that are most easily perturbed by their environment. The reason I am very pleased about this is that it gives me a tool for exploring my pet idea of what powers quantum information and computing: It is nothing to do with parallel universes or action-at-a-distance, or anything as science-fictiony as that, but simply quantum systems' greater sensitivity to external stimulus than classical systems.

The condition that must be satisfied for such a beautiful symmetry to be true is that one must be able to find a set of d^2 pure density operators $\Pi_i = |\psi_i\rangle\langle\psi_i|$ such that $|\langle\psi_i|\psi_i\rangle|^2 = 1/(d+1)$ when $i \neq j$. But does such a set of states exist? It is not obvious, and in fact the question is equivalent to the 33-year-old mathematical problem alluded to above—the problem of the "maximal number of equiangular lines"—which had its origin in classical coding theory. Furthermore, independently of the things reported here, the question of the existence of these states had already captured the interest of the quantum information community because of the set's intrinsic symmetry. The working conjecture of several groups, and the one we are on the tail of, is that the operators Π_i can always be generated by taking certain orbits of the standard set of unitary operators used in quantum error correction, the generalized Pauli operators. One thing that is remarkable about representing quantum states in terms of the Π_i is that then a general quantum error would be nothing more than an unknown permutation of indices—there are no longer any distinctions to be drawn between phase-flip vs. bit-flip errors, etc. Thus, viewing quantum errors in this way may open a further book of ways in which powerful classical error correction techniques can be adapted to the quantum domain. For all these reasons, proving the existence of these operators and giving a technique for actually constructing them would be a great achievement.

What has made my collaboration with Hoan Dang (now at Princeton) and Marcus Appleby (at Queen Mary) special in comparison to the rest of the pack considering the existence problem, is that we have opted for a basic—but steady—linear algebraic approach, rather than invoking abstract group theory and abstract number theory, as has been the fashion in most of the literature so far. We are banking that the problem does not really call for all that, and that these high-level approaches have clouded more than they revealed. For instance, until our work, the best numerical technique for checking for the existence of such a symmetric set scaled as a sum of d^{12} terms; we have been able to reduce that to a sum that scales as d^2. Concomitant with this, and more interesting actually, we have been able to reduce the exact problem to finding a single state vector, whose components must satisfy (roughly) $3d/2$ simultaneous quartic equations (of a very particular structure). We feel that we are not all that far from a complete solution to this problem. Personally, I feel this is some of my best work yet in quantum information theory.

What will come next is to ask in much greater detail what insight this kind of representation can give to quantum computing. On two fronts I hope to tackle this directly; one through a collaboration with Lov Grover in our own department, and one through a collaboration with Al Aho, a Bell Labs alumnus who continues to come into the lab often. Both of these collaborations are just starting up.

10-10-06 *Phone Tag* (to J. E. Sipe)

PS. Carl Caves is visiting me for a couple of days at the moment, and we're being forced to confront all our sins and indiscretions in this quantum Bayesian business. Not a pleasant way to spend one's time.

13-10-06 *Rabid Pragmatists (but in the sense of W. James, J. Dewey, and C. Fuchs)* (to R. D. Gill)

Thanks for calling my attention to your updates. I am impressed by how much we both seem to be attracted to the same ideas. (Of course, at this level of description—like the one found in your presentation—it is always easy to agree or to disagree. But I genuinely think there are similarities in our ontologies.)

Gillism 3: *Detector clicks in the past are real.*

Yes.

Gillism 4: *The future is a wave of possibilities.*

Yes.

Gillism 5: *As now moves relentlessly forward the past crystallizes out of it randomly.*

Yes.

Gillism 6: *There is no nonlocality problem, no Schrödinger cat problem, no measurement problem (time is measurement).*

Yes.

The only place where we significantly disagree is on the issue of probability in quantum mechanics. I too—i.e., like you—believe that quantum mechanics gives every indication of an *ontic* indeterminism or randomness in our world. I just part company with you in thinking that the concept of an *ontic probability function* sheds any light on this wonderful feature/fact of nature. In fact, I think it does just the opposite: Holding onto the idea of ontic probabilities—i.e., trying to make it make sense—only clouds our vision and gets in the way of proper description of this feature of nature.

In the note below (which I had originally written to a Princeton physics professor in one of my own more rabid moments), there are a few more details about what I actually mean when I speak of *ontic indeterminism* and *subjective quantum probabilities* in the same breath. The two ideas can coexist. (By the way, that guy really pissed me off. You never piss me off, so I wouldn't write you a rude note like that. But I forward the note to you because I do like the formulation I gave to the issue you and I happen to be talking about now.)

Tell me ultimately (i.e., as you get a chance), what you thought of the details of G. H. Mead's ideas.

14-10-06 *Pragmatism and the Weather, 1* (to R. D. Gill)

Gillism 7: *or do you mean pragmaticist?* `http://en.wikipedia.org/wiki/Pragmaticism`

No, I really meant the strain of pragmatism from William James and John Dewey (and an admixture of F. C. S. Schiller). Peirce's pragmaticism is quite a different thing, and not to my taste.

Talk about striking deep chords, I really liked this remark of yours:

Gillism 8: *I have this idea that if intelligent life would arise on Jupiter and develop mathematics then the most fundamental and deep mathematics would be probability theory (since Jupiter is basically weather and nothing else). The pure mathematicians would do statistics. The applied mathematicians would do number theory. It is just a cultural accident that probability is an embarrassingly tricky add-on to mathematics. It could have been the other way round.*

That is a beautiful idea! Danny Greenberger once wrote me an eloquent little passage about how it might have been the case that the real numbers would have been taken as mathematically primary (over the integers) if evolutionary pressures had been different—I'll include that passage below [see note to Greenberger, dated 06-12-02]—but I like your idea even better! And, in any case, you might not need Jupiter to pull it off. It may already be the case with us here on earth; we've just strayed a little from the natural progression of ideas in the last few centuries.

I'll be back in a couple of minutes after I dig up another passage.

14-10-06 *Pragmatism and the Weather, 2* (to R. D. Gill)

The other thing I had wanted to send you was a passage in my computer about the philosopher Chauncey Wright. He was a friend of James and Peirce and had been deeply influential on them (before ending his life early). The reason I send you this is because of your remark that "Jupiter is basically weather and nothing else." Wright was famous for saying on many opportunities, "The universe is only weather." The passage below gives some flavor of that.

> [Chauncey] Wright did not consider himself an evolutionist. To him the term denoted a belief that the world was getting, on some definition, "better." His loyalty was only to the theory of natural selection, which he thought corresponded perfectly to his notion of life as weather. "[T]he principle of the theory of Natural Selection is taught in the discourse of Jesus with Nicodemus the Pharisee," he explained in a letter to Charles Norton's sister Grace. The allusion may be a little gnomic today. The discourse with Nicodemus is in the Gospel of John, and the words of Jesus Wright was referring to are these: "The wind bloweth where it listeth, and thou hearest the sound thereof, but canst not tell whence it cometh, and whither it goeth: so is every one that is born of the Spirit." Wright was, in short, one of the few nineteenth-century Darwinians who thought like Darwin—one of the few evolutionists who did not associate evolutionary change with progress. "Never use the word[s] higher & lower," Darwin scribbled in the margins of his copy of the *Vestiges of the Natural History of Creation* in 1847. The advice proved almost impossible to follow to the letter, even for Darwin, but if anyone respected its spirit, it was Chauncey Wright.
>
> Wright's particular bête noire was the evolutionist Herbert Spencer, whose work seemed to him a flagrant violation of the separation of science and metaphysics. "Mr. Spencer," as he declared, "is not a positivist." Spencer's mistake was to treat the concepts of science, which are merely tools of inquiry, as though they were realities of nature. The theory of natural selection, for example, posits continuity in the sequence of natural phenomena (evolution does not proceed by leaps). But "continuity" is simply a verbal handle we attach to a bundle of empirical observations. It is not something that actually exists in nature. Spencer failed to understand this, and he therefore imputed cosmic reality to what are just conceptual inferences—just words. He did with the word "evolution" what Agassiz did with the word "creation": he erected an idol.
>
> "Mr. Spencer's philosophy contemplates the universe in its totality as having an intelligible order, a relation of beginning and end—a development," Wright said. But the universe is only weather.
>
> > Everything out of the mind is a product, the result of some process. Nothing is exempt from change. Worlds are formed and dissipated. Races of organic beings grow up like their constituent individual members, and disappear like these. Nothing shows a trace of an original, immutable nature, except the unchangeable laws of change. These point to no beginning and to no end in time, nor to any bounds in space. All indications to the contrary in the results of physical research are clearly traceable to imperfections in our present knowledge of all the laws of change, and to that disposition to cosmological speculations which still prevails even in science.
>
> "No *real* fate or necessity is indeed manifested anywhere in the universe," he wrote to a friend, "—only a phenomenal regularity."

16-10-06 *Paper* (to C. M. Caves)

Cavesism 80: *Chris, I have changed "prompting" to "eliciting". I think it captures much better what you have in mind, but won't mind going back to "prompting".*

Just noticed this PS, which I hadn't noticed before. Neither word, I think, in the end is completely adequate, but I don't know what's better at the moment. Elicit is fine. And you're probably right, it is closer to the mark at the moment.

16-10-06 *Our Professor* (to R. Schack)

Cavesism 81: *I think we ought to be thinking about a paper that lays out our entire approach, particularly, the idea Chris and I discussed of a three-pronged approach to subjectivity and objectivity: (i) quantum states and probabilities are wholly subjective; (ii) system attributes are wholly objective; and (iii) measurement outcomes are where the rubber meets the road, i.e., where subjective and objective meet to produce something that is not under the control of the agent, but is also not out there in the world.*

This paragraph of Carl's is one that took me absolutely by shock. I wish you could have been here for our first day of interaction, when Carl was still in his mode of saying how I've been "muddying things beyond belief." In fact, it all started with him going into a soliloquy his first day at the office. "I want to get one thing straight: With this business about 'fact for the agent', I'm not going there. Even if my position is not consistent and that one is, *I'm not going there*." That, I'm fairly sure, is an almost verbatim record of what was said. Then he outlined his story of "facts" being those things that are out there for all to see, and that *that's* what we've been talking about in this paper all along . . . yada, yada, yada.

It was hard to get past it, but I held my ground. I told him that you and I had both moved beyond that kind of understanding of "a fact." (I used some of your emails to bolster the point.) I explained how he would always be forced into a corner by Finkelstein with his present view, and similarly be absolutely stuck by the time he ever got to Wigner's friend.

It was a priceless scene actually, him rubbing his temples, going silent, looking extremely annoyed, rubbing his forehead, and then cutting off all conversation. The next day he woke up a fairly different man, saying, "We're going to make some progress on this paper today." I said, "Oh no," with complete dread. He said, "No, you'll be pleased." So, all of that was pretty cool. But then to see the proposal above was a complete shock.

I hope these new duties at your university aren't killing you too much: We need you man!

17-10-06 *The Book Proposal* (to D. M. Appleby)

I've finally read your book proposal. The words in it are nice, but I'm not sure that I got any insight toward answering these questions of yours: [. . .]

With regard to the content, there were a couple of parts that I marked to look at a second time.

1) I enjoyed the two mid paragraphs on page 8, the ones about how "It may seem . . . as though the epistemic interpretation must lead . . . to a depressingly positivistic view in which physics begins and ends with the task of predicting detector clicks. For a long time I had the same fear." That is a point, as you know, that is near and dear to my heart. It is a perception that I try to fend off at every point, and at every talk I give, to only small success. I'm out of inspiration for how to make

the point better, but it looks like you're not. Therefore, I think it'd be great if you were to expand those paragraphs even further. Find a way to really drive the point home.

2) The lines on page 9 that read, "Calling the statement epistemic only means that there is not, in addition to such features as the fact that one of the wings is on the verge of falling off, an additional mind-independent physical property called 'propensity for crashing'." This is because it reminded me of a conversation I had with Jos Uffink at our Konstanz meeting. I remember saying something like, "Why is it not good enough that the realm of the real world is already clearly marked out in a subjectivistic conception of probability? It is marked out in the facts, the clicks, the outcomes, which are distinct from the probabilities. The facts do not depend upon the agent. Someone once said, the world is everything that is the case—and there it is; it's in the form of the facts that the subjective degrees of belief happen to be about. But it seems that you, and all objectivists for probability, want more than that. It's as if you want a soul that lives behind the facts. The facts as they occur in nature are too secular, they're not good enough; they would be nothing if they didn't have a spiritual aspect that gives rise to them. It's as if you want a soul to be responsible for them." Jos simply replied, "That's right."

18-10-06 *Real Possibility* (to A. Shimony)

I was thinking about you the other day as I was writing Richard Gill for another purpose. I'll include that note below, which daisy chains to still another one (written to Kirk McDonald). [See 13-10-06 note "Rabid Pragmatists (but in the sense of W. James, J. Dewey, and C. Fuchs)" to R. D. Gill and 30-01-06 note "Island of Misfit Toys" to K. T. McDonald.] The subject matter is, in essence, the idea of "real possibility" and quantum mechanics' indication of it as a feature of our universe. I found myself thinking back on things you've said on the subject over the years and, in particular, during the Perimeter Institute meeting this summer.

In some ways, I feel there's really not that large a gap in our opinions about this pretty idea that "the universe is on the make" and in the directions we think physics should turn to to get a better grasp on it. We are both intrigued by the category of potentia. I know that I think (some flavor of) the conception is the greatest discovery in physics, certainly in the last 90 years, and maybe ever. But there is this one thing that keeps getting in our way of effectively communicating to each other and sharing our reverence for it—it is our feeling about quantum mechanical probabilities. Bayesian of a stripe though we both are, we certainly have different feelings for the meaning of probabilities that come from quantum mechanical pure states.

Thus I started to think that writing a careful statement of my view, and the arguments I use to support it, might be a worthy contribution to the festschrift Wayne Myrvold is organizing for you. In fact, the more I think about the idea, the more I am excited about it.

How can one have "chance" without "objective probabilities"? That's what the note to Gill (actually much more so the note to McDonald) starts to address. For me, chance is the primary ontic category and, as far as I can tell, it cannot be quantified. Quantum mechanics, and the probability relations it suggests, from my view, is a tool—a wonderful tool—for helping *physical* agents swim in this world of chance. The statement that we live in a world of chance or real possibility is indeed represented in the very structure of the quantum formalism, but as a kind empirical addition to de Finettian / Ramseyan coherence. Not in the specification of some kind of objective probabilities.

That's the idea, anyway, that I think I would like to expand upon. The groundwork has already been laid in some other things I've written—1) the note below, 2) a paper with Caves and Schack that I'll be able to send you a draft of in a couple of days, and 3) a sort of pseudo-paper (samizdat actually) that I'll attach to the present email. Particularly the last item goes into much more detail about what I mean by "objective indeterminism."

I thought it might be fun to start to get some feedback from you before I draw up the plans for the new paper in too great of detail. Your objections and queries could be part of the very structure and help me tailor my contribution to the things you find confusing or untenable. Anyway, that's my thought and I'd dearly love to get some feedback from you.

It was great seeing you again this year, and for me, the Perimeter Meeting was quite the most interesting conference I went to in some time. I'm very happy it happened (as it might not have in this world of real possibility).

18-10-06 *Back From My Long Silence* (to W. C. Myrvold and W. L. Harper)

This note is way, way, way overdue, but I have not forgotten about your kindness to me, and I wanted to thank you both for giving me feedback when I was stressed over my presentations and presentation style just after the AbnerFest. Both of your comments were very useful and helped me "make it through the night," so to speak, during that tough time. The reason this note is coming to you so late is that I got another comment from the fellow who had taken me aside at Perimeter, just after Wayne's writing and a little before Bill's leaving a message on my answering machine. Basically it was just an email version of what he had already said, but it caused me to feel even worse than I had before, and I decided to simply shut down for a while on the issue. (He is a physicist whose opinion I respect more than about anyone else's, and I got very confused about myself for a while.) Thus my philosophizing, my usual samizdating, and my emailing fell by the wayside for the summer, as I became a hermit and plunged myself deep into calculation like I had not done in quite some time. It was a form of crisis management. Anyway the fruits of the tactic were definitely good—I'll present them at the QCMC meeting Tokyo this Fall (in a different style! . . . I say facetiously)—and it allowed me to distance myself from the hurt. But it's time now for me to face the issues I left behind, and more importantly, to become a little more balanced in my research and thought again.

You two helped me see that my work and the choices I've made for trying to convey it were not made in vain, even though at the same time there is probably a lot of wisdom in what my old friend said . . . at least for particular audiences. All I can do is keep trying, making sure to try to take into account each datum that comes my way.

Concerning the points where we left off:

Myrvoldism 4: *I think that there is less disagreement between my view and yours than might appear. (I can explain this more if you'd like).*

Yes, I would like you to explain this more as you get a chance. I am curious (now that I've come back to being thoughtful again!). Surely, I know that we agree on some things. For instance, I think we would both say that quantum mechanics is a story of a world with inherent chance. But certainly there are two sticking points that always get in the way of our communication: 1) objective, *quantified* chance (i.e., the Lewisian stuff), and 2) nonlocality. As I understand you, you accept both. As I understand myself, I reject both. So there is some nontrivial distance between our views. But if an open universe is the more basic of the things we are both hoping to get a real handle on—i.e., the bigger goal—it is certainly worth the attempt to open up a dialogue, and I'd welcome better understanding what you mean.

It's probably not a coincidence that I happened to write all the stuff below to Abner this morning: These particular issues are on my mind again. [See 11-08-06 note titled "Real Possibility," to Abner Shimony.] And, actually, all this concerns you in two ways. As you'll see from my proposal to Abner, I think this subject will be the topic of my contribution to the festschrift you're editing.

Bill said,

Harperism 1: *At our discussions I suggested that you might find my introduction to and many of the papers in the volume Stalnaker, Pearce and I edited. Here is the reference: Harper, Stalnaker, Pearce, eds.* IFS: conditionals, belief, decision, chance and time*, D. Reidell Publishing Company, 1980. I think we have many informative things to talk about.*

As luck would have it, I just found a copy at Barnes and Noble online for only $42.00! The receipt says I should have it in my mailbox in 3 to 6 days.

Best wishes to both of you, and I apologize again for my long silence.

20-10-06 *Slammed by the Closet Door* (to N. D. Mermin)

Merminition 154: *My point was simply that you badly undersell yourself and what you're doing in your talks ... But jokes and cartoons and metaphors and lack of focus on specific controversial points (is "quantum nonlocality" nonsense? is there a "measurement problem"?) won't convey this to anybody. I mean you knew Leggett and Pearle, both of whom think the measurement problem is the most important unsolved problem in physics, would be there.*

I knew that Abner would be there.

And more than anything I wanted to make a talk that somehow touched base with Jamesian pragmatism for him. In the end, I failed my own standards (and I was aware of that). But I tried in the off hours of that interesting meeting to get it all into a self-contained package. You can't see it, but I actually worked very hard at that talk—much more than I have for any talk in quite a while. You had seen everything before? I should burn the transparencies and start completely over? Maybe. But, in fact there were 26 transparencies in there that were new; I just counted them again. You had never seen them before.

Leggett and Pearle? They don't mean as much to me as they mean to you. I know the protest you will surely give me over what I am about to say, but they are ideologues. They will carry the prejudice against epistemic quantum states with them to the grave. Bill Clinton said it very nicely in a speech a couple of days ago, "If you've got an ideology, you've already got your mind made up. You know all the answers and that makes evidence irrelevant and arguments a waste of time." They may be great scientists when it comes to other subjects, but with regard to this one subject, they are ideologues. The evidence is overwhelming that quantum states live in the mind rather than in the physical world (leaving aside, for the moment, the *detail* of whether they are knowledge, credence, information, belief, or whatever variation of such a kind of thing). The *only* thing those guys have motivating their research in the other direction—the direction that gives rise to a "measurement problem" and "nonlocality"—is the religion that a fundamental physical theory should not be IN PART about things to do with minds in the sense above. That is their one motivator. But the scale is completely imbalanced toward the other direction: It screams out in almost every piece of the phenomena of quantum information—the no-cloning theorem, teleportation, the "go ask Alice" point, and so on. And yet they don't see it. (And while I'm here, you, with all your admirable open-mindedness and nonprejudice, don't see it either.) The only thing that is really an open issue is getting a better answer to your question, "Information about what?" This was not the first time Leggett or Pearle were at one of my talks, or instead, one of Peres's, or one of Schack's, or Pitowsky's, or probably in the long past, one of John Wheeler's. Tony has been to at least five or six of my talks, and in particular was at the Seven Pines meeting in Minnesota for a week where the epistemic view of quantum states got the vast majority of the discussion. These guys are not ones that can be reached out to on this issue, my involvement or not. Where I can hope to make an impact is, as Zeilinger said, with the young ones. I may not be doing it successfully, but that's the only place I can hope to do it.

That aside, though, I did *try* to address directly those very questions you raise, and I did it with concrete examples too—think of the classical no-cloning theorem example I gave and the example of teleporting a probability distribution for the outcomes of a coin-toss. Even YOU don't see the force of those examples (I could hear it in your question that day), and because of that, they don't seem like concrete examples to you at all. But they carry the whole story. There is absolutely nothing inherently quantum mechanical about quantum teleportation. The only thing that is quantum mechanical is the answer to the question, "Information about what?" And that question is well-posed without anything to do quantum teleportation. That was the point.

It was that IF one can just get one's head around the idea that quantum states are epistemic, then there is no measurement problem and there is no need to talk of nonlocality. Peirce wrote this to James in 1904, "[P]ragmatism solves no real problem. It only shows that supposed problems are not real problems." And so too, this is the reason for adopting an epistemic conception of quantum states AS A STARTING POINT. It is to say, there never was a measurement problem.

You tell me I should have addressed these things. But I tried. I used the wrong language for some of you, but I tried.

22-10-06 *Wishful Thinking* (to C. M. Caves)

Well it was wishful thinking on my part. It's Sunday now, and I haven't done any tinkering on the paper; just looked at the draft and the comments. Birthday and NY City took more of my attention away than I thought it would. And now Appleby is arriving today. So, let me unlock the paper, so that you can return to tinkering on it if you wish. I much more liked our method of working on it together; too bad we didn't have just one or two more days of that.

I had a very thoughtful day in NY (they don't come very often to me any more), and I drew up all kinds of grand plans for how best to present the program ... to philosophers at least. I think the way I'll tackle it now is to start the presentation with all ontological sounding stuff—objects, events, indeterminism—get that established, and then throw in agents and subjective probabilities. Traditionally I always started with subjective quantum states first, how that alleviates the measurement problem, etc., and then worked backward toward an ontology. I sketched out a few pages of notes on the train and in coffee shops. At least right now, I'm decently excited about this, *dreaming* that maybe it will stave off the usual sorts of fears and confusions, like Finkelstein's and everyone else's. One thing I might try to do before Christmas with this is give a seminar at Princeton and see how it flies; another thing is I might try is to make it the format of my AbnerFest contribution. If it looks after those two preliminaries to be on the right track, I may lobby for that with you and Rüdiger as the right way to go for our next joint venture (where surely all the language will be toned down from my preliminaries).

Thinking how 42 is supposed to be the answer to life, the universe, and everything, I've decided I want to try to have a different kind of 42nd year than Lewis Carroll and Rüdiger Schack. That's probably the most important result of my last three days of thought.

25-10-06 *Maximum Uncertainty States* (to H. B. Dang)

It seems the price of SICs just went up again today! (Remember how you would say that every time we find a new question for which SICs are the answer, their price goes up.) Well, remember how I had speculated how the SICs shared some features with coherent states, and therefore might in some sense be "minimal uncertainty states" for measurements with respect to a complete set of mutually unbiased bases? Marcus and I finally drudged through the calculation today, and we found that I was just about right: They're not the minimal-uncertainty but the maximal-uncertainty states![112] (At least when $d =$ prime, this is true.) It's been a very exciting day. At the moment, we're trying to see if the SICs are the *only* maximal uncertainty states; for that we don't have an answer yet.

We still have not been able to figure out why the d^2 equations can apparently be reduced to only $\sim 3d/2$. That would probably be our main topic of conversation, to see if we can make any progress there.

28-10-06 *Please Email Abstract* (to K. H. Knuth)

This is a minor modification of the abstract I used for the Applied Maths colloquium at Princeton—making it look a little less mathematical and a little more physicsy, but I hope it will do.

> Title: Math Problems from the Far Side of Quantum Information
>
> Abstract: The field of Quantum Information has recently attracted great interest for the technological fruits it may bear. But there is a sect of its practitioners who think it stands a chance to have an even greater legacy than that—namely, that its theoretical tools will give us a means for exploring what quantum mechanics is really all about and for settling some of the deepest problems in physics. The roots of this optimism come from a very old thought: that quantum states have more to do with representing their users' information than any inherent physical properties of the systems they describe. What is new and nice is that quantum information teaches us how to formulate this idea precisely and even check its consistency. Nicer still is the number of juicy mathematical problems the consistency-checking process poses. In this talk, I will review some of the history of this and then quickly settle on a sample problem that has been annoying me a lot lately (the existence of a very symmetric, informative quantum measurement) and discuss the insights its existence would give for the structure of quantum mechanics as a whole.

01-11-06 *Plunging Ahead* (to W. C. Myrvold)

I never heard back from you concerning my proposal for an Abnerfest contribution. Anyway, I'm plunging ahead. Please let me know the last date you will be willing to accept the manuscript.

You know I always start my thinking with a slogan. Here's the slogan for this paper (which I'll also send to Bill H. in another note; I want to let him know I just got a copy of "Ifs"). The flavor

[112] Ha! We soon learned from Bill Wootters that we had made a minus sign mistake! They were minimal uncertainty states after all. See D. M. Appleby, H. B. Dang, and C. A. Fuchs, "Symmetric Informationally-Complete Quantum States as Analogues to Orthonormal Bases and Minimum-Uncertainty States," `arXiv:0707.2071v2`.

of quantum Bayesianism that Caves, Schack and I are trying to establish concerns in great part the following thought:

> The *formal structure* of quantum mechanics represents an empirical addition to Ramsey / de Finetti coherence, not an empirical addition to the setting of priors.

That, in a single sentence, is an attempt to capture the technical part of what I sent you last time.

I hope you're having fun at the PSA meeting. I really regret having to miss it this year.

01-11-06 *SBT, the Reprise* (to H. C. von Baeyer)

von Baeyerism 24: *So, to cover up my guilt, I'll go on the attack. I'm reading Caves, Fuchs, and Schack on quantum certainty, and thinking that I'm getting the drift, when I come up to an example – which is my way of understanding – and whammo, on page 10 you hit me with 33 states, which are (the cute little "of course" rubs salt in the wound) connected by 96 rotations and one shoulder separation. I can't envision that. Can't you use a simpler manifestation of KS like Mermin's very cogent* 3×3 *matrix, or maybe it was* 4×4*? As popularizer I'm always on the lookout for simpler versions. (I remember that when GHZ first came out in about 1991 it was such an improvement over EPR that I wrote an article about it which came out so well that even Anton requested a reprint just this Fall.)*

Well, that's just the tip of the iceberg of my own troubles with that paper. Rüdiger and Carl had convinced me that we should try to write it in a "metaphysically neutral" manner—saying the things the authors could all agree on, without too greatly exploring the meaning of the words "fact" and "truth" in the quantum domain (a place where we ourselves we had significant differences). But that was definitely the wrong thing to do: For it is precisely the place where the *four* referees picked on us the most. Clearly some clarity and commitment was called for! (By the way, of the reports, two thought it was an outstanding paper (though needing plenty of revisions), one was somewhat neutral but generally positive and also asked for revisions, and one thought it was so hopeless we should simply scrap it at the outset. The thing that tickled me most about the last report is that I had never before imagined that I could be wrong in so distinctly many ways.)

Anyway, I say this to warn you that I think a much better version of the paper is coming together now, and you shouldn't think too deeply about the present version. We intend to have the new version posted before December.

About the section you mention, one of our referees even said this:

> There is of course nothing wrong with the proof in section V, but at this point in the history of the philosophy of quantum theory, I confess that I don't see why it is there. Isn't a quick reference to the 10,000 other proofs of nonlocality sufficient? If not, can you say what is special or new about this one? I would personally rather see that space used for a more subtle discussion of the newer issues, perhaps addressing some of the points that I've made above.

In a way, he has a point; that section may bring us more trouble than it's worth. Its main point was really to say only that neither 1) Kochen-Specker, nor 2) Bell inequalities alone, lead to conclusive results with regard to the question of the pre-existence of "with-certainty" measurement outcomes. However, the conjunction of locality + KS (in the way of Stairs) does. To make that point, not all (or almost any) of the construction needed to be shown. References alone might well have done the trick. In any case, I'll share your concerns with my coauthors and treat your remark as a fifth referee report.

08-11-06 *November 8th* (to N. D. Mermin)

I apologize for my long delay in replying to you; this time it was not intentional. First I had Appleby here and we were completely single-minded on our simultaneous-quartic-equations problem to the exclusion of all else. Then I had the misfortune of getting a very bad case of food poisoning (cold left-overs from Taco Bell) which sent me to Emergency Care and knocked me out for several days. And, finally, yesterday was Election Day and I could not tear myself away from the television!

But like America as a whole on this glorious November 8th, I am recovering.

Merminition 155: *Don't be so quick to dismiss Leggett as an ideologue, just because he takes quantum states to be objective. It's hard to spend decades worrying about the phase diagram of helium-3 without believing that quantum states are objective. It's hard to spend a decade thinking about information theory without believing that quantum states are states of knowledge. Does that make you an ideologue? It seems to me (probably too superficial a thought) that neither of you has enough to say about the background that underlies the other's ideology.*

Your last sentence is fair enough: There is such a divide between me and the nuts-and-bolts, roll-up-your-sleaves-and-actually-do-something-with-quantum-mechanics physicists that there is certainly a communication barrier. (By the way, Tony happened to be visiting UBC in Vancouver during my interview there two and a half years ago. And just coincidental with that and the remark above, I remember after the interview Philip Stamp telling me that someone on the committee said, "Well, if he loves quantum mechanics so much, why doesn't he do anything with it!" I doubt it could have been Tony (not being on any UBC committees), but these separate strands bring it all together in my mind.) I don't know how to cross the divide; it's been a problem that has plagued me all of my career. I am just a very narrow specialist in physics (who, like Ringo Starr, was always just lucky to be there), and to bridge the divide would require a real generalist … one like you or David DiVincenzo. Particularly someone far more capable than me.

With regard to your phase-diagram remark in particular, I think it's got to be instructive to try to imagine what a turn-of-the-century physical chemist (trained in the worldview of classical physics) should have thought of the tables of heat capacities and whatnot that he had spent much of his life compiling. What can those numbers represent in the classical worldview? Why would two distinct chemists compile the same tables of numbers if those numbers didn't signify something objective and intrinsic to the chemicals themselves? Well, a Maxwell demon wouldn't compile the same tables. And there is surely a continuum of positions between us and him. What those tables represent is something about the relation between us, the coarseness of our senses, our inabilities to manipulate bulk materials, all kinds of things like that, and the materials themselves. Those numbers are just as much about us ("anyman") as about "it."

Anyway, forgetting about Leggett, how would I convince the classical physical chemist of such a point of view? It would be damned hard (as history has already shown). Instead of skirting so close to such an anti-Copernican idea—that knowledge gained in science is as much about "us" as about "it"—he'd probably spend his life trying to poke holes in the conceptually simple Maxwell demon idea and try to return his beloved thermodynamical quantities to their pristine objectivity (a place where they'll last forever).

I just think it'd be instructive to carry that out in more detail before tackling anything more specific to quantum mechanics.

On your other point, no I don't consider myself an ideologue (clearly). The reason is, I came to this position kicking and screaming … as Carl and Rüdiger, for instance, can attest to you. It has been a very slow transition and has really only gained momentum in the years that I've happened to know you. After the kicking and screaming phase—after Caves and Peres had laid the seed—"consistency" kicked in and carried me the rest of the way to the "radically subjective Bayesian" where I am now. And that's all that's happened. (Can you really imagine that Tony went in the reverse direction, kicking and screaming all the way himself?) In the years before I knew you, I had all kinds of crazy ideas about objective wave functions and objective probabilities and passion-at-a-distance and giving up on the notion of spacetime and .... And they all only expressed *one* thing: Fear of taking the observer seriously as a component of what quantum theory is about.

To let that "fear" or its equivalent be the one over-riding piece of "evidence" for shaping one's foundational program, I call an ideology. I've always liked the way Spekkens put it in his toy-theory paper:

> We shall argue for the superiority of the epistemic view over the ontic view by demonstrating how a great number of quantum phenomena that are mysterious from the ontic viewpoint, appear natural from the epistemic viewpoint. These phenomena include [about a million things]. ... The greater the number of phenomena that appear mysterious from an ontic perspective but natural from an epistemic perspective, the more convincing the latter viewpoint becomes. ...
>
> Of course, a proponent of the ontic view might argue that the phenomena in question are not mysterious *if* one abandons certain preconceived notions about physical reality. The challenge we offer to such a person is to present a few simple *physical* principles by the light of which all of these phenomena become conceptually intuitive (and not merely mathematical consequences of the formalism) within a framework wherein the quantum state is an ontic state. Our impression is that this challenge cannot be met. By contrast, a single information-theoretic principle, which imposes a constraint on the amount of knowledge one can have about any system, is sufficient to derive all of these phenomena in the context of a simple toy theory, as we shall demonstrate.

So, my statement is one about the mass of evidence. It's one about good scientific practice ... even in the context of quantum interpretations. To ignore a mass of evidence (in some context) and stick with one's gut is to be an ideologue (in that context).

Merminition 156: *Like my referee's report? You're right. I still don't get it. But I want to get it. The tension between what is known and what is a matter of judgment is somehow crucial to the whole business (and as far as I'm concerned, is what the "measurement problem" translates into in your way of looking at things. It's still there.) I guess there was also tension among the three coauthors which did not promote complete clarity of expression.*

Your referee report was fabulous. And much needed. It was by far the best of the lot (there were four of them, along with detailed comments from Jerry Finkelstein and Chris Timpson). Actually they were all needed. I blame the paper's lack of clarity on myself—it came about because I lost soul and didn't want to fight. The real sin of the presentation is that we tried to write the discussion in a "metaphysically neutral" way, if you will, without making too much commitment about these hard words like "fact" and "truth" (in the quantum context or out of it). In my gut, I didn't feel that was the right way, but I lost soul. And also Rüdiger assured me that in all the "unsaid things" we three really didn't differ. Well, the referee reports called us out on that: We had set ourselves up to confuse everybody. And when push came to shove I found out that Mr. Caves hadn't agreed with Rüdiger and me at all in the fine metaphysical details. So, no wonder the paper was confusing at a very deep level.

In fact, I actually had made a feeble attempt to say things a little more clearly when Jerry first commented. (I'll place the note below that I had written to Jerry at the time; it is relevant to some of your own points.) Carl responded with an angry, "I admit that my initial responses were too curt to be very useful, but now the waters are muddied beyond belief." At which point, I just curled up into a ball and went into hibernation.

Your report and the others at least helped dispel Carl's overwhelming obstinacy that nothing more needed saying. And that in turn—like a good marriage counselor—led us to start speaking to each other again. I think you will find a significant improvement in the paper when I can finally show it to you. At least I think it reached much better clarity when we were working on it last ... even though it is still written in the sparse style my two coauthors prefer. And I even gained ground: 1) it is not written in such a very "metaphysically neutral" way as it had been before (it attempts to make some statements of substance), and 2) Rüdiger and I even shockingly got this remark from Carl soon after our meeting:

> I think we ought to be thinking about a paper that lays out our entire approach, particularly, the idea Chris and I discussed of a three-pronged approach to subjectivity and objectivity: (i) quantum states and probabilities are wholly subjective; (ii) system attributes are wholly objective; and (iii) measurement outcomes are where the rubber meets the road, i.e., where subjective and objective meet to produce something that is not under the control of the agent, but is also not out there in the world.

Point (iii) represents a very deep shift in Carl's thinking indeed—I honestly thought I had long since pounded the opposite conception out of him—and I really don't think we would have gotten there if your referee report hadn't forced him to confront these issues that I've been babbling about for the last couple years.

... I just read that Donald Rumsfeld resigned while I was writing this note to you. I didn't know that a third of a president could resign? Quite a shock to me. Anyway, more evidence that it is a glorious day!

So let me leave you with one remark on my favorite line in your report. You say, "your real point, which you then expand on nicely, is that you have a rather nice, somewhat off-beat interpretation of the Born rule." Thank you. Because this really is the essence of where this research effort is now going. I tried to put it in a slogan the other day and this is what came out:

> The *formal structure* of quantum mechanics represents an EMPIRICAL ADDITION to Ramsey / de Finetti coherence, not an empirical addition to the setting of priors.

What survival in the quantum world is imposing on us agents is not some kind of "principal principle" (i.e., that we should adopt this or that objective probability), but rather an addition to "coherence" (in de Finetti's sense). When I am gambling on the sensations I will receive from interacting with a quantum system, my gambles should not only be coherent in de Finetti's sense, but more. For, considering all the ways I can imagine interacting with the system, all my gambles for each of those individual ways should also be related to each other. The precise way in which they should be related is an empirical statement: And in our contingent world it happens to be via the Born rule. (I.e., it could have been different if our universe were different.) And that is where we have our clearest window on quantum reality.

But I tried to say all that in my Waterloo talk (never to be mentioned again!); maybe I said it more clearly this time.

09-11-06 *Quantum Foundations at the APS March Meeting* (to a large distribution list)

Dear quantum foundations friends,

Please note that there will be a "special focus session" on Foundations of Quantum Theory at the APS March Meeting in Denver, Colorado, March 5–9, 2007. This session is organized by the TGQI—the Topical Group on Quantum Information, Concepts, and Computation—which hopes to promote the field of quantum foundations within the American Physical Society and the broader physics community.

We hope you will help make this session successful by submitting abstracts (to sorting category 23.8.1 "Foundations of Quantum Theory (GQI)"). The deadline for submissions is Monday, November 20. More information can be found at the very bottom of the page at

`http://www.aps.org/meet/MAR07/abs.cfm`.

Please spread the word to others you may know who are interested in quantum foundations.

The meeting announcement is at `http://meetings.aps.org/Meeting/MAR07/`. You can join the TGQI at `http://www.aps.org/units/gqi/`.

Best wishes,

Chris Fuchs
for the TGQI Program Committee

> 23.8.1 GQI Foundations of Quantum Theory
>
> Advances in both theory and experimental technique have made possible a golden age of investigations in the foundations of quantum mechanics. This session encourages both theoretical contributions that illuminate our understanding of foundational aspects of quantum physics and reports on the sophisticated new experiments that are probing the foundations of quantum theory.

13-11-06 *Zing! Went the Strings of My Heart* (to H. J. Bernstein)

Bernsteinism 3: *I had some thoughts about the last (co-authored) paper of yours that you shared; in fact I thought you were letting the pressure of admitting to a classical world get you a bit far over from what we really know we can say. Which is the beauty of Bayesian approaches: sure there is "zing" out there but updating our tables of probabilities and conditionals is about all we know if it.*

Rather than dig it out and try to get a recollection about the point I'd make, I know you must have at least almost finished the paper if not the negotiations with Carl & whomever, so could you send a newer version to me and my student Zac?

I'm not sure what you're saying, but it's always good to hear from you!

Actually, I'm working all this week to re-edit our paper once again. So any input you might have would be really good. Attached is the version of the paper that arose after David Mermin's detailed comments. Next, I've got to tackle the other three(!) referee reports, and maybe take into account Jerry Finkelstein's and Chris Timpson's comments. Why not join the club! It'd be great to hear which parts you think could be made clearer.

13-11-06 *'Many Worlds at 50' Invitation* (to A. Kent)

Thank you for the invitation to the Many Worlds meeting. I've marked my calendar in all worlds (i.e., one). Please put me down for a talk titled, "13 Direct Quotes from Everettian Papers and Why I Find Them Unsettling." I'm looking forward to this.

13-11-06 *Quantum Foundations at the APS March Meeting* (to F. E. Schroeck)

Schroeckism 2: *I got your message about the Q Foundations focus group of the APS to be held HERE, in Denver. I didn't know you had started this group. Anyway, congratulations; I hope it gets off the ground.*

I'm just a worker bee; the thanks for starting the group go to Danny Greenberger and Anton Zeilinger, and a big petition signed by over 400 people. You should join the group; we definitely need larger representation from the quantum foundations community.

Schroeckism 3: *I don't know what I could say that is new at this stage, but I'll think about it. Maybe something about informational completeness in measuring by placing holes in a "screen" at points in phase space in the mean. I have talked about q.m. on phase space until I'm blue in the face in Europe, but it still is mostly unknown here in the U.S.*

That sounds good.

Schroeckism 4: *What are you doing, researchwise now?*

Well, there's the things I have to do for Bell Labs and (dreaming of) building quantum computers.

But I'm also taken with informationally complete observables, particularly very symmetric ones. I'm writing a new paper on that at the moment, and I'll send you a copy when it is done in early December.

13-11-06 *Dove & Hudson Old Books* (to A. Caticha & K. H. Knuth)

By the way, Ariel, Kevin, I should have told you about the outcome of my book-buying excursion. I found this place called Dove & Hudson Old Books (on the corner of Dove and Hudson). It was a wonderful place, and I walked away with about 15 new books (on pragmatism and Heidegger). Anyway, on the small chance that you didn't know about it, I thought I should bring the place to your attention.

14-11-06 *No Stand-Alone Event Space at All* (to V. Palge & W. G. Demopoulos)

I'm going to cc this note to Bill Demopoulos since my reply to your technical question somewhat involves him. I don't think the papers of his that I refer to are posted on the web; so he would probably have to send them to you if you want to dig into this more deeply.

Palge-ism 1: *In the classical probability theory one assumes that the event space has the structure of a sigma-algebra. In their quest to formulate the probability theory for the quantum realm in an analogous manner, various approaches in the quantum logic tradition postulate non-commutative structures, e.g., partial boolean algebras and the like.*

The question then arises: what is the structure of the quantum event space according to your subjective Bayesian approach? Is it a sigma-algebra, like in classical probability theory, or is it perhaps some non-commutative structure? If the latter, perhaps you can specify in more detail what it is?

The best answer I can give you, I think, is that neither of these kinds of structures map onto what I'm thinking. The main reason for this is that I think it is incorrect to think of the process of "quantum measurement" either 1) as the *revelation* of a property inherent in the system under observation, or 2) as the *production* of such a property in the system. And without that, I don't think there is enough glue to bind the events occurring in quantum measurements together into an algebraic structure (say, a lattice or a Boolean algebra, or even a partial Boolean algebra, where there are Boolean algebras tied together at the edges)—at least not in any useful sense that intrigues me as a physicist.

Unfortunately, the only reading recommendations I can give you at the moment (where I try to make this kind of idea of measurement clear) are in the form of little personal essays. I am hoping to put this all together into a paper before the end of January (for the Abnerfest), but it is not there yet. So, let me recommend the following pieces:

1. "'Action' instead of 'Measurement'" (to van Fraassen)
2. "Snowflakes" (to Bub and others)
3. "Me, Me, Me" (to Mermin and Schack)
4. "Questions, Actions, Answers, and Consequences" (to van Fraassen)
5. "Canned Answers" (to van Fraassen)
6. "Incompletely Knowable vs 'Truth in the Making'" (to Bill Demopoulos)

Maybe it is best to read the pieces in that order.

Particularly in light of your question, maybe it is important for you to understand the type of point of view that Bill Demopoulos is trying to develop. (I give my report of it in Item 6 above, but you should probably get Bill's original papers.) I think there is a fruitful similarity between what he and I are seeking, even if we ultimately diverge. It is this: In both our views—and they are the only places I've ever seen this style of idea—even when two measurements share a common element (say a given projector P_i), there is no implication of a common truth value being imposed on P_i across the measurements. The reason for this for Bill is that the system's properties are bound up with the whole orthogonal family of projectors the individual P_i happens to be embedded within. In contrast, the reason for this for me is that I don't think of quantum measurement outcomes as signifying properties intrinsic to the system—they are simply consequences of actions for me. What makes the element P_i identified across measurements—for both of us—is not truth value, but that the *probabilities* for P_i are identical in both cases. What this means particularly from my Bayesian way of thinking is that a judgment is being made: I, the agent, am identifying *this* potential outcome of this measurement with *that* potential outcome of that measurement because I judge their probabilities equal under all imaginable circumstances. (See Section 4.1 of my `quant-ph/0205039`.) It is not that they are identified in Nature itself. Thus, for me, I think, there is no good sense in which they lie in the same event space at all.

I hope all of that helps. That's the best I can do at the moment.

14-11-06 *Visit?* (to A. Wilce)

Wilce-ism 3: *Any chance we can lure you out here to give a talk this spring? (The schedule's pretty open so far.)*

As a matter of fact, I had just started to accumulate my meeting schedule into a centralized view so I could contemplate the upcoming year. When I put it all together, it started to look pretty frightening! But I still owe you! And it would be very nice to see you again.

Not listed in there is that my in-laws will probably be in and out of our house from Jan 1–13.

Thus, what would you think of sometime in the week of Jan 17? Or maybe better, sometime in the week of Feb 7 or the week of Feb 14?

- "Quantum Communication, Measurement, and Computing," Tokyo, Japan, 27 November – 4 December 2006.
- "APS Sorters Meeting," Washington, DC, 7–9 December 2007.
- "Visit to Geraldine," Cuero, TX, circa 21–30 January 2007.
- "Pragmatism and Quantum Mechanics," Paris, France, 22–23 February 2007.
- "APS March Meeting," Denver, CO, 5–9 March 2007.
- "15th UK and European Meeting on the Foundations of Physics," Leeds, UK, 29–31 March 2007.
- "6th annual New Directions in the Foundations of Physics Conference," College Park, MD, 13–15 April 2007.
- "William James and Josiah Royce a Century Later," Harvard U., Cambridge, MA, 25–27 May 2007.
- "Vienna Symposium on the Foundations of Modern Physics," Vienna, Austria, 7–10 June 2007.
- "Workshop on Operational Probabilistic Theories as Foils to Quantum Theory," Cambridge, UK, 1–13 July 2007.
- "Many Worlds at 50," Perimeter Institute, Waterloo, Canada, 21–24 September 2007.

15-11-06 *Rethinking and Rethinking* (to W. G. Demopoulos)

This line of yours intrigues me:

Demopoulosism 7: *I've been slowly rethinking things of mutual interest to us but am not quite where I want to be about them; but maybe I'm getting close. I'll write soon in any case*

For some reason, "rethinking" always has a positive connotation with me. I myself am hoping to rethink a lot as I put together this Abnerfest contribution. Particularly, I want to get straight the extent to which I want to ascribe any ontological statements to the "quantum Bayesian" view—I think I may end up with several (or at least events and objects, all that Donald Davidson says I need anyway).

16-11-06 *Quantum Foundations at the APS March Meeting* (to J.-Å Larsson)

Larssony 1: *Will you be very angry with me if I submit to Quantum Cryptography instead?* ☺

That's funny; I just got a note this morning from Rob Spekkens that said, "Have you got a sense of whether many good foundations types will be submitting abstracts?" You would have counted as a "good foundation type"!

(You see a big worry of ours is that we get a sufficient number of serious people submitting ... since we want foundations to start looking legitimate in the APS's eyes. Last year we were lucky enough to have one of the two sessions completely full of interesting people, with only one filled with fringe people. If we can at least continue that kind of trend, we'll probably be OK.)

16-11-06 *Challenges to the Kierkegaardian Bayesian* (to A. Shimony)

Thank you for the continued kind words. [...]

Anyway, on to more fun things:

Shimonyism 5: *I can't possibly say in one letter all that I have to say in response to your ideas, just as you haven't said all you have to say. But I'll start, and in due time we shall carry the discussion further.*

Let me try to do that (at least a little) now.

Shimonyism 6: *Top of p. 3, point 4 of your list of similarities and differences with Gill: You don't give arguments that there is no measurement problem and no nonlocality problem. As to the first, if the entire dynamics of qm is linear, including the dynamics of macroscopic systems, then it is hard to see how definite events occur at the conclusion of a measurement – hence a problem. As to the second, I think quantum non-locality is a fact, and it is also a fact that it cannot be used to send signals superluminally. Does this show that there is tension between qm and relativity theory and nevertheless "peaceful coexistence"? I used to think so, but Bell convinced me otherwise – hence there seems to be a problem.*

It is true that I don't give any arguments there, but they have been peppered throughout my writings. The key to much of it is to realize just how *thoroughgoing* an epistemic view my closest collaborators and I take on QUANTUM STATES. Take for instance, your point that "if the dynamics of qm is linear, including the dynamics of macroscopic systems, then it is hard to see how definite events occur at the conclusion of measurement."

My reply to that is that the dynamics of quantum mechanics has nothing to do with how definite events occur. It is silent on the matter. It is silent on the matter in the same way that the formal structure of Bayesian probability theory is silent on the matter of where a datum d comes from by which a gambling agent updates from a prior probability $P(h)$ to a posterior probability $P(h|d)$.

It is simply not within the power of the formal structure of probability theory itself to say either 1) how a truth value for d comes about or from where it finds its origin, or 2) how the agent has become aware of d so that he may conditionalize.

Similarly, I will say of quantum states. For this clique of friends that I call "the quantum Bayesians" a quantum state represents a catalogue of Bayesian probabilities and has no independent existence beyond that. A quantum state is nothing other than the complete catalogue of probabilities. Probabilities for what? States of the world? No. They are probabilities for things concerning the very agent himself who has ascribed those probabilities. They are probabilities for something interactive: Namely, the agent interacts with a piece of the world external to himself and there are consequences of that interaction for him—at the very least there are various sensations that may come about. What the probabilities derived from a quantum state signify are the agent's expectations for the consequences of his actions.

What about the dichotomy of linear evolution and collapse? For the quantum Bayesians it is no conceptual problem—they are simply two modes of the same thing, namely updating probabilities for whatever reason. In the case of collapse it is of the cloth of conditionalizing. In the case of linear evolution it is more of the flavor of Jeffrey conditionalization (and some of the updating processes van Fraassen has described). But these are distinctions of detail, rather than conceptual distinctions.

I expand on some of this in the essays "Raining Down in Cambridge," "The Evolution of Thought," and "More Linearity," between pages 222 and 239 of my book *Notes on a Paulian Idea* (which I sent you a copy of). You might find some of that amusing, if not instructive.

Similarly, I could go on about your statement, "I think quantum non-locality is a fact." I would say it is a confusion that takes its origin in not properly appreciating the disconnection between quantum states (epistemic) and quantum events (ontic, but partial consequences of the agent himself). I tried to say this as best I could in Section 3 of "Quantum Mechanics as Quantum Information (and only a little more)" (`quant-ph/0205039`). I would be honored if you'd read that section.

Anyway, to sum up, none of these things are PROOF that a purely epistemic view of quantum states is the way to go, but instead it is a question of the mass of evidence. And you know that I am a pragmatist. I would think that any good Kierkegaardian Bayesian, too, would only adopt priors flexible enough that they could eventually be swayed by the mass of evidence if there is such. In that regard, I think one of the most outstanding arguments (which you should be aware of) is Rob Spekkens' paper "In Defense of the Epistemic View of Quantum States: A Toy Theory," which can be found at `quant-ph/0401052`. I've always thought Rob put the main point so nicely with these words:

> We shall argue for the superiority of the epistemic view over the ontic view by demonstrating how a great number of quantum phenomena that are mysterious from the ontic viewpoint, appear natural from the epistemic viewpoint. These phenomena include [about a million things]. ... The greater the number of phenomena that appear mysterious from an ontic perspective but natural from an epistemic perspective, the more convincing the latter viewpoint becomes. ...
>
> Of course, a proponent of the ontic view might argue that the phenomena in question are not mysterious *if* one abandons certain preconceived notions about physical reality. The challenge we offer to such a person is to present a few simple *physical* principles by the light of which all of these phenomena become conceptually intuitive (and not merely mathematical consequences of the formalism) within a framework wherein the quantum state is an ontic state. Our impression is that this challenge cannot be met.

> By contrast, a single information-theoretic principle, which imposes a constraint on the amount of knowledge one can have about any system, is sufficient to derive all of these phenomena in the context of a simple toy theory, as we shall demonstrate.

That's the way I see it in our own debate—the one between you and me. From my point of view, you are stuck with all these mysteries—the dichotomy of linear and nonlinear evolutions (when does one kick in and the other leave off?), passion-at-a-distance but no action-at-a-distance, and so on and so on. Whereas I would say a thoroughgoing epistemic view of quantum states alleviates so many of these difficulties automatically that it is already worth exploring for that reason alone. Here's the way Rob put it more eloquently in an earlier version of the paper already mentioned:

> Because the [toy] theory is, by construction, local and non-contextual, it does not reproduce quantum theory. Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. ... The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

Here's what I would love to get a better sense of from you: Does an argument like Rob's particular one (the paper is very easy reading, so I encourage you to give it a shot) ... does an argument like Rob's put any dent in your armor? And if not, why not?

Shimonyism 7: *I agree, of course, that there is ontic indeterminism, for that is at the heart of quantum potentiality, but I don't agree that "ontic probability function" throws no light on this indeterminism.*

Well, certainly my point is that it hinders our progress by presenting a false god—one that stands in our way of seeing a purer, more direct path to salvation! More seriously, in toned down language: That is my trouble. The examples I pointed you to above give I think overwhelming technical evidence that quantum states should be viewed epistemically. And that then has to trickle into my view of quantum indeterminism. There is something about this world that keeps me, the agent, from having too much certainty about too many things (particularly the consequences of the various actions I might take on a given physical system). This is expressed through the quantum probability rule—the Born rule (via Gleason's theorem). The more subjectively certain I am about the outcome of spin-x measurement, the less subjectively certain I should be about the outcome of spin-y measurement, say. This something—the thing that bars me from too much certainty about too many things—is, I would say, a direct handle on "quantum potentiality." But I don't need to go so far as to assume that particular quantum-probability values are ontic to express this idea. My very actions have uncertain consequences (this is undeniable under the assumption of quantum mechanics); no matter what I do I cannot gain a god-like foresight. That seems to be a property of the world, but it is a property that finds its expression in epistemic terms.

Here's one of the ways I put it in the past (in my `quant-ph/0205039`). The language is not quite as refined as what I would use now, but it carries a good piece of the point:

> The last seventeen years have given confirmation after confirmation that the Bell inequality (and several variations of it) are indeed violated by the physical world. The

Kochen-Specker no-go theorems have been meticulously clarified to the point where simple textbook pictures can be drawn of them. Incompleteness, it seems, is here to stay: The theory prescribes that no matter how much we know about a quantum system—even when we have *maximal* information about it—there will always be a statistical residue. There will always be questions that we can ask of a system for which we cannot predict the outcomes. In quantum theory, maximal information is simply not complete information. But neither can it be completed. As Wolfgang Pauli once wrote to Markus Fierz, "The well-known 'incompleteness' of quantum mechanics (Einstein) is certainly an existent fact somehow-somewhere, but certainly cannot be removed by reverting to classical field physics." Nor, I would add, will the mystery of that "existent fact" be removed by attempting to give the quantum state anything resembling an ontological status.

The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information. Put in the right mindset, this is *not* so intolerable. It is a statement about our world. There is something about the world that keeps us from ever getting more information than can be captured through the formal structure of quantum mechanics. Einstein had wanted us to look further—to find out how the incomplete information could be completed—but perhaps the real question is, "Why can it *not* be completed?"

Shimonyism 8 : *Certainly when one has systems simple enough to be characterized by pure quantum states, and those states govern the transition probabilities into other states – as in emissions and absorptions – then a quantitative character has been added to merely qualitative indeterminism. Doesn't that throw light on the character of quantum potentiality? And even more light is shed when one considers how these quantitative transition probabilities are related to the geometric structure of the Hilbert space of states, particularly to the inner product of vectors in the Hilbert space.*

Again there is a divergence between us because you are, at the outset, thinking of quantum states in ontic terms. For me, if a system has a pure state, it is because I have a very strong, particular belief about what might arise as a consequence from my interactions with it. If I could imaginatively ask the quantum system what its quantum state is, it would say, "I don't know; I don't have a quantum state. It's up to you to answer that question yourself—you're the only one who has a quantum state for me."

Still, I think there is much to be gleaned about the actual structure of our world from the Hilbert-space structure of the quantum probability rule—that's predominantly what my research program is about. It seems to me that the content of the Born rule is a statement about how to be "coherent" (in something like a Ramsey / de Finetti sense) when gambling on the consequences of our physical interactions. The Born rule, as I see it, is an empirical addition to probabilistic coherence . . . and that's where we stand a chance of learning something lasting about the precise nature of quantum indeterminism.

This idea is fleshed out in greater detail in the closing section of the attached paper (which Caves, Schack, and I are presently in the process of revising, so it is not ready for wide distribution yet). But I figure I will give you a copy now as this point will be the core of my planned festschrift contribution for you. If you have any particular criticism to this concept, I would welcome it.

Shimonyism 9 : *There remains a difficult question of whether the world view of qm accommodates indeterminisms that are not quantitative, such as the outcome of a war.*

Now, this point definitely intrigues me. For, as you may have already gathered of me, I would say ALL indeterminisms are of this variety *including* the quantum ones. In this regard I was deeply influenced by Pauli and Markus Fierz. If you go to pages 577–582 of my book *Notes on a Paulian Idea*, you'll find an essay of Fierz's on the subject, reprinted in full. I'd be curious to know what you think of it?

Shimonyism 10: *But there are many varieties of Bayesianism, including logical probability theory, personalism, tempered personalism, and a peculiar variety that I call "Kierkegaardian Bayesianism." I.J. Good has a taxonomy of Bayesianisms somewhere that is very diverse and still not complete. The important point is that some of the strategies for being rational are not prior to empirical science but depend upon the structure of the world and human adaptations to that structure: we not only learn by experience (using probability as a tool) but we learn how to learn by experience (and therefore how to use probability). As you see, I am not radically in disagreement with you, but I think there are nuances.*

And you are right. On this point, I think we have an issue of potential convergence, rather than divergence. I hope you'll see everything above in the light of this very point!

17-11-06 *Certain Comments* (to C. G. Timpson)

Like I said yesterday, thanks so much for the comments. Don't you mind sending me "excessively long emails." When long emails are thoughtful like yours, I love them!

Timpsonism 3: *I enjoyed reading your certainty paper. I'm glad I'll now be able to direct people to that now rather than encouraging them to read through all of 'Quantum States: WHAT'! I had a few comments, mainly about presentation really.*

Yes, it is long since overdue that Caves, Schack, and I have an official statement on this subject. I'm not very pleased with the paper because we made too much of an attempt to stay "metaphysically neutral" on what the h's in a probability assignment $P(h)$ stand for in the case of the quantum context. But still the paper has improved significantly since the first posted version: We had extremely detailed referee reports from two of our four(!) referees, and taking their points into account has improved the paper significantly … and helped me win back a little territory of what I would have liked to have seen emphasized more in the paper.

However, now that we will soon have a stabilized triumvirate statement out, I plan to start embellishing it into something more to my taste for the Abnerfestschrift (where I'll be going it alone). I plan to have that finished by end of January.

Timpsonism 4: *On the distinction between probability assignments and propositions. It seems to me that, while pedagogically useful, this distinction cannot be the whole story. To make a probability assignment may not be to express a proposition (or at least not one concerning objective facts in the world), but one can have truths concerning probability assignments: "He thinks that the probability is p", for example, could express a truth. De Finetti allows this, I notice (having just checked the sections of Probabilisimo you refer to towards the end): "That a fact is or is not a practically certain is an opinion, not a fact; that I judge it practically certain is a fact, not an opinion" (end section 19). Thus one does need to add a claim to the effect that judgements of probability aren't responsive to any facts about chances in the world, as there are no such facts as the latter, on this view.*

Yes, you are correct about that, and it probably should be reflected in better wording on our part. I'll try to do the point better justice in the revised paper.

Timpsonism 5: *On that reading it would be ok to say that stating that the measurement outcome is 1 with certainty does not express a proposition. But one could question whether it is necessary to go so far anyway; all that is required for the position is that any proposition expressed when making a probability assignment doesn't concern how things are in the world external to the agent.*

That, of course, is what we were shooting to express. So, ditto my last remark above: I'll work harder to make our language a little less clumsy.

Timpsonism 6: *p.2 "Certainty is a function of the agent, not of the system." That puts it nicely. The philosopher Alan White distinguishes between the certainty of people and the certainty of things, arguing that these are quite distinct things. Subjective probability concerns certainty or otherwise of people. (Did I send you a copy of his paper 'Certainty'? I have a vague feeling that I might have done, but I'm not sure.)*

No, I don't believe you ever told me about Alan White. I think I would have remembered that. What of his should I read? I didn't find anything in quick look on the web.

Timpsonism 7: *Against objective chance (Section 2). A quibble: I'm not sure I was 100% persuaded by the argument you give in the final few paragraphs here. Some accounts of objective chance – I am thinking particularly of Mellor's in* The Matter of Chance *– can deal with the problem of specifying initial conditions precisely. Mellor argues that a propensity is a dispositional property of a chance set-up, so that if the conditions allow the disposition to be manifest, then a certain probability distribution will be displayed. There would presumably be a range of detailed initial conditions that would allow the display of this disposition, and other ranges which would not.*

I think that is the weakest part of the whole paper. In fact, so weak, as to be almost irrelevant . . . or actually damaging. What you read was the residue that remained after we scrapped our attempt at a direct blow to the principal principle . . . one that was going to expand on Richard Jeffrey's against it. But we got lazy and gave up on the project, particularly because Carl kept saying, "We're writing this paper for physicists," and it started to look too hard to get any consensus between us.

Maybe I'll try to do this better for the Abnerfest thing. I think Jeffrey is right: The principal principle simply cannot be properly formalized, and to the extent that it can be, it just captures judgmental probabilities after all.

Timpsonism 8: *But there's another challenge close by – Howard's – which is: can one be certain that q when one knows that it is not certain that q (when one knows that there is no fact of the matter whether q)? It seems not; yet this is apparently the situation that a self-conscious Bayesian must admit. (There's a scope distinction in the negation here: one can still be certain that q when it's not the case that one knows that it is certain that q: here the form is "not (Knows(Certain q))"; but one can't be certain when one knows it's not certain that q: "knows (not (Certain q)".)*

So looking at the clause (p. 12 third para from bottom) 'an agent could not be certain about the outcome "Yes" without an objectively real state of affairs guaranteeing this outcome . . . , I think you are right to maintain that this is false. The assertion that there is such a state of affairs isn't needed for one to be certain. However, it seems we do require, in order to be certain, the absence of the assertion that there is no fact guaranteeing the outcome.

This discussion of yours is very helpful, and it helps emphasize the pitfalls of our sparse representation. I think it's more evidence that, despite Caves' objections, we really did need to get more metaphysical.

Here's something relevant that I wrote to David Mermin the other day:

> With regard to your phase-diagram remark in particular [I.e. "Don't be so quick to dismiss Leggett as an ideologue, just because he takes quantum states to be objective. It's hard to spend decades worrying about the phase diagram of helium-3 without believing that quantum states are objective."], I think it's got to be instructive to try to imagine what a turn-of-the-century physical chemist (trained in the worldview of classical physics) should have thought of the tables of heat capacities and whatnot that he had spent much of his life compiling. What can those numbers represent in the classical worldview? Why would two distinct chemists compile the same tables of numbers if those numbers didn't signify something objective and intrinsic to the chemicals themselves? Well, a Maxwell demon wouldn't compile the same tables. And there is surely a continuum of positions between us and him. What those tables represent is something about the relation between us, the coarseness of our senses, our inabilities to manipulate bulk materials, all kinds of things like that, and the materials themselves. Those numbers are just as much about us ("anyman") as about "it."
>
> Anyway, forgetting about Leggett, how would I convince the classical physical chemist of such a point of view? It would be damned hard (as history has already shown). Instead of skirting so close to such an anti-Copernican idea—that knowledge gained in science is as much about "us" as about "it"—he'd probably spend his life trying to poke holes in the conceptually simple Maxwell demon idea and trying to return his beloved thermodynamical quantities to their pristine objectivity (a place where they'll last forever).

and something more particular to our paper that I had written to Jerry Finkelstein a good while back:

> In our scenario there is only the agent and the external world. The way one should think of the measurement device is the same way one should think of a prosthetic hand—it is an extension of the agent.
>
> "Detector click" is shorthand for *sensation* in the agent, and in my personal view, that is all it is. The value of the "detector click" is beyond the control of the agent; it is not a function of his beliefs or his desires. His interaction with the external world gives the class of potential sensations, but it does not set the value.
>
> Thus, the value of the detector click—call it d—is objective for the agent. (There is one sentence in the paper where adopted almost that phraseology "an event is a fact for the agent," and it might have been wise to adopt it more consistently.) What is subjective from our view—i.e., a function of the agent and not the data or external world alone—is the POVM associated with those potential values. That is, the set of operators E_d. [...]
>
> There is an agent, there is an external world, there is an interaction between the two—the agent acts on the external world, it causes a reaction back in him (sensation). As far as he is concerned that reaction is not subjective; it's as real as anything he's ever seen.

I think the solution to your conundrum lies in extending these points. Particularly this. The reason the two statements

1. <u>One</u> is certain
2. <u>One</u> knows that “<u>It</u> cannot be certain”

are not inconsistent is because of the “metaphysical piece” that’s missing from most of our language in our paper. The outcome of a quantum measurement is a joint product of the agent and the quantum system. [I remember once writing on my notepad that the idea of quantum measurement—even in our conception of measurement (or, should I say, *particularly* in our conception!)—without a system to measure upon is as much of a koan as the sound of one hand clapping.]

When an agent expresses certainty about the outcome of some measurement, he is judging that, in effect, the quantum system’s contribution to the outcome will be negligible. (See the note titled “Philosopher’s Stone” for an expansion on this conception of measurement. Also, in a separate email, I’ll supplement this with a note I wrote to Veiko Palge recently; there are several references in it that still further expand on this point.) So the agent can self-consciously say “<u>I</u> am certain which will occur” at the same time as saying “<u>it</u> is not certain,” because “neither <u>it</u> nor <u>I</u> is the whole story.” The outcome comes out of a marriage of the two of us. “When <u>I</u> interact with <u>it</u> I am quite certain which sensation that interaction will lead to; <u>it</u> won’t surprise me.”

If I were to kick the cat after a hard day at the office, I am certain that it will lead to my satisfaction. (For whatever crazy reason, I am also certain that it cannot react quickly enough to spoil the moment by scratching me.) On the other hand, because of my fundamentalist Baptist upbringing, I might also believe that the cat cannot foresee how it will react to my kick, by running or fighting—“it’s just a soulless dumb animal.” Even more relevantly, it cannot foresee whether I will be satisfied or not. In other words, performing a quantum measurement on a system for which I am certain of the outcome is like kicking a cat.

Does that make any sense to you?

Going back to the last point of yours quoted above,

Timpsonism 9: *However, it seems we do require, in order to be certain, the absence of the assertion that there is <u>no fact</u> guaranteeing the outcome.*

what I said above, I think, may not contradict this. It just means that we should be a little more careful to say, “there is no fact *in the object* guaranteeing the outcome.” Of course, there is no fact in the agent guaranteeing the outcome either; otherwise his certainty wouldn’t be subjective certainty.

Timpsonism 10: *On a quite different matter: have you ever come across Diederik and Sven Aerts’ work?*

I’ve seen a very little bit of it, but even that I haven’t absorbed all that well.

Timpsonism 11: *They want to explain the appearance of quantum probabilities as arising from what they call a hidden measurement model, involving ignorance of some processes going on in measurements, rather than ignorance of some underlying properties of the object systems. [...] This seems to be close to what I think you have on occasion said might be one way – one of the less interesting ways – your and Rüdiger and Carl’s programme might turn out.*

I think you’re probably right in this assessment. “Hidden variables in the agent,” (maybe a more honest way of saying “contextual hidden variable theory”) is something that has crossed my mind from time to time as *possibly* consistent with my forming point of view of quantum mechanics. But as you say, it seems for me that it would be “one of the less interesting” potential endings to our research program. I clearly am immensely attracted to the idea that a quantum system and an agent, when put in conjunction with each other give rise to something that was in neither physical system alone. That a quantum measurement outcome is a birth of sorts. Even a "relational" viewpoint on measurements (say like Mermin or Rovelli, or what Spekkens desires) doesn't seem to be enough for me. There's something in my gut that says that anything weaker than the radical, Paulian direction (the thing that in a bar room I call "the sexual interpretation of QM") is ultimately inconsistent. But indeed I don't have a good argument for that right now.

On the other hand, I do think there is a pearl of wisdom in this quote you sent me:

> If one wants to interpret our hidden measurements as hidden variables, then they are hidden variables of the measuring apparatus and not of the entity under study. In this sense they are highly contextual, since each experiment introduces a different set of hidden variables. They differ from the variables of a classical hidden variable theory because they do not provide an 'additional deeper' description of the reality of the physical entity. Their presence as variables of the experimental apparatus has a well-defined philosophical meaning, and expresses the fact that we, human beings, want to construct a model of reality independent of our experience of this reality. The reason is that we look for 'properties' or 'relations between properties', and these are defined by our ability to make predictions independent of our experience. We want to model the structure of the world independently of our observing and experimenting with this world. Since we do not control these variables in the experimental apparatus, we do not allow them in our model of reality, and the probability introduced by them cannot be eliminated from a predictive theoretical model.

so maybe I should look at these guys more carefully.

Timpsonism 12: *My aim in the next couple of months is finally to write something more substantial about your approach (which will end up as the final chapter of my book), so I shall be sitting down to try and think hard about it. I'll try not to pepper you with emails, but you may get one or two. I see that there is a new collection of emails on your webpage which I imagine I ought to take a look at to see how your thoughts have been progressing.*

It would be very useful for us to have your commentary. So, please do pepper me any time you'd like. If I don't answer right away, it'll only mean that I'm busy or traveling or on holiday; it won't mean at all that I'm bothered by your queries or objections. I get more out of your emails than I've probably ever adequately expressed to you. I want to understand quantum mechanics before I die; that is first and foremost the goal ... and whatever tension will lead me there is precisely what I need.

I hope Rüdiger's inaugural lecture goes well. I wish I could be there too. I think what he's talking about (a quantum Bayesian definition of randomness) is very deep stuff, and it will be a while before it is appreciated. I myself have yet to incorporate it into my metaphysics, but I think it will be a crucial piece of it ultimately. Have a good time.

17-11-06 *March Meeting* (to R. W. Spekkens)

Spekkensism 32: *I was about to submit my abstract for the focus session on foundations, and then I reread the email I received from APS. It says: "the proposed title of your talk is 'Interpretations of Quantum Mechanics'." My best guess is that this wasn't meant to be a real proposal for my title,* *and that I should speak on a topic of my choice (I'm thinking of speaking on the toy theory), but I just wanted to make sure that this was the case.*

I think you should definitely talk on the toy model. Put it in the wider context of the interpretation debate of course, and I would hope that you *not* spend much time on the latest and greatest variant or new achievements. Instead, I hope you spend plenty of time getting the basic points across.

Was the paper ever published, by the way?

It's funny, I was just advertising the paper to Abner yesterday and rethinking that lovely closing line in the abstract of an earlier draft:

> A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

Yes, Bell and Bell-Kochen-Specker are clues—the most important ones. But not clues toward the epistemic nature of quantum states. They are clues about the event space, and that is a much deeper issue.

17-11-06 *Birds and Stones* (to S. L. Braunstein)

Braunsteinism 4: *I must be getting old. I no longer believe in mixed states — except as a convenient language/tool.*

Well, you know you shouldn't believe in pure states either. It's funny, just yesterday I wrote this to Abner Shimony:

> Again there is a divergence between us because you are, at the outset, thinking of quantum states in ontic terms. For me, if a system has a pure state, it is because I have a very strong, particular belief about what might arise as a consequence from my interactions with it. If I could imaginatively ask the quantum system what its quantum state is, it would say, "I don't know; I don't have a quantum state. It's up to you to answer that question yourself—you're the only one who has a quantum state for me."

21-11-06 *The Self-Preservation Society* (to M. S. Leifer)

I read a good bit of your paper last night, and I really, really enjoyed it. It's no-nonsense like your talks, and the whole thing is put together in a very compelling way. This pretty construction of yours makes my mouth water again for a kind of Gleason theorem behind trace-preserving completely positive (TPCP) maps—it's got to be there ultimately. We really ought to get together and think about this. (There is some chance I'll be visiting PI in late Jan or early Feb; will you be around?)

I checked with Andrei and he says it's OK to post things on `quant-ph`. So there you go.

But before posting, let me bring up one political point. I thought it was a bit unfair to me how you introduce the motivation for the paper:

> The situation is less clear when considering a unitary operation on the system of interest alone, since in this case the environmental state is irrelevant to the action of

> the operation on the system, and unitary operations do not cause convergence of distinct states. Thus, unlike the previously discussed cases, the subjectivity of unitary operations cannot be argued directly from the subjectivity of quantum states.

particularly

> This offers the opportunity for CFS to hold onto the objectivity of unitary operations, which might be tempting, since the specification of a Hamiltonian seems to encode the objective content of our most successful physical laws.

To be sure, your technical contribution is great and was needed, and I'll be advertising it to everybody as the best little piece of quantum foundations in a while. But your wording gives no indication that the subjectivity of unitaries had ever crossed my mind or the preliminary and very public arguments I've given for it. See, for instance,

- Section 7 in `http://www.arxiv.org/abs/quant-ph/0205039`,
- or Section 6 in `http://www.arxiv.org/abs/quant-ph/0404156` where particularly footnote 9 makes it clear,
- or Section 4 in `http://www.arxiv.org/abs/quant-ph/0307198` where I consciously did not single out unitaries as anything special among TPCPs,
- and there are certainly significant pieces in "Quantum States: W.H.A.T.?" that I could pinpoint where I think the point is strictly argued.

The arguments may not have been conclusive, or even, you might think, right (though I think they are), but they started the whole subject. The idea of the subjectivity of operations I consider my best work ever in physics—it was no easy wall to climb over. Your work is a nice turning point, giving us something far more solid and productive to work with, but don't forget the old hint-giver who is still himself struggling for a stable career. If that's my only role—and it's not too bad of a role—I still need to get credit for it, like the rest of you, to survive.

Matt's Reply

> You are right. Originally, I had wanted to say that this gives the C of CFS an opportunity to hold onto the objectivity of unitaries, since he's clearly the one who wants to, but I thought that was unfair because C has not yet written any paper to that effect. I didn't mean to imply that $C \cup F \cup S$ actually wanted to take this option, only that it would not be a logical contradiction for any of them to do so. Obviously, I am aware of your earlier discussions of the subjectivity of unitaries and these inspired my argument, but in my haste to finish the paper in time I may not have emphasized this.
>
> If you can give me a couple of extra days I should be able to fix this. It's not a big job, but unfortunately I have more than one deadline to deal with at the moment.
>
> As regards visiting PI, I should be there in Jan and Feb, apart from the week of the 11th Feb. I can see that a Gleason's theorem for TPP maps potentially follows from your tensor product argument, but I am still not completely sure what the motivation for studying it is. I mean, any conceivable dynamics must map density operators to density operators, so the only real question is linearity. For an operationalist, this would be argued from the possibility of forming mixtures. I am guessing you don't like this

> because it uses a notion that is analogous to a "probability of a probability". The more general issue of how to argue for convexity in the Bayesian approach without making use of the mixing argument is also worth discussing. I know you have an email to Lucien on this in your Samizdat, and Rüdiger also discusses it in his response to Lucien's papers, but I still think there is something more to be fleshed out here.

21-11-06 *The Self-Preservation Society, 2* (to M. S. Leifer)

Leiferism 4: *I can see that a Gleason's theorem for* TPCP *maps potentially follows from your tensor product argument, but I am still not completely sure what the motivation for studying it is.*

The motivation is the usual one: The search for a direct argument for something, rather than a roundabout or secondary one for it. It would just be nice to get a straight-out argument for CPMs without *first* invoking the idea of a single-time density operator. Moreover, it would be like a development of probability theory that defines conditional probabilities at the outset, without reliance on marginals and joints—it would be more radical probabilist at its core.

Leiferism 5: *The more general issue of how to argue for convexity in the Bayesian approach without making use of the mixing argument is also worth discussing.*

That would be good too.

22-11-06 *The Self-Preservation Society, 3* (to M. S. Leifer)

You made the old man happy! I think the paper's great. And you really do "significantly strengthen the case for the subjectivity of all TPCP," so we should uncork a bottle of bubbles when we're together.

My PI visit is now settled. I should be there roughly Jan 29 to Feb 11. It'll be good to see you.

This morning I dug up the notes I had written on trying to find a dynamical Gleason theorem. Almost to my shock, I saw that they were dated "1 December 2000, Vienna" ... but it's not really to my shock: I'm pathetic about ever completing anything. (My biggest character flaw is that I seem to need ridiculous amounts of interaction to get anything done ... and Bell Labs can be a lonely place.) At least I was pleased to see the way the notes start off:

> Let us take the following idea as fundamental: Hilbert spaces need not be associated with systems per se, but with measurements. Thus if I make a measurement on a 'single system' at two distinct times, I should consider that a POVM on $H \otimes H$. If, later, I might wish to consider H as the only relevant Hilbert space, I might do that — but that is a derivative step, at least from this conception.

If you give me a fax number where you can be reached, I'll fax you the notes (there's about 20 pages). Maybe you can start thinking about all my wrong turns—and how to fix them—in the background of your mind before we meet up.

Tell me as soon as your paper is complete to the point of posting. I want to bug Charlie Bennett about it before seeing him in person Monday. Have you discussed the closing idea in your paper with Lee Smolin? I should think if nothing else interested him about it (and all this subjectivity gobbledygook actually turned him off), he might still be intrigued by the formalism.

22-11-06 *A Place in the World, 1* (to C. H. Bennett)

I just read these words of yours:

Bennettism 28: *Fortunately public interest is now such that lay people are overcoming their fears and asking us questions like "What is a quantum computer?", "How can a particle be in two places at once?", or "How can observing one particle affect another particle?" By coming up with intelligible and respectful answers to these questions, we will be doing our profession, and our fellow humans, a great service.*

Made me feel like maybe I have a place in the world, after all.

Really looking forward to joking around with you in Japan next week. I hope you'll be there the whole time.

22-11-06 *A Place in the World, 2* (to C. H. Bennett)

Bennettism 29: *I'll be at QCMC there starting the late afternoon or evening of 28 November. I look forward to joking around with you and trying on some ideas about quantum Darwinism and the ontological status of Jimmy Hoffa, a little further developed from the ones you heard before.*

Then here's a good way to start preparing for the joust. I just got this paper from Matt Leifer [see `quant-ph/0611233`], which he'll be posting today. I think it's really very good. (And, of course, completely relevant to the ontological status of Jimmy Hoffa.)

See you the 28th. I'll be waiting for you in the hotel lobby!

22-11-06 *CFS Philosophy, Ontic and Epistemic States and Evolutions, Funes the Memorious* (to C. H. Bennett)

Bennettism 30: *Matt's paper deliciously deduces from your execrably anthropocentric CFS philosophy that if quantum states have no ontic existence, merely epistemic, then so do quantum evolutions. This would include the identity "evolution" which leaves all "states" the same. So even the process of staying the same is not a process, but a correlation between states of knowledge at two timelike separated events. To thus problematize the process of merely existing, of remaining the same, is to begin to think like Borges' character Funes in* Funes the Memorious*, who "was disturbed by the fact that a dog at three-fourteen (seen in profile) should have the same name as the dog at three-fifteen (seen from the front). His own face in the mirror, his own hands, surprised him on every occasion."*

I'm glad to hear you liked Matt's paper!!

Hey, tell Theo Happy Thanksgiving from all of us in Cranford. (I'm trying to get a big roaring fire going for tonight.)

22-1l-06 *The Usual Behavior of Chris* (to C. M. Caves & R. Schack)

Let me give you a progress report as of 2:00 PM Wednesday, Nov 22, just before I take off until Friday morning (for Thanksgiving), whereupon I will start up on the project again.

Truth is, I've been mulling over and working on our paper since late last week, and corresponding privately with some of our critics. Over the weekend I hit an extreme low point, and contemplated writing both of you that I would simply drop out of the project. Upon carefully reading the reports and carefully reading again the paper as a whole, I just started to feel icky about the whole thing. It dawned on me that the whole paper wasn't nearly as clear as I had pipe-dreamed it was and I didn't really feel like it represented my view on anything. So why propagate the trouble I'm going to have to try to dig myself out of? 1) The paper doesn't convey any sense of the *privacy* of the single agent's description of his beliefs and the outcomes of his measurements, yet to be consistent requires it. 2) The paper is not consistent about the complete disconnect between the epistemic and the ontic. ("Logical implication" should be viewed is simply an instance of "subjective certainty" again—to talk of "exceptions" kills our whole point.) 3) It bandies about this term—this enemy—"the Copenhagen interpretation" to complete distraction, when I myself think there are so many subtleties here that it would be best to never mention Copenhagen.

Anyway, that was my low point. Since then I've started to climb the hill of optimism again, and at the moment at least, I am feeling like with enough *subtle* editing work I can pull it into a convincing package that has everything that *each* and *every one* of us wants. I am trying.

Mermin wrote me this:

Merminition 157 : *Eager to see your revised version. Your problems confirm my long-time practice of never having any collaborators if I can possibly help it. Particularly on the kind of paper you guys are trying to write. Reporting calculations is one thing, but what you're doing is just too subtle for a committee to get right.*

He's right, you know. I think we should think much harder about this in the future.

Below is a sample of some the changes made in Section 1. Through them I am thinking that I am addressing the issues of three of the referees, but also Timpson, Finkelstein, and von Baeyer. I think I only have two days more work now that I'm on a positive, can-do note again. (And my attitude really is positive again, even if the above does not convey it.)

I have every intention of shipping the thing to you before leaving for Japan Monday morning.

25-11-06 *Your Bravery* (to M. S. Leifer)

It dawned on me yesterday when I saw your posting that you are a brave man! What a brave thing to get involved in any way with these loons, the subjectivist Bayesians! I commend you.

By the way, I already got a little feedback from Charlie Bennett on your paper. (Very dangerous to put "deliciously" and "execrably" in the same sentence.) [See Bennettism in 22-11-06 note "CFS Philosophy, Ontic and Epistemic States and Evolutions, Funes the Memorious" to C. H. Bennett.]

Guard your flank.

25-11-06 *Hoffa and the Bayesians* (to C. H. Bennett)

I had a lovely email exchange with Matt Leifer this morning. Below is Matt's reply, of which I asked him if I could forward it to you—I knew you'd enjoy the story at the end. (When I see you, I'll tell you the one about the Jewish mystic I met on a plane.) The ontological status of Jimmy Hoffa and subjective unitaries — what is Osamu going to think of us now? (I just hope he won't slap me on the rear this time around.)

Leifer Excerpt

Finally, I think I should relate the story of what happened to me last night. Around 7pm I decided to go to a little Italian café for dinner. I had been writing an application for a faculty position (which I should be finishing now), and was thinking about what I should write about Bayesian stuff in my research statement. Now, in this café the tables are quite close together and you often meet "interesting" characters. It turns out that the guy on the table next to me had some weird variant of Tourettes that caused him to mumble incoherently to himself the whole time. At one stage, the conversation became coherent for just a few moments and went as follows:

Him: Are you studying here?
Me: I'm a postdoc.
Him: What subject?
Me: Maths.
Him: Do you like your......Bayesian.........Analysis?
Me: Yes, I like it very much thanks.

Of course, I tried to ascertain how he had known to say that, given that I had not been talking out loud about this stuff anywhere nearby, but the responses were all incoherent, mainly involving "Bayesian arseholes", "lesbians" and comments to the effect that he was more handsome than me. In some ways this reminds me of your story about the guy you sat next to on the airplane. Is there some sort of curse associated with writing papers on quantum Bayesianism? Perhaps the savants of the world are trying to tell us something.

25-11-06 *The Subjectivity of Convincing* (to R. W. Spekkens)

Spekkensism 33: *I also wanted to mention that I am finally convinced that, within a Bayesian approach, even unitary maps must be considered to be subjective. (Of course, I myself am not so sure that the Bayesian approach is the correct one, but that's irrelevant to the question.) I remained unconvinced by your original argument, the recent CFS paper on certainty (actually it wasn't clear that it was arguing for this conclusion), and Matt's first draft of* `quant-ph/0611233` *(which he gave me a copy of). But, after discussing it with Matt, he finally came up with an argument that did convince me (well, I haven't made a concerted effort to find fault with the argument, but intuitively, it seems right). This is the one that is now found in sec. 5 of* `quant-ph/0611233`*. Perhaps this argument was implicit in a previous argument made by one of you, but if so, it wasn't spelled out in enough detail for me to really get it until now. If you haven't had a look at Matt's paper, you really should.*

Yeah, I'm very proud of Matt's paper. That's very nice work, and I am very impressed by him.

The CFS certainty paper made no mention of the issue. 1) It didn't need to for the point at hand (that objective facts don't determine quantum states), but 2) because Caves is still an obstinate hold out on the issue of unitaries. I'm hoping Matt's paper will finally make some dent in his armor.

Regarding your "unconvinced" point, that's fine. But you shouldn't forget that "being convinced" depends upon a prior. All argument of this variety is a function of 1) one's prior, and 2) the mass of evidence in comparison to the prior. It is, just as you argue in your toy theory paper. I became "convinced" a long time ago, not through one killer argument, but through several inconclusive arguments—programmable quantum circuits, teleportation of unitaries, the non-uniqueness of ancilla representations of quantum operations, the Choi-Jamiołkowski isomorphism (though without a strong operational interpretation), various analogies to stochastic evolution equations (not as strong as Matt's), and so on. But then the real cap was beauty and a sense of adventure. The Bayesian edifice just looked sturdier and simpler without the Cavesian cycles and epicycles (i.e., that "true, there are some subjective, 'effective' unitaries, but there are also the objective ones too"). I understood that a conceptual leap might need to be made—i.e., that maybe we would never be able to make a strict logical argument *from* the subjectivity of states *to* the subjectivity of unitaries, i.e., that maybe it was an independent assumption. But if anything, that thrilled me a little.

It gave me a small chance to play pretend to be William James, and I could let W. K. Clifford be the proxy for my Carl Caves:

> The talk of believing by our volition seems, then, from one point of view, simply silly. From another point of view it is worse than silly, it is vile. [[As that]] delicious enfant terrible Clifford writes: "Belief is desecrated when given to unproved and unquestioned statements for the solace and private pleasure of the believer. . . . Whoso would deserve well of his fellows in this matter will guard the purity of his belief with a very fanaticism of jealous care, lest at any time it should rest on an unworthy object, and catch a stain which can never be wiped away. . . . If [a] belief has been accepted on insufficient evidence [even though the belief be true, as Clifford on the same page explains] the pleasure is a stolen one. . . . It is sinful because it is stolen in defiance of our duty to mankind. That duty is to guard ourselves from such beliefs as from a pestilence which may shortly master our own body and then spread to the rest of the town. . . . It is wrong always, everywhere, and for every one, to believe anything upon insufficient evidence."

Whereupon I could clench that, by his fears and lack of foresight, he was giving up the chance to make real progress in quantum foundations: A REAL leap is needed to understand quantum mechanics—not just for us, but for any foundational direction in QM. Pure logic won't do it; not for any of us (not for me, not for David Deutsch). And to the extent that we quantum Bayesians really believe we smell the right direction, we should be willing to take a leap in it:

> Believe truth! Shun error! — these, we see, are two materially different laws; and by choosing between them we may end by coloring differently our whole intellectual life. We may regard the chase for truth as paramount, and the avoidance of error as secondary; or we may, on the other hand, treat the avoidance of error as more imperative, and let truth take its chance. Clifford, in the instructive passage which I have quoted, exhorts us to the latter course. Believe nothing, he tells us, keep your mind in suspense forever, rather than by closing it on insufficient evidence incur the awful risk of believing lies. You, on the other hand, may think that the risk of being in error is a very small matter when compared with the blessings of real knowledge, and be ready to be duped many times in your investigation rather than postpone indefinitely the chance of guessing true. I myself find it impossible to go with Clifford. We must remember that these feelings of our duty about either truth or error are in any case only expressions of our passional life. Biologically considered, our minds are as ready to grind out falsehood as veracity, and he who says, "Better go without belief forever than believe a lie!" merely shows his own preponderant private horror of becoming a dupe. He may be critical of many of his desires and fears, but this fear he slavishly obeys. He cannot imagine any one questioning its binding force. For my own part, I have also a horror of being duped; but I can believe that worse things than being duped may happen to a man in this

> world: so Clifford's exhortation has to my ears a thoroughly fantastic sound. It is like a general informing his soldiers that it is better to keep out of battle forever than to risk a single wound. Not so are victories either over enemies or over nature gained. Our errors are surely not such awfully solemn things. In a world where we are so certain to incur them in spite of all our caution, a certain lightness of heart seems healthier than this excessive nervousness on their behalf. At any rate, it seems the fittest thing for the empiricist philosopher.

Anyway, that was my little sense of adventure. And Matt's work is a great cushion at the bottom of the leap. Actually it's much more than that: For it gives us a proper formal structure to start building upon.

By the way, with regard to this whole method of reasoning—i.e., beating down the opponent with a BIG mass of inconclusive evidence—I was reminded of your example (i.e., the toy theory paper) by a passage in Marcus Appleby's submission for the Växjö proceedings with regard to interpreting probability. Let me dig that up and paste it in for you.

> The complaint of the Bayesians about the orthodox statistical methodology has always been that it is (in the words of de Finetti) "ad hoc" and "arbitrary". Jeffreys makes the point with characteristic irony when he says of Fisher (one of the founding fathers of the orthodox methodology)
>
>> I have in fact been struck repeatedly in my own work, after being led on general principles to the solution of a problem, to find that Fisher had already grasped the essentials by some brilliant piece of common sense. [Jeffreys, p. 393]
>
> This is, in a way, a compliment. However, the compliment is distinctly back-handed: for what Jeffreys is really saying is that Fisher, notwithstanding his confusions and inconsistencies, often contrives to get the right answer owing to the power of his intuition. It is rather as if a physicist were to congratulate a snooker player on his ability to pot a ball notwithstanding his ignorance of Newtonian mechanics; or to congratulate a fish on its ability to swim notwithstanding its ignorance of the principles of hydrodynamics. I have argued elsewhere that that criticism is amply justified. Generally speaking what drives the Bayesian school of thought is a desire for clarity and logical cogency. By contrast the orthodox statistical methodology is driven by what Jaynes describes as an ideological conviction that, if statistics is to be scientific, then probability distributions must be conceived as objectively real entities. To attain that ideological end orthodox statisticians are willing to make whatever sacrifice of logical coherence seems necessary.

Happy Thanksgiving.

26-11-06 *Jimmy Hoffa's Bones* (to C. H. Bennett)

> "The hypothesis that there is an external world, not dependent on human minds, made of *something*, is so obviously useful and so strongly confirmed by experience down through the ages that we can say without exaggerating that it is better confirmed than any other empirical hypothesis."
>
> — Martin Gardner [and Chris Fuchs]

Just packing up and saw this on one of my transparencies, and thought of you.

26-11-06 *Sample Talk* (to R. E. Slusher)

By the way, here's a sample of my present talks, just in case you would need it for anything.

> Quasi-Orthonormal Bases for the Space of Density Operators
>
> Recently there has been much interest in the quantum information community to prove (or find a counterexample to) the existence of so-called symmetric informationally complete measurements (SICs). In this talk we show that there should be even more interest. For, under a robust measure of orthonormality for operator bases (one that does not build in any symmetry at the outset), one can show that SICs, if they exist, come as close as possible to being orthonormal bases for the space of density operators. Moreover, in contrast to the usual expression of the superposition principle (where bases are taken to be orthogonal sets of state vectors), writing a superposition principle in terms of SICs leads to a more intrinsically-quantum representation for quantum states. This is because the basis states, rather than being the easiest to eavesdrop upon (as the usual ones are), are actually the hardest. Furthermore, such states fulfill a few other extreme non-classical properties that make them very interesting. Because of all this, writing the quantum-state space in these terms gives hope for a direct derivation of it from a plausible information-theoretic constraint principle. Various aspects of this problem will be discussed.

28-11-06 *Funes + Borges* (to C. H. Bennett)

On the flight over, I read the Borges story you sent me. It was great and much more meaningful than I had expected it to be. That guy really builds atmosphere. Anyway, you were right: there is certainly a sympathy between my thoughts and Funes's. But in another aspect of it, there is also a significant sympathy with Borges's. Particularly the line "To think is to forget a difference, to generalize, to abstract. In the overly replete world of Funes there were nothing but details, almost contiguous details," struck me. Though, for me, the importance is not quite in "forgetting a difference" but rather in paying no attention to it.

You'll see the similarity between what I think of quantum measurements and this point of Borges's if you dare enter the attached file [my paper "Delirium Quantum"]. The appropriate part is Section 3, titled "Snowflakes."

28-11-06 *Whose Lagrangian?* (to M. A. Nielsen)

I'm shamefully ignoring a talk at QCMC in Japan at the moment. I'm writing because I just looked at the scheduled and discovered that you're not here. I don't know why, but I had it in my head that you were going to be here (maybe you were on an earlier schedule?).

Anyway, I was looking forward to talking to you. I had wanted to discuss a paper with you posted by Matt Leifer last week—its number is `quant-ph/0611233`. Particularly, I guess I wanted to know whether it alleviates any of your troubles about this whole Bayesian quantum state and operation business that Carl, Rüdiger and I are trying to develop. I remember your asking in Konstanz what good is the 'search' for a fundamental Lagrangian if they're subjective (in the Bayesian sense) anyway. And I remember your not looking too pleased with my answer (I also talked to Menicucci about this later)—fair enough, it wasn't a very good answer. But now we've got a little more to work with, so it seemed like a good time for me to *try* to develop some answers.

(Hideo's sitting beside me doing his email too. Two times shameful between the two of us.)

Michael's Reply

I haven't seen Matt's paper. I'll be curious to have a look, although it may take a while [...]

On the general issue: as I recall, I essentially asked whether or not the people who'd been searching for a single universal Lagrangian had been in search of a chimera. Your answer was, I think it's fair to say, rather equivocal — you didn't say "yes", but you didn't give a clearcut "no", either. Given the astounding success the program of searching for a Lagrangian has had, I think any answer short of a resounding "No" spells big trouble for the Bayesian program.

29-11-06 *Whose Lagrangian?, 2* (to M. A. Nielsen)

Quick question, upon which I'll then chew on the answer more slowly ... being careful to write you again only after you have some time to breathe. Suppose a classical world, as was supposed, for instance in the days of Gibbs and Boltzmann. Given the astounding success (at that time) of the canonical distribution for a wide range of statistical mechanical and thermodynamical calculations and predictions, would that have spelt big trouble for a Bayesian view of statistical mechanics?

Michael's Reply

I don't think the situation is at all analogous, for at least two reasons.

First, much of the importance of the "quest for a Lagrangian" is as a guide to how we as physicists should work, and not so much as a theory of how the world is (unlike stat mech). The Lagrangian viewpoint might be quite wrong, yet that wouldn't change the fact that it's been hugely useful as a guide to making progress. Indeed, even if it does turn out to be wrong, it's reasonably likely to be replaced by some similar guiding principle. If the Bayesian point of view says we shouldn't look for a Lagrangian, I therefore view that primarily as a strike against Bayesianism, not Lagrangians.

Second, the Bayesian point of view explicitly addresses some shortcomings of the old stat mech formalism. I doubt very much that you're claiming that this point of view is a serious contender for explaining some shortcoming in the modern approach to quantum field theory.

29-11-06 *The Funesian Path* (to C. H. Bennett)

Now that you've got me thinking about this issue more than I had been before, it seems I keep running across things that bring it back to the top of my attention. Reading the book *The Pragmatic Humanism of F. C. S. Schiller* in this lovely Japanese bath tub, I came across this passage:

> It is Aristotle's logic which is usually taken as the archetype of formal logic. However, the 'logical analysis of judgment' in anyone's hands is bound to come to failure if it discounts the personal intention and circumstances of the maker of the judgment. Thus Hegel's formula of judgment as identity in difference disregards the fact that identities

are always made or postulated by us through conscious disregard of those differences which we take for the moment to be irrelevant to the purpose of judgment.

29-11-06 *Thinking of Brad* (to S. J. Lentz)

Hi from Japan. I'm so glad to hear that Brad seems to have made it through the operation OK; I told so many people about it here yesterday, it became a topic of conversation. Tell Brad that there were a load of quantum information physicists in Japan worried about his health!

Thinking of Brad's interest with the *Eats Shoots And Leaves* thing, as I was lying here with jet lag, it dawned on me that you might take him this class of examples to think about. (It was one of the topics of the dinner conversation last night.)

- "Buffalo buffalo buffalo buffalo."

If you parse it right, you'll see that it's a proper English sentence! Not a lie. (Don't tell him right away, but one reading of it can go like this: "The buffaloes from the city Buffalo bamboozle their fellow buffaloes everywhere." The reason this can be made to work is because one spelling of the plural of the noun "buffalo" is "buffalo". When buffalo is used as a verb, it means bamboozle or baffle.)

But it gets worse, buffalo can also be an adjective. My Webster dictionary gives it this definition as an adjective, "of the kind of style prevalent in Buffalo, NY." So,

- "Buffalo buffalo buffalo buffalo buffalo."

is also a proper English sentence. (Those "Buffalo buffalo" are a very self-deceptive lot.) And it can actually be made still worse, with even more repetitions of buffalo, if you work at it.

And now here's a different kind of example—this one invented by Charlie Bennett many years ago. If you cycle through the words "toll, house, cookie, delivery, and truck" you can make an infinite number of meaningful, distinct concepts.[113] For instance,

- Toll (that's meaningful enough)
- Toll House (meaningful too)
- Toll House Cookie (you know what those are)
- Toll House Cookie Delivery (if we only had a company like that in Cranford!)
- Toll House Cookie Delivery Truck (a truck that delivers those delicious cookies)

Now here's where it starts to get fun.

- Toll House Cookie Delivery Truck Toll
  (it's silly but it's meaningful; it's a special charge required for toll house cookie delivery trucks to use the road)
- Toll House Cookie Delivery Truck Toll House
  (ok, those trucks have to take a special lane when they pay their tolls)

[113] Charlie also has a method of modifying this example so as to produce a *continuous infinity* of meaningful, distinct concepts!

- Toll House Cookie Delivery Truck Toll House Cookie
(a toll house cookie that happens to be in the toll house cookie delivery truck)
- Toll House Cookie Delivery Truck Toll House Cookie Delivery
(not much market for it, but a very specialized company that delivers toll house cookies from the original toll house cookie delivery truck)
- Toll House Cookie Delivery Truck Toll House Cookie Delivery Truck
(and that's the truck employed by that weird delivery service)

You get the pattern now. And it can be made to go on forever, if you think about it. For instance, those weird delivery trucks from the last example could also be tolled. And there must be a place from where that toll is taken. And so on, and so on, and so on.

When Brad's ready to think a little, see if he enjoys these.

30-11-06 *Early Morning Rabbits* (to D. Gottesman)

It's not very conclusive evidence, but it is along the lines of what I thought I remembered hearing:

> There is another myth that says that there are rabbits on the moon making mochi, pounding away with their mochi maker, since there is a rabbit shaped series of dark areas on the moon, according to Japanese myth ... then there is another myth that Tsuyoiko's rabbit is descendant from the rabbit on the moon ...

The more important outcome of this, though, is that I learned that the rabbit I have been "seeing" all these years is quite different from the "official" image: See:

`http://en.wikipedia.org/wiki/Moon_rabbit`.

01-12-06 *Chris's House from Space* (to K. R. Duffy)

Ahh Ken, I miss you! I myself wouldn't say it's burnt Lamborghini orange—a little more subtle than that—but if having Lamborghini in the name will help me sell the place when the time (eventually, surely) comes, I'll use it liberally. Attached is a colorful picture for your further pub stories.

And while I'm here, let me advertise the opening of the Orange House Quantum Information-Foundations Seminar Center. That's what's in the second picture. My girls have agreed to rent the space out from time to time, as the needs of science require. Since that picture was taken, the two front columns have been rigged with fasteners for holding a large (green) chalkboard across the front, when meetings are in session. (If you look carefully, you'll notice that the main walls are constructed completely of doors; there are 12 in total. Kiki ripped them out of a nearby 1890's house. And bless her soul, she built the whole place by herself, from floor to roof, without an ounce of help from her worthless husband or anyone else.)

I should send you a picture of the newest member of the family too, but I won't clog the airwaves. Kiki decided that our retriever Murphy shouldn't be alone. So, he now has a spry little border collie companion, Beamish. Now, don't you think Ireland made some impression on us?

Finally to answer your question: Yes, the house can be seen on Google Earth ... but not because of the color! It's an old photo, most likely from a time before we purchased the house (the old roof can still be seen, and that is one of the first pieces of construction we had done). Here are the coordinates, which I had written up for a relative some time ago: [...]

Too bad you can't see the orange.

Greetings from Tsukuba Science City! Wish you guys were here. I'll try to get Charlie Bennett to send you pictures of the (before-cooked) menu selections he, Bill Wootters, and Lev Levitin had the other night. I don't recall the animal, but three of the items were "womb", "rectum", and "kidneys"—I nearly fainted at the sight.

03-12-06 *Our Stuff* (to M. Sasaki)

I found the Bartlett paper on the web, i.e., the one he presented to me at the poster session. You might enjoy it too; it is: `http://www.arxiv.org/abs/quant-ph/0608037`.

As you find some free time, let us develop this theory of ours more!

Thank you again for providing me with a wonderful week. I truly enjoyed this meeting. I think it was a great success.

05-12-06 *The Future of Quantum Information* (to O. Hirota)

I just wanted to write you a short note of thanks for making it possible for me to come to Japan. The meeting was wonderful; I learned so much from it. Particularly I was happy for the venue it gave me to continue my debate with Charlie and the progress I made therein. The issue is not just one of philosophy, I think, but rather something that will give rise to deep technical issues, and through them, a new means to move quantum information forward. So, I should thank you too in proxy for "the future of quantum information"!

In that regard, let me point you to two excellent papers that demonstrate the more technical side of the debate:

- `http://www.arxiv.org/abs/quant-ph/0401052`
- `http://www.arxiv.org/abs/quant-ph/0611233`

If you have any students interested in such matters, I hope you will put them in the service of this army! More seriously, not everything between Charlie and me is a joke. Some of it leads to good equations!

06-12-06 *Fourth and Fifth* (to J. Barrett)

I'll address your points "Fourth" and "Fifth" right away, and I'll come back with a longer, more specific reply to your note as a whole maybe by the end of next week. (I'm just in the middle of significantly revising `quant-ph/0608190`, and that has to be done before I go to the APS sorter's session, which will block out my time through the end of this week.)

Funny that you bring these things up, because I've provisionally titled my Shimony festschrift contribution, "Even Quantum Bayesians Like a Little Ontology from Time to Time." Anyway, for a provisional (though overly poetic) answer, see

1. Sections 6 and 7 of my paper "Delirium Quantum"
2. Maybe also Section 4 in it, picking up at the paragraph that starts with "Why is this not solipsism?", and

3. the paragraphs below that were written in response to one of the points of one of the 4(!) referees for the present paper.

If these passages provoke any other thoughts or other questions in you, feel free to write me with them, and I'll try to incorporate the issues into my larger response next week. (I.e., feel free to mail me any further thoughts, just be prepared for my not replying for a little while.)

Remarks to a Referee of `quant-ph/0608190`

This paper does not emphasize it, but no, we do not mean "the facts we are talking about here are facts for everybody." We mean that the facts too are personal, though in a very careful sense.

We have tried to meld the phrase "fact for the agent" a little better than previously into its surrounding text—so that it is there at least as some shadow of our presently agreed upon view—but it will have to remain somewhat awkward in the present context.

A better exposition would have emphasized the full setting of our view, including some discussion of the ontological—i.e., noninstrumental—pieces of it. 1) There are two physical systems in the story of this paper. One takes the role of the agent—the possessor of subjective degrees of belief and the activator of the measurement process. The other takes the role of the object system. 2) What is being discussed when one speaks of gathering data in the quantum context is an interaction or transaction between the agent and the object. 3) Without the agent, there would be no quantum "measurement" to speak of, but without the object, there would be no means for the agent to obtain the data, the spikes upon which he pivots his probabilities (his subjective degrees of belief). 4) There are no other agents in this story. 5) We do not go any further than points 1-5 warrant by giving the data obtained in a quantum measurement an autonomous existence—for instance, as something beyond the agent's sensations. That would run into inconsistencies in a "Wigner's friend" scenario. Nonetheless, quantum measurement outcomes are beyond the control of the agent—they are only born in the interaction—and thus are not functions of the agent in the way that his degrees of belief are. The degrees of belief find their source in the agent (the subject); the outcomes find their source in the external quantum system (the object). But the outcomes lead back to the agent in that they are personal to him.

This does not mean that the agent-object interaction and its fuller consequences are not autonomous events in spacetime, but it does mean that if there are any such things, quantum theory is not directly concerned with them. In CAF's view, quantum theory takes its whole definition as a normative theory for organizing an agent's *personal* probabilities for the *personal* consequences of his interactions with external physical systems. The structure of the agent-independent world that lies behind quantum mechanics is, in the end, still codified by the theory, but only in a higher-order, more sophisticated fashion than had been explored previously—it is through the normative rules, rather than quantum states and Hamiltonians.

Unfortunately a more detailed account like this will have to wait for another paper.

Jon's Preply

Here's my own take. Parts at least are different from yours, so you might not like it.

First, both probabilities and quantum states are descriptions of degrees of belief and are thus subjective.

Second, quantum theory doesn't need to specify what these beliefs are about, or What is a measurement? any more than classical probability theory needs to specify a sample set, or What is a measurement? Classical probability theory has such broad application just because it does not specify the sample set, it can be applied to any betting scenario. Classical probability theory may be derived with Dutch book arguments. The hope is that quantum theory can be derived from similar arguments supplemented with a small number of physical postulates, which get us to Hilbert space, whence Gleason's theorem can take over. Quantum theory could then be applied in any betting scenario in which those postulates are true.

Third, classical probability theory on its own is not a theory of physics, it is a tool that can be applied in physics. A theory of physics needs to specify an ontology of events that agents can, at least in principle, bet on. Thus Newtonian mechanics gives an ontology of events (system at particular point in phase space at time t) that one could in principle bet on. Newtonian mechanics plus classical probability theory gives Liouville mechanics. By the same lights, quantum probability theory needs to be supplemented with a theory of physics that provides an ontology of events to bet on.

Fourth, at present we have only a provisional, half-baked theory in which these events are "outcomes of interventions". But a fundamental formulation of a complete theory must not use such vague terminology, and will eventually supply an ontology of primitive events that agents can in principle bet on.

Fifth, the relationship between this fundamental theory and current theory need not be anything so simple minded as a hidden variable interpretation of quantum theory, in which the fundamental theory supplies instruction sets, and quantum states are distributions over instruction sets. In this light, worries along the lines of, "but hidden variables must be contextual" are too shallow. In a fundamental theory, space and time themselves may well be emergent, in which case worries along the lines of "but hidden variables must be nonlocal" are also too shallow.

As I understand you would resist the fourth and fifth here, insisting that quantum theory is complete and fundamental, that the objective events I speak of are not supplied by further physical theory, but are simply "outcomes of interventions", and that pace Bell, "Against Measurement" etc, there is nothing wrong with this. But might I ask why? The view I outlined seems supported by many of your arguments, in so far as quantum states are subjective. It is relatively humble (physics is not there yet, it's not even close). It also avoids mysticism (we will never understand the universe, it's too ineffable, thus don't aim for a god's eye view, just predict measurement outcomes).

06-12-06 *Made Wrong* (to J. Barrett)

Another quick point before I fade out of contact. With regard to:

Barrettism 1: *What is my point? Just that beliefs, described by quantum states, can be objectively right or wrong, and we are forced to this point by the need for objective outcomes of bets. This need not imply a complete instruction set for every quantum system. But it does imply that a theory must specify an ontology of events that give objective wrong status to (at least some) quantum states.*

Go to the file titled "Cerro Grande II" at the very bottom of my webpage, and in it do a search on the phrase "made wrong". The essays surrounding that phrase may go a little way toward explaining my take on what you just said. This very point is why I keep stressing the resemblance between quantum mechanics and pragmatic theories of truth—James, Schiller, Dewey—rather than correspondence theories, which, I think, lies in the background of your remark.

07-12-06 *0611283* (to C. H. Bennett)

Bennettism 31: *I never liked GRW much, but this paper, rather rudely refuting the infamous "Free Will" paper, mentions our differences in its introduction, e.g. my seeking for a quantum theory in which the observer plays no role.*

Well, I'm all for getting rid of the observer. But my strategy has always been to try to isolate which parts of quantum theory are explicitly about observers, and which parts aren't. I don't believe the latter is an empty set. But I don't believe the former is an empty set either—thus it needs to be carefully excised, and that is what I've been shooting for for a long time now. Perhaps I'm wrong, but I view it as a very careful approach, whereas things like GRW strike me as sheer speculation, based on rather fantastic ideas.

I'll look at the paper you recommend later tonight (I'm in DC, gonna go see some monuments before it gets dark).

In the meantime, do send me a copy of your Tsukuba talk. I want to think about it again.

07-12-06 *Adding and Subtracting* (to C. H. Bennett)

Here's another way to put what I wrote to you in the last note that strikes me as a possibly useful formulation. Traditionally the tack that's been taken in trying to recover an observer-free quantum mechanics (i.e., the thing you say you're seeking) has been in trying to *add* something to the raw formulation:

Bohm $\longrightarrow$ hidden variables
GRW $\longrightarrow$ a collapse mechanism
Everett $\longrightarrow$ a plethora of unseen worlds

What I say of myself is that I too am seeking an observer-free formulation of the content of quantum theory. But where I differ from the pack above is that, rather than adding anything to it to get the job done, I think it's just the opposite: Some elements need to be taken away. There's too much stuff in quantum theory as it stands—for, some of it is explicitly about observers. (Otherwise there wouldn't be such a resemblance to good Spekkens' toy theory ... which is, by construction, about states of knowledge.)

07-12-06 *The Cantankerous Colleague* (to R. Schack & C. M. Caves)

It's a little after 12:30 AM, December 7, and I think I'm going to call it quits for now. I'll ship you all the changes I've made, along with replies to the referees, and a list of most changes (with explanations for why I made them). It's my hope that Carl will be able to download these things and read them before we meet this afternoon. (Be careful to take your blood pressure pills first.)

I think I have pretty much covered everything, except:

1. Referee 2's point about extending the discussion of the distinction between "truth" and "probability 1". That's probably an important point, but given the contention between us, I didn't feel like going into this now. And maybe we shouldn't touch it at all ... ever.

2. The needed extension of text around the sentence, “The main issue here is whether there is any difficulty with the idea of an utterly certain belief about an admittedly contingent fact.” I think we can profitably build something based on a reply to Timpson’s criticism (pasted in the referee replies document for your convenience). It’s quite an important point I think, but I’ve just lost steam now.

Finally, I think it would be useful to give Mermin some more extended replies. Particularly we should say something about why this point he keeps bringing up—the one about how we should simply focus on never making probability-1 statements—is irrelevant, and just plain off the point.

In total, at this late stage, I don’t like the paper very much. Mermin’s report, but maybe more so Referee 4’s, made a big impression on me. I became ashamed of myself, knowing that I knew better and that if I had just been more engaged this year, maybe everything would have been OK. Rüdiger is right:

Schackcosm 94: *But without the joint effort it would not have been written at all. And are you really confident that any of us would have written a better paper by himself?*

It likely would not have been written, but I don’t know—the main delay on it, I think, was the issue of consensus. Certainly, I am not confident that I would have written a better paper by myself, but it would have been a very different paper. I’ve come to detest this business about the Copenhagen interpretation that runs throughout it. I’ve tried my best to cover us against the community, but in my heart I know it’s basically a lie (there’s plenty of stuff out there on Bohr and Pauli that slaps us right in the face). I don’t like that we never got “metaphysical” in the paper anywhere—without the imagery in the background, most readers will think that there’s little but another version of positivism in it (the world is sense impressions) and/or they’ll bring in their prejudices about what “facts” and “truth” are (correspondence and coherence theories), ones that I certainly don’t share and I know that I could guard myself against in a paper that didn’t demand consensus. You think you guard against those misimpressions by being quiet about them; I think it’s just the opposite. Mermin’s frustration was acute. And the issue of the exception to the subjectivity of probability—I find it so dangerous and so distasteful.

The trouble is my mother didn’t raise a very self-confident son. For some reason, on this project—this now 5-year-old project—I urgently felt the need for consensus between the three of us. Consensus was never reached in any but a superficial way. We would have been better off to all write our own papers on our own thoughts. The papers would have been finished long ago, and we could think in a civilized way how we agree or disagree with each other and maybe make some progress that way.

07-12-06 *Changes I Made To Certainty* (to R. Schack & C. M. Caves)

Changes in Section 1

- Abstract. Why did I change the abstract? Predominantly because I was bothered by this sentence: “In this paper we investigate the concept of certainty in quantum mechanics, *because* it is the with-certainty predictions of quantum mechanics that highlight the fundamental differences between our Bayesian approach on the one hand and Copenhagen . . . ” This paper has slowly been morphing into a referendum on us against Copenhagen and I don’t like that. For one thing, I don’t know what the hell the Copenhagen interpretation is and no one else does either, and that is just setting ourselves up for years of pain (as effectively Referee

4's report attests to). I wish I hadn't allowed it to get this far, but I did and I can't back out now. All I can try to do now is temper the damage, and then run for the hills as soon as I'm finished with this project. In Rüdiger's very first draft the word Copenhagen appeared 17 times; by the time we submitted to `quant-ph`, the paper contained 28 instances. Now, with Carl's latest modifications, we have 33. We would have been better to never mention it once.

- Abstract and throughout paper. I replaced all instances of "with-certainty" with "probability-1". I'm not adamant about this, but the new phrase did strike me as unneeded, and that the reader would more easily have a sense of what "probability-1" means and where the paper is going than taking a time to ponder over what we might mean with this never-before-seen adjective "with-certainty." There were only four instances of "with-certainty" in the whole paper anyway.

- Abstract. Left Stairs reference, but took out phrase "originally due to" and replaced it with "of." Don't want our abstract to seed a priority battle (even though Stairs says that there is a version of the argument in his 1978 thesis, predating Heywood-Redhead significantly). Anyway, in this way, our statement is certainly true: I don't understand the Heywood-Redhead paper (so we can't be giving a version "of" their argument).

- par. 2. Too repetitive, so I trimmed it down. Also made the logic flow a little better, and tried to tone down the "fact for an agent" which sticks out like a sore thumb. See discussion with Referee 4 for this point. Got rid of the parenthetical "(or results)" because we just don't use that terminology so much—doesn't need to be introduced. Modified footnote on "eliciting"—I don't like power-packed long sentences.

- par. 3. Got rid of the "generally." Didn't see any need for it, as we make no admission to other kinds of conditionalizing, like Jeffrey, etc., in this paper. Changed "probabilistic argument" to make Referee 4 happy. Also changed "functions of" to "depend upon" because Referee 4 raised a flag.

- par. 3. Also changed "probabilities must obey" to "gambling commitments should obey" to make my Darwinian side feel better, also to head off the reading of the statement as a tautology.

- par. 5, 6. "theory" $\longrightarrow$ "world". Added "purview" and all kinds of stuff like that. I took out the parenthetical "(the Born rule)" because people are not always so specific—they just like to flippantly throw out the phrase "specified by physical law" and act as if that means something obvious. I know that you added that, Carl, to help introduce the next paragraph and to help it not seem so out of place (which it indeed is). In general, I tried to round a lot of edges, connect things better, and build up to the next paragraph.

- par. 7. Fundamentally reworked.

- par. 8, 9. In line with what I said above in the abstract, I changed descriptions to "our main aim" and "along with this." The reason I added "more carefully address" is because previously we had already said, "We have shown in a series of previous publications that ... all probabilities in quantum mechanics can be interpreted as Bayesian degrees of belief .... A consequence of the Bayesian approach is that all quantum states, even pure states, must be regarded as subjective." I also got rid of a little bit of repetition. Also I backed off some instances of "the Copenhagen interpretation", and in places where the phrase "and similar interpretations" were invoked, I replaced it with the more compact "Copenhagen-like interpretations." Note also that here and there (this paragraph and others) I peppered the phrase "agent-independent" to get across the idea of what is different in the Copenhagen-like interpretations.

- (what had been) par. 10. I've always thought this comment about not commenting on other interpretations had no natural place. So, I've decided to place it in a footnote. I think it much less breaks the flow this way.
- 4th par. from last. Added, "even when derived from the "ultimate measuring instruments" of Ref. [114]," again to address Referee 4's concerns.
- Sect. 1, 3rd and 2nd par. from last. Tightened up greatly to reduce repetition of previous paragraphs and also modified some language to try to alleviate confusion (of the sorts Timpson had). Changed "originally due to Stairs" to "of Stairs" and also gave credit to Heywood-Redhead and Brown-Svetlichny as "independent" variations on the theme. Also inserted "noncolorability" to better set apart the KS result from itself being a full no-preexisting-property proof.

Changes in Section 2

- par. 1, last sentence. Changed to "we shall say that that facts are objective in the sense that ..." to make it a little clearer that this is our definition of objective, rather than a categorical statement (potentially carrying the reader's own connotations, rather than ours).
- par. 2, last sentence. Removed 'in probability theory' because a) we don't elaborate on what that means, and b) because it created too many questions in Ref. 2.
- par. 3. 'takes this category distinction seriously' $\longrightarrow$ 'takes this category distinction as its foundation'.
- par. 4. Added "predominantly" because I modified (extended) the quantum discussion at the end of the section.
- 2nd par. of PP discussion. Changed 'typical' to 'exchangeable' because typical conveys the sense the there is a correct class of priors. Why not be specific? Stripped away the closing sentence, "The whole idea, however, is plagued with a fundamental conceptual difficulty" because it is not at all justified by our nonquantum, coin toss example. It can't be a "fundamental conceptual difficulty" if it potentially works in the quantum world. So, I played up the point that instead that chance simply serves no useful role.
- 3rd par. of PP discussion. Built a new transition into this paragraph.
- Last par. Drastically reconstructed to take into account Referee 4's point about how we have the logic of the issue backward. Also humbled the presentation somewhat.

Changes in Section 3

- par. 4. I removed the parenthetical phrase "(often called the *model*)" (and the single further instance of "model" later in the paper), because I saw no use for the terminology. No need to introduce an essentially unused term. I also italicized "exception" and introduced an endnote

[114]Letter from Niels Bohr to Wolfgang Pauli, 2 March 1955 (provided to us by H. J. Folse).

to distance myself from the "exception pack"[115]. By the way, I think the paper would be distinctly better if we could turn off this annoying feature of the REVTEX 4 style file that turns footnotes into endnotes. Independent of my definitely wanting this note of mine to be seen, it would help ensure that *all* notes aren't simply overlooked as references, and the reading would flow easier. Is there anything in the paper that distinctly depends on the REVTEX 4 style? We are almost surely going to have to change style for submission to SHPMP anyway.

Just to be snide, by the way, I will point out that the following sentences of Carl's latest draft (i.e., the one just before my present modifications)

1. "We first review the main arguments for the general claim that probabilities always represent degrees of belief." [in the abstract]
2. "The updated probabilities always depend on the agent's prior probabilities as well as on the data and thus can be different for agents in possession of the same data." [in the introduction]
3. "We argue, following de Finetti [116], that in the last analysis probability assignments are always subjective in the sense defined earlier." [again in the introduction]
4. "We emphasize that certainty is always an agent's certainty . . . " [once more in the introduction]
5. "Probability assignments are not arbitrary, but they always have an irreducibly subjective component." [in section 2]

are in direct conflict with the *exception*. Similarly, without a more careful statement, this

6. "Probabilities for outcomes or data are not facts." [back in the introduction again]

is in conflict too: For one would have to be careful that the probabilities being spoken of here are distinctly not conditional probabilities, or at the very least, when they are conditional probabilities, a further analysis of their subjective status is required.

Now, that said, *I do not advocate changing statements* 1 *to* 6. All I'm trying to do in my snideness is give you a feel for how *ugly* a formulation the paper would take on if you were consistent with yourselves. For instance,

1'. "We first review the main arguments for the general claim that probabilities sometimes represent degrees of belief."

2'. "The updated probabilities mostly depend on the agent's prior probabilities as well as on the data and thus can be different for agents in possession of the same data."

[115] This was the footnote (it did not ultimately appear in print): "One of the authors—CAF—strongly dislikes exceptions on matters of principle, and the present exception is no exception. The problem, *as he sees it*, with the formulation that there is a case whereupon a fact strictly determines a probability is that it puts the horse before the cart: It acts as if abstract 'logical implication'—a statement of the form "$d \Rightarrow h_0$"—has a meaning independent of the *subjective* judgment "$\Pr(d|h) = 0$ for $h \neq h_0$." (See for instance, F. C. S. Schiller, *Formal Logic: A Scientific and Social Problem*, (Macmillan, London, 1912), and F. C. S. Schiller, *Logic for Use: An Introduction to the Voluntarist Theory of Knowledge*, (G. Bell, London, 1929), for a development of this point.) Therefore the reader should associate neither the sentence of text cited by this endnote, nor the three sentences following it, with the views of CAF. Since it is CAF's belief that these sentences undermine the very consistency of the quantum Bayesian point, it is a lucky thing that, *from his point of view*, they are not actually used anywhere in the paper's argumentation. Instead, they should be treated as dangling appendages that will be surgically removed in any subsequent CAF publications."

[116] B. de Finetti, "Probabilismo," Logos **14**, 163 (1931); translation: "Probabilism," Erkenntnis **31**, 169–223 (1989).

3'. "We argue, following de Finetti, that in the last analysis probability assignments are fairly often but not always subjective in the sense defined earlier."

4'. "We emphasize that certainty is *always* an agent's certainty ... except in those cases when it is not"

5'. "Probability assignments are not arbitrary, but here and there they have an irreducibly subjective component."

Remind me: Just what "possible misunderstanding" is this *exception* supposed to be saving the reader from? I've never seen that it leads anywhere other than to weakening our argument.

- Fourth par. from last. I instated a parenthesis around the 'nontrivial'. I figured if you allowed the parenthesis once a few paragraphs above, you'd allow it again.

- Par. where state preparation is given an equation. Deleted "essentially," because it struck me as apt to confuse—implying that there are exceptions. Also took out "thus the posterior state is independent of the prior state" for reasons of repetitiveness.

- Par. starting with "The quantum operation depends, at least partly ... " I have changed the logic of this argument because I feel that Referee 4 is right—it's just the problem of trying to say all this crap about the "Copenhagen interpretation." It is dangerous business. For instance, it's really hard—probably impossible—to reconcile these things we say with a passage like this one (drawn from Plotnitsky's book *Complementarity*):

  > Bohr insists that indeterminacy affects the interaction, and thus the possibility of sharp distinction, between the measured object and the measuring instrument, since both must be treated as quantum systems (PWNB 1:11; PWNB 2:25-26, 72-74).

  And anyway, I have always felt uncomfortable putting so much weight on a "church of the larger Hilbert space" view of state preparation. As far as I am concerned, the really only conclusive argument (for the subjectivity of operations) is the one we present in the two paragraphs preceding this one:

  > It is tempting to conclude that objective facts, consisting of the measurement outcome d and a classical description of the preparation device, determine the prepared quantum state σ. This would violate the category distinction by allowing facts to fully determine (nontrivial) probabilities derived from σ. What this can only mean for a thoroughgoing Bayesian interpretation of quantum probabilities is that the posterior quantum state σ *must* depend on prior beliefs through the quantum operation.

  It was fine to have the church argument lying around as a heuristic to help convince people (like Carl) if needed, but when it is touted as 1) the ultimate reason for the subjectivity of operations and 2) an explicit contradiction to Bohr and the like, then it gets dangerous. Because then every amateur reader of Bohr out there can be in a position to pounce.

  We say in the last draft,

  > Classical facts cannot suffice to specify a preparation device completely because, among other things, a complete description must ascribe to the device an initial quantum state, which inevitably represents prior beliefs of the agent who is attempting to describe the device.

Now, it was I who inserted the phrase "among other things," but that really didn't go nearly far enough to save my conscience from agony. Delete it, as almost surely no readers will take note of it anyway, being such a slight deviation from the rest of the flow, and what do you get?

> Classical facts cannot suffice to specify a preparation device completely because a complete description must ascribe to the device an initial quantum state, which inevitably represents prior beliefs of the agent who is attempting to describe the device.

Would Bohr himself buy that? (Carl will be quick to say that he doesn't give a damn what Bohr would say. But without some firm foundation for this great enemy that we're supposed to be fighting, 'the Copenhagen interpretation', we're just erecting a straw man that we're taking pleasure in kicking down. Mermin alluded to this, and so did Referee 4, and, honestly, in my own head I've always thought it.)

Here's a nice passage from Asher Peres's paper "Karl Popper and the Copenhagen Interpretation":

> Quantum mechanics provides statistical predictions for the results of measurements performed on physical systems that have been prepared in specified ways (Peres, 1995). (I hope that everyone agrees at least with that statement. The only question here is whether there is more than that to say about quantum mechanics.) The preparation of quantum systems and their measurement are performed by using laboratory hardware which is described in classical terms. If you have doubts about that, just have a look at any paper on experimental physics. The necessity of using a classical terminology was emphasized by Bohr (1949) whose insistence on this point was very strict:
>
>> However far the [quantum] phenomena transcend the scope of classical physical explanation, the account of all evidence must be expressed in classical terms. The argument is simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics.
>
> The keywords in that excerpt are: classical terms . . . unambiguous language . . . terminology of classical physics. Bohr did not say that there are in nature classical systems and quantum systems. There are physical systems for which we may use a classical description or a quantum description, according to circumstances, and with various degrees of approximation. It is according to our assessment of the physical circumstances that we decide whether the q-language or the c-language is appropriate. Physics is not an exact science, it is a science of approximations. Unfortunately, Bohr was misunderstood by some (perhaps most) physicists who were unable to make the distinction between language and substance, and he was also misunderstood by philosophers who disliked his positivism.
>
> It is remarkable that Bohr never considered the measuring process as a dynamical interaction between an apparatus and the system under observation. Measurement had to be understood as a primitive notion. Bohr thereby eluded questions which caused considerable controversy among other authors (Wheeler and Zurek,

1983). Bohr willingly admitted that any intermediate systems used in the measuring process could be treated quantum mechanically, but the final instrument always had a purely classical description (Bohr, 1939):

> In the system to which the quantum mechanical formalism is applied, it is of course possible to include any intermediate auxiliary agency employed in the measuring process [but] some ultimate measuring instruments must always be described entirely on classical lines, and consequently kept outside the system subject to quantum mechanical treatment.

So, I think, Bohr would reject our very starting point. How can we really argue that he'd be wrong in rejecting it? We'd say, "But you're not really giving a complete description of the ultimate measuring device because a complete description must ascribe to the device an initial quantum state." He'd say, "Young men, you just don't understand the practice and scope of quantum mechanics."

OR, if I wanted to play like Bohr anticipated us all along, I think I could actually pull that off too. For I can think of a completely different reading of Bohr where he might have been in essential agreement with us and even gone on to refine the point. For instance, he might say,

> Right you are young men that there is an essential subjective character in state preparation—that's what Plotnitsky was reporting on my behalf—but what do you think I meant by saying, "The argument is simply that by the word 'experiment' we refer to a situation where we can tell others what we have done and what we have learned and that, therefore, the account of the experimental arrangement and the results of the observations must be expressed in unambiguous language with suitable application of the terminology of classical physics."? I meant that two agents have to have common enough priors that they can actually speak to each other—there has to be enough intersubjective agreement that they're in the same 'language game.' Didn't you read Bernardo and Smith, who say:
>
> > There is an interesting sense, even from our standpoint, in which the parametric model and the prior can be seen as having different roles. Instead of viewing these roles as corresponding to an objective/subjective dichotomy, we view them in terms of an intersubjective/subjective dichotomy. To this end, consider a *group* of Bayesians, all concerned with their belief distributions for the same sequence of observables. In the absence of any general agreement over assumptions of symmetry, invariance or sufficiency, the individuals are each simply left with their own subjective assessments. However, given some set of common assumptions, the results of this chapter imply that the entire group will structure their beliefs using some common form of mixture representation. Within the mixture, the parametric forms adopted will be the same (the *intersubjective* component), while the priors for the parameter will differ from individual to individual (the *subjective* component). Such intersubjective agreement clearly facilitates communication within the group and reduces areas of potential disagreement to just that of different prior judgements for the parameter. As we shall see in Chapter 5, judgements about the parameter will tend more towards a consensus as more data are acquired, so that

> such a group of Bayesians may eventually come to share very similar beliefs, even if their initial judgements about the parameter were markedly different. We emphasize again, however, that the key element here is intersubjective agreement or consensus. We can find no real role for the idea of objectivity except, perhaps, as a possibly convenient, but potentially misleading, "shorthand" for intersubjective communality of beliefs.
>
> When I am talking about the ultimate measuring instrument, I am talking about precisely the situation where there is intersubjective agreement between agents. So, you can purify the description of measurement if you wish (i.e., ascribe a quantum state to the preparation device), but then there is no further subjective freedom in those quantum states, for those hypothetical agents would be so in disagreement that they will not be able to communicate with each other. That is to say, I say it's time for a classical description of a measurement device just at the moment where your argument about the subjectivity of states no longer holds for two disparate agents.
>
> A complete description of a preparation device is a fact? Who said anything about facts? I didn't.

This is why I think we have really worked ourselves into a corner—and done our relatively clear initial thoughts a disservice—by making the phrase 'the Copenhagen interpretation' such a focal point.

But there's not much that can be done about this now, without scrapping the whole project. Thus rather than attempting to delete the argument (which I'm sure would create an outcry like never heard before), what I propose is that we change the logic of the argument (and then only slightly ... enough to get us through the night). The flow used to be "a complete description of a preparation device *requires* that a quantum state be ascribed to the device." To at least stave off the Bohrians-in-the-know a little bit at this point, and to frame the argument more in terms that I can agree with, here's what I changed it to. 1) Take any purported agent-independent, complete classical description of the preparation device. 2) That description can always be 'quantized' into one in the church of the larger Hilbert space, i.e., one of the variety we discuss. But then, 3) the latter description of the process clearly has a subjective element in it in the form of a prior quantum state for the device. 4) Since there is no operational distinction between the descriptions, either the initial quantum state in the latter description is in fact objective (contrary to our assumption), or the initial purported classical description had a subjective element in it that was unacknowledged previously.

By the way, let me just put it down for the record why I'm not all that attracted to going to the church of the larger Hilbert space to justify the subjectivity of preparation operations. The reason is that it is effectively like saying that to prove the subjectivity of conditional probabilities you have to start from the subjectivities of the joints and the marginals. But the conditional should not require the existence of a joint distribution before its own existence—it is a stand-alone entity, as Rüdiger emphasizes in his classes. And so, its subjectivity is stand-alone too. Similarly, I would say, for quantum operations.

Changes in Section 4

- par. 2. Changed tense in this paragraph. Deleted the use of "with certainty" that confused Referee 2.

- par. 3. Changed 'incomprehensible' to 'inconsistent' for Referee 4.

- last par. Deleted 'the quantum state of' because 1) my previous railing on church-of-the-larger-Hilbert-space justifications, and 2) because it gives the impression one can have beliefs about the (true) quantum state, rather than admitting forthrightly that quantum states are beliefs. Also added "that is true or false of the system" to "The statement that the measurement outcome is 1 with certainty is thus not a proposition, but an agent's belief—and another agent might make a different prediction." for the purposes of Referee 2.

Changes in Section 5

- par. 3. Added "external to the agent" to guard against those who would ask, "What, the agent is not part of the world?" I think that's as far as we want to go anyway.

- last par. "slippery slope" ⟶ "path" The former seemed too alliterative for the Rüdigerian style we had settled on. Also, it seemed too judgmental to be stated without back-up.

Changes in Section 6

- par. 6. Added reference to Garrett.

- par. starting "It might still be ..." Add "if he is bound by a classical worldview" to the last sentence.

- par. starting "For instance, take a complete ..." Changed "raw" to "bare"—seemed less harsh that way (I'm sensitive because of Carl's "meaty" jokes). Also added "law-of-thought" as an adjective to help emphasize the point. "shows its head" ⟶ "shows itself"

- Last paragraph. Deleted "of the world"—didn't seem needed in that context. "dogged" ⟶ "plagued" (I had gotten carried away previously). "finally" ⟶ "most importantly" (didn't want an overuse of finals). Changed "what features of the quantum formalism" to "what features of the quantum formalism beyond the ones discussed here" just to make it clear that we had already discussed some.

07-12-06 *(Select) Replies to Referees* (to R. Schack & C. M. Caves)

Replies to Referee 1

> **Referee:** In fairness to Copenhagen shouldn't you add that though Copenhagen holds the quantum state to be objective in the sense you describe, this (by itself) does not imply that the state is an objective property of the system to which it is assigned. Surely Bohr would never have said such a thing. It is an objective property of the "whole experimental setup" which includes both the system and the preparation device. You do indeed say this a little further down the page, but only reluctantly ("perhaps supplemented by..."). You seem to be trying to make Copenhagen sound worse than it is.

Agreed. We softened the language and made the enemy more specific throughout the paper. You are correct that Bohr would have likely called the quantum state (though he rarely spoke of quantum states) as an objective property of the "whole experimental setup". What he failed to realize though—in CAF's reading of Bohr—was the dependence of the phrase "whole experimental setup" upon an agent's judgment. (An interesting exchange between Bohr and Pauli from 1955 bears this out to some extent.)

> **Referee:** "The main issue here is whether there is any difficulty with the idea of an utterly certain belief about an admittedly contingent fact." Yes indeed. And this is the issue that underlies most of my concerns about your not paying enough attention to what constitutes a fact, and not giving any example of the certain beliefs of two agents that underly different state assignments. This is central to your argument. Is a reference to de Finetti all you're going to give the reader? (This reminds me of a friend who was receiving Catholicism lessons from a priest so he could be married in a church. He kept asking the priest why he should believe in the immaculate conception, and then why he should believe that an angel had announced it, etc., until he got to a point where the priest said triumphantly, because German scholarship has established it.) Surely you owe your readers some gloss on de Finetti, or an explanation of why he is not central to your case.

You are quite right about this, and initially we tried to fill in the gap here. But then we realized it was too large a task to try to bite off in a "committee report" and, we backed off. So we removed the paragraph completely. Further elucidation will have to await another publication.

> **Referee:** I once read somewhere in Jaynes that no Bayesian would take $p(h) = 1$ for any h other than a logical tautology, because no acquired data whatever could then permit updating the probability to anything less than 1. I assume that what he meant was that since $p(d)$ in (2) is the sum on h of $p(d|h)p(h)$, then $p(d)$ is necessarily equal to $p(d|h)$ when $p(h) = 1$.
>
> Given this, why isn't your entire paper about a situation (certainty) that no self-respecting Bayesian would ever find himself in? Presumably the reason it interests you is that quantum theory does assign $p = 1$ to certain data given certain state assignments, and this requires some interpretation. But then shouldn't the position of the Bayesian be that pure state assignments are ideal limiting cases that one can never encounter in practice? Analogous to initial conditions specified to infinite precision in chaotic classical dynamical systems. (You have seduced me into making dubious classical analogies.) All physically achievable states are mixed states. A pure-state assignment should not be viewed as subjective, but as impossible. A quantum operation may well be subjective but this has nothing to do with pure states being subjective, because there is no physical quantum operation that prepares a pure state. The best you can ever do is prepare a mixed state of very low entropy.

One of us—CAF—has never seen the relevance of this point (which the referee has made previously in other contexts) for what is at issue here. CMC, on the other hand, says, "This is an interesting point that we are well aware of, but it is not a point we agree with, nor is it one we wish to deal with in this paper."

But, back to CAF. Your concern seems to be this. If one should, methodologically, never assign a pure state, then there are no handles by which to address pure states' subjectivity or objectivity. (In fact it seems, one should just write them out of the theory.) But that's not true. The Born rule tells us how to calculate probabilities from pure states—so the issue has to be adressed, and in fact there's something to be learned from it. If one has accepted all the arguments for (or simply the beauty and simplicity of) interpreting ***all*** probabilities as subjective, Bayesian degrees of belief, then one must conclude that the pure state has a subjective element or the measurement description has a subjective element or both have a subjective element.

Replies to Referee 2

> **Referee:** The category distinction between facts and probabilities is explained quite nicely. Nonetheless, the authors might wish to emphasize this point further. The confusion I have in mind (which might be my own) is the following one. It is typical to assert that assignments of probabilities 0 and 1 to a proposition are equivalent to assignments of truth and falsity to that proposition. If this is so, then one cannot say that "the truth value of a proposition is a fact" and simultaneously that no probability assignment is a fact. Presumably, the mistake is in identifying probability 0 and 1 with truth and falsity. Or, perhaps there is a more subtle distinction to be made between the actual truth value of a proposition and the truth value that an agent assigns to it (which might be different from the actual value). In any case, treating propositional logic as a special case of probability theory is sufficiently ubiquitous that more explanation seems in order here.

Indeed we all agreed with the referee on this point, but we could never come to great agreement among ourselves on how to handle the issue in the context of this paper—where much of the groundwork required to address it properly had not been laid. Thus, we added a small—perhaps confusing—footnote to nonetheless mildly address the issue. We will try to do your point better in further publications.

> **Referee:** It should be emphasized by the authors that they are making the strong claim that the Bayesian approach allows one to hold on to the assumption of locality. This is likely to be a very controversial claim and consequently should not be left implicit. Wiseman and others have argued that maintaining locality is only possible if one is willing to deny the reality of distant observers, that is, it is only possible if one is willing to adopt a solipsist view. The authors may choose not to address such issues in detail in this paper, but should certainly say more than has been said about locality in the Bayesian approach.

Wiseman (personal conversations with CAF) has never understood the category distinction we are making—and that is the root of his further misunderstandings. Since we have laid out the category distinction fairly carefully in this paper, we think we have said enough for the time being. There's only so much that can be done in one paper.

Replies to Referee 3

> **Referee:** The authors attempt to defend the following general thesis. There are no facts about probabilities in the world (see e.g. Section II). Attempts (e.g. Lewis's PP) to connect between probabilities and facts are highly problematic. Moreover, in principle the gap between probabilities and facts is unbridgeable. Probability assignments always have an irreducibly subjective component, essentially because they involve priors. This holds for both deterministic (e.g. classical statistical mechanics) and indeterministic theories (where the probabilities are given by dynamical laws, e.g. some versions of quantum mechanics).

This captures part of our paper relatively accurately.

> **Referee:** I agree that PP-style and similar approaches have problems. But this does not mean that probabilities are not grounded by facts. Here is a counter-argument to the above. Under quite general circumstances, it is provable that if an agent updates probabilities in accordance with Bayes' rule the probabilities will converge to the long term relative frequencies (for large enough samples), no matter what the prior probabilities are (except for priors equal to 0 and 1). The dependence on the priors (for large enough samples) washes out in the long run, and so the priors, however subjective, don't matter. The authors seem to be aware of this point (see p. 4, last paragraph), but they don't directly address it. The same argument applies also to the quantum mechanical probabilities and state assignments (Section III).

The counter-argument holds no water for a subjective Bayesian. 'Quite general circumstances'? For whom? For sufficiently distinct priors, there need be no convergence. What you point out is only true if two agents for whatever reason—perhaps simply because of their common genome—happen to adopt 'compatible' priors of one variety or other. We explore this issue in detail (particularly in the quantum case) in C. M. Caves, C. A. Fuchs, and R. Schack, "Conditions for Compatibility of Quantum-State Assignments," Phys. Rev. A **66**, 062111 (2002)—so, yes, we are aware of it.

The book by Bernardo and Smith that we cite in the present paper puts the issue very nicely:

> [T]here is an interesting sense, even from our standpoint, in which the parametric model and the prior can be seen as having different roles. Instead of viewing these roles as corresponding to an objective/subjective dichotomy, we view them in terms of an intersubjective/subjective dichotomy. To this end, consider a *group* of Bayesians, all concerned with their belief distributions for the same sequence of observables. In the absence of any general agreement over assumptions of symmetry, invariance or sufficiency, the individuals are each simply left with their own subjective assessments. However, given some set of common assumptions, the results of this chapter imply that the entire group will structure their beliefs using some common form of mixture representation. Within the mixture, the parametric forms adopted will be the same (the *intersubjective* component), while the priors for the parameter will differ from individual to individual (the *subjective* component). Such intersubjective agreement clearly facilitates communication within the group and reduces areas of potential disagreement to just that of different prior judgements for the parameter. As we shall see in Chapter 5, judgements about the parameter will tend more towards a consensus as more data are acquired, so that such a group of Bayesians may eventually come to share very similar beliefs, even if their initial judgements about the parameter were markedly different. We emphasize again, however, that the key element here is intersubjective agreement or consensus. We can find no real role for the idea of objectivity except, perhaps, as a possibly convenient, but potentially misleading, "shorthand" for intersubjective communality of beliefs.

Particularly, there is no requirement of nature that there must be intersubjective agreement of the type discussed. If it happens, it is often nice—it leads to less violent worlds for instance—but nature cannot decree it.

> **Referee:** p. 4, last par – I think that the authors don't address the real problem faced by any purely subjectivist interpretation of probabilities. Take for example classical statistical mechanics. The problem isn't about whether the uniform probability measure brings about the melting of the ice cube. Of course it doesn't.

Of course it doesn't? Like you, we would have said so. But you should not mock. To quote the reference [35] you bring up in your Point (8) below (i.e., J. North's paper) at length:

> There are two main, and to my mind fatal, problems with the epistemic view [of probabilities in statistical mechanics]. The first stems from the role the uniform probability distribution plays in explanations of thermodynamic phenomena. Consider the explanation of an ice cube's melting towards the future. If we take an epistemic view of the uniform distribution that is placed over its current macrostate, then part of the reason for the ice's melting will be our ignorance of its initial microstate. On the assumption that explanations of physical phenomena ought to be objective—and in any case not rely on our epistemic state—we should not maintain that part of the explanation that entropy increases (that ice melts, that coffee cools, that gases expand) is the extent of our knowledge. How could our epistemic state have anything to do with the ice cube's melting? This would be like saying that if we happened to be the kinds of beings who did have epistemic access to the initial microstate of the ice cube, then it might have behaved differently. It is also a consequence of this view that no matter what kind of world we live in, we must assume that the ice is most likely to melt towards the future. All of that seems crazy: we are after an objective, scientific explanation of thermodynamics.

We have added a more explicit reference to this paper, so that perhaps a reader will be led to it.

> **Referee:** The authors seem to imply (Sections V and VI) that the Bayesian approach to QM is local, since it doesn't assign determinate instruction sets (even in eigenstates). This, however, is completely mistaken. Assuming that measurements have outcomes, any local theory is committed to Bell's inequality. So any theory that produces the statistical predictions of quantum mechanics must be nonlocal, no matter whether or not it admits instruction sets.

'Completely mistaken' is a rather strong expression for a referee who hasn't shown many indications of having understood much of the point of view of the paper.

> **Referee:** The authors claim that there is no fact of the matter about probability assignment (including quantum states; Section VI). But they accept the Dutch-book argument (Section I). This is a contradiction. The Dutch-book argument presupposes that there are factual constraints (sure loss) on probability assignments.

It is not a contradiction. We view the Dutch-book argument as a normative principle—not a statement of fact or a property of the physical world. Our view is along the lines of these words from our Bernardo and Smith reference:

> What is the nature and scope of Bayesian Statistics ...?
>
> Bayesian Statistics offers a rationalist theory of personalistic beliefs in contexts of uncertainty, with the central aim of characterising how an individual should act in order to avoid certain kinds of undesirable behavioural inconsistencies. The theory establishes that expected utility maximization provides the basis for rational decision making and that Bayes' theorem provides the key to the ways in which beliefs should fit together in the light of changing evidence. The goal, in effect, is to establish rules and procedures for individuals concerned with disciplined uncertainty accounting. The theory is not

> descriptive, in the sense of claiming to model actual behaviour. Rather, it is prescriptive, in the sense of saying "if you wish to avoid the possibility of these undesirable consequences you must act in the following way."

Dutch-book coherence is something to strive for. For if not satisfied, an agent with any logical powers will see that his gambling commitments could lead, in the appropriate circumstances, to sure destruction.

To help abate misunderstandings like yours, we have changed the sentence in our Introduction reading

> The Dutch-book argument shows that, to avoid sure loss, an agent's probabilities must obey the usual probability axioms.

to

> The Dutch-book argument shows that, to avoid sure loss, an agent's gambling commitments should obey the usual probability axioms.

> **Referee:** Some items in the references list don't appear in the text, e.g. [10], [35].

We thank the referee for pointing out his likely identity.

Replies to Referee 4

> **Referee:** A final point about this passage: you write
>
> > The occurrence or nonoccurrence of an event is a fact for the agent. Similarly, the truth or falsehood of a proposition is a fact. Facts are objective; they are not functions of the agent's beliefs.
>
> What work, precisely, is the modifier 'for the agent' supposed to be doing here? It comes across as slightly odd to say that occurrence is a fact 'for the agent' and then go on to emphasize the non-dependence of these fact[s] on the agent. (Of course, there is no inconsistency here: apparently, facts are *facts for everybody* on your view, and so *a fortiori* they are facts 'for the agent'. But still [the] way you put things here strikes me as misleading somehow.)

This paper does not emphasize it, but no, we do not mean "the facts we are talking about here are facts for everybody." We mean that the facts too are personal, though in a very careful sense. There is only so much work one can put into a paper as a view is evolving (and as the authors are *slowly*, ever so very *slowly*, coming to agreement), without scrapping the whole project and starting over.

We have tried to meld the phrase "fact for the agent" a little better than previously into its surrounding text—so that it is there at least as some shadow of our presently agreed upon view—but it will have to remain somewhat awkward in the present context.

A better exposition would have emphasized the full setting of our view, including some discussion of the ontological—i.e., noninstrumental—pieces of it. 1) There are two physical systems in the story of this paper. One takes the role of the agent—the possessor of subjective degrees of belief and the activator of the measurement process. The other takes the role of the object system. 2) What is being discussed when one speaks of gathering data in the quantum context is an interaction or transaction between the agent and the object. 3) Without the agent, there would be no quantum "measurement" to speak of, but without the object, there would be no means for the agent to obtain the data, the spikes upon which he pivots his probabilities (his subjective degrees of belief). 4) There are no other agents in this story. 5) We do not go any further than points 1-5 warrant by giving the data obtained in a quantum measurement an autonomous existence—for instance, as something beyond the agent's sensations. That would run into inconsistencies in a "Wigner's friend" scenario. Nonetheless, quantum measurement outcomes are beyond the control of the agent—they are only born in the interaction—and thus are not functions of the agent in the way that his degrees of belief are. The degrees of belief find their source in the agent (the subject); the outcomes find their source in the external quantum system (the object). But the outcomes lead back to the agent in that they are personal to him.

This does not mean that the agent-object interaction and its fuller consequences are not autonomous events in spacetime, but it does mean that if there are any such things, quantum theory is not directly concerned with them. In CAF's view, quantum theory takes its whole definition as a normative theory for organizing an agent's *personal* probabilities for the *personal* consequences of his interactions with external physical systems. The structure of the agent-independent world that lies behind quantum mechanics is, in the end, still codified by the theory, but only in a higher-order, more sophisticated fashion than had been explored previously—it is through the normative rules, rather than quantum states and Hamiltonians.

Unfortunately a more detailed account like this will have to wait for another paper.

Referee: You write:

> The Copenhagen assumption that a preparation device can be given a complete classical description neglects that any such device is quantum mechanical and thus cannot be specified completely in terms of classical facts.

I said above that I wouldn't quibble about what the 'Copenhagen Interpretation' says, and I won't but I will argue that Bohr, at least, is not fairly described in this way. For one thing, Bohr does not deny that the physical instruments that play the role of measuring devices can be described quantum-mechanically. (Bohr says this in several places. I was quickly able to locate one (1949 Schilpp volume): "… of course, the existence of the quantum of action is ultimately responsible for the properties of the materials of which the measuring instruments are built and on which the functioning of the recording devices depends".) Indeed, it must be possible to do so, on Bohr's view, since they could themselves become the object of measurement. (The quotation just above even indicates that Bohr will agree that in order to analyze the functioning of the measuring device, one must use quantum theory.) What Bohr will insist on is: (1) that the only 'facts' about the physical world to which we ultimately have access are 'phenomena', by which he means "observations obtained under specified circumstances, including an account of the whole experimental arrangement" (also from the Schilpp volume); and (2) the 'specification of circumstances' must be given in terms of concepts from classical physics. Now, one could well dispute either of these claims, but if we accept them for the moment, then Bohr has an easy reply to the point that you are making here: for the purposes of describing the phenomena that a given measuring device is capable of eliciting, the device must be described classically, but if we are to investigate how or why it functions as it does, we describe it quantum-mechanically. (Of course, an *empirical* investigation of the device will itself involve observations of the device, and these observations will be made by means of yet other devices, which will be described classically, for the purposes of revealing phenomena that involve the original device.) Given Bohr's (highly debatable) assumptions, it seems to me that he has a reasonable response to the point that you make here.

Actually we take explicit issue with the point you make near the end of this paragraph:

> Bohr has an easy reply to the point that you are making here: for the purposes of describing the phenomena that a given measuring device is capable of eliciting, the device must be described classically, but if we are to investigate how or why it functions as it does, we describe it quantum-mechanically.

The logic goes like this. 1) Take any purported agent-independent, complete classical description of the preparation device. 2) That description can always be 'quantized' into a quantum mechanical one on a larger Hilbert space, i.e., one of the variety we discuss, where there is only unitary evolution. But then, 3) the latter description of the process clearly has a subjective element in it in the form of a prior quantum state for the device. 4) Since there is no operational distinction between the descriptions, either the initial quantum state in the latter description is in fact objective (contrary to our assumption), or the initial purported classical description has a subjective element in it that was unacknowledged previously.

We have tried to emphasize the logic of this argument by rewriting this section somewhat. We have also bolstered the point by quoting Bohr directly, where he says things similar to your summary above, and saying a little on how we differ.

> **Referee:** It is important not to give the impression that in 'the Copenhagen' view, there is *no room* for subjective probabilities.

That is probably true, and we hope that we have taken this into account by being more careful to say that our concern in this paper are "realist readings" of Copenhagen. Finally, to defuse the issue of what is or what is not "the Copenhagen interpretation" a little, we have replaced most instances of that phrase with "the objective-preparations view" which better identifies the issue at hand in any case.

> **Referee:** After all, a Copenhagen-ist can still agree that there is some measure of uncertainty about whether a device really prepares the state that we generally think it prepares. (Perhaps it is true for them that the *true* classical description of the device fixes the state, but they are not committed to the claim that we know what the true description is.)

But we would never say something like this. Remember, for us, a "preparation" is a synonym for an initial quantum state (pure or mixed). There is never a question about whether a device *really* prepares the state that we generally think it prepares—such a statement ontologizes states in a way that we're trying to get away from.

> **Referee:** I'm more concerned about the presumption that there must be facts that guarantee the outcome in the first place, *even* for the Copenhagen view. Indeed, in at least one common form, the Copenhagen view is more or less an anti-realist, more specifically instrumentalist, view. Now, anti-realists will exactly *deny* that there must be 'facts' that underwrite the accuracy of the predictions made by theory. Copenhagen-ists of this stripe will not have any problem with your conclusion that "we must abandon explanations in terms of pre-existing properties" – they couldn't agree more! See, for

example, van Fraassen's and Fine's essays in the volume "Philosophical Consequences of Quantum Theory" (ed. Cushing and McMullin). Both of them argue explicitly for the view that even predictions with certainty do not have to be 'underwritten' by facts about the world, of whatever sort. As van Fraassen puts it, succinctly, "There does not have to be a reason for everything."

This is a good point. We have tried to take into account your concern by defining a little better what we are calling the "Copenhagen-like interpretations."

On the other hand, we wonder whether you have van Fraassen right, at least in this particular context. We say this because one of us—CAF—had the following email exchange with him on 14 November 2005.

> **CAF:** [With regard to van Fraassen's sentence]
>
>> Writers on the subject have emphasized that the main form of measurement in quantum mechanics has as result the value of the observable at the end of the measurement – and that this observable may not even have had a definite value, let alone the same one, before.
>
> your phrase "MAY NOT even have a definite value" floated to my attention. I guess this floated to my attention because I had recently read the following in one of the Brukner/Zeilinger papers,
>
>> Only in the exceptional case of the qubit in an eigenstate of the measurement apparatus the bit value observed reveals a property already carried by the qubit. Yet in general the value obtained by the measurement has an element of irreducible randomness and therefore cannot be assumed to reveal the bit value or even a hidden property of the system existing before the measurement is performed.
>
> I wondered if your "may not" referred to effectively the same thing as their disclaimer at the beginning of this quote. Maybe it doesn't. Anyway, the Brukner/Zeilinger disclaimer is a point that Caves, Schack, and I now definitely reject: From our view all measurements are generative of a NON-preexisting property regardless of the quantum state. I.e., measurements never reveal "a property already carried by the qubit." For this, of course, we have to adopt a Richard Jeffrey-like analysis of the notion of "certainty"—i.e., that it too, like any probability assignment, is a state of mind—or one along (my reading of) Wittgenstein's—i.e., that "certainty is a tone of voice"—to make it all make sense, but so be it.
>
> To which he responded:
>
> **van Fraassenism 11:** *Suppose that an observer assigns eigenstate $|a\rangle$ of A to a system on the basis of a measurement, then predicts with certainty that an immediate further measurement of A will yield value a, and then makes that second measurement and finds a. Don't you even want to say that the second measurement just showed to this observer, as was expected, the value that A already had?*

Thus at least on a surface reading of this, it seems we are saying something in this paper on quantum certainty that even takes van Fraassen by surprise.

> **Referee:** There is of course nothing wrong with the proof in section V, but at this point in the history of the philosophy of quantum theory, I confess that I don't see why it is there. Isn't a quick reference to the 10,000 other proofs of nonlocality sufficient? If not, can you say what is special or new about this one? I would personally rather see that space used for a more subtle discussion of the newer issues, perhaps addressing some of the points that I've made above.

There is a folklore in the physics community that a paper without equations will not get read. We wanted physicists to read our paper too. More seriously, though, the context of this use of KS is quite different from previous uses of it. You yourself call it a "proof of nonlocality." That's not what it is to us: It is a proof that, under the assumption of locality, probability-1 predictions in quantum mechanics correspond to subjective certainty rather than objective certainty.

07-12-06 *Getting It Right* (to S. J. van Enk)

Attached is a draft of a revised version of my "certainty" paper with Caves and Schack. I remember you thinking I didn't capture your point about the Tsirelson bound correctly at the end of it. Could you give me a recommendation for a better more accurate statement?

Steven's Reply

> What I meant was this: a Bayesian is not surprised as much as a realist is by violations of the Bell inequality because he considers the correlations are (at least partly) in his head, not really in the quantum systems (oversimplifying it a bit). That's good, you solved part of the mystery. But what I say then is why are the correlations in your head still restricted by Tsirelson's inequalities? That should be much harder to explain.

07-12-06 *Catching Up on a Southbound Train* (to D. M. Appleby)

I'm sorry I've been silent. Things have just been a little overwhelming, and since I didn't have much to tell you, I didn't write. At the moment, I'm on a train making my way to Washington, DC. I've got to work for the APS tomorrow to get the quantum information topical group sessions organized. (At last count, there were 214 abstracts.)

The talk in Tskuba went fine. I checked the essential part of your minimum-uncertainty state derivation, and I found no mistakes. So, it was part of the overall presentation. [...]

Going back to science a little, the discussions last week with Bennett were the most fruitful I've ever had with him (foundation-wise, that is). Lately he's been worried about information erasure again, and what quantum mechanics adds to the idea. Taking a strong many-worlds kind of stance as his starting point, he asks, if "facts" are just correlations between branches, what happens when those correlations move away from the original systems to which they were concerned? Are the facts as "real" as they used to seem to be? Well, of course this gets him into all kinds of trouble—the sort of trouble he's not used to since he normally shuns all discussions of foundations. So it was an absolute surprise to me one morning when he calls down to say that our discussions had caused him to think a lot, and, quote, "... it almost pains me to say this ... but I can see why you might use the language of calling a quantum state a state of knowledge." OK, he's coming up a little toward the Chris of about 10 years ago (not the radical Bayesian), but given what I know of him, it was almost a miracle to hear this! It was an essentially infinite step really! Here's what I wrote Kiki that night:

> But over all, it was a good day. Probably one of my proudest moments ever: Charlie took me aside early this morning and said that because of our discussions yesterday, he was starting to adopt a view of quantum mechanics that comes closer to mine—he wouldn't go so far as me, mind you, or at least he said so—but it was "closer." [. . .] I was proud and humbled at the same time.

Now, to nurture this seed!

11-12-06 *Chris's House from Space* (to J. Racette)

Thanks for the holiday letter. I enjoyed it, and it was great to learn where you guys are at in your lives now. Please give Kim a big Merry Christmas from Kiki and me. (Include yourself too.)

Of course, you were always more literary than me: In all these years I've never put together a holiday letter. Maybe the closest I can come at the moment, is to forward you a letter (with the associated photos attached) that I had written to an Irish friend recently. [See 01-12-06 note "Chris's House from Space" to K. R. Duffy.] (It was spurred by him telling another colleague in a pub that the orange of my house could be seen from space.)

Our two kids are growing too. Emma is setting herself to conquer the world. She reads like a scanner, and works out like a gymnast. And Katie's coming along quite nicely too, though her enjoyments are pulling weeds with the old man and cooking. Kiki is constructing and reconstructing the house constantly—partially restoring it to 1896 and partially just Kikifying it. Me, I'm just trying to understand quantum mechanics as usual, and traveling the world while I do it. This year I went to Portugal, Sweden, Italy, Canada and Japan. (Last year was busier; I went to Australia, Poland, Austria, Sweden, Ireland, Canada, and Germany (twice).)

Sadly I don't have satellite TV yet (still with trudgy old cable): Too bad I'm not in central New Mexico!

11-12-06 *Quantum Brunch* (to P. Grangier)

Grangierisme 21: *Following our "quantum breakfast" in Tsukuba, here is the talk which includes the arguments with Gleason and Wigner theorems, given at ENS in Paris in November 2004 (see* `http://www.spectro.jussieu.fr/Seminaires/Resumes_2004/Resu041116.html`*).*

I have various improvements as drafts in my files, but I did not find time to write them down.

I enjoyed the quantum breakfast indeed. Thanks for sending your presentation. I shall study it, and maybe we can get together for a quantum brunch in February.

I'm just back from the APS sorters meeting, preparing for the March meeting. That was a lot of hard work. But it was also a lot of hard work in the many fights Carlton Caves and I had about our own interpretive stuff. I wonder why I get involved in these things sometimes.

As I say, I will study your presentation. It would be nice if it turned out that there is more common ground between us than was initially apparent.

12-12-06 *More Stable Version of Manuscript on Operational Axiomatization of QM* (to G. M. D'Ariano)

Thanks for the note. I too much enjoyed our discussion. I'm sorry about the notes; when I saw you at breakfast, I was in such a hurry everything spilt out of my mind. Anyway, I am working on the paper with Appleby now, and hope to get it posted early next week.

Thanks for the coordinates of your latest posting. You are right that there is probably much latitude for joining our ideas. I admire your axiomatic system—of all that I have seen, it is the one that smells the most right for leading to a deeper understanding of the essence of quantum phenomena. On the other hand, my Bureau-of-Standards measurement idea is only a small piece of the puzzle, and I certainly need to wed it to a larger structure.

I will inform you of the coordinates of the thing with Appleby just as soon as I can.

12-12-06 *Small Changes* (to R. Schack)

Since we haven't heard from you, I went ahead and made some small changes to Carl's last draft. I'm guessing that they won't interfere with anything that you may be doing to it.

1) I modified the last paragraph to better capture van Enk's point (he had remarked that I didn't quite get the point across that he thought was important). On top of that, with this modification, the flow of the logic is a better than it was before anyway.

2) I wimped out on a longer exposition of the Principal Principle troubles again, which I was going to put in a footnote. So, I just deleted the flagged footnote.

I'd probably like to do a few more *small* (literally) changes, but I'll go ahead and send you this much in case you do happen to be working on the draft.

The biggest issue probably—the one that will require the most work—is about de Finetti's previous statements on certainty. As it stands, we say this:

> The main issue here is whether there is any difficulty with the idea of an utterly certain belief about an admittedly contingent fact. This issue arises in the classical setting, where it has been dealt with masterfully in Bruno de Finetti's "Probabilismo" (Sect. 18-19). Nothing of de Finetti's argument loses force when we come to quantum mechanics; indeed, the quantum denial of instruction sets seems to give it yet more force.

I don't think that quite frames it right. (Plus—but this is slightly an aside—Bruno de Finetti's world was neither 'classical' nor 'quantum' in the usual physics sense.) I think the real issue is closer to the one that Timpson brings up: The agent is certain of the outcome, even though he is also certain that the system *cannot* be (objectively) 'certain'. That is the apparent conundrum. I'm not now sure that de Finetti considered such a stronger situation. Or is my memory/understanding now failing? (You can read what I had written previously on the issue—when I thought de Finetti had indeed done the job—at [now defunct pages].) So the question is what to write. Do you want to tackle this one?

My present feeling is that the solution—or at least one solution—is implicit in our ontology (though I'm not all that happy with what I had written Timpson previously). All that's being recognized with a Bell-KS argument is that quantum outcomes cannot be a function of the world external to the agent alone. They come about, in part, by the participation of the agent himself.

On the previous point of contention—this phrase "Bayesian updating is, as it should be, consistent with logical deduction of facts from other facts"—I'm now willing to leave Carl's draft alone with regard to it. But I don't think it is nearly as meaningful as you two think it is. In fact, I still think our disagreement is more than linguistic. I'll explain in more detail in a later note.

13-12-06 *Seeking an Objective Substructure, Yes; Theory of Fundamental Events, Maybe No* (to J. Barrett)

<u>NOTE</u>: This letter was apparently never finished or sent off. I suspect it was composed roughly on the date listed here.

I've finally gotten back to reading your note again and thinking about it for a day. I'm sorry for the delay. I have enjoyed your questions and your thoughts. Let me reply to a few of them, though I don't think I have as much to say as I had originally thought I would.

First let's go back to the issue of "fundamental events," since I've already referred you to some of my ruminations on the subject. You wrote this in your note:

Barrettism 2 : *at present we have only a provisional, half-baked theory in which these events are "outcomes of interventions". But a fundamental formulation of a complete theory must not use such vague terminology, and will eventually supply an ontology of primitive events that agents can in principle bet on.*

And let me recall the thing I wrote for the referee of our "certainty" paper:

> [Q]uantum measurement outcomes are beyond the control of the agent—they are only born in the interaction—and thus are not functions of the agent in the way that his degrees of belief are. The degrees of belief find their source in the agent (the subject); the outcomes find their source in the external quantum system (the object). But the outcomes lead back to the agent in that they are personal to him.
>
> This does not mean that the agent-object interaction and its fuller consequences are not autonomous events in spacetime, but it does mean that if there are any such things, quantum theory is not directly concerned with them. In CAF's view, quantum theory takes its whole definition as a normative theory for organizing an agent's *personal* probabilities for the *personal* consequences of his interactions with external physical systems. The structure of the agent-independent world that lies behind quantum mechanics is, in the end, still codified by the theory, but only in a higher-order, more sophisticated fashion than had been explored previously—it is through the normative rules, rather than quantum states and Hamiltonians.

Also a piece of the Preamble section of "Delirium Quantum"

> I think the greatest lesson quantum theory holds for us is that when two pieces of the world come together, they give birth. [Bring two fists together and then open them to imply an explosion.] They give birth to FACTS in a way not so unlike the romantic notion of parenthood: that a child is more than the sum total of her parents, an entity unto herself with untold potential for reshaping the world. Add a new piece to a puzzle—not to its beginning or end or edges, but somewhere deep in its middle—and all the extant pieces must be rejiggled or recut to make a new, but different, whole. That is the great lesson.
>
> But quantum mechanics is only a glimpse into this profound feature of nature; it is only a part of the story. For its focus is exclusively upon a very special case of this phenomenon: The case where one piece of the world is a highly-developed decision-making agent—an experimentalist—and the other piece is some fraction of the world that captures his attention or interest.

So, clearly, I have sympathy for *something* that goes beyond quantum theory. But what is that something? The idea behind the Preamble passage has its origin in a kind of Copernican principle.

13-12-06 *Offline Discussion* (to R. Schack)

Here are the passages you and Carl wrote:

Schackcosm 95: *I am, however, totally puzzled by your problem with what you call the "exception". I have said over and over again that from our perspective this is a trivial issue. I think you have some kind of mental block here. All probabilities are degrees of belief. All probabilities reside in the agent, not in the world. But the agent has to be coherent. The agent's probabilities generally depend on facts (these are facts for the agent) and his prior. Given a prior and some facts, the agent is forced by coherence to a particular probability assignment. There are trivial cases, however, where the prior doesn't come in. If $x = 5$ is a fact for the agent, then the agent is forced to the probability assignment $P(x = 5) = 1$ by coherence alone. There is nothing deep here at all.*

and

Cavesism 82: *The point of the exception is to make clear that we are not saying that probability theory is inconsistent with logic. It is consistent with logic. Now in logic, you could say that some facts imply other facts. When thinking in terms of probabilities, you would say the same thing as some unit probabilities imply other unit probabilities via Bayes's rule. Now you can always maintain a distinction—and we would want to maintain one—between the facts themselves and certainty about facts, and I personally would be happy to change things so that we're saying the right thing.*

The main point is that sometimes in the case of certainties, facts do imply some other unit probabilities by logic, and there is no escaping that. For example, suppose we observe that one thing has the value two and another thing has the value three. These are now facts (for the agents), but the agent cannot escape assigning unit probability to the sum of the two having value five. That's the only thing I want to guard against.

And here's what I had written in the now deleted footnote:

> The problem, *as [Fuchs] sees it*, with the formulation that there is a case whereupon a fact strictly determines a probability is that it puts the horse before the cart: It acts as if abstract 'logical implication'—a statement of the form "$d \Rightarrow h_0$"—has a meaning independent of the *subjective* judgment "$\Pr(d|h) = 0$ for $h \neq h_0$." (See for instance, F. C. S. Schiller, *Formal Logic: A Scientific and Social Problem*, (Macmillan, London, 1912), and F. C. S. Schiller, *Logic for Use: An Introduction to the Voluntarist Theory of Knowledge*, (G. Bell, London, 1929), for a development of this point.)

Let me try to say the same thing, but in a little more detail.

In my weakest criticism of this exception business, I would point out that you two seem to be drawing no distinction between analytic and synthetic truths (facts). It seems you're wanting to apply a blanket rule to both, and that's partially what's getting under my skin:

> We may … adopt the unsophisticated definition that a synthetic statement has meaning only in terms of matter of fact, whereas an analytic statement has meaning independently of matter of fact (roughly, the Leibnizian distinction between *vérités de fait* and *vérités de raison*). An analytic statement, as has been charged, may still be considered as relative to the language or symbolism in which it is stated … However, these considerations should not affect the essence of the distinction, which is that, in theory, an analytic statement, since it is concerned only with the relations of meanings, cannot

be verified or falsified by any conceivable human experience, whereas a synthetic statement, regardless of its "certainty," may always, in theory, be upset by experience. A typical analytic statement is, "two plus two equals four." If the meaning of the words "two," "plus," "equals," and "four" is made clear, it is impossible for this statement not to be valid. Synthetic statements are the material of science, and their degree of certainty may approach any assignable limit, with the qualification that they are always subject to verification in experience, i.e., may conceivably be falsified.

Propositions corresponding to quantum measurement outcomes are not analytic statements (nor are classical measurement outcomes for that matter). And so, so-called "logical implication" between any two such statements can be nothing other than a subjective judgment of certainty, $\Pr(d|h) = 1$. Logical implication in this kind of situation has no independent definition.

As previously written in the paper, Carl's choice of words seemed to indicate a contradiction to this point. (Actually, it went further than that—and may still in Carl's mind—as evidenced by all the earlier attempts of Carl to draw a distinction between classical and quantum based on such ideas.)

My heels dug in when I looked at the passage on Bayes' rule as it had been written:

Classically, Bayes's rule,

$$\Pr(h|d) = \frac{\Pr(d|h)\Pr(h)}{\Pr(d)}\,, \tag{68}$$

is used to update probabilities for hypotheses h after acquiring facts in the form of data d. The posterior probability, $\Pr(h|d)$, depends on the observed data d and on prior beliefs through the prior probabilities $\Pr(h)$ and the conditional probabilities $\Pr(d|h)$. The only exception to the dependence on prior probabilities occurs when the observed data d logically imply a particular hypothesis h_0, i.e., when $\Pr(d|h) = 0$ for $h \neq h_0$, thus making $\Pr(h_0|d) = 1$.

and saw nothing but subjective judgments in every slot on the right-hand side, yet talk of *exceptions*. To me, it sent the wrong message about our whole enterprise. Once one exception, why not two? Particularly if some subjective judgments are forced by facts (without prior judgment), why not others? Why not, in fact, allow that some nontrivial probabilities—quantum probabilities—can even be forced by facts (without prior judgment)?

Since the real subject matter of our paper was for h and d that are measurement outcomes, I saw no reason to sew a seed of doubt.

But—you might say—"That was just a little trouble in the way Carl wrote up the point. What we really meant to convey is that $\Pr(h|h) = 1$, and that holds true without subjective judgment even for synthetic statements. It's just coherence."

And I say, "NO, even that is wrong—or at least irrelevant—as long as h is synthetic."

It is Aristotle's logic which is usually taken as the archetype of formal logic. However, the 'logical analysis of judgment' in anyone's hands is bound to come to failure if it discounts the personal intention and circumstances of the maker of the judgment. Thus Hegel's formula of judgment as identity in difference disregards the fact that identities are always made or postulated by us through conscious disregard of those differences which we take for the moment to be irrelevant to the purpose of judgment.

If the right-hand entry and the left-hand entry of $\Pr(h|h)$ are meant to be two distinct empirical instances of "h" (the quotes important), then it is a subjective judgment. If the right-hand entry is synthetic, while the left-hand is treated as (now) analytic, I don't know what the very expression $\Pr(h|h)$ means.

Take your particular point:

Schackcosm 96: *If $x = 5$ is a fact for the agent, then the agent is forced to the probability assignment $P(x = 5) = 1$ by coherence alone.*

Why would the agent write down $P(x = 5) = 1$ unless he were contemplating gambling on whether $x = 5$ (and is quite certain of the outcome)? The only sense I can make of that situation is that what is really happening is that the agent is contemplating gambling on a NEW instance of $x = 5$, and because of the previous observation he is certain he will find it again. But that is a subjective judgment. If you take away my right to think of it as a new instance, then I will say he cannot be gambling on it at all—the statement $x = 5$ is a dead fact; it has passed away. And what you must mean by $P(x = 5) = 1$ is something other than a gambling commitment.

The same can be said for Carl's example above (though he does slip in a little "2+2=4" issue on the side, which as far as I can see is irrelevant for the observation he really wants to make ... which is the same as the one you made, or at least he told me so).

So, now we get to the point. My present opinion is that probability statements are really only ever meant to be made about "live options" (to use a phrase of William James, though I may not be using it exactly the same way as he did). One can indeed write $P(x = 5) = 1$, but then $x = 5$ had better a live option, rather than a "fact for the agent." If it is a fact for the agent, then we are reinstating the category error. The only role for the "facts for the agent"—despite their very real consequences, like 'life' or 'death'—are in pivoting the agent's subjective probabilities. They are never used for the setting of probabilities unconditionally.

Could I make the criticism stronger? Yeah, I think so, if I were to argue along the lines of Schiller that there simply are no such things as analytic judgments anyway. But that's probably further than I need to go.

Do you see my concern any more clearly now?

Rüdiger's Reply

> Let me try to tell you what I understand about facts and certainty. You say that once A is a fact for the agent, the agent can't be certain of it in the sense of assigning probability 1 to it. When something is a fact for the agent, it doesn't normally make sense for him to ask "Am I certain of the fact?". This is one of Wittgenstein's points. And I fully appreciate it. In the quantum context, this has to do with the status of "clicks", which are directly given to the agent. Facts are facts for the agent and don't have an independent existence out there. This is why the agent should not assign probabilities, not even 0 or 1, to facts.
>
> But where does leave the analytic/synthetic distinction? Isn't that a pre-Wittgensteinian distinction? I think the analytic/synthetic distinction moves the whole discussion in the wrong direction.

13-12-06 *More on Identity* (to R. Schack)

Let me supplement what I just sent you with a little email conversation I had with Charlie Bennett on the very point of 'identity'. [See 28-11-06 note "Funes + Borges" to C. H. Bennett.] The messages below are in reverse time order. What Charlie is referring to is Matt Leifer's paper, `quant-ph/0611233`; and CFS refers to you, Carl, and me.

There's a funny anecdote that goes with it too. One evening while walking back from dinner in Tsukuba, Charlie said to Daniel Gottesman while pointing up to a traffic light, "For instance, Chris wants to say that that traffic light right now is not the same the traffic light as ..." And, by wonderful fate, just as Charlie gestured up to it, the light changed color! We all had a very good laugh, and it's clearly already become one of Charlie's palette of stories: I heard it at least three times over again at the meeting.

13-12-06 *Analytic/Synthetic* (to R. Schack)

Schackcosm 97: *But where does leave the analytic/synthetic distinction? Isn't that a pre-Wittgensteinian distinction? I think the analytic/synthetic distinction moves the whole discussion in the wrong direction.*

And a pre-Jamesian and pre-Schillerian distinction too. Yes, it does move the discussion in the wrong direction. But I left my exposition couched in terms of it because I thought it might strike a chord more quickly in you that way. (It was a first assault.) That's why I said, "In my weakest criticism of this exception business, I would point out ..." Later I hinted at the corrective with this:

> Could I make the criticism stronger? Yeah, I think so, if I were to argue along the lines of Schiller that there simply are no such things as analytic judgments anyway. But that's probably further than I need to go.

Schiller, too, rejects the distinction.

Blending that point into the mix, the simplest solution to the dilemma caused by the "exception" was to have never imagined it to exist in the first place. No mention of analytic/synthetic, but no mention of exceptions either. (We had tried that solution once in an earlier draft, but it only made Prof. Caves that much more bitter.)

14-12-06 *What a Friend!* (to J. A. Smolin)

I just sent these words off to Mr. Khrennikov for part of the Preface to his latest volume. Now, read that and tell me I'm not a good friend! At some stage relatively soon, we've got to get back to working that out. ... At the moment though SIC-existence is more important.

> **Swedish Bayesian Team.** Eight of the meeting's participants came to Växjö as a task group to further develop an understanding of quantum probabilities as Bayesian degrees of belief and to examine various technical issues surrounding the idea. These were D. Marcus Appleby, Hans C. von Baeyer, Ariel Caticha, G. Mauro D'Ariano, C. A. Fuchs, Matthew S. Leifer, Rüdiger Schack, and John A. Smolin. Quantum Bayesianism is a view of quantum mechanics that has been making much progress and is becoming widely recognized; it was thus good to be able to come back to Växjö, which played a pivotal role in its development already with the "Shannon meets Bohr" session of the 2001 meeting. As concerns the talks, D. M. Appleby gave a nice introduction to the Bayesian conception of quantum-state tomography and explored the extent to which complete sets of mutually unbiased bases (so-called MUBs) and symmetric informationally complete measurements (so-called SICs) are optimal measurements for tomography. A. Caticha gave a potential derivation of the quantum formalism along lines inspired

by R. T. Cox's derivation of the axioms of probability theory. G. M. D'Ariano presented some very fresh work on an axiomatic system for quantum mechanics inspired by the practice of quantum tomography and many of the work-a-day features of quantum information theory. C. A. Fuchs gave a presentation aiming to clarify some the ontological aspects of the quantum Bayesian position, arguing that, though quantum states are subjective from this conception, the Born rule for transforming probabilities from one measurement context to another represents an objective feature of the world. M. S. Leifer presented a new and very useful isomorphism between bipartite quantum states and quantum operations and discussed its implications: Particularly, he argued that from a Bayesian conception, to be consistent, quantum operations should be interpreted as of the same subjective character as quantum states. R. Schack discussed the issue of what it could possibly mean for a quantum random number generator to be better than a classical one and gave a rather deep answer: When an observer writes down a pure state for a quantum system, he is not only expressing his certainty for the outcome of some measurement, but he is also expressing his certainty that no one else can have what he would deem as "inside information." Finally J. A. Smolin posed what was perhaps the deepest question of the conference (for the Bayesians at least!): Forget about better and better Bell inequality violations; to what extent has the basic Born rule for calculating probabilities ever been independently tested, and indeed, can it be experimentally tested?

We might add that three other participants of the meeting count as *honorary* members of the Swedish Bayesian Team (whether they would like such an association or not!). First, there is Arkady Plotnitsky, whose riveting talk on Bohr's reply to EPR, helped make clear the single-case conception of all quantum phenomena (thus seemingly requiring a Bayesian notion of probability). Then there is Ingemar Bengtsson, who discussed the still-open (for >30 years!) problem of the existence or nonexistence of MUBs for non-prime-power dimensional Hilbert spaces. This kind of work and related constructions are likely to be crucial for better Bayesian reconstructions of the quantum formalism. And finally Jan-Åke Larsson, for his role as the conscience for Bell-violation experimentalists; we fear that one day he will be the conscience of the quantum Bayesians!

15-12-06 *Background Independent Bayesians* (to L. Smolin)

I know he's already in your neighborhood, and you probably know him better than I do, but I just thought I'd tell you that you guys have an excellent postdoc in Matt Leifer. I've been editing the volume for the Växjö proceedings and discovered I really like Matt's submission: `quant-ph/0611233`. (Well, of course I would like it—and you'll easily catch why if you read the Introduction.) But because of the closing remarks in the paper it struck me that you might have some interest in the paper too. I think it is a good point, and if this quantum Bayesian—i.e., me—were to try to dabble in quantum gravity, it might be the first observation I'd try to get some traction from.

Also, in spite of the motivation for the formalism (i.e., if you can't stomach those Bayesians), you might find the formalism independently useful.

15-12-06 *Swedish Delirium* (to D. Gottesman)

I don't know if you could see it in my face, but I was taken quite aback when you said at the Tsukuba banquet (something like), "The trouble with *all* the interpretations is that, by hook or crook, they always try to describe quantum mechanics in some classical way." Well ... ahem ... of course I thought that about every *other* interpretation, but I had never imagined such a criticism being leveled at me!

Anyway, I thought of you again last night as I was hurriedly annotating the attached "pseudo-paper" for the wacky Swedish proceedings. And I wondered whether after reading it you'd still drop me in the same pot with all the rest. (Maybe I just want to think I'm "more anti-classical" than the rest so that some one will call me MAC-daddy some day.) But seriously ... You probably will (drop me in the same pot, that is). But I'd like to better understand why, particularly in light of the peculiarities of the attached document.

On a different subject, might I ask you to send me a copy of your Tsukuba presentation? I gave your paper a perusal, but didn't find your nice little example in it with the transition matrices written explicitly. I think I'll need that kind of gentle leading to absorb your point.

18-12-06 *I and It* (to N. D. Mermin & C. G. Timpson)

I'm going to cc this note to Chris Timpson, since it is directly relevant to him. (I.e., I will treat it as out of the context of my other notes to you today.)

Merminition 158: *Footnote 25. I can't imagine how a successful gambler can be certain that red will turn up next without also feeling that it is certain that red will turn up next. So this footnote just confuses me.*

I think your confusion here comes from not parsing the sentence the right way. (We probably need to add something more to the text to make sure no one else has the same trouble as you.)

It comes from some things that Chris Timpson brought to our attention. Let me paste in Timpson's explanation.

Or maybe that's not the trouble at all—as I keep staring and staring at your sentence, I start to wonder. Maybe it is actually the same trouble that Timpson expressed below?

Here's a far-fetched example of how it could possibly be. Schrödinger knew—nay, was certain—that with a fresh lover he would be able to make a great scientific discovery. The lover is the catalyst for the discovery. No lover, no discovery. Yet, ask the lover, and she will have no clue about what her presence does for his science. He is certain, but she is not.

From our view—let me be careful, from my view—a quantum system is a catalyst. It brings about a transformation in the agent who interacts with it. Sometimes the agent is certain (in the subjective sense) about the transformation that will be the result; sometimes he is not. But even in those times in which he is certain, there is nothing in the system itself that is informative of his certainty.

Buy it?

18-12-06 *Bennett's Hoffa Talk and Certainty* (to R. E. Slusher)

By the way, here's a copy of Bennett's painful (to him) Tsukuba talk.

Also attached is a much better version of our certainty paper than you had seen before. [C. M. Caves, C. A. Fuchs, and R. Schack, "Subjective Probability and Quantum Certainty," Stud. Hist. Phil. Mod. Phys. **38**, 255–274 (2007).] It's what gave rise to the Schrödinger story below.

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20-12-06 *All Unambiguous Account* (to C. M. Caves)

Cavesism 83: *One thing I discovered as I was doing this. Shouldn't that quote from Bohr to Pauli start with "In all unambiguous accounts" instead of "account"?*

Nope, that is just as Bohr said it. I was very careful in entering those words into my computer, which come from a fascinating set of letters between Bohr and Pauli (written in English!) that Henry Folse supplied me with.

Bohr's letters can be found on pages 14–16 of the attached document. (And a quote of Folse quoting the phrase "in all unambiguous account" too can be found on page 39.) Pauli's more interesting (by my estimate) replies can be found on pages 132–135.

We could add an "[sic]" to the Bohr quote.

20-12-06 *New Jersey Atoms* (to D. Gottesman)

Thanks for sending me your talk.

Gottesmanism 2: *When you talk about an "event", that is a classical concept. Less obviously, "information", "knowledge", "belief" are also all classical concepts. Of course, the actual classical concepts don't quite fit the quantum objects you'd like to assign them to, so you have to start to modify them, to give them more quantum properties.*

I pretty much expected you'd say that, at least with "event"—you said about as much in Tsukuba (though I think you used the word "data"). But I guess I was hoping you'd expand on what you mean by all this. For instance, I don't know what you mean when you say "belief is a classical concept." Classical in the sense of classical physics? For myself, I would think terms like "belief" have essentially nothing to do with any particular conception of the world. For instance, one could speak of beliefs (and even rudimentary probability theory) at a time when the world was thought to be animated with little spirits or when rocks were thought to fall according to Aristotelian rules. Or in Hinduism where the world is but the dream of four brothers dreaming Vishnu dreaming the world containing four brothers dreaming Vishnu dreaming . . .

If I see you in early February, maybe I can get you to tighten up what you mean.

Gottesmanism 3: *This, I think, is not at its root different from the way most interpretations work: They take some classical concept and try to modify it to allow it to describe quantum mechanics. I don't know that there's anything* wrong *with that, but I also don't know why one would expect there to be a uniquely "best" way of doing it.*

True enough. I understand your point on this. But I do think the quantum formalism is trying to tell us something unique about the world, and despite our using the formalism every day, we don't yet have a good grasp of what "it" is trying to get at. Whatever it takes to move forward on that project is, in the end, the important thing. (And so: I'm ready to be Darwin'd out if it so happens, but at the moment the Bayesian way strikes me as the best way to proceed.)

Gottesmanism 4: *While there are certainly many ways in which atoms can be distinct, they also can, in fact, be exactly the same. The very notion of "separateness" of atoms breaks down in a BEC.*

There's something in the way you pose this that makes me think you don't quite get what I was shooting for in that rumination. (And it's no wonder, because I don't like the way I wrote it now either.) From my *toyed-with* conception, atoms (and events) are not something larger bodies (and histories) are built up from. But rather atoms are residues of in-common judgments about the bodies. So, even from this point of view atoms can be exactly the same (as you would have them). But now the statement almost turns into a tautology, rather than being empirical. That is to say, if one has adopted an epistemic view of quantum states as a working hypothesis (as Caves, Schack, and I have), a symmetrized or anti-symmetrized quantum state is still an epistemic state, rather than a state of the world.

In connection with that point, I'll attach the latest Caves-Fuchs-Schackism in case there's an evening when you find you can't fall asleep. (This version is significantly different from the one we put on the web earlier under duress.)

20-12-06 *Paper* (to R. Schack)

Schackcosm 98: *Not that I disagreed with what it says, but I thought it would disturb some readers unnecessarily. So I have some sympathy with the idea of dropping it.*

Disturb some reader unnecessarily in what sense?

The whole point of our paper is that we say that quantum mechanics leads us to think that there can (validly) be subjective certainty without an underlying objective certainty. What is wrong with citing[117] someone who backs up a similar idea—or at least an idea that moves in a similar direction—and, in a clearer way than we do, in another context?

[117] The citation in question was this one: More philosophical, quantum independent, precedents for this notion of 'certainty' can be found in A. White, "Certainty," Proc. Aristotelian Soc. **46**, 1 (1972). For example, White puts it this way,

> The certainty of persons and the certainty of things are logically independent of each other. Somebody can be (or feel) certain of something which is not itself certain, while something can be certain without anybody's being (or feeling) certain of it. 'He is certain that p' neither implies nor is implied by the impersonal 'It is certain that p'. The same thing cannot be both certain and not certain, though one person can be certain of it and another of its opposite ... People can become more or less certain of something which itself has not become any more or less certain. ... A gambler need not feel that *it* [our italics] is certain that red will turn up next in order to feel certain that it will.

See also, L. Wittgenstein, *On Certainty*, (Blackwell, Malden, MA, 1975).

Timpson may be wrong that this is relevant, but he is not dismissive and is thinking hard about the issue. We need people like him, and if it helps to put our thoughts a little into the context of his world (and people's like his), I don't see the harm. Saying the truth—as we understand it at any moment—is the only thing that's ever going to make us free (almost a JC quote).

Carl says this paper is "written for physicists." What physicist is going to look at a journal like SHPMP? The only people who will look are the ones who really want some clarity, or have a philosophical bone to pick. Either way, it seems to me, we ought to be providing them with as much material as we can, when we can.

It strikes me that you guys always work from a starting point of the "the less said the better". But I think we're tearing a huge whole in these guys' (our reader's) worldview and if we don't put something in its place—some kind of metaphysics, some kind of world picture—we're just asking for trouble after trouble upon ourselves. Wisemans calling us solipsists; Bubs calling us instrumentalists; Stamps calling us behaviorists; Pereses calling us positivists. Everyone thinking we're making the world smaller, rather than bigger. It is true that we may never get rid of all those biting, deaf dogs, but if we don't try, we've really only got ourselves to blame.

That said, I am willing to remove the quote of White (rather than adding a little more to it to help set the context) and leave only a citation of he and Wittgenstein if you really wish. But I'd hope you'd do it out of a reason deeper than fear. (Fear, that is, that some jerk out there who says, "This is not physics; I know what physics is. This is philosophy with no impact on physics, and I am therefore warranted to close my mind." It is that kind of arrogance that keeps such people from making the really big discoveries, and I want a community around us that will make big, really big, discoveries, even if we never do.)

20-12-06 *I and It, 1* (to R. Schack)

Mermin's replies on I and It. The first was to my note (about Schrödinger and his lovers). The second was to Timpson's long linguistic analysis on the Moore-type sentences. (Which—I disagree with you—are relevant if one thinks of quantum measurement in the common "question-answer" motif, *rather than* in the alchemical motif I keep trying to refine. In fact, the questions he raises give extra impetus, give extra reason for the alchemical/Paulian motif. For that view, I think, sidesteps the issue he fears. Timpson's distrust, I believe, has its source in what I railed about earlier: Our tearing too much out of his worldview without replacing it with a new image.)

23-12-06 *Real Data!* (to G. L. Comer)

Comerism 14: *If you take a look at* `astro-ph/0605007` *you'll find a paper of Nils, Reinhard Prix, and me referenced. This is quite unique for me, because our model for neutron star glitches is being put to the test via real data analysis. And ... we're still in the hunt! It turns out that glitch data and our model share a certain linearity.*

That's exciting! What a way to go into the Christmas season.

I've never had anything comparable in my life—to the extent I have made any positive predictions at all, they've always been of use for engineering, never explaining any naturally occurring phenomenon. I can tell you one funny story in that regard that happened recently. A couple of weeks ago in Japan I was walking through the poster session at QCMC, and ran across two guys from MIT with a very professional looking poster titled, "Experimental Realization of the Fuchs-Peres-Brandt Probe." I thought, "what's that?" and stopped to talk. Well, they tell me all about my original paper with Asher—or so I thought that's what they were telling me at first—and about how they used the calculations there to design a real-world optimal eavesdropping setup. I ask them how much it cost to perform the experiment and that kind of stuff—about $10K in parts plus a very, very expensive laser that was already in the lab, etc., they tell me. Then, at some point, it all starts to sink in: Whatever they've done, they've used it for eavesdropping on BB84. Whereas my paper with Asher had solely to do with B92! So, I explain this, and they say, "No, there's a part on BB84." I say, "No, no, there's nothing on BB84 in it." They say, "Yes there is, Professor Shapiro told us so!" Well, like a big Emily Litella, I think the whole experiment—or at least the title of it!—was a big "Never mind."

To think, there's a neutron star out there that knows the name of Greg Comer. (Newton's Third Law ultimately says knowledge is reciprocal, you know.)

24-12-06 *Spekkens and Kant* (to R. W. Spekkens)

Happy holidays!

Rob Spekkens on wave functions:

> We shall argue for the superiority of the epistemic view over the ontic view by demonstrating how a great number of quantum phenomena that are mysterious from the ontic viewpoint, appear natural from the epistemic viewpoint. These phenomena include [about a million things]. ... The greater the number of phenomena that appear mysterious from an ontic perspective but natural from an epistemic perspective, the more convincing the latter viewpoint becomes. ...
>
> Of course, a proponent of the ontic view might argue that the phenomena in question are not mysterious *if* one abandons certain preconceived notions about physical reality. The challenge we offer to such a person is to present a few simple *physical* principles by the light of which all of these phenomena become conceptually intuitive (and not merely mathematical consequences of the formalism) within a framework wherein the quantum state is an ontic state. Our impression is that this challenge cannot be met.

Immanuel Kant on ghosts:

> I do not dare wholly to deny all truth to the various ghost stories, but with the curious reservation that I doubt each of them singly, but have some belief in them all taken together.

24-12-06 *Christmas Conversations in My Head* (to C. H. Bennett)

I read the lines below (which I'd like stored in my computer anyway, from Reuben Abel's book *The Pragmatic Humanism of F. C. S. Schiller*), and I thought again of our conversations over the years. Particularly I wondered whether the quote of Schiller (the very last sentence in the paragraph below) might characterize our relationship.

> This amount of indeterminism does not upset the structure of science. Alternative modes of conduct are, at least partially, calculable. Continuity of character is a limitation on complete freedom. But this is not equivalent to saying that, since character and habits affect choice, choice is therefore not free. It is commonplace that people

> frequently do things on impulse, or without reason, or "unexpected of them." Moreover, character is not a fixed entity, but a growing, evolving, changing concept. Is it ever completely formed? And besides, there exist situations in which the motives to contrary actions balance each other. A choice can frequently be shown to have, in retrospect, rational connections with the antecedent circumstances. But this does not imply that another choice in the same circumstances would not also have, in retrospect, such rational connections. Alternative courses may be equally free, yet they need not be haphazard. Determinists have argued that if the course of events were not completely and rigidly determined, it must be indeterminable. This, Schiller shows, is not necessary. The two postulates do not clash. There is a certain amount of indeterminism in the world which is determinable in alternative ways. The choice between these is a real freedom which does not involve the dire consequences of either complete determinism or complete indeterminism. Scientific calculability and moral freedom are not in conflict. Determinism as a scientific postulate is not endangered; as an ontological dogma, like all other statements of metaphysics, one may freely take it or leave it. "As we cannot vindicate our freedom unless we are determined to be free, so we cannot compel those to be free who are free to be determined, and prefer to think it so."

Just my weird way of saying: Merry Christmas; you're never far from my thoughts.

27-12-06 *My Christmas Present?* (to G. L. Comer, S. J. van Enk, and J. W. Nicholson)

To my three most open-minded friends,

I've just got to share this thing I discovered with someone. Go to Google and do a search on the three terms: "Chris Fuchs", "crazy", and "quantum". On the results page, click on the link at the bottom that'll take you to page 3(!) of the results, and look at the very last hit. It's to something titled, "Captain Stabbin – Captain Stabbin Interpretation of Quantum Mechanics." If you're not on a company machine, and you're feeling brave, click on the link. You'll be quite surprised!

Happy holidays!

27-12-06 *New Year's Delirium* (to H. J. Bernstein)

Hey Herb, you old reality maker,

I hope you're having a nice holiday, despite the meanness Charlie and I heaped on you the other day. I'm having a nice time, thinking a lot about what quantum mechanics is trying to tell us, and reading up on F. C. S. Schiller's flavor of pragmatism. (I'm giving a talk in Paris, Feb 23 at a meeting *Pragmatism and Quantum Meeting*, titled "William James, F. C. S. Schiller, and the Quantum Bayesians".) Schiller's ideas are fascinating and I think, perhaps, deeper than James's even.

Anyway, I was thinking I should send you a holiday letter and I came across a quote today that's giving me an opportunity. Ultimately, it's about the "big bang" being right here, right now, all around us. I'll give you the idea in three flavors, first in the form of a transcribed conversation between John Wheeler and R. Q. Elvee, second in an excerpt from my pseudo-paper "Delirium Quantum" (which is attached in its final form, just sent to the publisher), and third in my newfound quote from Schiller. It comes from his 1924 book, *Problems of Belief*.

Wishing the best to you and your family for the coming year. It sure would be nice to see you again sometime soon. (Will you be going to the Zeilinger foundations meeting in Vienna in June?)

Flavor 1:

ELVEE: Dr. Wheeler, who was there to observe the universe when it started? Were we there? Or does it only start with our observation? Is the big bang here?

WHEELER: A lovely way to put it—"Is the big bang here?" I can imagine that we will someday have to answer your question with a "yes." If there is any conclusion that follows more strongly than another about the nature of time from the study of the quantum nature of space and time, it is the circumstance that the very idea of "before" and "after" is in some sense transcended.

There are two aspects of this idea. First, Einstein's theory of space and time tells us that in order to predict all of space and time for time to come, we have to know what the conditions of space are now and how fast they're changing. Only then do we have enough information to predict all the future. The uncertainty principle of quantum theory tells us that if we know the condition of space now, we cannot know how fast it's changing. Or if we know how fast it's changing now, we cannot know what the geometry is now. Nature is so built with this complementary feature that we cannot have the information we need to give a deterministic account of space geometry evolving with time.

That deterministic account of space evolving in time is what we mean by spacetime. Everything that we say in everyday language, about time is directly built on that concept. And with determinism out, the very ideas of before and after are also out. For practical everyday matters, this indeterminism, this indefinability of spacetime is of no concern. The uncertainties only show up effectively at distances of the order of 10^{-33} cm. Nobody at present has equipment fine enough to reach down to a distance so small.

What does all this have to do with the big bang? At the very beginning of time we know that—according to Einstein's account—the universe was indefinitely small. Things were indefinitely compact. When we talk about time when the universe itself is so fantastically small, we deal with a state of affairs where the very words "before" and "after" lose all meaning. This circumstance puts one heavy restriction on the usefulness of the word "time." There is another.

When we do our observations in the here and the now on photons, quanta of light, hunks of energy coming from distant astrophysical sources, we ourselves have an irretrievable part in bringing about that which appears to be happening. We can put it this way: that reality is, in a certain sense, made up of a few iron posts of definite observation between which we fill in, by an elaborate work of imagination and theory, all the rest of the construction that we call reality. In other words, we are wrong to think of the past as having a definite existence "out there." The past only exists insofar as it is present in the records of today. And what those records are is determined by what questions we ask. There is no other history than that. This is the sense in which we ourselves are involved in defining the conditions of individual elementary quantum phenomena way back at the beginning of the big bang.

Each elementary quantum phenomenon is an elementary act of "fact creation." That is incontestable. But is that the only mechanism needed to create all that is? Is what took place at the big bang the consequence of billions upon billions of these elementary processes, these elementary "acts of observer-participancy," these quantum phenomena?

Have we had the mechanism of creation before our eyes all this time without recognizing the truth? That is the larger question implicit in your comment. Of all the deep questions of our time, I do not know one that is deeper, more exciting, more clearly pregnant with a great advance in our understanding.

Flavor 2:

The way I see it, quantum measurement outcomes are ultimate facts without specific call for further explanation. And indeed the quantum formalism supplies none. Thus there is more to the world than the quantum formalism can supply. Nothing to do with hidden variables. ...

How does the theory tell us that there is much more to the world than it can say? It tells us that *facts* can be made to come into existence, and not just at some time in the remote past called the "big bang" but here and now, all the time, whenever an observer sets out to perform (in antiquated language) a quantum measurement. I find that fantastic! And it hints that facts are being created all the time all around us. But that now steps out of the domain of what the quantum formalism is about, and so is the subject of future research. At the present—as a first step—I want rather to make the interpretation of the quantum formalism along these lines absolutely airtight. And then from there we'll better know how to go further.

Doesn't that just make you tingle? That (metaphorically, or maybe not so metaphorically) the big bang is, in part, right here all around us? And that the actions we take are *part* of that creation! At least for me, it makes my life count in a way that I didn't dare dream before I stumbled upon Wheeler, Pauli, and Bell-Kochen-Specker.

Flavor 3:

On a very minute scale, but in a very real sense, our preferences and our acts are contributing to the shaping of the world, and sharing in the unceasing process of creation, which did not come to an end 5,928 years ago, but is continuously manifested in the all-pervasive creativeness which engenders ... novelties in every region of the universe.

If Humanism then is right, human agency is *not* the illusion it is so tempting to make it. Truth may be, like the other values, like our moral and aesthetic ideals, a real contribution to reality, which the real might not possess unless we had made it.

27-12-06 *Bayes, Born, and Everett* (to F. J. Tipler)

Tiplerism 1: *You probably have never heard of me, but I was a post-doc of John Wheeler's from 1979 to 1981.*

Of course I've heard of you. I read your book with Barrow on anthropic principles about 20 years ago and enjoyed it immensely. (My copy burned up in the Cerro Grande fire that came through Los Alamos six years ago, but I used to keep one of Sam Hurt's Eyebeam cartoons in it as a bookmark. It was a scene of Eyebeam explaining anthropism to Ratliff; when each would turn his back on the other, the other would disappear from the scene.)

Your sentence indicates though, that you thought John Wheeler might be a hook for my interest. Indeed I have the greatest respect for some of John's more outlandish ideas on quantum mechanics. (You might enjoy the Wheeler story on page 149 of my `quant-ph/0105039`. And you can find more

Wheeler stories by looking in the index; there's a better indexed version on my website.) I wish I had known you were one of John's postdocs; I would have invited you to the little meeting Marlan Scully and I organized at Princeton last year for John on quantum information and quantum foundations. Schumacher, Wootters, Unruh, and various of John's other quantum-interested associates came. John even made an appearance for about 30 minutes. It was quite nice to see him even at this stage in his life. You could tell he was understanding the emotion of the occasion, even if not much else. His smile and the thrusting of his fist into the air made it all worth it.

Tiplerism 2: *In reference to your "Being Bayesian in a Quantum World," conference last year, I've just put a paper on the* `arXiv` *(*`quant-ph/0611245`*), wherein I derive the Born interpretation using the Bayesian approach to probability, namely that probabilities are precise quantitative measures of human ignorance.*

I have downloaded your paper and will study it carefully. As it turns out, I'll be speaking at the "Many Worlds at 50" conference in Waterloo in September, and the title I gave the organizers for my talk is, "13 Direct Quotes from Everettian Papers and Why I Find Them Unsettling." Not a lie. I hope your paper doesn't make my list!

You certainly have a different use of the Bayesian idea of probability in mind with regard to quantum mechanics, than my closest colleagues and I do. For fun, I'll attach two pieces of writing that aren't posted on the web yet—one's a paper, and one's a pseudo-paper—so you might see what I mean by this remark. However, I might also send you to `quant-ph/0205039` which is the paper I'm most pleased with at the moment. (It is not a mistake that the number above is `0105039`, while this one is `0205039`; it's just an interesting coincidence.) Anyway, of the two papers attached, the pseudo-one tries to express what I don't like about block-universe (in the sense of William James, say) conceptions of the universe—the Everettian idea is a species of this. I'd rather think the universe is not complete in any sense, and I think quantum mechanics gives us plenty reason for exploring this turn of thought. The other paper, particularly in its concluding section, tries to express what my colleagues and I see as the import of the Born rule: The idea we are trying to develop is that it is not a rule for *setting* probabilities, but rather a rule for *transforming* Bayesian/personal/subjective probabilities.

27-12-06 *Residue of the Category Error* (to S. J. van Enk)

van Enkism 8: *Do I get this right, at least with high probability?*

So, it's a live option for you?! Not a fact?

Anyway, indeed you are on the right track (probably just using mildly different language than I use). h cannot both be a fact for the agent and a live option. One makes the category error (i.e., thinking a probability assignment can be implied by a fact alone, and therefore itself is a fact) if one thinks $h \rightarrow h$ (a statement about a fact) enforces $P(h|h) = 1$. The latter is a judgment because, in order for $P(h|h)$ to be meaningful at all, the left-hand h must be a live option. That is, one judges that the live option will be the same as the dead fact.

Steven's Preply

What you say here sounds right to me:

> So, now we get to the point. My present opinion is that probability statements are really only ever meant to be made about "live options" (to use

a phrase of William James, though I may not be using it exactly the same way as he did). One can indeed write $P(x=5)=1$, but then $x=5$ had better a live option, rather than a "fact for the agent." If it is a fact for the agent, then we are reinstating the category error. The only role for the "facts for the agent"—despite their very real consequences, like 'life' or 'death'—are in pivoting the agent's subjective probabilities. They are never used for the setting of probabilities unconditionally.

In particular, I needed the part about "reinstating the category error" to be convinced.

"$x=5$" could be either in the part labeled "d", or in the part labeled "h" in probabilities like $P(h|d)$, but they have different meanings then, one is a fact, the other is a live option. So, writing $P(h|h)$ or $P(d|d)$ are both category errors.

Do I get this right, at least with high probability?

28-12-06 *I and It, 2* (to R. Schack)

Schackcosm 99: *I am just not sure what the problem is with simultaneously (a) knowing there is nothing objective in the world guaranteeing an outcome and (b) being certain of the outcome.*

I'm finally replying to this. Though, not much to say actually. It is just that I think analyses of the Timpson type are valuable for showing how twisted things can become under the "question-answer" motif for quantum measurement—that is, viewing a quantum measurement as asking a question of a system, and the system coughing up an answer. With that imagery, how could the agent be certain of the outcome if he acknowledges that the system is not certain? I can see his quandary. But if one takes the alchemical view, as far as I can tell, it doesn't seem to be any big deal at all—it is not a great quandary. The quantum system's role is that of a catalyst, not an answerer of questions—there is no reason for it to be "certain" of the outcome, i.e., there is no reason to have a fact residing in the system that indicates the outcome of the measurement. And so my desire for Timpson to run through a pretty thorough analysis of the opposing conception is a bit self-serving. It's a useful exercise and someone should do it.

28-12-06 *The Scandal of Bayesianism* (to A. Hájek)

Once upon a time, you showed me a draft of a paper that contained these words:

> The scandal of Bayesianism is that no rational constraints on subjective probabilities beyond probabilistic coherence have been widely accepted. ... Bayesians maintain and even celebrate a remarkably anarchistic attitude to priors, (and derivatively to posteriors): as long as they are coherent (or appropriately derived from a coherent prior), anything goes.
>
> ... Just as our beliefs aim for more than consistency, our probabilistic judgments surely aim for more than coherence. What, then, do they aim for? What does such a judgment's 'fitting' the world amount to? What plays the role for subjective probability analogous to the role that truth plays for all-or-nothing belief? Presumably something plays this role, for probabilistic judgments seem also to be governed by a norm of veracity. How else can we explain the fact that we regard some weather forecasters as better than others, even when they couch their predictions probabilistically? And when different people assign different probabilities to the same event—as the managers and

> engineers did the day before the ill-fated space shuttle launch in 1986—we think that they are *disagreeing* about something. There is surely some fact about the world that a subjective probability assignment strives to track. Beliefs strive for truth; probabilistic judgments strive for <u>(fill in the blank)</u>?

Could I ask you to email me a copy of that paper if you still have it around?
Thanks!

Alan's Reply

> [CAF said:] "Thanks for the paper. Was it never published?"

> Not yet. I did send it to a journal. The referee's report wrote that I "didn't even discuss the obvious objection that nothing fills in the blank". Not only DID I discuss this objection, but it got a section to itself, with an italicized heading. Maybe next time I should put it in bold face as well. When I complained to the editors, they were very good about it — apologetic, and they invited me to resubmit the paper. I eventually will, when I've revised it further.

28-12-06 *Prepare Yourself* (to N. D. Mermin)

Sorry for my holiday absence (not that you probably minded!). I started gearing up this morning to reply to your 12/18 note, particularly what I deem to be your main trouble expressed in it:

Merminition 159: *All I'm saying is that this is rotten pedagogy, because it gives the student no basis whatever for getting the whole process started. Part (b) of the figure makes this dramatically clear. All the circuit does is transfer the state from the ancilla to the Qbit. It doesn't begin to answer the student's question of how you make an initial state assignment independent of any prior state assignment. Whether the initial state assignment is objective or subjective, the grounds for making one remain utterly obscure.*

and

Merminition 160: *With reference to what you say below, the prior, as I understand Figure 1 is that the damned ancilla on the left has the state assignment* $|0\rangle$*, so you haven't told the [poor] student anything about what it means to make such a state assignment, either objectively or subjectively. It's all circular.*

So, I did a few Google searches. For instance, I did one on "Bayesian" and "prior come from." Well, there's some tasty quotes out there:

> Bayes' Rule is central to modern economics and modern psychology. According to Bayes' Rule, a rational person starts with some beliefs about probabilities (his "priors") and changes them in a particular way as new information arrives, in order to reach new beliefs (his "posteriors"). Psychologists usually emphasize that people should use Bayes' Rule; economists are more likely to assume that people do use Bayes' Rule.
>
> The main problem with Bayes' Rule is that it doesn't say where priors come from, or which ones you should have. It is tempting to say that every prior is just your last posterior. But where did your first prior come from? If you picked the wrong one, then everything based on it could be wrong as well.

Maybe more relevantly I dug into my personal archives and found, for instance, this nice thing by Alan Hájek (from a 1998 draft of something):

> The scandal of Bayesianism is that no rational constraints on subjective probabilities beyond probabilistic coherence have been widely accepted. Bayesians maintain and even celebrate a remarkably anarchistic attitude to priors, (and derivatively to posteriors): as long as they are coherent (or appropriately derived from a coherent prior), anything goes.
>
> … Just as our beliefs aim for more than consistency, our probabilistic judgments surely aim for more than coherence. What, then, do they aim for? What does such a judgment's 'fitting' the world amount to? What plays the role for subjective probability analogous to the role that truth plays for all-or-nothing belief? Presumably something plays this role, for probabilistic judgments seem also to be governed by a norm of veracity. How else can we explain the fact that we regard some weather forecasters as better than others, even when they couch their predictions probabilistically? And when different people assign different probabilities to the same event—as the managers and engineers did the day before the ill-fated space shuttle launch in 1986—we think that they are *disagreeing* about something. There is surely some fact about the world that a subjective probability assignment strives to track. Beliefs strive for truth; probabilistic judgments strive for ___(fill in the blank)___?

But then I went back to bed … and so now this is a restart. (I hate it when I do that, for it forces me to change the planned titles of my notes. The present title was meant to be the second title in the series. But I've decided that maybe I'll just go for broke and put my complete reply in the present note.)

What I read into your note is simply a species of this general worry about Bayesianism. "Where does the prior come from? Without telling me where a prior comes from, how can you have given me a complete solution to the meaning of probability?"

Compare: Where does the initial quantum state come from? Without telling me where the initial quantum state for a quantum computer comes from, how can you have given me a solution to the meaning of quantum states? Particularly, how can you make any sense of what a quantum computer is doing if you don't give an answer for where its initial quantum state comes from other than saying that it's conditioned from (effectively) a previous quantum state?

Remember, for me, a quantum state is *nothing but* a single probability distribution (for the outcomes of a singled-out informationally complete observable). Thus, to the extent that a subjective Bayesian leaves the ultimate origin of *any* probability assignment unanalyzed, precisely the same point holds for quantum states.

You call that circular. The subjective Bayesian calls it necessary: It is the cutting of a Gordian knot. Without the stark recognition that all (ultimate) priors are personal to the agent, one would have an infinite regress. Not circularity, but an infinite regress. So, one says, "There's a starting point, I can't get past that."

For myself, I think, "Why is that admission any more mysterious than what one finds in Newtonian mechanics?" A pendulum, for instance, has an equation of motion and an initial condition. Why doesn't anyone in a classical physics class complain, "But where does the initial condition come from? Without telling me the origin of the initial condition, you have not told me the complete story of the pendulum!" Or why don't the instructors complain to the textbook writers, "You think this is good pedagogy? How am I going to explain to the students that they must just take initial conditions for granted … and that it is beyond the bounds of the problem to ask where the initial conditions come from?"

As far as I can tell, in the two classes of problems the *only* difference of any note is in what the initial conditions are about. There's no great conceptual or philosophical difference between the two. The ordered pair (x_0, p_0) is the initial condition of the pendulum. The prior $P(h)$ is the initial condition of the agent. Dynamics leads to (x_1, p_1); conditioning leads to $P(h|d)$. Why does the initial condition of the agent call for any more explanation or justification than the initial condition of the pendulum?

Or maybe let's put the two examples together. (x_0, p_0) is the initial condition of the pendulum, but $P_0(x, p)$ is the initial condition of the classical observer using Liouvillian mechanics (the agent). Both systems, the pendulum and the agent, have unexplained initial conditions — what's wrong with that?

Above I said "only difference of any note," but of course there is a difference of detail here and there. When we come to the quantum case particularly, there is this difference between the classical Liouvillian observer and the quantum observer. When the classical observer writes down $P_0(x, p)$, it is his initial uncertainty of a preexistent reality for the pendulum; when the quantum observer writes down a quantum state $|\psi_0\rangle$, it is his initial uncertainty for *what will come about for him* in the case that he takes a certain fiducial action upon the system (i.e., makes a certain measurement)—it is not an uncertainty for a preexistent value.

Is that difference of detail enough to require that $|\psi_0\rangle$ be given more explanation or justification than $P_0(x, p)$? We say (over and over), no. What I should better understand about you is why you think it needs more. My guess at this point is that it is nothing other than the usual fear of Bayesianism (even in the classical context of $P_0(x, p)$): "Surely there is a 'right' prior, and whatever makes it 'right' is what should be delineated." That's what you are asking for.

Now, before moving on to another point, let me come back to part of your second quote above:

Merminition 161: *With reference to what you say below, the prior, as I understand Figure 1 is that the damned ancilla on the left has the state assignment $|0\rangle$, so you haven't told the [poor] student anything about what it means to make such a state assignment, either objectively or subjectively.*

That is just incorrect. We have told the poor student over and over what it means to make the state assignment $|0\rangle$. It means that for his subjective assignment that IS the measurement gate (that particular quantum operation), he is quite certain what the consequence of its action upon the quantum system will be for him—he will get the datum 0. He can predict the outcome with (subjective) certainty. That is the *meaning* of $|0\rangle$: It is the statement of certainty. It is the statement that the student is prepared to gamble his life on the outcome.

You don't have to like our answer, but that is the answer we give. It is clearly a meaning for the state $|0\rangle$—a very personal one, with operational consequences. What we don't do is give a larger story that explains the origin of the agent's state of mind, $|0\rangle$. Where did he get that from? I don't know. It might be a good question to ask, but it is not one that the calculus of probability can answer. For, the calculus of probability requires as its very input a probability assignment—just like the calculus of Newtonian mechanics requires as its very input an initial state for the system under discussion.

So, why did I title this note "Prepare Yourself" (other than for its expected length)?

Just what is happening, you ask, for the quantum computer scientist with the initial preparation gate in your depiction of a quantum computer? What is he doing when we *say* he is preparing the qubit? From the Bayesian point of view, he is preparing himself. He is tightening his state of belief for what will come about in future interactions with the qubit.

I guess that's why I showed such dismay at your talk in Konstanz:

Merminition 162: *As I understand you it is unacceptable because that would give the state an objective character. (The basis for the remark I made early in my talk in Konstanz that made* *you say I still hadn't understood a goddam thing. Probably I still don't. If the reading of the measurement gate is a fact, then the only room for subjectivity is whether the measurement gate is indeed a measurement gate — e.g. is it properly aligned, or, in Figure 1, the subjectivity of making the state assignment $|0\rangle$ for the ancilla.)*

I remember you had a big question mark as the output of the preparation gate—seemingly implying that the quantum computer scientist would not know what the output of the gate was. But he knows exactly what it will be. The "preparation gate" is an action whose consequence will prepare his state of mind to something he wants it to be. The subjectivity of the measurement gate (or preparation gate, I can't remember your exact terminology) means that it is *his* description of his action upon the qubit. Making it *his* description—rather than a fact about ultimately remote parts of the universe external to him, as the conception you are shooting for does (see my previous note)—makes it no less useful to him. (Maybe it makes it more so.)

At the moment, I fail to see how this would be so very mysterious to a computer scientist. To perform a (quantum) computation, you build your confidence about some things to do with a machine (what you will see if you press this button). Then you bank on that to let the machine help you build up various inverse probabilities for something you are not very sure about (say the factors of some large number).

Does any of this answer anything for you?

P.S. In return for all this labor this morning, I would love a reply from you on what I'm getting wrong with my critique of your "disembodied facts" account of quantum states.

28-12-06 *Goose and Gander* (to R. W. Spekkens)

Spekkensism 34: *On the subject of the ontic/epistemic status of the quantum state, at the end of a talk to the history and philosophy of physics group at Leeds, Ian Lawrie (a physicist) had a comment which, after I had finally understood it, impressed upon me (more strongly than before) that there's a sense in which the distinction breaks down when one tries to treat agents as physical systems. I'm curious what you think about the following argument. Suppose one devises a physical theory for the ontic states of the world – no external observer is required for the theory to be well-defined; it is a "god's eye view" type of theory. And suppose that this theory does in fact provide a useful model of known physics and, in particular, of observers and the process of observation. In other words, all observers are treated internally to the theory. Assuming a materialist position on the mind-body problem, everything that is known to any observer is encoded in the ontic state of the world according to this theory (either in the intrinsic properties of their brains, or, more likely, in the relational properties of their brains and the rest of the world). According to the epistemic view of quantum states, different quantum states represent different beliefs. However, in such a physical theory, different beliefs correspond to different ontic states of the world, and so different quantum states correspond to different ontic states of the world.*

Personally, I don't see much of a problem with granting that quantum states are ontic in this sense. They are simply not ontic in the traditional sense of describing the reality of the system to which the state is assigned. Furthermore, even though they may be ontic in this sense, they certainly will not correspond to a complete description of reality in such a theory. In other words, quantum theory can't be this "über"-theory. What's your take on it?

Well, I don't think much of such hoped-for god's-eye-view theories, because I doubt the concept is even consistent at all. They don't have enough fire in the loins, so to speak, to power a real universe. (That's part of what I think quantum theory is trying to tell us.) But I'll grant you the possibility, for the purpose of getting this discussion off the ground.

If you limit me to that context, then I'd say I probably pretty much agree with your second paragraph above. In my presentations, I know that I often use language like this (the presentational Chris Fuchs is usually not as radical as the Chris Fuchs of correspondence—I tend to think it's better to be a little less than true-to-myself in such a forum for the purpose of more progress and less confrontation; Lord knows I already have enough confrontation). I find myself saying things like I said to Greg Comer in a 4 July 2003 note:

> A wave function and its evolution are not properties intrinsic to the system for which they are about. Rather, if they are properties of anything at all, they are properties of their user's head—for they capture all his judgments about what might occur if he were to interact with the system of interest.

Indeed, that's not so very different from calling a quantum state an ontic state of the agent (when treated as an agent per se). [Just for completeness sake, let me point out the difference between that and a situation where *I* treat *you* as a physical system (rather than an agent) of which I can contemplate performing measurements on. For instance, I might ask you what you believe of that electron for all measurements you could perform on it. Your answer to me will be in the form of a density operator—that is to say, the outcome of my quantum measurement will be something that symbolizes a quantum state in your possession. Since I generally won't know what your answer to me will be—treating you as a physical system—I encode my uncertainty about the answer you'll give my questions in a density operator. That density operator—which is an epistemic state for me—has nothing to do with the ontic state we are talking about above. Nor is the outcome of my quantum measurement directly so either—for the outcomes of quantum measurements are not revelational of properties intrinsic to the system (they are a joint product of the observer and the system). This, I think, is another way of saying what you said above, that quantum theory will not be its own über-theory.]

Or to give a more recent example of my using this kind of imagery, I might paste in the note I composed for David Mermin this morning. Throughout it, I lean on the idea that a quantum state is effectively an ontic state for the agent (along the lines of how x and p are ontic states for a pendulum). I think the note is relatively self-contained and you'll see its relevance for the present issue. The figure Mermin refers to is in the attached paper—a revised version of the CFS paper "Subj Prob and Q Certainty". The issue that seems to bother Mermin is that Bayesians give no account of the origin of a prior; I say that is no deeper a problem than Newtonian mechanics giving no account for the origin of any initial condition. What's good enough for the goose, should be good enough for the gander.

Spekkensism 35: *On a different topic, I recently finished a paper on pooling quantum states. The central result is something that I worked out during the BBQW workshop after Todd Brun's talk and have since developed with Howard Wiseman. It appeared on the* `arxiv` *on Christmas day (I'm attaching a copy). I'm sure that you will be unhappy with it for the same reasons that you're unhappy with the Brun-Finklestein-Mermin compatibility criterion. Still, as someone who likes to think about quantum analogues of features of classical probability theory, you might be interested in the problem.*

Thanks for sending the paper. I'm quite OK with exercises like this—I think they're essential—as long as they are advertised for what they are: Examples of conditions under which disparate agents will move closer to agreement. One would like to identify those cases! If Agent X believes this, and Agent Y believes that, then they will be willing to pool their beliefs for a sharper belief. But to demand that beliefs always be poolable—I say—effectively transforms them back into ontic states (defeating the purpose of trying to draw an ontic/epistemic distinction in the first place)—they become incomplete perspectives on the "true" ontic state and regain an ontic character in that way. BFM could just never get that into their heads (it's really hard to give up the idea of an ontic quantum state).

I'll certainly study your paper. I hope you advertise the result the right way!!

Spekkensism 36: *Matt Leifer and I are writing a follow-up paper which makes extensive use of Matt's "conditional density operators", and goes much further on the problem than my paper with Howard does.*

Glad to hear about that. Reflecting on Bayesianism, one knows that the foundation is really in the idea of conditional probability $Pr(h|d)$ rather than unconditioned probabilities $Pr(h)$. Similarly I'm starting to think perhaps quantum mechanics is focusing on the wrong mathematical object for a smooth development along Bayesian lines. It is not the quantum state, but something *like* (if not exactly) Matt's conditional density operators. So, it'll be good to get a better feel for these objects in as many ways as possible.

Happy New Year to you too!

29-12-06 *Your Amazing Efficacity* (to A. Plotnitsky)

I came into Bell Labs today and found to my great surprise the package containing your new book! You really are quite amazing. Prolific. I wish I had a fraction of your productivity! I'll certainly study your new material.

When will you be in New York again? I'd love to get together with you one day for a long lunch.

Attached you will find my own latest, with Caves and Schack: `quant-ph/0608190`. [...]

For the moment though, I hope you will hang on the words "fact for the agent" which I introduced into this article. I think it is the key for much of what I want to say in the next paper. Here's a little discussion I had with one of the referees of the present paper.

> This paper does not emphasize it, but no, we do not mean "the facts we are talking about here are facts for everybody." We mean that the facts too are personal, though in a very careful sense.
>
> We have tried to meld the phrase "fact for the agent" a little better than previously into its surrounding text, but a better exposition would have emphasized the full setting of our view, including some discussion of the ontological—i.e., noninstrumental—pieces of it. 1) There are two physical systems in the story of this paper. One takes the role of the agent—the possessor of subjective degrees of belief and the activator of the measurement process. The other takes the role of the object system. 2) What is being discussed when one speaks of gathering data in the quantum context is an interaction or transaction between the agent and the object. 3) Without the agent, there would be no quantum "measurement" to speak of, but without the object, there would be no means for the agent to obtain the data, the spikes upon which he pivots his probabilities (his subjective degrees of belief). 4) There are no other agents in this story. 5) We do not go any further than points 1-5 warrant by giving the data obtained in a quantum measurement an autonomous existence—for instance, as something beyond

the agent's sensations. That would run into inconsistencies in a "Wigner's friend" scenario. Nonetheless, quantum measurement outcomes are beyond the control of the agent—they are only born in the interaction—and thus are not functions of the agent in the way that his degrees of belief are. The degrees of belief find their source in the agent (the subject); the outcomes find their source in the external quantum system (the object). But the outcomes lead back to the agent in that they are personal to him.

> This does not mean that the agent-object interaction and its fuller consequences are not autonomous events in spacetime, but it does mean that if there are any such things, quantum theory is not directly concerned with them. In CAF's view, quantum theory takes its whole definition as a normative theory for organizing an agent's *personal* probabilities for the *personal* consequences of his interactions with external physical systems. The structure of the agent-independent world that lies behind quantum mechanics is, in the end, still codified by the theory, but only in a higher-order, more sophisticated fashion than had been explored previously—it is through the normative rules, rather than quantum states and Hamiltonians.

When we get together, I'd like to come back to your old 'efficacity' concept. Ever more I feel it's relevant to what I want to say ... but I wonder if that is one of the things you have changed your views on (as I don't see the word appearing in your index).

29-12-06 *The Finish Line* (to S. Hartmann)

Hartmannism 2: *Could you send me the final version by the end of December? This would be great.*

OK, we just barely made your deadline! Attached, please find our paper (both in LaTeX form and PDF) and our replies to the referees (they are all in one single PDF file).

I can tell you, you have no idea how much (and how deeply) we fought in constructing this paper. The whole thing was built out of negotiation—and you'll get some sense of that if you read our replies to the referees (particularly our remarks to Ref 1—i.e., Mermin—and Ref. 4). And every single word was pained over. Thus, we pretty much beg of you not to ask for any further changes to the text. Very seriously. My word processor shows our word count to be 10,582—it is over the 10,200 word limit you and Roman set, but only by a little.

I hope you enjoy the new version of the article. I think you got much more than you bargained for: You asked for a review, but we gave you a new research article. Thanks for giving us this opportunity.

By the way, I found Referee 4's comments excellent (as I hope my replies to him show). If he is amenable, I would like to further the conversation with him, as I think I would get much out of the exercise and because I have many points to add to what I already said to him (that I couldn't express when speaking for Carl and Rüdiger). Thus, if you will, please give him my card (i.e., this paragraph), and tell him to contact me if he has any further interest.

Best wishes to you and your wife for the coming year!

31-12-06 *Tipler Speaks* (to R. Schack)

I don't think I'm going to reply to Tipler's reply to my reply to Tipler, but I am curious in what pithy way you would answer his sentence, "Anyone with the same information would assign exactly the same probabilities." I'm afraid we have Ed Jaynes to blame for this one, and so we'll keep hearing it. It might be good to develop a good canned answer.

> **Chris:** The other paper, particularly in its concluding section, tries to express what my colleagues and I see as the import of the Born rule: The idea we are trying to develop is that it is not a rule for *setting* probabilities, but rather a rule for *transforming* Bayesian/personal/subjective probabilities.

> **Tipler:** Here I think you are indeed taking the wrong approach to probability theory. There is nothing personal or subjective about assigning probabilities. Anyone with the same information would assign exactly the same probabilities. This was also Laplace's view, and he was a determinist like me. Probabilities are not in nature, they are only precise measures of human ignorance. Which is why I prefer to call my approach "Laplacean probability" rather than "Bayesian probability." And I think that you will have difficulty improving the priors that come from quantum indistinguishability.

03-01-07 *David Signals* (to N. D. Mermin)

Judging from your present note, it doesn't look like you'll like the latest incarnation of the footnote. (I'm sorry; I just realized that I had not sent you the finalized manuscript. It is attached.) In this version we had said:

> N. D. Mermin (private communications, 2003 and 2006) characterizes as "dangerously misleading" the idea that the post-measurement quantum state $|\psi\rangle$ of B is an objective property of system B alone. He "reject(s) the notion that objective properties (must) reside in objects or have physical locations." Yet the quantum state of B, if objective, is a property of the world, external to the agent, and since it changes as the world changes, it is hard to see how it can have only the disembodied objectivity Mermin is describing. If the objectivity of $|\psi\rangle$ *does* reside somewhere, say, in the entire experimental setup, including the device that prepares A and B and the measurement on A and its outcome, then consider a measurement of an observable of B for which $|\psi\rangle$ is an eigenstate with eigenvalue λ. If λ is an objective property, how can it fail to reside in B (under the assumption of locality), thus making $|\psi\rangle$ a property of B after all?

For, clearly here we use that bad word "property" again.

But, while we're on this subject, you say:

Merminition 163: *The naughty things I'm searching for are facts — in this case the facts of the "preparation procedure". There's nothing wrong with some of the facts being facts about us. (Can't have "correlations between the manifold aspects of our experience" without invoking us.)*

In what way do you distinguish the phrase "facts about us" from "properties of us"? I would normally think of these two phrases, "facts about" and "properties of," as synonyms, and I think everything I have written can be interpreted that way. But you seem to reject that usage.

However, there's a still more worrisome issue here. Previously I would have *never* thought you would allow the facts for which you take quantum states to be short-hand of to refer to the agent in any way (or the preparer, if you will). But now it seems that is part of your very formulation. Has it always been so?

Because the quantum state—from our view—depends on the AGENT as well as the data, we have insisted on calling it subjective. As I understood you, you wanted to think that quantum states could be made to refer to facts alone—agent-independent facts particularly—and you therefore wanted to call (pure) quantum states objective. If you are willing to explicitly renounce the requirement of agent-independentness (which I had thought you had always been making), in what substantial way does your view differ from ours?

Well, I would guess it still differs deeply in that you say:

Merminition 164: *I would say that the "something in the world ... that guarantees the outcome YES" has to be the whole state preparation — all the relevant history of the system prior to the measurement.*

For even if you were to substitute into that:

> I would say that the "something in the world ... that guarantees the outcome YES" has to be the whole state preparation — all the relevant history of the system AND THE AGENT prior to the measurement.

CFS would still get hung up on your idea of "guarantees."

Let me paste in a note that I wrote to Rob Spekkens (just after writing you last) [dated 28-12-06, titled "Goose and Gander"] that I think is directly relevant to the issue. In that note to Rob I say one might validly call a quantum state an ontic state of the agent (though maybe I should have called it 'ontic aspect'). Is that all that your way of phrasing things is trying to capture?

If it is, then in a later note I'll try to argue that our way of phrasing things in better, but first I've got to understand this NEW (I think) twist I'm hearing from you.

03-01-07 *New Year's Alchemy* (to H. C. von Baeyer)

Thank you for the sweet New Year's note. Though I've only known you for a short while now, I've learned that when I need some reassurance that I'm not wasting my life away, I can at least look to your emails. That's more resources than I had before! The idea of the Republica Literaria sounds charming.

So what should I call my upcoming lump of collected emails if not samizdat? Your description was inspiring, but I need to distill the idea into a name that won't make me appear to be self-aggrandizing. At the moment, I run dry.

Have you had a chance to read through (or translate) any more of the Pauli-Fierz correspondence? I've been thinking about modern alchemy again—aka quantum measurement—as I've been thinking about how to put together my Shimonyfest contribution and also reading F. C. S. Schiller in preparation for my lecture in Paris. (Will you still be in Paris Feb 22 and 23? If so, you should come; the meeting is titled "Pragmatism and Quantum Mechanics," and my talk is titled "William James, F. C. S. Schiller, and the Quantum Bayesians." Schack will be there too.) Maybe I'll be able to get a quote of Schiller into my computer in the next few days and send it to you; it'll help reveal a little of what I see as the connection between Schiller's metaphysics (of a plastic reality) and Pauli's alchemy.

Attached is a copy of the finalized version of my recent paper with Caves and Schack on certainty. (We haven't re-posted it yet; that'll have to wait until Rüdiger is back from vacation.) It is much changed since the version you last saw; though not too much with regard to the particular point you made. Sorry about that. Still I hope the paper makes a little more sense this round. I'm not all that pleased with the paper, but it'll have to do. Particularly, the next stage of development really has to be an explicit and open discussion of the ontology that underlies our view of quantum mechanics. That is what my Shimonyfest contribution will attempt to be about; I want it to be a much more detailed development of the point I made to one of our referees, which I'll paste in below. Measurement involves *two* systems, so it is not solipsism (or positivism) even if the outcomes make reference only to the observer's sensations. The quantum system is the philosopher's stone that transforms the agent. (In that way, there is a bit of Pauli in me; but the passage also shows where I differ from Pauli—his and Bohr's 'outcomes' were automatically public, for the quantum Bayesians, they remain private … at least till communicated.)

Hans Christian's Preply, "Happy New Year," 30 December 2006

Dear Chris, for the New Year I wish you new insights, renewed certainty in your employment, and much joy. Also good wine.

I am in gray, cold, wet Paris, which is nevertheless brightly illuminated, snug, and exhilarating indoors. In the course of reviewing two new biographies of Emilie du Châtelet, Voltaire's lover and Newton's translator, I am reading a book that reminds me of you. In *The Republic of Letters* the historian Dena Goodman describes the 18th century forerunner of today's "scientific community", which is distinct from both the university world and the international system of professional societies. Both of the latter already existed during the Enlightenment, but the Republic of Letters was broader in both subject matter and membership.

The Republic of Letters was a network (you see where this is going?) of people interested in the arts, humanities, and sciences. As early as 1699 it was described as follows: "The Republic of Letters is of very ancient origin. It embraces the whole world and is composed of all nationalities, all social classes, all ages, and both sexes [!]."

Goodman singles out three features: In the 18th century the RoL depended on two fundamental inventions of the modern world – the printing press and the postal system – which together made possible the cosmopolitan polity of the RoL. Further on she quotes another historian: "… it was the strict duty of each citizen of the Republica Literaria to establish, maintain, and encourage communication, primarily by personal correspondence or contact." Citizens considered it their duty to bring others into the republic through the expansion of their correspondence. And finally: "Reciprocity is the distinctive feature of correspondence as a mode of communication …. Reciprocity was the fundamental virtue of the republic."

It was this last sentence that struck me – the difference between your style of research and the conventional style of publication. The RoL was a social construct that co-existed but contrasted with the monarchy, and, according to Goodman, eventually helped to bring about the revolution. This is certainly a more cogent reading of history than the fairy tale of the masses rising up spontaneously against the evil king, but it's not what interests me here.

I once told you that I didn't think samizdat was a good description of your work, but I didn't have a better suggestion. Now I realize that you are restoring the Republic of Letters by means of the latest technology, which replaces both the printing press and the postal system. This insight puts your work into historical context and, in my mind at least, enhances its value and importance. I doubt that you intend to or will start a revolution, but I do think that your style will flourish.

06-01-07 *Free Will, William James, NY Times* (to me)

From Maureen Dowd, "Monkey on a Tiger," *New York Times*, 6 January 2007:

> There was a touch of parody to the giddy Democrat takeover this week: Nancy Pelosi indulging her inner Haight-Ashbury and dipping the Capitol in tie-dye, sashaying around with the Grateful Dead, Wyclef Jean, Carole King, Richard Gere, feminists and a swarm of well-connected urchins.
>
> The first act of House Democrats who promised to govern with bipartisan comity was imperiously banishing Republicans from participating in the initial round of lawmaking. Even if Republicans were brutes during their reign, Democrats should have shown more class, letting the whiny minority party offer some stupid amendments that would lose.
>
> Perhaps the Democrats' power-shift into overdrive is a neurological disorder, or neuropolitical disorder.
>
> If free will is an illusion – if we are, as one philosopher put it, "nothing more than sophisticated meat machines," doomed to repeat the same mistakes over and over – that would explain a lot about the latest trend in which everyone is reverting to type.
>
> William James wrote in 1890 that the whole "sting and excitement" of life comes from "our sense that in it things are really being decided from one moment to another, and that it is not the dull rattling off of a chain that was forged innumerable ages ago."
>
> But in Science Times this week, Dennis Overbye advised Dr. James to "get over it," observing that "a bevy of experiments in recent years suggest that the conscious mind is like a monkey riding a tiger of subconscious decisions and actions in progress, frantically making up stories about being in control."
>
> As Mark Hallett of the National Institute of Neurological Disorders and Stroke told Mr. Overbye, "Free will does exist, but it's a perception, not a power or a driving force. … The more you scrutinize it, the more you realize you don't have it."
>
> That would explain why, after voters insisted that the president wrap it up in Iraq, he made a big show of pretending to listen, then decided to do a war do-over.
>
> Is this just the baked-in stubbornness of one man, or is W.'s behavior evidence that he has no free will? Is the Decider freely choosing another huge blunder or is he taking instructions from his genetic and political coding, fearing that if he admits what a foul hash he's made of Iraq, he'll be labeled a wimp, as his dad was?

09-01-07 *Harsanyi Principle* (to P. Grangier)

Thank you for your note; let me think about how I will reply. In the meantime, you can read one of my mumbles on the principle Jaynes is pushing here: `http://netlib.bell-labs.com/who/cafuchs/nSamizdat-2.pdf`. See pages 251 (note to David Poulin) to 253.

Philippe's Preply

> I stepped down on Jaynes's book on (strongly Bayesian, actually he tells Laplacian!) probabilities, and it does contain a lot of funny and interesting things (plus a few weird ones). Here is one (up to you to decide in which category it fits):
>
> > "Subjective" vs. "Objective"
> >
> > These words are abused so much in probability theory that we try to clarify

> our use of them. In the theory we are developing, any probability assignment is necessarily "subjective" in the sense that it describes only a state of knowledge, and not anything that could be measured in a physical experiment. Inevitably, someone will demand to know: "Whose state of knowledge?" The answer is always: "The robot - or anyone else who is given the same information and reasons according to the desiderata used in our derivations in this Chapter." Anyone who has the same information but comes to a different conclusion than our robot, is necessarily violating one of those desiderata. While nobody has the authority to forbid such violations, it appears to us that a rational person, should he discover that he was violating one of them, would wish to revise his thinking (in any event, he would surely have difficulty in persuading anyone else, who was aware of that violation, to accept his conclusions). Now it was just the function of our interface desiderata (IIIb), (IIIc) to make these probability assignments completely "objective" in the sense that they are independent of the personality of the user. They are a means of describing (or what is the same thing, of encoding) the information given in the statement of a problem, independently of whatever personal feelings (hopes, fears, value judgments, etc.) you or I might have about the propositions involved. It is "objectivity" in this sense that is needed for a scientifically respectable theory of inference.

So: a pure quantum state (my "modality") is "subjective" in the sense that you certainly have to know something (and in particular the "context") to speak about it. But it is "objective" in the sense that every "robot" (or whatever "thinking observer") which has that information can predict results with certainty, repeatedly, and without changing the system.

So is that Jaynes enough to convince a Bayesian that a pure quantum state is "contextually objective"?

With Best Wishes for 2007

09-01-07 *Harsanyi Principle, 2* (to P. Grangier)

Grangierisme 22: *Here the mumbles do not shake me too much. You write:*

> I will agree to your definition of "state of knowledge." But, backtracking from that, an initial state upon which everyone agrees? If one is taking a subjectivist approach (or what I had been calling a "Bayesian approach") to interpreting the quantum state, there is nothing in nature to enforce an initial prior agreement. God does not come down from on high and say to all the agents (i.e., all the observers), "Your starting point shall be the quantum state $|\psi\rangle$." Everyone is left to fend for himself.

But for QM, don't you think that there IS something to "enforce an initial prior agreement", it is simply what I call the "context", which is more or less what most people would call "reality"? (in the classical sense = if you don't believe in the wall, try to kick in it . . .)

So again I am tempted to think that your "extreme subjectivism" denies ALL KINDS of "realities", quantum as much as classical.

Actually, I start liking this idea of "prior knowledge", but in Jaynes' sense, which apparently is not exactly yours (I hoped so!), but is closer to this "Harsanyi doctrine", which has the infamous goal of trying to be "objective" . . .

So we have to try again!

Maybe not, we shall see.

Also let me send you the latest version of my paper with Caves and Schack. (It's quite different than the posted version; can't update it until Schack is back from vacation.) The closing section of it again comes back to the issue of what—in our view—is objective in the quantum world. It is more than nothing, but it is probably less than you are hoping for with your contextual objectivity.

But that's only a short reply. If one will allow "levels of objectivity" maybe the differences between us will disappear after all.

09-01-07 *Talk about Anthropocentric!* (to D. Bacon)

I just enjoyed your anti-Wolfram [`http://dabacon.org/pontiff/?p=1402`] and "Why I'm Not a Bohmist" [`http://dabacon.org/pontiff/?p=1339`] columns with my lunch. Nice. I agree with both points. Particularly your "I do not believe in computers" put a big smile on my face. It has some of the flavor of the de Finetti phrase I co-opted for our quantum Bayesian program. De Finetti said, "Probability does not exist," and I keep advertising "Quantum states do not exist." The point is, for Caves, Schack, a few others, and me, (all) probabilities and quantum states are anthropocentric constructions. To say that is not to give up on the project of figuring out what physics is telling us about the world in itself (i.e., the world without us), but simply to recognize those *particular* mathematical objects for what they are (and what they are has something intrinsically to do with us). Thus, when we say something like that, we're simply saying, "Don't look here" if your question is "What is the universe made of?" Look somewhere else.

Similarly I would say of a computer: It's as anthropocentric of a concept as a hammer or screw driver. A computer is a tool that satisfies a need; it is a *piece* of nature that has been harnessed for an external purpose. There is no more indication that computers are any more likely to be parts of the fundamental constituents of the world than are hammers. You didn't emphasize the anthropocentric aspect of it, but I think your point is pretty much the same.

Let me paste in a couple of pieces of my own writing that I hope will similarly put a smile on your face. The first is a nasty referee report that I wrote on a Bohmian funding proposal. I think the last paragraph of it gets at what really bugs me about what they're doing, and I think it makes much the same point as you were shooting for. The other is a letter I wrote to a professor at Princeton when he pissed me off by saying that what Caves, Schack and I am up to is trying to turn physics into a branch of psychology. I came out swinging! Anyway, he too is one of those people who says "the universe is a computer" and thinks that's saying something significant. Since I view that contrarily as an attempt to anthropocentrize the universe, I was doubly offended when he made the remark about psychology!

09-01-07 *Politically Correct* (to D. Bacon)

Baconism 3: *So every few months I try to come up with some new way of phrasing foundational problems, so that I have something to write about on my blog :) Today I've decided my next one will be "why is a qubit so much like a probabilistic bit?"*

Sounds like a good blog. When you write it send me an announcement, as I don't visit your blog on a regular basis. (Today's visit occurred because I wanted to see what your website name is; I'm thinking I need to get one for myself ... for when Bell Labs eventually lays me off.)

On a technical point, maybe your question should be "why is a qubit so much like two probabilistic bits?" For, if one wanted to think of a qubit's quantum state as a simple probability distribution (for the outcomes of an informationally complete measurement), the minimal event space has to have four outcomes. Similarly, a qudit requires an event space worth $2 \log d$ classical bits. It's probable that you've seen the paper, but in case not, have a look at Rob Spekkens' "toy theory" `http://www.arxiv.org/abs/quant-ph/0401052`. It's just a nice list of similarities between qubits and partially known cbits (maybe I'll call them pcbits). And he, like you, suggests that the place where we're really going to learn something about quantum mechanics is in its deviation, not from cbits (i.e., the event space), but from pcbits (the space of allowed probability distributions over the event space). So there: quantum theory's real value comes in precisely where it is not pc! (Those bad boys of quantum mechanics.) Who would have expected anything else?

09-01-07 *Facts-in-Themselves* (to H. C. von Baeyer)

von Baeyerism 25: *My Fierz translation is on hold. When I planned it, I thought that in retirement I would have nothing else to do. Then retirement turned into an avalanche of other things to do, and besides, Fierz died (though that should really spur me on all the more).*

That is too bad; I hope your conscience will send you back to the project eventually. That's because it's not Fierz, the man, that is important (and so the project should have nothing to do with his death, one way or other), but the set of ideas he and Pauli were exploring. The issue is getting those ideas into the consideration of a wider audience. I'm always about building workforce; what can I do to get you to think that way too?

Thanks for the Russian analysis:

von Baeyerism 26: *The big difference between samizdat and your technique, besides the issue of secrecy, is that the former obliges you to copy, whereas letters oblige you to reply. This principle of reciprocity is really fundamental, and should be reflected in the name of your work. But I have not coined a sexy slogan yet.*

You have me intrigued. I hope one of us can dream up a term.

You're right that reciprocity is really fundamental. It is in letter writing and in the world. Below is the F. C. S. Schiller quote I had promised you (originally from a 1903 Int. J. Ethics paper). I don't agree with it completely, but I like it much for the things I do agree with—particularly the part about reciprocity.

In fact I think I can localize the precise transition in the words that I don't like. He writes, "The simple fact is that we know the Real *as it is when we know it*; we know nothing whatever about what it is apart from that process. It is meaningless therefore to inquire into its nature as it is in itself." Particularly, I agree with the first sentence, while disagreeing with the second. I will agree that quantum measurements reveal nothing of the thing-in-itself, nor do they even generate facts-in-themselves—they are "facts for the agent." I think Schiller's description is quite nice in getting that kind of idea across. It's another take on the Paulian alchemical idea. But what of the Born rule for transforming probabilities from measurement to measurement? Of that, I would say, when we postulate it, we are taking a guess about some property of the world—a property of the world as it is in itself. It's an indirect statement, but it's a statement nonetheless. What's left is figuring out the precise nature of that guess (and its rather tight connection to some of the other things in Schiller's paragraphs!).

That the Real has a determinate nature which the knowing reveals but does not affect, so that our knowing makes no difference to it, is one of those sheer assumptions which are incapable, not only of proof, but even of rational defence. It is a survival of a crude realism which can be defended only, *in a pragmatist manner*, on the score of its practical convenience, as an avowed fiction. In this sense and as a mode of speech, we need not quarrel with it. But as an ultimate analysis of the fact of knowing it is an utterly gratuitous interpretation. The plain fact is that we can come into contact with any sort of reality only in the act of 'knowing' or experiencing it. As *unknowable*, therefore, the Real is *nil*, as *unknown*, it is only potentially real. What is there in this situation to sanction the assumption that what the Real *is* in the act of knowing, it is also outside that relation? One might as well argue that because an orator is eloquent in the presence of an audience, he is no less voluble in addressing himself. The simple fact is that we know the Real *as it is when we know it*; we know nothing whatever about what it is apart from that process. It is meaningless therefore to inquire into its nature as it is in itself. And I can see no reason why the view that reality exhibits a rigid nature unaffected by our treatment should be deemed theoretically more justifiable than its converse, that it is utterly plastic to our every demand—a travesty of Pragmatism which has attained much popularity with its critics. The actual situation is of course a case of interaction, a process of cognition in which the 'subject' and the 'object' determine each other, and both 'we' and 'reality' are involved, and, we might add, *evolved*. There is no warrant therefore for the assumption that either of the poles between which the current passes could be suppressed without detriment. What we ought to say is that when the mind 'knows' reality both are affected, just as we say that when a stone falls to the ground both it and the earth are attracted.

We are driven, then, to the conviction that the 'determinate nature of reality' does *not* subsist 'outside' or 'beyond' the process of knowing it. It is merely a half-understood lesson of experience that we have enshrined in the belief that it does so subsist. Things behave in similar ways in their reaction to modes of treatment, the differences between which seem to us important. From this we have chosen to infer that things have a rigid and unalterable nature. It might have been better to infer that therefore the differences between our various manipulations must seem unimportant to the things.

The truth is rather that the nature of things is not *determinate* but *determinable*, like that of our fellow-men. Previous to trial it is indeterminate, not merely for our ignorance, but really and from every point of view, within limits which it is our business to discover. It grows determinate by our experiments, like human character. We all know that in our social relations we frequently put questions which are potent in determining their own answers and without the putting would leave their subjects undetermined. 'Will you love me, hate me, trust me, help me?' are conspicuous examples, and we should consider it absurd to argue that because a man had begun social intercourse with another by knocking him down, the hatred he had thus provoked must have been a pre-existent reality which the blow had merely elicited. All that the result entitles us to assume is a capacity for social feeling variously responsive to various modes of stimulation. Why, then, should we not transfer this conception of a determinable indetermination to nature at large, why should we antedate the results of our manipulation and regard as unalterable facts the reactions which our ignorance and blundering provoke? To the objection that even in our social dealings not all the responses are indeterminate, the reply is that it is easy to regard them as having been determined by earlier experiments.

10-01-07 *Hyle* (to A. Shimony)

I'm very happy to hear that you are OK. I did start to get worried because the silence seemed a little unlike you.

Shimonyism 11: *I suggest that you read certain of my epistemological papers which are relevant but perhaps not familiar to you: In my collection* Search for a Naturalistic World View *vol. 1 (Cambridge U. Press 1993), papers 1, 2, 3, 9 (sections 3, 4, 5), and 10; and "Some Intellectual Obligations of Epistemological Naturalism" in* Reading Natural Philosophy*, ed. by David Malament.*

I will do. In fact, I just ordered your book from Amazon.com. They say it will ship by tomorrow.

The last two weeks I've been reading F. C. S. Schiller in preparation for the talk I'm giving at the "Pragmatism and Quantum Mechanics" meeting in Paris Feb 22–23. (I hope I do much better than I did in my talk in Waterloo for you this year.) Now he is an obscure philosopher!—I wonder if you've ever read him at all—but I find that I'm even more attuned to him than James or Dewey ... probably why he is a forgotten philosopher. He too was a fan of Aristotle's hyle, but in a much different way than Peirce.

10-01-07 *Anti-Algebra, the Reprise* (to V. Palge)

I'm sorry to take so very long to reply to your email. I'm behind on everybody's email.

Palge-ism 2: *It seems that one of the main reasons you reject a well-structured event space is that it assumes a realistic interpretation: its elements would correspond to intrinsic properties of quantum systems (either possessed by systems before a measurement or being generated by a measurement). However, I wonder if one is forced to such realistic commitment. Can't one understand the e.g. partial Boolean event space in an instrumentalistic and hence more Bayesian spirit? Can't one just take the elements as corresponding to clicks and blips in the measurement apparatus? What I have in mind is Pitowsky's 2003 ("Betting on the outcomes of measurements: a Bayesian theory of quantum probability," SHPMP,* **34***). As you probably know, he assumes that quantum event space is a partial Boolean algebra and then lays down the rules a Bayesian must follow in a world whose event space ultimately has this structure.*

In short, I am wondering if your argument applies to the Pitowskian Bayesian picture which is free from a realistic understanding of properties?

The reason this issue seems important to me is that I believe eventually a Bayesian should say something about the structure of the event space—this seems like a minimal condition for calculating probabilities. And what this structure is has interesting implications on further issues in QM.

Let me start with your first sentence: "It seems that one of the main reasons you reject a well-structured event space is that it assumes a realistic interpretation: its elements would correspond to intrinsic properties of quantum systems." That is the wrong direction of reasoning—though I am probably the cause of this misimpression through the restrictive choices of readings I recommended to you. It is not that I reject a well-structured event space because it *assumes* its elements would correspond to intrinsic properties of quantum systems, but rather this is the *result* of a thoroughgoing subjective interpretation of probabilities within the quantum context. What cannot be forgotten is that quantum-measurement outcomes, by the usual rules, *determine* posterior quantum states. And those posterior quantum states in turn determine further probabilities.

Thus, if one takes the timid, partial move that Itamar and Jeff Bub, say, advocate—i.e., simply substituting one or another nonBoolean algebra for the space of events, and leaving the rest of

Bayesian probability theory seemingly intact—then one ultimately ends up re-objectifying what had been initially supposed to be subjective probabilities. That is: When I look at the click, and note that it is value i, and value i is rigidly—or I should say, *factually*—associated with the projector Π_i in some nonBoolean algebra, then I have no choice (through Lüders rule) but to assign the posterior quantum state Π_i to the system. This means the new quantum state Π_i will be as factual as the click. And any new probabilities (for the outcomes of further measurements) determined from this new quantum state Π_i will also be factual.

So, the starting point of the reasoning is to *assume* that there is a category distinction between probabilities and facts (this is the subjectivist move of de Finetti and Ramsey). Adding the ingredient of the usual rules of quantum mechanics, one derives a dilemma: If there is a rigid, factual connection between the clicks i and elements Π_i of an algebraic structure, then probabilities are factual after all. Holding tight to my assumption of a category distinction between facts and probabilities, I end up rejecting the idea that there is a unique, factual mapping between i and Π_i.

Let me point you to two papers of mine that emphasize (and make a little more formal) this kind of reasoning. The first is `quant-ph/0404156`, and the particular place to look in it is Section VI, starting on page 22. The second is attached to this note. It is a fairly drastic revision of `quant-ph/0608190` that we haven't posted yet (can't re-post it until Rüdiger gets back from vacation with the passwords). In large part, this paper is on the very point we are discussing here. Finally, let me recommend an excellent recent paper by Matt Leifer that extends these points: `quant-ph/0611233`.

But let me return to your paragraph before signing off. If I read this question of yours in isolation: "Can't one just take the elements as corresponding to clicks and blips in the measurement apparatus?" Then at one level my answer is, "Of course; I've never said otherwise." What is at issue here is whether the events—the clicks and blips themselves—fall within the kind of algebraic structure you speak of, or whether it is something else (something a conceptual layer above the events) that falls within it. From the CFS point of view, for a single device with clicks i, one agent might associate the clicks with a set of orthogonal projectors Π_i, and another agent might associate them with a set of noncommuting effects (i.e., POVM elements) E_i. This was the sense in which I meant there is no stand-alone event space at all: For us, the algebraic structure of the events (the clicks and blips), their level of commutivity or noncommutivity and whatnot, is just as subjective as the quantum state. The clicks i themselves are objective (in the sense of not being functions of the subject's beliefs or degrees of belief), but their association with a particular set of operators E_i is a subjective judgment.

I hope this completely answers your question now.

But let me extend the discussion a little to try to give you a more positive vision of what we're up to. The starting point is the category distinction between facts and probabilities applied to the quantum measurement context. From this we glean that quantum operations and quantum states are of the same level of subjectivity. But that is not our ending point. Because, implicit in everything we have said there are these autonomous, realistically-interpreted quantum systems: The agent has to interact with the quantum system to receive his quantum measurement outcome. No quantum system, no measurement outcome. Thus the CFS position is more than a kind of positivism or operationalism. The objects of the external world with which we interact have a certain kind of active power, and we become aware of the presence of that active power particularly in the course of quantum measurement. When we kick on a quantum system it surprises us with a kick back.

Can we say anything more explicit about the active power? Can we give it some mathematical shape? Yes, I think we can, but that is a research project. Still, I think the hints for it are already in place ... and indeed they are in the quantum formalism, as we would expect them to be. One of the hints is this: The Born transformation rule. When we make probability assignments in quantum mechanics, we are assuming more than de Finettian / Ramseyian coherence. We assume that if we set the probabilities for the outcomes of this measurement this way, then we should set the probabilities for the outcomes of that measurement that way and the rule of transformation is a linear one. This, from the CFS view, is the content of the Born rule (see the last section in the attached paper). And it is empirical, contingent: A different world than the one we live in might have had a different transformation rule. So, if we're looking for something beyond personal probabilities in quantum mechanics, that is a point to take seriously. It hints of some deep property of our world, and I'd like to know what that property is.

10-01-07 *Anti-Algebra, the Moral* (to V. Palge)

I walked into the hall to get a glass of water, and immediately felt that I had not properly clenched off the last two paragraphs of my last note. There were several points made there, but among them is this, and I just want to be explicit about it:

Though CFS banish the algebraic structure of Hilbert space from having anything to do with a fundamental event space (and in this way their quantum Bayesianism differs from the cluster of ideas Pitowsky and Bub are playing with), they do not banish the algebraic structure from playing any role whatsoever in quantum mechanics. It is just that the algebraic structure rears its head at the conceptual level of coherence rather than in a fundamental event space. It is not that potential events are objectively tied to together in an algebraic way, but that our gambling commitments (normatively) *should be*. This is another point of contact between my view and Bill's.

10-01-07 *Mulling* (to N. D. Mermin)

I've been mulling over your letters "State Preparation" and "The Grangier Letter" from 1/2/07 for several days now, but the more I think about it, I don't know how to respond in any detail until I hear your thoughts on my note "David Signals" from 1/3/07. Particularly, much will hinge on you respond to the part that starts off, "However, there's a still more worrisome issue here."

I know it's getting very close to your leaving for vacation; so I am not betting strongly to hear back from you on the subject for quite a while (probably not till you're visiting Princeton). Basically, you just give me the signal when you're ready (by answering those questions), and we'll pick back up where we left off.

At the moment I'll only observe this much, with regard to these two things you said:

Merminition 165: *You have to understand that much of my attitude towards quantum foundations these days really does grow out of the past six years of teaching the subject to computer scientists. From this narrow perspective quantum mechanics is a set of rules enabling us to describe important aspects of the operation of a particular bounded, isolated, piece of machinery.*

and

Merminition 166: *And it wipes out the beautiful symmetry between measurement gates being crucial at both the start and the end of the computation, which I can't help think is telling us something (about correlations between aspects of our experience.)*

In some ways, you seem to be roughly where I was when I started writing that ill-fated piece for Physics Today with Asher in 1999—basically the position of Asher in his 1980s paper, "What

Is a State Vector?" (Does this mean I've devolved since then?) Anyway, I paste in a bit of the correspondence I had with Asher at the point when I started to turn away from the master.

Best wishes, and stay clear of the lava,

Excerpt of: 15-11-1999, to A. Peres, "Have a Good Trip"

One point that we may need some private side discussion on before we set it in stone, is captured among other places in your sentence:

Asherism 68: *The notion "state" refers to a method of preparation, it is not an intrinsic property of a physical system.*

In general I have noticed in this manuscript that you lean more heavily on the word "preparation" than we did in our letter to Benka. (In fact, I can't find any mention at all of the word "preparation" in that letter.) Unless I misunderstand your usage of the word, it may actually be a little too anthropocentric even for my tastes. The problem is this: consider what you wrote in the paragraph about the wave function of the universe. It seems hard to me to imagine the wave function of those degrees of freedom which we describe quantum mechanically as corresponding to a "preparation." Who was the preparer?

It is for this reason that Carl Caves and I prefer to associate a quantum state (either pure or mixed) solely with the compendium of probabilities it generates, via the Born rule, for the outcomes of all potential measurements. And then we leave it at that. Knowing the preparation of a system (or the equivalence class to which it belongs) is one way of getting at a set of such probabilities. But there are other ways which surely have almost nothing to do with a preparation. An example comes about in quantum statistical mechanics: when the expected energy of a system is the only thing known, the principle of maximum entropy is invoked in order to assign a density operator to the system. There may be someone beside me in the background who knows the precise preparation of the system, but that does not matter as far as I am concerned—my compendium of probabilities for the outcomes of all measurements are still calculated from the MaxEnt density operator.

To help ensure that I was not jumping to conclusions on your usage of the term, I reread today your paper "What is a state vector?" [AJP **52** (1984) 644–650]. There was a time when I agreed with everything you wrote there (in fact, I think it was the first paper with which I got to know you). But as of today at least, I think a more neutral language as in our letter to Benka is more appropriate. [...]

Excerpt of: 20-10-2000, to P. Benioff, "Not Instructions, but Information"

Benioffism 2: *If I recall correctly you wrote a paper with Peres on QM without interpretation but stated that a quantum state corresponds to an algorithm for preparing it.*

In this, though, you're confusing two things. Asher wrote a paper in the early 1980s for AJP titled "What is a State Vector?" and in that paper he took the point of view you mention. That is, that a quantum state corresponds to nothing more than the (equivalence class of) instructions for preparing a system one way or the other. I've never felt completely comfortable with that point of view, so in the paper that Asher and I co-wrote for *Physics Today* last year we worked around that. I think also that I may have even swayed Asher's opinion on this issue, but you would have to check with him directly. (Perhaps I'll just carbon copy this letter to Asher.) Here's how I put it in a note to Asher 15 November 1999: [...]

I believe this point of view is adequately expressed in the article Asher and I wrote for *Physics Today*: a quantum state is nothing more and nothing less than one's best (probabilistic) information on how a system will react to our experimental interactions with it. How we may have come by that information—be it through a preparation, through a sheer guess based on all available evidence, or the principle of maximum entropy—is something I view as largely outside quantum theory proper. The structure of quantum theory instead codifies how we should manipulate our information (this is what time evolution and the collapse rule is about) and enumerates the varied ways with which we may gather new information (this is what the structure of observables or POVMs is about).

In this sense, I would call what we are talking about an "information interpretation" or "Bayesian interpretation" of quantum theory, rather than an "algorithmic interpretation." For the most part, however, I would like to avoid the wording of an interpretation.

I will go further—and this is one point where I may diverge from Asher—and say that I do suspect that we will one day be able to point to some ontological content within quantum theory. But that ontological statement will have more to do with our interface with the world—namely that in learning about it, we change it—than with the world itself (whatever that might mean).

10-01-07 *Pragmatism in Paris* (to A. Grinbaum)

Grinbaumism 2: *First, all the best wishes for 2007! It's been long time since we talked–hope all is well. I was told that you'll be in Paris around February 22 for a seminar: what are the exact dates? It would be wonderful if you had time to talk and perhaps visit our new foundations of physics lab at the CEA, LARSIM, where I now work.*

Good to hear from you. That's quite interesting: A nuclear physics research center investing in quantum foundations?! That's very nice. And congratulations on your new position. Is it permanent? Or is it a limited appointment? I hope it is the former and affords you much leisure to do good work.

I will arrive in Paris just in time to make it to the start of the meeting, Feb 22. I'll be coming from London with Schack on a Eurostar. Then I fly directly out of Paris on the 25th. Thank you on your kind invitation to visit your lab, but I think I will take a rain check on it this time. It is true that I'll be in Paris an extra day beyond the meeting, but I had already drawn up grand plans for the day: To walk around the city all alone and use its lonely sounds to try to think deeply and possibly provoke some new thoughts in my head.

Happy new year to you too, and see you in February.

11-01-07 *Pyroclastic Surge* (to N. D. Mermin)

Merminition 167: *The hope is that quantum foundations will be clarified by a spectacular sunset and not by a pyroclastic surge. But my main reading project is* Origin of Species*, which I somehow missed growing up.*

Worthy book. Read it, and then take all those ideas to physics. Might I recommend as supplementary material:

Database:	Cornell University Library
Author/Creator:	Wiener, Philip Paul, 1905–
Title:	Evolution and the founders of pragmatism / by Philip P. Wiener ; with a foreword by John Dewey.
Published:	Cambridge, MA : Harvard University Press, 1949.
Description:	xiv, 288 p. ; 25 cm.
Location:	Library Annex
Call Number:	B818 .W64
Status:	Not Charged
Location:	Uris Library
Call Number:	B818 .W64
Status:	Not Charged

11-01-07 *Matter, Money, and Change* (to A. Shimony)

Shimonyism 12 : *I am pleased that you have ordered my papers. I felt frustrated engaging in a written debate with you when my full position cannot be presented briefly, and when I have already spent years trying to present it adequately. After you have read the papers you may be convinced by me, or you may have cogent criticisms which will persuade me.*

Yes, you are probably right that that is a more efficient method. By the way, your book only cost me $13.29 (and is supposed to be in "essentially new" condition). Since my Mom sent me $20 for Christmas, I still have almost $7 left! Thanks for providing the form to the hyle, and thanks for the change!

12-01-07 *Out of the Blue* (to H. Halvorson)

I wonder: 1) if I can ask a favor of you that is surely beneath a professor, and 2) if I might have a conversation of deeper value with you as I pick up on favor 1 (if indeed you take up that task)? (I'll explain below.)

First the favor to ask: I'm giving a talk at the "Pragmatism and Quantum Mechanics" meeting in Paris, Feb 22–23, titled, "William James, F. C. S. Schiller, and the Quantum Bayesians," and partly because of that I'm doing everything I can to strengthen my knowledge of Schiller. It's not easy, as he's a pretty obscure philosopher—though you can guess he stirs me like no one else!—and particularly I haven't been able to get my hands on one of his most important papers: "Axioms as Postulates." This is the favor that's beneath a professor, but I wonder if I can ask it of you anyway (for a friend)? The Princeton Library has a copy of the volume that contains the paper, but it's archived at some off-site facility. Could I ask you to recall the volume from there?

If you wouldn't mind doing that, I could drop by and copy the paper one day next week.

13-01-07 *Conference "Pragmatisme et Mécanique Quantique"* (to M. Bächtold)

Thanks for making the reservation for me. I'll forward your note to Schack; I'm sure the accommodation will be fine with him.

One thing about the program: The title of my talk should be "William James, F. C. S. Schiller, and the Quantum Bayesians" rather than the original title you have listed. (I sent you a note earlier changing the title, but maybe you didn't get it.) The talk will be on an updated, quantum mechanical rendering of this quote of Schiller:

> That the Real has a determinate nature which the knowing reveals but does not affect, so that our knowing makes no difference to it, is one of those sheer assumptions which are incapable, not only of proof, but even of rational defence. It is a survival of a crude realism which can be defended only, *in a pragmatist manner*, on the score of its practical convenience, as an avowed fiction. In this sense and as a mode of speech, we need not quarrel with it. But as an ultimate analysis of the fact of knowing it is an utterly gratuitous interpretation. The plain fact is that we can come into contact with any sort of reality only in the act of 'knowing' or experiencing it. As *unknowable*, therefore, the Real is *nil*, as *unknown*, it is only potentially real. What is there in this situation to sanction the assumption that what the Real *is* in the act of knowing, it is also outside that relation? One might as well argue that because an orator is eloquent in the presence of an audience, he is no less voluble in addressing himself. The simple fact is that we know the Real *as it is when we know it*; we know nothing whatever about what it is apart from that process. It is meaningless therefore to inquire into its nature as it is in itself. And I can see no reason why the view that reality exhibits a rigid nature unaffected by our treatment should be deemed theoretically more justifiable than its converse, that it is utterly plastic to our every demand—a travesty of Pragmatism which has attained much popularity with its critics. The actual situation is of course a case of interaction, a process of cognition in which the 'subject' and the 'object' determine each other, and both 'we' and 'reality' are involved, and, we might add, *evolved*. There is no warrant therefore for the assumption that either of the poles between which the current passes could be suppressed without detriment. What we ought to say is that when the mind 'knows' reality both are affected, just as we say that when a stone falls to the ground both it and the earth are attracted.
>
> We are driven, then, to the conviction that the 'determinate nature of reality' does *not* subsist 'outside' or 'beyond' the process of knowing it. It is merely a half-understood lesson of experience that we have enshrined in the belief that it does so subsist. Things behave in similar ways in their reaction to modes of treatment, the differences between which seem to us important. From this we have chosen to infer that things have a rigid and unalterable nature. It might have been better to infer that therefore the differences between our various manipulations must seem unimportant to the things.
>
> The truth is rather that the nature of things is not *determinate* but *determinable*, like that of our fellow-men. Previous to trial it is indeterminate, not merely for our ignorance, but really and from every point of view, within limits which it is our business to discover. It grows determinate by our experiments, like human character. We all know that in our social relations we frequently put questions which are potent in determining their own answers and without the putting would leave their subjects undetermined. 'Will you love me, hate me, trust me, help me?' are conspicuous examples, and we should consider it absurd to argue that because a man had begun social intercourse with another by knocking him down, the hatred he had thus provoked must have been a pre-existent reality which the blow had merely elicited. All that the result entitles us to assume is a capacity for social feeling variously responsive to various modes of stimulation. Why, then, should we not transfer this conception of a determinable indetermination to nature at large, why should we antedate the results of our manipulation and regard as unalterable facts the reactions which our ignorance and blundering

> provoke? To the objection that even in our social dealings not all the responses are indeterminate, the reply is that it is easy to regard them as having been determined by earlier experiments.
>
> In this way, then, the notion of a 'fact-in-itself' might become as much of a philosophic anachronism as that of a 'thing-in-itself,' and we should conceive the process of knowledge as extending from absolute chaos at the one end (before a determinate response had been established) to absolute satisfaction at the other, which would have no motive to question the absolutely factual nature of its objects. But in the intermediate condition of our present experience all recognition of 'fact' would be provisional and relative to our purposes and inquiries.

It captures a good piece of what we quantum Bayesians are up to. But it also contrasts some—for we would say that the quantum formalism is telling us *something* of the world in itself. Below is how I wrote Veiko Palge on the subject recently. [See 10-01-07 note "Anti-Algebra, the Reprise," to V. Palge.] Anyway, that's what the talk will be about, and I'll tie it all in with a) the quantum formalism, and b) with William James too.

16-01-07 *Axioms as Postulates* (to H. Halvorson)

I don't know if you looked any at the document you copied, but already in the outline, Section 1, I love it! Matter is the resisting medium. That is just the right conception, I think, for a quantum system.

Read it as a novel (not for literal truth, but for inspiration) as you get a chance!

16-01-07 *Don't Forget Schiller!* (to H. Price)

I've only read the first page of your Federation Fellow proposal, but in that little bit I already like it much. And said to myself, "I've got to print this out." In the meantime, I'll fly this note off to you.

When I read your listing of Peirce, James, and Dewey, I thought, "Don't forget Schiller!" My recent fascination with his thinking is the partial source of the title of my last note. I'm now starting to surmise that his take on pragmatism perhaps better tacks with what I'm aiming for in quantum mechanics than even James. We shall see.

Anyway, two notes below on my plans with Schiller that you may enjoy. Particularly the long Schiller quote itself at the bottom. [See 03-01-07 note "New Year's Alchemy" and 09-01-07 note "Facts-in-Themselves" to H. C. von Baeyer.]

I'll head toward the printer now.

16-01-07 *The Information *Was* in the Warning* (to H. Price)

I just finished the "Aims" section, and I'm still with you. Very nice. Now, I'm off to the "Background" section.

I must have shown you this montage I put together once, playing on the words of Tilgher (some early Italian pragmatist, I think):

> A quantum state is not a mirror in which a reality external to us is faithfully reflected; it is simply a biological function, a means of orientation in life, of preserving and enriching it, of enabling and facilitating action, of taking account of reality and dominating it.

17-01-07 *Ego* (to A. R. Calderbank)

It's of no consequence now, but let me just record this for my own satisfaction. As I was driving home this evening, reviewing the day in my head, I thought: "Wow did I ever undersell myself with that line about how 'my main interest in a cross appointment arose because I'm neither a great mathematician, nor a great philosopher, but a little good at each'." Rest assured, I think much more of myself than that! Particularly, I'm quite confident that the school of thought in quantum foundations I am building up around the world is deeply significant and paving the way for the next big step in physics. All I really meant to imply is that I fit no department per se, and these are the closest fits. But I don't doubt for a minute that I would inspire the students around me to do some dazzling work in these fields (by those departments' own standards).

Thanks, by the way, for talking to me. Your clarity was much appreciated. As we can schedule it (with my fairly full travel schedule this Spring), I'd be more than happy to give a seminar for you guys.

18-01-07 *Pragmatism Old and New* (to R. E. Slusher)

Keeping with the idea of 'something old, something new . . . ', I thought you might like to take this old book with you down to Georgia. Certainly you'll need pragmatism (with a little p) to make big things happen in your lab. But perhaps the book will also give you a chance to reflect on how *Pragmatism* (with a big P) finds its best argument in our quantum world.

Good luck, and many more years of good science from you.

29-01-07 *Toy Model, 1* (to S. J. van Enk)

Well, I've finally gotten your paper printed out. I'll be taking it to Toy Model Central (i.e., PI) with me on my flight tomorrow. I want to understand this. . . . [some days]

Now, I've started putting work into understanding your paper. I still think it's nice (very nice actually), but I'm not sure to what extent it has a claim to being called a toy model for epistemic states. Once the "probabilities" come out of the "information theoretic principle" (both quotes have to be taken seriously now, because those words cannot be interpreted literally any more) as having to be negative, they lose their epistemic meaning. They're something else now.

I'm thinking, though . . .

30-01-07 *Polchinski Story* (to L. Smolin)

As it turns out, I just finished reading your new book as I landed in Toronto today. (Literally so! I was reading the last paragraph of the last page as the airplane wheels hit the pavement.) I enjoyed it and got a lot out of it, particularly the chapters on "the anthropic solution" and "what is science." I also thought your last chapters made a powerful argument for the place and style of foundational work—nice.

Let me tell you a story about Joe Polchinski that one of your pages brought back to mind. On page 137, you write:

> . . . from 1984 to 1995, we were like amateurs . . . [missing] most of what was necessary to make the system work until Polchinski discovered the missing essentials. . . . In the fall of 1995, Polchinski showed that a string theory, to be consistent, must include not

> only strings but surfaces of higher dimensions moving in the background space. ... If a string, which is a one-dimensional object, can be fundamental, why can't a two-dimensional surface, be fundamental? In higher dimensions, where there is a lot of room, why not a three-, four-, or even five-dimensional surface?

Well, sometime around 1985 or 1986 ('87 at the latest), I was at a physics colloquium at the University of Texas and Joe Polchinski was the speaker. Steven Weinberg was in the first or second row of the auditorium in his usual seat. Polchinski talked about strings. What was funny was that early in the talk, Weinberg interrupted to ask, why stop with strings? Why not two- or higher dimensionsal objects? And, in particular, he ended, "Once you move away from the particle concept, why not just entertain a universe filled with gummy bears?" Polchinski at first just paced back and forth on the stage, looking down ... a bit of a silence. Then Weinberg said, "I'm not being facetious! Why not gummy bears? What in the theory constrains you to strings?" The audience, of course, laughed. And I remember Polchinski saying, "I know you're not being facetious." However, I don't remember his answer beyond that. Just a cute story you might like (and I might record).

31-01-07 *Nature Giving a Flip* (to me)

> "Perhaps nature doesn't give a flip about such questions, because c is finite."

Good point. John Wheeler used to say of counterfactual games like this (at least with quantum mechanics): "Why talk of them? They're worlds that never were and never could be."

01-02-07 *Metaphysics of the Time Process* (to L. Smolin)

Here's some words from the paper I was telling you about, F. C. S. Schiller, "The Metaphysics of the Time-Process," originally published in Mind, 1895.

The abstract to the paper reads:

> Significance of Dr. McTaggart's admission that the Hegelian Dialectic cannot explain the reality of succession 'in Time.' The reason of its failure, viz. that Time, Change, and Individuality are features of Reality we abstract from in our formation of Concepts. Hence abstract metaphysics always fail to account for Reality. Must we then either accept scepticism or reject a procedure on which all science rests? No; for to admit the defects of our thought-symbols for reality need merely stimulate us to improve them. As for science, it uses abstractions in a radically different way, to test and to predict experience. Thus 'law' is a methodological device for practical purposes. Science practical both in its origin and in its criterion, and ethics as the science of ends conditions metaphysics. Such an ethical metaphysic accepts and implies the reality of the Time-process. And *therefore* it has a right to look forward to the realization of its ends in time, and forms the true Evolutionism.

More to the point with regard to yesterday's considerations after Tegmark's talk are these. (In the long quote Schiller says about the same thing five times over, but I think I'll record it all in case there are some subtle distinctions I'm missing at the moment.)

> [T]he incompatibility between the assertion of the reality of the Time-process and its comprehension by any system of 'eternal' logical truth (whether Hegel's or any one else's) has its origin in very simple and obvious considerations.

Dr. McTaggart cannot find room for the reality of the Time-process, i.e. of the world's changes in time and space, within the limits of Hegel's Dialectic. But is this an exclusive peculiarity or difficulty of Hegel's position? Is the Time-process any more intelligible on the assumptions of any other purely logical[118] system, as, for instance, on those of Plato or Spinoza? I think the difficulty will be found to recur in all these systems. And this shows that it is not accidental, but intrinsic to the *modus operandi* of all systems of abstract metaphysics.

They cannot account for the time-factor in Reality, because they have *ab initio incapacitated* themselves from accounting for Time as for change, imperfection and particularity—for all indeed that differentiates the realities of our experience from the ideals of our thought. And their whole method of procedure rendered this result inevitable. They were systems of abstract truth, and based on the assumption on which the truth of abstraction rests. They aimed at emancipating philosophy from the flux to which all human experience is subject, at interpreting the world in terms of conceptions, which should be true not here and now, but 'eternally' and independently of Time and Change. Such conceptions, naturally, could not be based upon probable inferences from the actual condition of the world at, or during, any time, but had to be derived from logical necessities arising out of the eternal nature of the human mind as such. Hence those conceptions were necessarily *abstract*, and *among the things they abstracted from was the time-aspect of Reality.*

Once abstracted from, the reference to Time could not, of course, be recovered, any more than the individuality of Reality can be deduced, when once ignored. The assumption is made that, in order to express the 'truth' about Reality, its 'thisness,' individuality, change and its immersion in a certain temporal and spatial environment may be neglected, and the timeless validity of a conception is thus substituted for the living, changing and perishing existence we contemplate. ...

The true reason, then, why Hegelism can give no reason for the Time-process, i.e. for the fact that the world is 'in time,' and changes continuously, is that it was constructed to give an account of the world irrespective of Time and Change. If you insist on having a system of eternal and immutable 'truth,' you can get it only by abstracting from those characteristics of Reality, which we try to express by the terms individuality, time, and change. But you must pay the price for a formula that will enable you to make assertions that hold good far beyond the limits of your experience. And it is part of the price that you will in the end be unable to give a rational explanation of those very characteristics, which you dismissed at the outset as irrelevant to a rational explanation. Thus the whole contradiction arises from a desperate attempt to eat one's cake and yet have it, to secure the eternal possession of absolute truth and yet to profit by its development in time! ...

If these considerations are valid, the idea of accounting for the time-process of the world on any system of abstract metaphysics is a conceptual jugglery foredoomed to failure, and must be declared mistaken in principle.

02-02-07 *Something Fun* (to C. H. Bennett)

I wonder if you'd do something fun for me. I'm visiting the Perimeter Institute, and I've been bragging about how their cafeteria could take some lessons from IBM Research's cafeteria.

[118]I.e. intellectualist.

Particularly the little paper and pencil boxes you have sitting on the tables. (It came to me as I was having great difficulty writing on a napkin.) Would you take a nice picture of one of them, or show how they're situated on the tables, so I can make a case to the people here that they should do the same thing.

Wish you were up here; I'm having a great time, just hanging out and being the old Chris again.

03-02-07 *Bill's Thoughts on QL and QI Frameworks* (to W. G. Demopoulos)

Demopoulosism 8: *I've had an idea which I would like to run by you. It was prompted by several things: your exchange with Veiko Palge, a re-reading of some of our previous correspondence, and some more general reflections suggested by work of Lucien and others. (Among "others," I ran across two very suggestive papers of Colin Howson on Ramsey on probability and logic which I can send you the details of if you're interested.)*

Yes, I would like to know what you're talking about here. I remember being extremely disappointed when I first met Howson at the LSE a couple of years ago. For despite his Bayesian credentials—I had cut my eyeteeth on his book with Urbach—he seemed to hold fast to the idea that subjective probability *had to be* supplemented with objective chance … and for the silliest of reasons: To explain how predictable frequencies could come about. I was taken aback! In case, just in case, you harbor your own reservations about this issue, let me recommend Marcus Appleby's papers "Facts, Values and Quanta," and "Probabilities Are Single-Case, or Nothing," `quant-ph/0402015` and `quant-ph/0408058`, respectively. He makes the argument there about as decisive as I've ever seen it.

Demopoulosism 9: *What I have to say is pretty abstract and at the level of the "framework" which I believe is implicit in your approach, and I think that for just this reason it may clarify a little further the connection between us.*

You definitely get several of the elements right, I would say. Though I do have some quibbles, as you'll see below. Of course, these may just correspond to parts of the framework you reject. We'll see.

Demopoulosism 10: *I want to contrast what I'll call the quantum logical (QL) and quantum informational frameworks (QI), where by a framework I mean something very general, having to do with the language or conceptual structure within which one seeks to formulate things, rather than any particular thesis. One thing that I think emerges from looking at things in this way is that it makes it clear that realism vs. anti-realism is not an issue, and I think it also shows that some of the classical foundational issues, like whether values of observables are created on measurement, are also not to the point.*

Well, I certainly agree with the first part of this sentence, i.e., "that realism vs. anti-realism is not an issue," but the jury for me remains out on the second issue … at least until I better understand precisely what you mean by "values of observables are created on measurement."

Demopoulosism 11: *It may raise the question of instrumentalism, but it does so in a sense that doesn't contrast with realism.*

I've always been impressed by this passage of Richard Rorty's (from the introduction to his volume, *Objectivity, Relativism, and Truth: Philosophical Papers Volume 1*):

> The six papers that form Part I of this volume offer an antirepresentationalist account of the relation between natural science and the rest of culture. By an antirepresentationalist account I mean one which does not view knowledge as a matter of getting reality right, but rather as a matter of acquiring habits of action for coping with reality.
>
> Philosophers in the English-speaking world seem fated to end the century discussing the same topic—realism—which they were discussing in 1900. In that year, the opposite of realism was still idealism. But by now language has replaced mind as that which, supposedly, stands over and against "reality." So discussion has shifted from whether material reality is "mind-dependent" to questions about which sorts of true statements, if any, stand in representational relations to nonlinguistic items. Discussion of realism now revolves around whether only the statements of physics can correspond to "facts of the matter" or whether those of mathematics and ethics might also. Nowadays the opposite of realism is called, simply, "antirealism."
>
> This term, however, is ambiguous. It is standardly used to mean the claim about some particular true statements, that there is no "matter of fact" which they represent. But, more recently, it has been used to mean the claim that *no* linguistic items represent *any* nonlinguistic items. In the former sense it refers to an issue within the community of representationalists—those philosophers who find it fruitful to think of mind or language as containing representations of reality. In the latter sense, it refers to antirepresentationalism—to the attempt to eschew discussion of realism by denying that the notion of "representation," or that of "fact of the matter," has any useful role in philosophy. Representationalists typically think that controversies between idealists and realists were, and controversies between skeptics and antiskeptics are, fruitful and interesting. Antirepresentationalists typically think both sets of controversies pointless. They diagnose both as the results of being held captive by a picture, a picture from which we should by now have wriggled free.

At least when it comes to quantum states, I know that I am thinking of them in an antirepresentationalist way. Thus, the issue of realism vs. antirealism with regard to the quantum-Bayesian view (which asserts outright that quantum states are subjective) is just off the mark.

Demopoulosism 12: *I'm imposing this on you as a way of getting my head around some part of what I will say in Maryland in April, so I hope I'm catching you at a time when you are able to send me your reactions.*

The idea of QI that I want to focus on is its rejection of the QL framework of propositions and their structure in favor of an emphasis on what I'll call "effects," a term whose significance will I hope become clear.

I include with QI's rejection of the propositional framework all related and to my mind equivalent ideas. I know that you sometimes distinguish between propositions which are true or false vs. events which happen or not, just as one also distinguishes both of these categories from properties which hold or fail to hold, but I can't really see any basis for marking such distinctions and see them all as of one piece.

I don't know if this will help, but let me at this point include something I wrote to Rüdiger Schack, when we were having a fiery debate on what to include in our "certainty" paper, `quant-ph/0608190`. [See 12-12-06 note to Schack titled "Offline Discussion".] The distinction I keep drawing and that you don't like—I think—has to do with all that.

Demopoulosism 13 : *QI rejects them all. By contrast, the QL tradition takes as its starting point the classical Kolmogorov theory of probability which is naturally regarded as formulated within a framework based on an algebraic structure of propositions, events or properties; these latter notions are respectively about, associated with, or belong to the objects whose probabilistic behavior is described in a classical probability framework. QI doesn't deny that elementary particles sustain relations to propositions, events and properties, it simply does not take the propositions, events or properties associated with them to be the proper objects of quantum probability assignments.*

I like this last sentence very much.

Demopoulosism 14 : *What is conventionally described as a property of a particle is, for the purposes of the probability theory, replaced by the notion of an effect which the particle produces at the observable level of experimental results; and what is conventionally understood as the probability that a particle has a particular property is re-conceived as the probability that a particular effect will be produced under certain experimental conditions. The particle contributes to this effect and is not equated with the totality of its effects, but it is effects—rather than properties of particles—that are the proper subject-matter of quantum probability assignments.*

Yes.

Demopoulosism 15 : *Effects are all describable at the macroscopic level of experimental results; they are not properties of the particles that produce them, although their occurrence is a function of particle behavior. Hence, in one sense, there is no question of realism, since the framework of QI assumes the reality of the micro-level. I think QI can even assume the observer-independent reality of the micro level without in any way compromising what is novel and important about its framework, but I'll leave this to another letter.*

Yes, I agree with all this too. But I would put it differently. I don't like drawing the distinction between the micro and macro levels. It sends one on the goose chase (like Zurek's decoherence program) of thinking that the distinction is a physical one, rather than a categorical one. The distinction to be drawn—as far as I am concerned—is only between the *object* and the *agent*.

Demopoulosism 16 : *One reason why Bayesianism is so attractive as an approach to probability within the QI framework is because of the naturalness of the idea of a fair betting quotient as an account of the meaning of probability and the fact that this is readily paired with the primacy which QI gives to the notion of an effect which is conditional on the performance of an experiment: probabilities are regarded as the fair odds associated with conditional bets, in the present case with bets which are conditional on an effect occurring should a particular experiment be performed.*

You're right about it being *one* reason. As a matter of history, I came across it from the other end. I started out as a strong objectivist about quantum probabilities, thinking that that conception could eventually be made well-defined. But in reading everything I could on objective chances and propensities (from '91 to '96), I slowly came to the belief that those notions were contentless. Eventually the Ramseyan notion struck me as the only one with any operational content.

Demopoulosism 17 : *As with conditional bets in general, if the experiment is not performed, the bet is off. This is the basis for the claim, often made by some proponents of the QI framework, that unperformed experiments have no outcomes. So understood, the claim is neither contentious nor surprising, and has no obvious implications for anti-realism about the micro level. It does suggest an element of instrumentalism, however.*

Instrumentalism for the quantum state, indeed. But such is true for *all* Bayesian probabilities: they are never representational of "matters of fact" about their objects. Thus, along the lines of Rorty, it seems to me the wrong distinction to make. Bayesian probabilities never had a chance to represent anything about their objects.

Demopoulosism 18: *A useful way of putting the difference between the two frameworks is the centrality which one gives to* <u>*conditional probability*</u> *and the other to* <u>*conditional bets*</u>*. Within the QL framework of propositions or events we can show that the [Lüders] rule generalizes the notion of conditionalization from a classical or Boolean algebraic context. This is a formally elegant aspect of the QL framework, but its interpretation is elusive for reasons connected with the Kochen and Specker theorem. (Part of what I am thinking is that perhaps this theorem should make us re-evaluate the QL framework. I'm unsure whether the QI framework relieves the tensions that the theorem poses for QL.) We can perhaps retain some of the formal elegance of the QL account of conditionalization without incurring its interpretational difficulties by focusing on the probabilities of effects. The idea is that conditional bets have this advantage: whenever a conditional bet takes place, there is no question of the determinacy of the effects bet upon; their determinacy raises no puzzles and is beyond question.*

I think I need you to explain what you mean by "determinacy." I've heard you say this word many times, but I've never pushed on you to explain what you mean. What do you mean?

And does what you mean have anything to do with Rorty's use of the word in the same Introduction that I already quoted. (I don't know what Rorty means either.) Here's his usage of it:

> The later Wittgenstein, Heidegger, and Dewey, for example, would all be as dubious about the notion of "truth-makers"—nonlinguistic items which "render" statements determinately true or false—as they are about that of "representation." For representationalists, "making true" and "representing" are reciprocal relations: the nonlinguistic item which makes S true is the one represented by S. But antirepresentationalists see both notions as equally unfortunate and dispensable—not just in regard to statement of some disputed class, but in regard to all statements.
>
> Representationalists often think of antirepresentationalism as simply transcendental idealism in linguistic disguise—as one more version of the Kantian attempt to derive the object's determinacy and structure from that of the subject. This suspicion is well stated in Bernard Williams's essay "Wittgenstein and Idealism." Williams says there that a Wittgensteinian view of language seems committed to the following chain of inference:
>
> > (i) 'S' has the meaning we give it.
> > (ii) A necessary condition of our giving 'S' a meaning is Q.
> >
> > *Ergo*
> >
> > (iii) Unless Q, 'S' would not have a meaning.
> > (iv) If 'S' did not have a meaning, 'S' would not be true.
> >
> > *Ergo*
> >
> > (v) Unless Q, 'S' would not be true.
>
> Since the values of Q will typically include human social practices, the conclusion of this set of inferences is, indeed, reminiscent of transcendental idealism. But the

antirepresentationalist will reply that (v) merely says that unless certain social practices are engaged in, there will be no statements to call "true" or "false." [*Though, this part starts to sound a little bit like what you are saying.—CAF*] Williams, however, rejoins that "it is not obvious that for the later Wittgensteinian view … we can so easily drive a line between the sentence 'S' expressing the truth, and what is the case if S." His point is that antirepresentationalists typically do not think that, behind the true sentence S, there is a sentence-shaped piece of nonlinguistic reality called "the fact that S"—a set of relations between objects which hold independently of language—which makes 'S' true. So, Williams concludes, antirepresentationalists, and in particular the later Wittgenstein, are committed to the idea that "the determinacy of reality comes from what we have decided or are prepared to count as determinate."

The trouble with this conclusion is that "comes from" suggests causal dependence. The picture called up by Williams's terminology is some mighty immaterial force called "mind" or "language" or "social practice"—a force which shapes facts out of indeterminate goo, constructs reality out of something not yet determinate enough to count as real. The problem for antirepresentationalists is to find a way of putting their point which carries no such suggestion. Antirepresentationalists need to insist that "determinacy" is not what is in question—that neither does thought determine reality nor, in the sense intended by the realist, does reality determine the thought. More precisely, it is no truer that "atoms are what they are because we use 'atom' as we do" than that "we use 'atom' as we do because atoms are as they are." *Both* of these claims, the antirepresentationalist says, are entirely empty.

Demopoulosism 19 : *A principal difference between QI and QL is that QL seeks to explain the quantum probability assignments by appealing to the algebraic or logical structure of quantum propositions. (Here I'm thinking of Jeff and Itamar; my recent work is different in just this respect, since I don't credit the logical structure (i.e., the "basic" rather than the "derived structure" of my incompletely knowable domain paper) with that kind of explanatory power.) This idea is a reasonable one in view of Gleason's theorem. For it to be wholly successful, it would be necessary to show how the structure of Hilbert space can be recovered from logical or algebraic constraints on the family of physical propositions, which is the central goal of the quantum logical axiomatic program. It would also be necessary to show that this is the correct direction of explanation—i.e. we should go from logic to probability, rather than in the opposite direction or in neither.*

By contrast, I take it that QI envisages no such explanation of the probabilities. For QI, the situation is the same as with classical probability theory: classical logic is insufficient for explaining the coherence of classical probabilistic reasoning. Its explanation may plausibly be said to depend on the interpretation of probabilities as fair betting quotients and the theorem of de Finetti. So also in the context of quantum probability theory, where one also seeks a general strategy for all possible gambles—but where significantly, not all gambles can be taken simultaneously—if one's betting quotients do not obey the quantum probabilities, one is sure to lose.

That is the idea. Now, Itamar claims to prove just this. But he takes the quantum logical structure of propositions as given. The quantum Bayesians of the CFS variety, however, do not want to take that as given. And in that regard, what you say—"if one's betting quotients do not obey the quantum probabilities, one is sure to lose"—is only an idea for an idea: It is a research program.

Demopoulosism 20 : *The remarkable and striking fact here is not the suggestion that logical relations explain probability assignments, but that an empirical theory of the probability of outcomes* *of experiments should inform the consistent distribution of probabilities over a family of hypothetical gambles,* [. . .]

I agree to this point.

Demopoulosism 21 : [. . .] *a distribution which contravenes the classical constraints on their distribution.*

But I disagree here. I don't think there's any sense in which the Born rule specifying probabilities for all measurements contravenes the usual Ramsey / de Finetti coherence. As I see it, the extra quantum restrictions are an addition on top of usual coherence. (Remember, as I see it, quantum measurement outcomes for distinct measurements are not within a single algebra of any interesting variety.)

Demopoulosism 22 : *Following tradition, I call the puzzling phenomena which comprise the first grouping, quantum paradoxes, and the second, the measurement problem. I depart from tradition by calling the problem of hidden variables the theorem which is often invoked as a solution to what usually passes for this problem; by the problem of hidden variables I mean the problem of understanding the significance of this result in the formulation which has become canonical since the work of Kochen and Specker.* [. . .]

I claim that the classification is nevertheless a useful one because it isolates a conceptual issue that is more purely philosophical from those that are peculiar to quantum theory, and which are therefore more properly regarded as foundational issues specific to that theory; the more purely philosophical issue is the problem of hidden variables. Even though the theorem of Kochen and Specker might never have suggested itself were it not for the development of quantum mechanics and the presumption that it might bear on the foundational problems mentioned earlier, the conceptual issues the theorem poses are rightly distinguished as philosophical rather than foundational because the notions on which their formulation depends are of such generality that they are held in common by virtually every theory.

Aha! You may not agree with me in detail, but you seem to agree with me in sentiment. Recall what I wrote in the old Caltech proposals:

> **Quantum Mechanics as a Powerful Hint.** In my opinion, the most profound statement yet to come out of quantum theory is the Kochen-Specker theorem. For it licenses the slogan, "Unperformed measurements have no outcomes." This is just a beginning. If one canvasses the philosophic traditions for one that has significantly developed this slogan, one will find the now mostly-forgotten tradition of pragmatism fathered by William James and John Dewey. As a source of ideas for what quantum mechanics can more rigorously justify, no block of literature is more relevant: The connections between the two fields cry out for systematic study. Quantum mechanics holds the promise of drastically changing our worldview on the wide scale. It is time to let that happen.

and

> **Quantum Mechanics and Anti-representationalist Philosophies.** There are various threads connecting the quantum research program proposed here to a wider philosophical tradition, which to my knowledge has never been greatly examined in

> this context. The tradition comes under the rubric of what Richard Rorty calls 'anti-representationalist philosophies.' This tradition, spearheaded by the pragmatism of William James and John Dewey, also includes thoughts of (the later) Ludwig Wittgenstein, Martin Heidegger, Donald Davidson, Hilary Putnam, Rorty himself, and several others. How else can one understand the implications of the Kochen-Specker theorem than by realizing it hints at something like James' analysis of the concept of 'truth'? How else can one make sense of a Bayesian take on pure quantum states than to explore the same paths as Wittgenstein in his book *On Certainty*?
>
> Since becoming immersed in the subject, I have found nothing more exciting than these trains of thought. For they indicate the extent to which quantum foundations research may be the tip of an iceberg—indeed, something with the potential to drastically change our worldview, even outside the realm of physical practice.

You may become a pragmatist yet!

Demopoulosism 23: *The idea I propose to develop is based on the assumption that no satisfactory interpretation of quantum probabilities is possible so long as the probabilities are understood to be defined over propositions "belonging to" a particle. I say that a proposition belongs to a particle if its constituent property is a possible property of the particle. I claim that such an interpretation is compelling only when the propositions form a Boolean algebra.*

That sounds good.

Demopoulosism 24: *The essential idea of my positive proposal is that the probabilities of "quantum probability theory" are not defined over the totality—or even a sub-totality—of the propositions belonging to a particle, but over the totality of effects which are induced by an interaction with an experimental set-up and that are registered in the experimental apparatus.*

I hope you'll give some credit to old Asher Peres here, "Quantum Theory Needs No Interpretation." This is a formulation he would have accepted:

> Our purpose here is to explain the internal consistency of an "interpretation without interpretation" for quantum mechanics. Nothing more is needed for using the theory and understanding its nature. To begin, let us examine the role of experiment in science. An experiment is an active intervention into the course of Nature: We set up this or that experiment to see how Nature reacts. ... What [quantum theory] does is provide an algorithm for computing *probabilities* for the macroscopic events ("detector clicks") that are the consequences of our experimental interventions. This strict definition of the scope of quantum theory is the only interpretation ever needed, whether by experimenters or theorists.

and

> We surely agree with Brun and Griffiths that "in science, one cannot rule out alternatives by fiat; one must evaluate them on their merits." We do not find any merit in the various alternatives that were proposed to the straightforward interpretation of quantum theory: It is a set of rules for calculating probabilities for macroscopic detection events, upon taking into account any previous experimental information. Brun and Griffiths may think this a "straitjacket," but it prevents the endless conundrums that arise solely from shunning quantum theory's greatest lesson—that the notion of experiment plays an irreducible role in the world we are trying to describe.

Demopoulosism 25: *Effects, which comprise the macroscopic historical record of such interactions, are the proper objects of quantum probability assignments. My claim is that it is this shift in conception that is mandated by the classical hidden variable theorems. Because these theorems are formulated in a propositional framework, they possess a kind of "dialectical" significance by showing the necessity of replacing the propositional framework on which they are based. As a point about the conceptual framework of quantum theory, the claim lends itself to the following formulation: The logical form of the representation of an elementary particle is not that of a class of propositions whose constituent properties are possible properties of the particle, but that of a function which, when subjected to a specified class of operational procedures, produces a family of effects.*

I tentatively like the sound of this formulation, but I'm not completely sure yet that I know what you mean.

Demopoulosism 26: *The representation as a function implicates the particle in an essential way in the production of the effects associated with its interactions with a class of experimental procedures. But it is not committed to the idea that an effect would have been the same had the particle been presented with a different experimental set-up than the one with which it was in fact presented. By contrast, the view of the particle as a class of propositions is committal on this counterfactual claim in just the way that the functional representation is not.*

Within this framework the problem of determinism is posed as follows: Given a class of experimental procedures, to predict their effects with perfect knowledge, i.e. to predict uniformly, and without foreknowledge of the experimental procedure to which the system will be subjected, the answer to every question regarding the occurrence of a possible effect. The no hidden variable theorems show that the quantum probabilities of such effects are not compatible with the existence of a function which predicts every possible effect with probability 0 or 1; hence, neither is it possible to construct a reconstruction of the state of a particle on which to base such predictions: the probabilities of the effects are logically inconsistent with the existence of such a state. The representation of an elementary particle as a function which, when presented with an experimental configuration, yields an effect, is interchangeable with its representation as a class of propositions only when the effects are predictable with 0-1 probability. The fact that the particle-effects described by quantum probability theory are not so interchangeable with propositions belonging to them should be implied by, and perhaps itself implies, the impossibility of cloning.

You're getting off track with this last sentence: The no-cloning theorem has no power to imply the Kochen-Specker result. No-cloning is always true for probability distributions, no matter what the character of arguments of the probability functions (i.e., whether they are "effects" or classical (Kolmogorovian) propositions belonging to the particle). Furthermore, no-broadcasting is generally true too, as long as the probability functions are not allowed to become too peaked. Simply see Spekkens' "toy model" paper.

Kochen-Specker is far deeper than no-cloning ... and far deeper than many of the standard phenomena of quantum information theory (teleportation, superdense coding, entanglement monogamy, etc.).

03-02-07 *My First Fortune Cookie in Waterloo* (to L. Hardy)

"You are going to pass a difficult test."

04-02-07 *QT Needs No Interpretation* (to W. G. Demopoulos)

Boy my ideas have significantly moved on since then!! (Or at least gotten more subtle/resilient ... if you can imagine such a combination.)

04-02-07 *The Painful Ambiguity of Language* (to W. G. Demopoulos)

<u>**NOTE**</u>: This letter was never finished or sent, though the part below was composed on this date.

Reading your notes, now I am VERY confused. I thought I had understood your point of view, but now I wonder. And when you had said in your earlier note, "What I have to say is pretty abstract and at the level of the 'framework' which I believe is implicit in your approach," I thought you were roughly right, but now I wonder about that too.

Demopoulosism 27: *The idea behind determinacy: Nothing can be colored without being some specific color. The terminology is perhaps derivative from W. E. Johnson (whom I've never read, but likely should have) who was apparently an important influence on Keynes and Ramsey. Johnson introduced the terminology of <u>determinates</u> falling under a common <u>determinable</u>, from which my example evidently derives.*

This must be the terminology Schiller was using in that quote of his from 1903 that I like so much [F. C. S. Schiller, "The Ethical Basis of Metaphysics," in his *Humanism: Philosophical Essays*, second edition, (Greenwood Press, Westport, CT, 1970)]:

> That the Real has a determinate nature which the knowing reveals but does not affect, so that our knowing makes no difference to it, is one of those sheer assumptions which are incapable, not only of proof, but even of rational defence. It is a survival of a crude realism which can be defended only, *in a pragmatist manner*, on the score of its practical convenience, as an avowed fiction. In this sense and as a mode of speech, we need not quarrel with it. But as an ultimate analysis of the fact of knowing it is an utterly gratuitous interpretation. The plain fact is that we can come into contact with any sort of reality only in the act of 'knowing' or experiencing it. As *unknowable*, therefore, the Real is *nil*, as *unknown*, it is only potentially real. What is there in this situation to sanction the assumption that what the Real *is* in the act of knowing, it is also outside that relation? One might as well argue that because an orator is eloquent in the presence of an audience, he is no less voluble in addressing himself. The simple fact is that we know the Real *as it is when we know it*; we know nothing whatever about what it is apart from that process. It is meaningless therefore to inquire into its nature as it is in itself. And I can see no reason why the view that reality exhibits a rigid nature unaffected by our treatment should be deemed theoretically more justifiable than its converse, that it is utterly plastic to our every demand—a travesty of Pragmatism which has attained much popularity with its critics. The actual situation is of course a case of interaction, a process of cognition in which the 'subject' and the 'object' determine each other, and both 'we' and 'reality' are involved, and, we might add, *evolved*. There is no warrant therefore for the assumption that either of the poles between which the current passes could be suppressed without detriment. What we ought to say is that when the mind 'knows' reality both are affected, just as we say that when a stone falls to the ground both it and the earth are attracted.

> We are driven, then, to the conviction that the 'determinate nature of reality' does *not* subsist 'outside' or 'beyond' the process of knowing it. It is merely a half-understood lesson of experience that we have enshrined in the belief that it does so subsist. Things behave in similar ways in their reaction to modes of treatment, the differences between which seem to us important. From this we have chosen to infer that things have a rigid and unalterable nature. It might have been better to infer that therefore the differences between our various manipulations must seem unimportant to the things.
>
> The truth is rather that the nature of things is not *determinate* but *determinable*, like that of our fellow-men. Previous to trial it is indeterminate, not merely for our ignorance, but really and from every point of view, within limits which it is our business to discover. It grows determinate by our experiments, like human character. We all know that in our social relations we frequently put questions which are potent in determining their own answers and without the putting would leave their subjects undetermined. 'Will you love me, hate me, trust me, help me?' are conspicuous examples, and we should consider it absurd to argue that because a man had begun social intercourse with another by knocking him down, the hatred he had thus provoked must have been a pre-existent reality which the blow had merely elicited. All that the result entitles us to assume is a capacity for social feeling variously responsive to various modes of stimulation. Why, then, should we not transfer this conception of a determinable indetermination to nature at large, why should we antedate the results of our manipulation and regard as unalterable facts the reactions which our ignorance and blundering provoke? To the objection that even in our social dealings not all the responses are indeterminate, the reply is that it is easy to regard them as having been determined by earlier experiments.

I don't know that I would go as far as Schiller and say that every aspect of matter is "determinable indetermination," but I certainly think the idea corresponds decently with the situation in quantum measurement. Take a complete orthonormal basis that corresponds to some quantum measurement that can be performed on a system. The basis as a whole (i.e., not the subspace spanned by it, but the collection)—I would say—corresponds to a determinable belonging to the object. (This is a variant of your terminology of "belonging to.") That is to say, the basis as a whole is among the hypothesized properties of the object—for it has something to do with how any agent can interact with the object. But what do the individual elements of the basis correspond to? Truth values of propositions belonging to the object? I would say no (just as apparently you would say no). Thus I would say they are "indeterminate of the object."

But I think that's probably a very different use of the words "determinate" and "determinable" than here:

Demopoulosism 28: *In philosophy of QM it's thought to be the lesson of K-S that some propositions are not determinately true or false. Since however their disjunction may always have probability* $= 1$*, it is said that the disjunction is true, but without any [disjunct?] being true or false. Thus the determinable property holds without any determinate which falls under it holding.*

For I think what you have in mind is that some quantum logicians would take the disjunction (i.e., the subspace spanned by the vectors) to be true OF THE OBJECT. That the disjunction's truth value is a property of the object (a property that belongs to the object) even if . . .

04-02-07 *Giving Up for the Night* (to W. G. Demopoulos)

I started to construct a reply to your notes from today, and then it became long and unwieldy and mostly confused. (I don't even want to show you the partial construction—it's pretty bad.) So, I think I'm just going to give up tonight … and indeed just give up until I see you in person. I think at this stage, it might be more fruitful to have a conversation at a blackboard rather than write.

06-02-07 *Physics, Economics, and Death by Interpretation* (to D. M. Appleby, C. M. Caves, and R. Schack)

How would you respond to someone who wrote this:

> Economics and physics fundamentally diverge, however, on an entirely different front: people. To a physicist, our existence is profoundly irrelevant to the structure of the universe: we are made of the same basic building blocks as both quinces and quasars and the universe would unfold according to precisely the same structural laws if we were to suddenly disappear.[1] Economics, on the other hand, is entirely different. Without people, economics is not merely "abstract"; it is simply non-existent, nonsensical. One obviously can't study the wealth of nations or the consumption of goods and services without tacitly assuming that there are humans[2] to produce and consume the goods.
>
> [1]I imagine there will be some astute reader who will point out that our present understanding of quantum mechanics argues for some special role of human observers. Without in any way wading into this particular miasma, the point is that the current ambiguity which makes such an argument defensible is nearly universally regarded by physicists as a problem with quantum theory, or at least our current interpretation of it.
>
> [2]Or aliens sufficiently similar to humans as to be, to all intents and purposes, the same thing.

Just wondering out loud within the earshot of some potentially like minds.

07-02-07 *Research Program* (to P. Busch)

Buschism 2: *I have read Marcus's papers but as I told him; I have more or less come round to seeing probabilities as a "logical" tool, for making uncertain or incomplete inferences (paraphrasing a quote Marcus gave from Maxwell's writings). But then this still has to be brought together with the ontology of the world out there.*

Yes, absolutely. That's the nub of the research program, the part that has the greatest potential. But to get this far—to this starting point really—we have had to have a hard, hard fight with the community to get the epistemic view of quantum states even to be taken seriously.

07-02-07 *Epistemic/Ontic – Subjective/Objective . . . and All That* (to P. Busch)

Buschism 3: *Well, well . . . it did take you guys a while to realize that you were in danger of "throwing the baby out with the bath water", didn't it?* ☺

I'm not sure I'm understanding the correct intonation I should be reading this with (e.g., I can see a smile on your face), but under the assumption that I am:

I would say, NO, it was the *community* that took the long time to *recognize* (or finally listen) that we had no intention of "throwing the baby out with the bath water". See, for instance, the bottom of the chart on page 5 and particularly the larger discussion on pages 5 and 6 of `quant-ph/0205039`. That was five years ago, and the goal was already articulated quite clearly there. What more can a poor worker do?

10-02-07 *Toy Model, 2* (to S. J. van Enk)

van Enkism 9: *If you want to violate Bell inequalities you either give up locality or realism: I think if you give up realism then you can't keep your underlying ontic state anymore . . . don't you agree?*

Yes . . . But what does that have to do with your paper? The reason you call it a "toy model" is 1) because there are underlying ontic states, and 2) there are "epistemic" states (for the ontic states) that satisfy some information theoretic requirement. No? What you find is that the requirement cannot be satisfied with "epistemic" states that are true-blue probability distributions—they must go negative. The trouble is then I don't know how to really think of these things as epistemic states any more. Thus, I'm not really sure in what sense this is an instructive "toy model" for QM after all.

10-02-07 *Inside and Outside* (to S. J. van Enk)

van Enkism 10: *I agree it's no longer knowledge about the ontic states. But I can view it as knowledge about the observables (i.e. the Q's): there is a constraint on how much one can know about those. In the case that the P's are all positive, the two constraints are equivalent; if not, then you have to use a trick: namely use Zeilinger/Brukner's measure of information.*

But you're right, going to negative probabilities changes the character. On the other hand, I don't see how you can ever violate Bell inequalities, if you don't change the character of the Spekkens model!

Have you ever thought about putting "half of the hidden variable" inside the observer? I'm thinking something like this: 1) The system has an ontic state (say like in the Spekkens model), but 2) the observer also has an ontic state (say, perhaps buried deep in his belly). And though the value of the observer's belly state is within him, it is completely inaccessible to his knowledge. That's what I mean by "half of the hidden variable" being inside the observer.

Now what about a measurement on the system? Maybe *instead* of a measurement giving partial information about the system's ontic state, like in the Spekkens model, the kind of measurement of interest tells something about the *relation* between the observer's belly state and the system's ontic state. For instance, a measurement might reveal partial information about the parity of the inside bits and outside bits.

Anyway, the crucial idea here is that when an observer makes a measurement on the left member of a bipartite system, there is no good sense in which there is a local hidden variable at the right-hand member signifying the truth value of the measurement. The outcome of a measurement only has some existence with respect to the hidden variable within the observer. In other words, the observer has to take his belly variable over to the right-hand system before a *local* hidden variable account of the right-hand situation is complete.

Maybe another way to put it is that there is a sense in which this is a nonlocal hidden variable theory (the observer always carries half of the hidden variable)—and he is always with either the left-hand system or the right-hand system, but never localized at both. So, though the hidden variable theory is nonlocal, it is nonlocal in an innocent kind of way.

Do you think one might get a Bell inequality violation out of toying with a kind of idea like that?

Just an idea; probably crazy.

12-02-07 *Lord Zanzibar* (to J. Christian & L. Hardy)

Dear Lord Zanzibar and Sir Lucien,

Here's that lovely quote I was telling you two about. But I misremembered it a little; it's from G. K. Chesterton's book *Heretics* and only quoted by James.

> There are some people—and I am one of them—who think that the most practical and important thing about a man is still his view of the universe. We think that for a landlady considering a lodger it is important to know his income, but still more important to know his philosophy. We think that for a general about to fight an enemy it is important to know the enemy's numbers, but still more important to know the enemy's philosophy. We think the question is not whether the theory of the cosmos affects matters, but whether in the long run anything else affects them.

My view of the universe is that it is many—that it ultimately cannot be unified, for it is alive and changing and creative in a very deep sense. Moreover, that reality is, to some not-yet well-understood extent, plastic: It can be molded by our actions. Thus, though humanity is quite well a Darwinian accident, now that it is here, it is a significant component of the universe that must be reckoned with.

The question is, with that in the open would a landlady let me through the door!

13-02-07 *Tegmark, 101 Years Ago* (to L. Smolin)

I'm sorry I missed your talk last Thursday; I had to give my own talk in London that day. It sounds like I quite missed something! But we will have a chance to discuss these things many times, as I have made a firm decision now that I will be coming to PI for an extended stay as soon as possible. More than discussing, though, I hope we will find a way to put some of these pragmatist ideas into solid, lasting physics—physics made of fantastic new equations and not just words.

At the moment though, let me send you some old words again. You know I'm preparing for this "pragmatism and qm" conference in Paris next week, and because of that I'm rereading some James and Schiller. Well, look at this nice tidbit I found in James today. It was as if he had been listening to Tegmark's talk himself. Really, not a lot has changed in 101 years for a certain strain of mind. (James' lecture was in 1906.) A modern like Tegmark uses a phrase like "logical system," "mathematical system," or "multiverse," but it boils down to nothing essentially different from the "Absolute" and the "mind of God" that had taken hold of the "rationalists" at the time. You'll see what I mean:

> [I]f you are the lovers of facts I have supposed you to be, you find the trail of the serpent of rationalism, of intellectualism, over everything that lies on that side of the line. You escape indeed the materialism that goes with the reigning empiricism; but you pay for your escape by losing contact with the concrete parts of life. The more absolutistic philosophers dwell on so high a level of abstraction that they never even try to come down. The absolute mind which they offer us, the mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below. And the theistic God is almost as sterile a principle. You have to go to the world which he has created to get any inkling of his actual character: he is the kind of god that has once for all made that kind of a world. The God of the theistic writers lives on as purely abstract heights as does the Absolute. Absolutism has a certain sweep and dash about it, while the usual theism is more insipid, but both are equally remote and vacuous.

14-02-07 *Rivers of Nows* (to G. L. Comer)

Hollow Flow

In Einstein rapids,
Instants, rivulets,
Shadows shake
Beneath the foam.

In carved canyons,
Sifted sandbars,
Imperfect memories
Attend the self-aware.

In quantum pools,
Non-waves crash,
For milky ways
And silt-filled streams.

In recollections,
Our reflections,
Are rivers of nows
And melting glaciers.

Gregory Lee Comer
02/13/07

Thanks for the poem. I'm glad I checked gmail! (I usually only use gmail when I'm away from home, at a really backwoods place.)

Rivers of nows. I love that phrase. Rivers made of droplets? So are there droplets of now? Carriers of now? Carriers of now. Whose now? My now. I am the carrier of my now. Maybe I should say, I'm the maker of my now. Makers of now. Whereupon synchrony, then? Communication. No communication, no global now; not even a quasi-local now.

Oh, you've carried me off today! And this snow-locked day at home probably isn't helping either. But I must resist. Too much work needs to be done.

15-02-07 *The Kochen-Specker Spectre* (to G. L. Comer)

Thanks for the article. Gregor Weihs will be one of my colleagues in Waterloo (though, he's at the university). He's very reasonable and thorough kind of guy.

Couple of tiny points about the article. I really disagree with blanket statements like this:

> Thus, unless one allows the existence of contextual hidden variables with very strange mutual influences, one has to abandon them — and, by extension, 'realism' in quantum physics — altogether.

It's the 'by extension' part that bugs me. For, I don't think that we quantum Bayesians have given up on realism at all. Quantum mechanics allows for quantum systems—things out there independent of the experimentalist. If they weren't out there for the experimentalist to perform experiments on, his actions in the laboratory would be as much of a koan as the sound of one hand clapping. And yet, there are no hidden variables (from the quantum Bayesian view). So, there is no 'by extension'—it's just a non sequitur.

I think John Sipe does a good job of summarizing how we qB's can let go of hidden variables at the same time as holding on to a deeper, more interesting realism. I'll paste in the section he wrote for his book below (wherein I've toyed with a couple of phrases to bring it further in line with my own thoughts).

> We know this for photons already, but the corroboration in a different system should help to convince doubting Thomases, as well as assure the rest of us.

A quibble with regard to the need for funding these kinds of things: For the skeptics who have not been convinced by experiment yet, they never will be. Nor, does a positive result really assure anyone the likes of me—if one is already certain of something, one doesn't go out of his way to test it. If there is a reason to do these experiments, it lies elsewhere: And that is to develop laboratory techniques for quantum information processing. Some QI protocols rely on KS for their very existence, and that's the real argument for using taxpayer money to do these things.

15-02-07 *Bayesian Strategies* (to R. Schack)

I'm looking forward to spending some time with you next week. I feel like I'm bubbling over with ideas, but of course can't articulate a darned thing.

I'm writing this note to list a few things that I'd like to think about while together with you.

First an operational note: I hope you know Paris well enough that we'll be able to negotiate the trip between the train station, the hotel (to check in and put away our suitcases), and the conference site in the allotted time. We arrive in Paris Nord at 12:53, whereas the conference starts up at 3:00.

Now, for things to think about.

1. I'd like to definitely further explore the conversation we started in completing the certainty paper, about "live" versus "dead" propositions. Also, to discuss what better to say to Timpson.

2. The issue that John Sipe wants to draw out of us: to what extent are we "consequence Bayesians" exclusively, or dualists between that position and "classical Bayesianism" (in his terminology).

3. I'd like to figure what to say to Lucien Hardy who repeatedly challenges me on his paper with Galvao, "Substituting a Qubit for an Arbitrarily Large Number of Classical Bits" (`quant-ph/0110166`). He somehow thinks this is a challenge to a Bayesian view of quantum states, and I just don't know what to say in defense.

27-02-07 *Ref Help* (to W. C. Myrvold)

Myrvoldism 5: *While you were here I asked you for a reference for the classical no- cloning theorem that goes via the invariance under Liouville evolution of the fidelity of two classical densities—that is, integral of* $\sqrt{f(x)g(x)}$*. You referred me to Caves and Fuchs, "How Much Information . . . ?",* `quant-ph/9601025`*, but it's not in there.*

Well, it's mentioned, even if it's not spelled out in detail. See the last paragraph of the paper, at the end of page 31 and the start of page 32. That paper was started in summer of 1995, just at the time that I had first realized the point. (I remember the moment perfectly; I was sitting in front of "tangelo," my computer, writing on my thesis, and my jaw dropped.) But I hadn't recalled that we waited so long before putting the paper on `quant-ph`. (Carl was really dragging his feet on that one, and I remember saying, "If we don't get this paper out quickly, Nathan Rosen may well never see it." Well, Nathan never saw it.)

I guess two publicly earlier references on the point are:

1. `quant-ph/9511010` (the no-broadcasting paper, pages 2 and,

2. `quant-ph/9601020` (my thesis, submitted Fall '95, pages 113 and 114)

There's probably longer discussions buried somewhere in my *Notes on a Paulian Idea*—if you want me to dig in there and find the appropriate stuff, let me know.

I was in Paris this week at a very nice meeting on pragmatism and QM. I met Hacking there for the first time; a wonderful guy. More importantly though, Rüdiger and I had many long discussions on the points you brought up at dinner in London. I will try my best to put those thoughts down for you, along with a reply to your last note, soon.

28-02-07 *Chris* (to A. Y. Khrennikov)

Khrennikovism 15: *Yes, it would be great! Thus I shall put you in the list of invited speakers that we shall print today. Then you will contact with me about whom will come?*

I'm just off the phone with Rüdiger Schack, and I asked him if he would be available in my stead. I told him that you would be able to pay his local expenses, but not the travel expenses. He became very intrigued about the meeting, and is working to rearrange his schedule—he does not know for sure whether he can yet. I think Rüdiger would be particularly good for the subject chosen for this year's meeting in that we have just prepared a paper making a direct comparison between our Bayesian view and older Copenhagen style views, and Rüdiger will have a presentation prepared on just that subject.

01-03-07 *Soul Tired* (to D. M. Appleby)

You are a powerhouse of living scientists: Your stamina and creativity amazes me. You have reached so far further on this problem (and allied problems) that I wonder if I will ever catch back up. I like your "navigation" analogy, which is the part of the note that I understood.

See what a horrible state I am in? Actually, I am quite excited—extremely excited—about the move to PI. It is absolutely clear that it will give me a new lease on life, and effectively set back the clock to the days when I first knew you (say, at the Oviedo meeting): Life, and particularly quantum foundations, was full of possibility then. But it is now getting there, and all the uncertainty in between, that is taxing my soul.

On other fronts, Paris was an extremely intense meeting for me. I was surrounded by pragmatists, real pragmatists, for once and it was a completely new experience. There were Deweyians, Peirceans, and even one Jamesian. There was a fellow who could recall detailed points in Wittgenstein's "On Certainty". That, plus the general intensity of Paris (along with the general hope of PI), too made me feel that the future will be bright again. Particularly useful were all the discussions with Schack about an alchemical world and the "potentest of all my premises."

In that regard, let me ask you this: Can you articulate why you feel that the study of SICs is profoundly important? I would like to hear your answer without tainting it with any contemporary statements of my own, i.e., why I think SICs are profoundly important. I reported all of those to Schack and for the most part I think they left him cold. So, I'm wondering how your version of things will come out. Your stamina on this problem, I suspect, arises from a deep understanding of something—something I myself have not yet grasped—and I would like to pinpoint that source in you.

Besides the various tidbits above, my working life has been filled mostly with the politician's work: Writing letters of recommendation, working behind the scenes to secure positions for good Bayesians (you're not the only one), getting the student-award program set up for the APS March meeting, working with PI to put together the foundations summer school, catching up on my editorial duties for QIC, crap like that. And like a good American politician, I even got stuck in a snowstorm overnight during a recent flight: Though in my case, I really did get stuck in a snowstorm, rather than slipping off with a mistress.

Kiki has been scrambling to complete all her uncompleted projects on the house, so that we might command a higher price. And daily we look through the real estate adverts for the Waterloo area, tabulating distances and whatnot.

On my flight from Paris, I read a small book *Wittgenstein: A Memoir* that contained a small biography by G. H. von Wright, a memoir by Norman Malcolm, and a complete set of Wittgenstein's letters to Malcolm. One thing struck me from the ebb and flow of W's letter-writing style: It had a certain something in common with yours. I never could put my finger on it, but at times (briefly), I felt like I was reading one of your letters. It made me wonder if Wittgenstein had influenced you deeply indeed.

01-03-07 *More Progress on New Directions* (to W. G. Demopoulos)

Thanks for the latest draft. I read a little of it during lunch—up to page 9—just before my lunchtime knap. Let me make some completely superficial remarks at this stage. I'll come back later with any more substantial remarks, after I get a chance to complete the document. [...]

I'm just recovering from my trip to Paris, where I had a thoroughly enjoyable, but intense time. Rüdiger Schack and I spent much time discussing what I keep calling "the potentest of my premises." I also met several very knowledgeable living pragmatists, which was useful for me, and finally had several conversations with Ian Hacking, whom I got to know for the first time at the meeting.

11-03-07 *Quantum Certainty* (to G. Quznetsov)

Quznetsovism 1: *On your "Subjective probability and quantum certainty" in LANL: You already can be not worried about these topics because all these your problems are eliminated by my book Logical Foundation of Theoretical Physics, Nova Science Publishers, N.Y. (2006).*

Thank you for saving us a lot of work. I will look at your book right away.

12-03-07 *Remarks on Ludwig* (to D. M. Appleby)

I've finally sat down to read your long Wittgenstein note seriously. I am really, really sorry about this:

Applebyism 11: *I think that after all this time (more than 20 years) I have got the poison safely neutralised, but with something like this one can never be totally sure. Which is why your comment, about similarities between my style and Ludwig's, worried me. I feared that the toxins were breaking out.*

It was only an offhand remark of mine!! Really. I certainly didn't mean to cause such a fear in you. You never speak like an oracle. If either of the two of us is guilty of it, it is I. So, please rest your soul: You are not *that* much like Wittgenstein. As long as you are a repository of some of his better thoughts, and can bring them to bear on physics, everything will be OK.

Applebyism 12: *I am not like your friend at Paris: I can't quote from the man verbatim (a pretty pointless achievement, it seems to me).*

Well, I didn't go quite that far. I had only said, 'There were Deweyians, Peirceans, and even one Jamesian. There was a fellow who could recall detailed points in Wittgenstein's "On Certainty".' I don't see anything wrong in being a Wittgensteinian in that sense.

Applebyism 13: *Wittgenstein was a ball-crusher: he tended to destroy the intellectual virility of those around him (I use the word "virility" deliberately: I get the impression that it was specifically the men around him who were chiefly at risk (Anscombe, I get the impression, was pretty weird, but I suspect she was like that before she met Ludwig)). And the effect wasn't dependent on personal contact. I mentioned the logical positivists, and the ordinary language philosophers. But aside from these there is a third school: the Wittgensteinians as they are often called. These are people who have fallen for the man completely. They're a pretty sad bunch of nonentities. But they are not just sad in the way that epigones are usually sad. They have lost something vital, located in the region of the crotch, and they all sing in the same kind of distinctively precious voice. Ludwig's castrati.*

I wonder if I can infer from this that you'll think I wasted $15 in buying G. H. von Wright's *Philosophic Logic* in a Denver bookstore the other day? I bought it for his chapter "The Epistemology of Subjective Probability", but of course have not read it yet. The squirrel keeps gathering nuts for the long Canadian winters.

12-03-07 *More Serendipity* (to G. L. Comer)

Comerism 15: *Do you know what I am now studying? Reichl's* Statistical Physics *book, in an attempt to understand better things like the Boltzmann equation. I used to hate that book. But now I really like it. Anyway, I've been reading about Liouville equation, etc.*

So the book was unexpected and serendipitous!

Actually more serendipitous than you might think. I've been thinking a little about what sorts of research pushes I might start to make after arriving at PI. And one thought that has occurred to me is that I think it is time to start working on a "quantum Bayesian" understanding of Bose-Einstein and Fermi-Dirac statistics. You see from a q-Bayesian point of view, symmetrization or anti-symmetrization is a *judgment*, nothing less, nothing more. I.e., it is nothing less or nothing more than a property of a proposed quantum state, and a quantum state is a judgment. So, the question then must be, if one *judges* either BE or FD, what is one judging? I want to get a much deeper understanding of that. (BE certainly looks as if it can be made to come out of an strengthier version of a finite quantum de Finetti theorem; but FD—maybe to be expected—is a completely wild mystery.) These considerations have been hitting me from several directions lately. One was discussions with the (eminent) philosopher Ian Hacking in Paris, who has recently taken a philosophical interest in the "making" of BECs. (If you don't recognize the name, see `http://en.wikipedia.org/wiki/Ian_Hacking`.) The other is discussions with Daniel Gottesman at PI; see this paper of his: `cond-mat/0511207`. But going back to Hacking, he asks the reasonable question: Surely the stability of cold, far away stars does not depend on anyone's judgment. The q-Bayesian needs a good, detailed answer to that question.

13-03-07 *Recent Book Review?* (to N. D. Mermin)

Someone at the APS March meeting (I can't remember who) told me that you had written a book review that had something tangentially to do with quantum Bayesianism. I thought they told me the most recent issue of Physics Today, but I can't seem it. (It could be that it appeared in January, in which case, I can't even find that issue of *Physics Today*!) Curiosity is killing the cat. Could you just send me the file if such a thing exists?

13-03-07 *Recent Book Review?, 2* (to N. D. Mermin)

That was a very nice review. Makes me want to read the book (and scoff at part of it). I liked several of your turns of phrase this time around.

I was also struck by this part:

> On the contrary, the reader is assured that quantum states are real states of affairs. "In some very real sense, the wavefunction of an object is the object." Or, less guardedly, "'The wavefunction of the atom' is a synonym for 'the atom'."

You know Charlie Bennett has been guilty of almost identical sentences before (and certainly remains guilty in spirit, as evidenced in Japan in November). I've told you the story before, haven't I?

15-03-07 *Attributed to Einstein, Incorrectly?* (to G. Will)

I am a regular reader of your column and enjoy it very much, even if I disagree with it often. This morning's column, however, left me in a position in which I could *finally* disagree with some authority! Thus I thought I would write you: One takes one's merit badges where one can.

Today's column started with the slogan "The only reason for time is so that everything doesn't happen at once," which you "attributed to Albert Einstein." However, I don't think that attribution is correct. I have been familiar with a variation of the phrase for many years, one promoted by the Princeton physicist (and Einstein associate from Einstein's later days) John Archibald Wheeler. "Time is nature's way to keep everything from happening at once," he would say. Wheeler was very fond of the phrase and put it in several publications, however he always attributed it to a graffito found on a wall in the Pecan Street Cafe in Austin, Texas. For instance, in Wheeler's 1989 paper, "Information, Physics, Quantum: the Search for Links," he cites it this way, "Discovered among graffiti in the men's room of the Pecan Street Cafe, Austin, Texas."

Of course, it could be the case that the phrase made it from Einstein to Wheeler indirectly, via a men's room, but I find it unlikely given Wheeler's involvement in the development of general relativity in the late 1950s onward.

With regard to the history of ideas, let me point out one other variation of the theme that comes from Louis Menand's book, *The Metaphysical Club*, a wonderful quadruple biography of William James, Charles Sanders Peirce, John Dewey, and Oliver Wendell Holmes Jr. and a very good introduction to American pragmatism. With regard to one of the meetings, Menand has this to say about Peirce,

> They assembled. Peirce did not come; they waited and waited; finally a two-horse carriage came along and Peirce got out with a dark cloak over him; he came in and began to read his paper. What was it about? He set forth ... how the different moments of time got in the habit of coming one after another.

15-03-07 *Attributed to Einstein, Incorrectly?, 2* (to C. M. Caves)

I sent the note below to George Will this morning, only to stupidly do a Google search immediately thereafter. I.e., I should have done it immediately before! Anyway, the web reveals the quote variously attributed to Einstein, Wheeler, and Woody Allen. My guess is that Woody Allen really is the originator, now that I've seen it suggested. Trouble is, no one seems to pin down when/where he said it. Do you or Karen have any friends who are big Woody Allen fans that might recall the source of the phrase?

16-03-07 *Sorry, Sorry, Sorry, Sorry* (to N. D. Mermin)

I was very impressed with your book review. I think it is because it expressed better than ever before what you see as the lesson of quantum mechanics. Or at least I felt like it got to the nub. Most particularly for me, I think it helped me pinpoint what I feel uncomfortable with in your view. If you can give me some time, I'll try to articulate that. (And tell you the Bennett story, etc.)

You are a good man David Mermin, and I feel sorry about being such a sorry disciple. I'll try to write more on more interesting matters soon.

19-03-07 *Quantum Computing* (to L. Ketchersid)

I don't mind your quoting me (or anything in my writings), but I don't see how the present quote ties in with the subject of your article.

I read your draft. I'll just make a couple of trivial comments that may be useful to you, and then sign off. (Sorry, but you've caught me at a bad time these last couple weeks, with some rather big life changes in the works. I will start to be more sociable again in the Fall, if you want to revisit these issues then.)

1) Your sentence, "A qubit is represented as a quantum object's nuclear spin (rotation)," directly contradicts your later sentences, "... qubits are of course represented by quantum particles. Photons, electrons, quantum dots and the current flow across weakly coupled superconductors (Josephine Junctions) are a few of the quantum materials tried as qubits." The second of the two is truer: A qubit can be made from just about anything, so long as it has at least two distinguishable states. Schrödinger's cat is a perfectly good qubit, as it can be found alive or dead under the appropriate measurement procedure. A qubit does not have to be a nuclear spin.

2) In the same sentence already quoted, those are Josephson junctions, not Josephine junctions. Josephine Junction is a town in Ontario.

3) I don't much like the imagery of "quantum parallelism" and think we have been missteering the public (and ourselves) a good long time with it. It's just a cheap way out; that's why everyone uses it. Some models of quantum computation—for instance measurement-based QC—clearly have nothing to do with parallelism ... so that already gives a proof-of-principle contradiction to the whole imagery. Have a look at these articles for your general education: `http://www.arxiv.org/abs/quant-ph/0003084` "A Quantum Computer Only Needs One Universe", and `http://www.arxiv.org/abs/quant-ph/0504097` "Cluster-State Quantum Computation".

4)

> "The roadmap presented by D-Wave Systems, Inc., at their demonstration evidences that quantum computing may follow a similar pattern, with an 'Online Quantum Computing Service' projected to be available in Q1 2008 and 'Enterprise Deployable System' available one quarter after that."

To propagate something clearly so farcical is dangerous, I think. A more reliable timeline, I would say, should you wish to quote something, can be found in the ARDA document, "'Quantum Computation Roadmap," which can be found here: `http://qist.lanl.gov/qcomp_map.shtml`. Look at page 4 of the Overview chapter, to see where we will probably really be by 2012. That's a far cry from an Online Quantum Computing Service.

5) On a different subject—i.e., I don't see how it's related to your present article—in your second note to me, you wrote, "I'd still be interested in putting some thoughts in this article from your paper on quantum mechanics as quantum information, specifically around the fact that some non-mathematical concepts are going to be required to describe what "it" is in order to get the computing community to take on quantum computing." I'm not quite sure what you're talking about, but if you are interested in what I see as the "essential (and ineradicable) incompleteness" of a quantum description of the world, let me recommend the following two (almost lay-) pieces to you. Maybe they'll tell you a little more on the subject, or at least give you some thoughts.

1. See Sections 4 and 5 of `http://www.arxiv.org/abs/quant-ph/0204146`.

2. See Sections 1, 5, 6, and 7 of the attached paper (which I don't have posted, but appeared in

this conference proceedings:
`http://proceedings.aip.org/proceedings/confproceed/889.jsp`).

But that's about all I can do for you now. If you want to "pelt" me with questions (as you said in your first email), it'll have to wait until sometime in the Fall.

Good luck on your article.

Larry's Preply

> My name is Larry Ketchersid, I am an old (in duration and age) friend of your father in law, Brad Lentz (from our Compaq days together). I have an interest in quantum mechanics and he has forwarded me one of your papers in the past.
>
> I run a security software company (`www.mediasourcery.com`), but in my spare time (usually between 1am and 2am) I write. I've written a book, *Dusk Before the Dawn*, and I am writing technology articles for a newsletter called *The Global Intelligencer* (`www.theglobalintelligencer.com`).
>
> I am working on my next article for the *Intelligencer*, and would like to do one on quantum computing. With D-Wave Systems demonstrating a quantum computer recently, it is a timely topic and one I have some personal interest in.
>
> The audience is not the most technical in the world, but I like to cite good sources (as I cited Mr. Schneier in my last article).
>
> Would you mind being pelted with questions and potentially quoted?

28-03-07 *An Anti-Fodor?* (to W. G. Demopoulos)

Thanks for the Fodor review of Frayn. I got a lot out of it, even though I read it in the wee hours (as I couldn't sleep—something that always happens when I really need to). Some remarks regarding our New York discussion later, but for the moment let me tell you about how his sentence, "For better or worse (I think, in fact, it's much for the better), almost nobody has 'a philosophy' any more," reminded me of a note I wrote to Joy Christian and Lucien Hardy recently. I'll paste it below. [See note dated 12-02-07, titled "Lord Zanzibar".]

It sounds like I don't go as far as Frayn goes, but on the other hand, it does seem pretty clear that I'm an anti-Fodor nevertheless.

More piffle later (his words), when I have some light to see by.

30-03-07 *Symmetry Considerations* (to C. H. Bennett)

Let me record a story while it's on my mind. You seem like a good repository for this one. The story's on my mind because the family and I are at Niagara Falls making our way to Waterloo for a short visit, and we saw the most brilliant rainbow through the Falls yesterday afternoon. They're just waking up as I write this. The Horseshoe Falls is 32 floors and a little hill's worth below me, just out the window to my left.

St. Patrick's Day was a couple of weeks ago, and Emma and I were having a discussion about leprechauns. I asked her if she thought they were real, and she said yes, though it seems for no strong reason. "And they'll take you to their gold if you catch them?" "Yep," she replied. "But, I don't believe there's a pot of gold at the end of the rainbow." "Why's that," I asked. "Because both sides of the rainbow are exactly the same. How could you make a distinction between the beginning and the end?"

I was a very proud father.

Say hello to Theo and the family for me.

01-04-07 *Bohm and Cash Value* (to W. G. Demopoulos)

Demopoulosism 29: *Any thoughts on this? It's from a recent email from Jeff.*

I think the criticism from the Bohmians and the Everettians will be something like this: "You've given your analysis of the significance of the quantum revolution. That's interesting, but there is a less radical alternative: we can continue to adopt the standard view of systems characterized in terms of their properties on Bohm's interpretation if we take position, and properties that supervene on position, as the only properties; alternatively, we can hold onto the standard view if we interpret superposition in terms of multiplicity on the Everett interpretation. So why should we accept your view?"

I had pretty much this reaction from Simon Saunders, David Wallace, and Harvey Brown to my talk at Oxford. And I have to say that I am still struggling with this – i.e., why I don't accept the Everett interpretation (it seems to me if you are going to be a Bohmian, you might as well be an Everettian, since you don't really need the trajectories – assuming you buy their decision-theoretic analysis of probability).

I won't answer you on Everett at the moment, but I've got a quick temporary solution with regard to Bohm. It's in the form of a note originally written to Hans Halvorson. Pasted below.

The main point I try to make is that one can make up *unobservable* ontologies underneath quantum mechanics ad infinitum. Bohmian mechanics is not unique in that regard: I could just as well say there is an individual angel guiding each quantum system, and that would be an ontology. But the Bohmians pretend to be doing science rather than something so fanciful because they have an equation. But so what?, I ask. If the equation is unobservable, it is no better than the angel if it doesn't have some further cash value in the sense of facilitating physical calculations. If there were some evidence that it did do that—facilitate calculations—I'd be all for it. (If the angel ontology facilitated calculations, I'd be all for it too.) But I've never seen any evidence of that sort.

The psychological origin of this criterion comes from my early interactions with Jeff Kimble. When I was first a postdoc at Caltech, he would never speak to me. I mentioned this to Mabuchi once and he said, "Oh you just have to beat him at something, and then he'll take you seriously." And as it happened, one day during one of his group meetings, I disagreed with Kimble on some physical point. When I was able to defend myself, he became a different person to me. And that's my point about Bohmianism. Let me take a standard problem of quantum mechanics—one still unsolved by all known means. If the addition of Bohmian trajectories were to facilitate the solution, I would take the trajectories seriously. They would remain, of course, unobservable, but their cash value would be clear enough to me and, hence, worth contemplating.

You see, for the physicist—as opposed to what I take for the average philosopher of science—consistency of a theory is not enough to give it credibility. It has to feel like it's going to give something new in the *next* step, not just tie up a loose end in an *old* one. I like the way Scott Aaronson put it in his blog a while ago. See the entry "Mistake of the Week: Belief is King" at `http://scottaaronson.com/blog/?cat=3D9`. Two relevant paragraphs in there are:

> The reason I'm harping on this is that, in my experience, laypeople consistently overestimate the role of belief in science. Thus the questions I constantly get asked: do I *believe* the many-worlds interpretation? Do I *believe* the anthropic principle? Do I

> *believe* string theory? Do I *believe* useful quantum computers will be built? Never what are the arguments for and against: always what do I *believe*? ...
>
> In my view, science is fundamentally not about beliefs: it's about results. Beliefs are relevant mostly as the heuristics that *lead* to results. So for example, it matters that David Deutsch believes the many-worlds interpretation because that's what led him to quantum computing. It matters that Ed Witten believes string theory because that's what led him to ... well, all the mindblowing stuff it led him to. My beef with quantum computing skeptics has never been that their beliefs are false; rather, it's that their beliefs almost never seem to lead them to new results.

My beef with Bohmian mechanics is where does it lead us further than the angel theory?

In the case of an ontology of objects and effects, or whatever you want to call it—i.e., the stuff you and I have been talking about—I feel it will lead to the NEXT step. And its cash value has already long been apparent to me. The conception told me that I should look for counterparts to almost all quantum information phenomena (e.g. no cloning theorem, teleportation, superdense coding, etc.) in Liouville mechanics. I and several others looked, and we have found. (See for instance the Spekkens paper I recommended to you.) One would like to see the conception lead to many more things still, but that's what should be required of any potential ontology. Bohmianism seems to have been stillborn in that regard. It's had a 50-year shot at it; it's time to move on.

That's my answer.

10-04-07 *Title and Abstract* (to Č. Brukner)

Let me apologize again for being so late on this. Title and abstract below. I hope they will work for you.

> **Good Coordinate Systems for Quantum Foundational Questions?**
>
> Recently there has been much interest in the quantum information community to prove (or find a counterexample to) the existence of so-called symmetric informationally complete measurements (SICs). In this talk we show that there should be even more interest. For, under a robust measure of orthonormality for operator bases (one that does not build in any symmetry at the outset), one can show that SICs, if they exist, come as close as possible to being orthonormal bases for the space of density operators. Moreover, in contrast to the usual expression of the superposition principle (where bases are taken to be orthogonal sets of state vectors), writing a superposition principle in terms of SICs leads to a more intrinsically-quantum representation for quantum states. This is because the basis states, rather than being the easiest to eavesdrop upon (as the usual ones are), are actually the hardest. Furthermore, such states fulfill a few other extremal properties that make them very interesting. Because of all this, writing the quantum-state space in these terms gives hope for a direct derivation of it from a plausible information-theoretic constraint principle—a principle along the lines of the Brukner-Zeilinger proposal of information limitation, but applied solely at the single system level. Various aspects of this problem will be discussed.

10-04-07 *The Story of the No-Cloning Theorem* (to A. Wilce)

Wilce-ism 4: *During our talk last week, you mentioned that you and Carl Caves were responsible for the law-of-large numbers strategy for proving the no-cloning theorem. I'd like to include a* *reference to this in our paper – where does it appear?*

The paper is this one: `http://www.arxiv.org/abs/quant-ph/9601025`. Its final resting place is:

- C. M. Caves and C. A. Fuchs, "Quantum Information: How Much Information in a State Vector?," in *The Dilemma of Einstein, Podolsky and Rosen – 60 Years Later (An International Symposium in Honour of Nathan Rosen – Haifa, March 1995)*, edited by A. Mann and M. Revzen, Annals of The Israel Physical Society **12**, 226–257 (1996).

It looks like Section 7 is the place, though I now wonder how much of the argument was us and how much of the argument was already folklore already long before then.

10-04-07 *Squeamish on Everett* (to W. G. Demopoulos)

Demopoulosism 30: *I'll look forward to what you say about Everett. Regarding it, I would have thought that no one would want to be stuck with such an absurd view unless they were hammered into it. But that appears not to be the case. I recall Harvey and Jeremy once trying to argue that it was no less absurd than the thought that there is a continuum of events, but I've never understood why they see this to be the same. If the issue were one of the continuum, then perhaps so. But that isn't what makes one balk at Everett.*

That is because it is a *temperament* that has essentially nothing to do with the details of quantum mechanics. And that temperament has only found a recent home in quantum mechanics. Read the James quote below to see what I mean. If you listen to one of Max Tegmark's talks on the beauty of many-worlds, you'll see little difference between his take on it and James' description of the Hegelians.

I've written a couple of things contra-Everett. They're not as tight of formulations as they should be, but they reveal my essential squeamishness with the idea. See Section 4, "Psychology 101," of `quant-ph/0204146`. Another place to look is in my *Notes on a Paulian Idea*; see notes to Howard Barnum dated 30 August 1999 and 5 September 1999, "It's All About Schmoz" and "New Schmoz Cola."

The problem, in the end, is that Everettism is simply empty.

12-04-07 *Effects of Effects* (to W. G. Demopoulos)

I'm sure this note will get to you too late for you to see it before the Bubfest, but let me wish you the best of luck with your talk. I'm biased of course, but I think it'll be one of the most important at the meeting. Let's hope this will be the beginning of a long list of effects arising from the recognition that effects are central for our understanding what QM is about.

I write this because I won't be able to come to the meeting after all. What's going on is that it's become clear that just too many things have to be done before we can put our house on the market [. . .] So, though the Bubfest represents probably the most important meeting for me all year long, I couldn't come to it in good conscience. I hope you will understand.

I hope you'll give me a full report of your talk and the reaction to it as you get a chance. And when I get to Waterloo, let's conquer the quantum world!

13-04-07 *Happy Bubfest* (to J. Bub)

It's with some sadness I have to write you this note: Happy Bubfest! Paradoxical, but necessary. The reason is I won't be able to come to the New Directions meeting this year. I really, really apologize and am kicking myself that this had to happen. What's happening is that I'm in the middle of buying/selling houses, in preparation for my move to the Perimeter Institute, and everything hit me at once and at last minute. My wife and I were up until after midnight last night negotiating the Waterloo house (our third offer finally got accepted), and our house here in Cranford now has to go on the market tomorrow. And with the latter it's been a mad dash to the finish line in getting parts of the house freshly painted and decluttered, etc., for showing tomorrow morning. Finally, yesterday afternoon it just became completely clear that I'd have to make a decision to stay at home this weekend. That's the story.

I know I'm missing the best foundations meeting this year, and it hurts (hurts me, hurts the program, everything). Still, at least I am confident you'll enjoy this well deserved honor of a meeting in your name. Thanks for all the inspiration over the years.

13-04-07 *Happy Bubfest* (to H. Barnum, A. Wilce, & M. Leifer)

I'm sorry I'm going to miss all you guys today and this weekend. (The story of my absence below.) [See 13-04-07 note "Happy Bubfest" to J. Bub.] The line-up of talks at this meeting looks fantastic, and on the top of my list is Howard's. Good luck with it. It has a chance of being a wake-up to the community, and I hope it will be. The philosophers don't seem to be very good at seeking counterexamples: All this talk about C^*-algebras "covering a *vast* range" of theories really grates, and I hope your talk will be a good splash of cold water for them. There's so much to be done, and it'd be great if they'd start using their thoughts more wisely. Lead the way!

13-04-07 *Relations and Information* (to B. C. van Fraassen)

I am so sorry I'm going to miss your talk at the Bubfest. I got caught up in all the stresses of a house sale/purchase, and it became clear that I could not afford to be away from home this weekend. Many of the talks look fantastic, but particularly the remark in your abstract, "It is a fascinating world in part because of Rovelli's reliance on the information-theory approach to the foundations of quantum mechanics ... " has me intrigued. If you have a paper version of the talk, I'd very much like to see it.

11-05-07 *Information Gain Disturbance* (to R. W. Spekkens)

Spekkensism 37: *What is a good reference for the following well-known result? The only set of pure states about which one can get information without creating a disturbance is a set of orthogonal states.*

That would be the Bennett, Brassard, Mermin PRL that came out soon after the Ekert protocol:

> C. H. Bennett, G. Brassard, and N. D. Mermin, "Quantum Cryptography without Bell's Theorem," Phys. Rev. Lett. **68**, 557–559 (1992).

11-05-07 *Information Gain Disturbance, 2* (to R. W. Spekkens)

Spekkensism 38: *I just had a look at it. They do show (around eqs. 3 and 4) that if two nonorthogonal states are left undisturbed, then no information is gained about them. It's obvious that the result doesn't apply if the states are orthogonal, but unfortunately, they don't say so explicitly. It would be great to find a reference that did say this explicitly. Can you think of one?*

Now that I think about it, for mixed states, the condition is no doubt that the set be a commuting set. Surely, somebody has pointed this out somewhere.

Just a short note: I've come in for a small break between mowing the left side of the yard and the right.

For mixed states one can gain information without disturbance even if they are noncommuting. So long as they are block diagonal with the same blocks. See Section 5 of `http://www.arxiv.org/abs/quant-ph/9611010`. Koashi later proved if and only if somewhere.

I'll be able to think about the fine details of your question and trawl my memory tomorrow morning. We've got to show the house to a potential customer tonight. That's why I'm scrambling for all to look beautiful.

11-05-07 *Big Drags and Little Drags* (to W. G. Demopoulos)

Demopoulosism 31: *I extracted a significant part of the paper and read it; I don't often do that, but the nature of the argument seemed to warrant this style of presentation.* [...] *I found the question period a little disappointing; Paul Teller said that he couldn't see how I was saying anything different from Bohr.*

Maybe he's right ... but the holy scriptures can be interpreted many ways. More importantly that doesn't bar us from trying to say things more clearly than Bohr ever could ... and we are definitely saying them more clearly. Anyway, seriously, I think I would say Paul is probably close to the right judgment. In fact, it strikes me that in our own discussions—the discussions between you and me—you are to Bohr what I am to Pauli. I.e., their divide is our (present) divide. For Bohr, the "device" was the repository of the effect; for Pauli the "device" should be considered a prosthesis of the agent.

14-05-07 *Information Gain Disturbance, 3* (to R. W. Spekkens)

Spekkensism 39: *I just had a look at [BBM]. They do show (around eqs. 3 and 4) that if two nonorthogonal states are left undisturbed, then no information is gained about them. It's obvious that the result doesn't apply if the states are orthogonal, but unfortunately, they don't say so explicitly. It would be great to find a reference that did say this explicitly. Can you think of one?*

Is this more like the statement you were looking for:

> On the other hand, if Eve definitely knows that the initial $|\psi\rangle$ is one of the orthonormal vectors $|e_n\rangle$, but she does not know which one of them it is, she can unambiguously settle this point by a non-demolition measurement [4], which leaves the state of the system unchanged.

It is from the top of page 3 of my old paper with Asher: `http://www.arxiv.org/abs/quant-ph/9512023`. I don't know how reference 4 had said things.

... just catching up.

14-05-07 *More Info Gain Disturbance Tradeoff* (to R. W. Spekkens)

Spekkensism 40: *All of this talk has jarred my memory. I now remember how at the end of one of your papers on state disturbance you show that the condition for information gain without disturbance is not commutativity, but rather commutativity on a subspace. I also remember that I had a problem with this argument, so I was led to dig up some old notes on this question of infogain-disturbance for mixed states which I wrote way back when I was working on cryptography with Terry. Here is what I said:*

At the end of his paper, Fuchs suggests that all that is required for a measurement to be non-disturbing is for the state of the system to be unchanged. However, this seems to me to be necessary, but not sufficient. A sufficient condition is that the state of any larger system of which the system is a part is unchanged. The reasoning is as follows: if the correlations between A and B are disturbed due to a measurement on B, then clearly one's predictions about the outcomes of measurements on B suffer, specifically, one has reduced predictability for measurements on the composite of A and B.

A natural measure of this revised notion of disturbance is the probability of failing a test for a purification of the state.

So, whereas Fuchs argues that one can gain information without disturbance for commuting density operators, and even for density operators that fail to commute (but do commute on some subspace), I would say that the notion of disturbance considered by him is an unnatural one. Under the revised notion of disturbance, I suspect that the only sets of density operators about which one can hope to gain information without disturbance are orthogonal density operators.

I think your question is an interesting one, and I don't know the answer. But I don't think—for a q-Bayesian, at least—that there can be a most-natural / least-natural distinction. These problems are driven by the assumption of what the agent knows and which systems he has in his possession and control. You imagine a situation where the sending agent has a reference system lying about and that he has a declared quantum state for the totality, even though he only sends part in one shot. I imagined a situation where the agent sent everything he has per shot. Just a different situation; neither is more fundamental or natural as far as I can see. It would be very nice to find info-disturbance questions for which the answers are 1) "iff orthogonal density operators" and 2) "iff commuting density operators." I always liked the achievement of the Holevo bound on accessible information in that it was "iff commuting."

15-05-07 *Couldn't Slice It* (to R. Schack)

Unfortunately, no matter how I tried to slice it, I couldn't make a small stay in London viable. So, I guess I won't be seeing you this trip after all. The trouble of course is my damned insistence on restricting myself to American Airlines. Bypassing any time in London and arriving in Newark at 10:00 PM, made the cost of the trip within ethical bounds. (Basically the lowest price I could have found anywhere, though of course other airlines would have given much better schedules.) Arriving for a small visit to you in London at 4:00 PM would have upped my price \$120, and arriving at 1:00 PM would have upped it \$220—and besides, I would have still gotten into Newark relatively late in the day. So, I just dropped the idea, and swallowed the idea of arriving at 10:00 PM (but a day earlier).

My existential state is complete misery at the moment. It is the true evil of money—not living the life of an Erdos, or a Wittgenstein, or a Tolstoy. Hardly a couple has even looked at our house, and it is already mid-May. And as you can guess, monetarily, I've got everything tied up in this house. I have no fluidity at all. So, between the Waterloo house pending (July 31 latest) and all the expenses of the flood damage (waiting even to file insurance claims, much less collecting on them), I am up in arms. And there's no great sign of relief in the real estate market here. So my behavior has become one of OCD for this dirty subject; not at all the life of an academic. Pray I survive to do science once again one of these days!

On better things, have a look at John Baez's blog "This Week's Finds in Mathematical Physics" (week 251, the latest edition). He gives a small explanation (very small, but not infinitesimal) of the quantum de Finetti theorem.

29-05-07 *Foils Is a Very Broad Term* (to D. M. Appleby)

Applebyism 14: *Rob did get in touch and I said I did want to go. However, I am feeling a bit worried. When you originally asked me I didn't pay much attention to the title* ["Operational Probabilistic Theories as Foils to Quantum Theory"]*. It is a workshop on quantum mechanics, you are going to be there, along with several other people I find interesting, so I automatically said "yes". But Rob has now sent me this:... At the end he speaks of bringing "everyone up to speed with what others are working on". But I am not working on operational theories.*

Anyway, I don't think you'd be an imposter at that meeting at all. And, looking carefully at Rob's call for abstracts, I couldn't find the word "operational" or "operationalism" at all. Probably it shows in the official title of the meeting ... but then that should have tweaked your fears long before the abstract request.

That aside, I view the issue of foils to quantum theory a very general question—a game that can be played whatever one's interpretational stance. And in the list of attendees that I saw, I would say at least Wootters, Kent, Caves, Barrett, and Leifer could not be counted as "operationalists." And in the case of Spekkens, and maybe Barnum, I'm pretty sure they only see it as a means to an end anyway—a kind of stopgap method for bringing them ultimately to some kind of realism.

In my own case, what I want to know, and what I want to focus thinking on during the meeting is what is the significance of the damned $\mathrm{tr}\, A^2 = 1$ and $\mathrm{tr}\, A^3 = 1$ conditions for defining the quantum state space. Who ordered that? Why? Particularly, when written in terms of an SIC expansion, what do they mean? For, I want to see the two operator equations as information theoretic constraints on sets of probabilities, and—I think—the best way to translate them into something of that flavor is to use SICs as the intermediary. Then the first condition is a constraint on a Rényi information. But what is the second equation?

What would be a foil theory to QM in this case? Drop the one condition while retaining the other. What would the resulting theory entail? At least, that was the sort of thing I envisioned as good busy work while waiting for a better or more natural question.

And because I was thinking I wanted to think about those kinds of things, much more so than "nonlocal boxes" and the like, I thought it'd be great if you were there—that maybe you'd share some of my interest in these questions. Moreover, I thought that you might get something out of thinking about all your SIC-existence work within this kind of context. So, scientifically I think the meeting would be fruitful for you.

31-05-07 *Vienna Traffic Lights* (to C. H. Bennett)

Noticed you're on the list for the Vienna meeting next week. When do you get there; when do you leave? We'll have to have some conversations near a traffic light and see if we can reproduce the phenomenon we noted in Tsukuba. (Contradictory to the point of debate though that sentence be.) [See 13-12-06 note to Schack, titled "More on Identity," for an explanation of this.]

11-06-07 *Objective Indeterminism* (to Č. Brukner)

I enjoyed talking to you the other night; I hope it is the sign of much more to come. At the moment I write this, I am over the Atlantic working my way back home. Particularly, I've been thinking about our little exchange on how one might have objective indeterminism at the same time as subjective probabilities—my view of course, that is, the one you took issue with. Anyway, it led me to dig through my old correspondence to see what I had actually written on the subject. Apparently not as much as I thought I had. Still, I thought I'd compile for you what I found, even as incomplete as the thoughts are at this stage. The file is attached.

The main letter to read is the second one, to Kirk McDonald. I wrote it in a fit of passion, as he had written an insulting letter to me (saying that my view boiled down to "physics = psychology"), but nonetheless, I think that letter best captures the point. Bayesian probabilities need not solely be "ignorance probabilities" (as I think you said) where the ignorance is of some actual existent, but rather probability in any context of uncertainty ... whatever the cause of the uncertainty. In the case of quantum measurement, the uncertainty is there because the outcomes do not pre-exist the measurement. Measurement is an act of creation, bringing something new into the world: That is the ultimate reason for the uncertainty. And what more can a poor quantum gambler do but express his beliefs about what will come into existence? Alice has her beliefs about an event; Eve has hers. They are not in conflict because the beliefs are not objective properties of the event.

Actually, while I'm on this subject, I think I'll also attach a little pseudo-paper of mine, "Delirium Quantum" (it appeared in an APS conference proceedings, but I never posted it on `quant-ph`). It describes in more detail the sort of sense in which I think nature is "on the make"—that is, being hammered out, being created, in quantum measurement interactions.

Thanks for organizing a great conference. I thoroughly enjoyed it and got so much out of the discussions with so many. Despite your weary words the evening of the banquet, I hope you'll organize another one soon!

12-06-07 *The Sweep and Dash of Mathematical Structures* (to M. Tegmark & C. H. Bennett)

I'm flying over the Atlantic, slowly making my way home, and I'm using the time to take care of various loose ends that arose at the conference. Below is the quote by William James with the highfalutin' words I told you both about. I thought it captured the point Charlie brought up with his question, but maybe not. Charlie can be the judge of that.

What James was talking about directly was a certain strain of philosophy at the time—a belief that the world arose from something called the Absolute. The name to look up in an encyclopedia if you're interested is "Absolute Idealism." Hegel's thought was an example of it, as was F. H. Bradley's.

James wrote these words in 101 years ago in 1906:

> [I]f you are the lovers of facts I have supposed you to be, you find the trail of the serpent of rationalism, of intellectualism, over everything that lies on that side of the line. You escape indeed the materialism that goes with the reigning empiricism; but you pay for your escape by losing contact with the concrete parts of life. The more absolutistic philosophers dwell on so high a level of abstraction that they never even try to come down. The absolute mind which they offer us, the mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular

> from the notion of it. It is compatible with any state of things whatever being true here below. And the theistic God is almost as sterile a principle. You have to go to the world which he has created to get any inkling of his actual character: he is the kind of god that has once for all made that kind of a world. The God of the theistic writers lives on as purely abstract heights as does the Absolute. Absolutism has a certain sweep and dash about it, while the usual theism is more insipid, but both are equally remote and vacuous.

In place of "absolute mind", as far as I can tell, you could insert Max's "mathematical structures" and little of the meaning would change. Now, of course, "mathematical structures" makes reference to equations, symbolic logic, and the like, whereas the Absolute does not. So it would seem to have a certain sweep and dash about it, far beyond anything the Absolute could muster. More accurately, it gives the appearance of being more impersonal and scientific than the theistic God. But, like James, I cannot help but feel that this is only an appearance. When a plan of the multiverse becomes so disconnected from what is seen in our actual universe, one might as well invoke a bearded old God—the two conceptions have no operational distinction, precisely because, as you pointed out (celebrated even), the ultimate, all-inclusive mathematical structure has zero information content.

James, by the way, in that same lecture makes the first use of the term "multiverse" that I know of—though he meant it in quite a different way than you guys.

Max, I did enjoy your talk (and even more so the PI version of it) and many of your comments, which gave me lots of giggles. Charlie says that's the best way to do science, and listening to you helped me clarify several of my own thoughts. I also liked your comments to the students at the round table too; they were on the mark.

12-06-07 *Quantum Chris* (to M. Tegmark)

You also asked me about my own view of quantum mechanics. For some reason, at that moment, I felt a little reluctant to talk about it. Let me try to make up for that, in case you're still interested in the answer.

Maybe the single best place to look for a starter is this paper: `quant-ph/0205039`. But that can maybe be supplemented with: `quant-ph/0404156` (particularly the introductory and concluding sections), and `quant-ph/0608190` (particularly the concluding section). A couple of other significant pieces of the story are told in these papers by Marcus Appleby and Matt Leifer: `quant-ph/0402015` and `quant-ph/0611233`. (FQXi, by the way, partially funds Leifer ... and thus, I would guess this kind of thought? Maybe you should watch where your dollars are going!!)

The view is a kind of realism about quantum objects, but one that strikes reality from the wave function. The significant, stand-alone property of quantum systems is instead that they are catalysts. They are catalysts that take part in the creation of events. It is a view in me that owes much to the John Wheeler of the 1970s and 1980s.

I hope that helps some.

16-06-07 *Article for Nature* (to M. Buchanan)

Funny you should ask for my thoughts on many-worlds; I'm not exactly a sympathizer. For instance, my talk at the *Fifty Years of Everett* conference coming up in September is to be titled "13 Direct Quotes from Everettian Papers and Why I Find Them Unsettling" (or at least that's the title I sent the organizers a few months ago). But, yes, we can talk if you like—though Tuesday or Wednesday would be better dates for me.

¿ I've read a few things that suggest to me that the Everett view (and I ¿ suppose there are many variations of it) has been gaining adherents in ¿ recent years. Is this true, and if so why?

It is true that the Everett view is gaining adherents. But I think it's powered mostly by a lack of imagination and a usual historical cycle working its way through. Putting it bluntly, it's just the newest species of the "serpent of rationalism." Read this little piece William James wrote 101 years ago for his Lowell Lectures:

> [I]f you are the lovers of facts I have supposed you to be, you find the trail of the serpent of rationalism, of intellectualism, over everything that lies on that side of the line. You escape indeed the materialism that goes with the reigning empiricism; but you pay for your escape by losing contact with the concrete parts of life. The more absolutistic philosophers dwell on so high a level of abstraction that they never even try to come down. The absolute mind which they offer us, the mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below. And the theistic God is almost as sterile a principle. You have to go to the world which he has created to get any inkling of his actual character: he is the kind of god that has once for all made that kind of a world. The God of the theistic writers lives on as purely abstract heights as does the Absolute. Absolutism has a certain sweep and dash about it, while the usual theism is more insipid, but both are equally remote and vacuous.

"The multiverse which they offer us, the mathematical structure that makes our universe by containing it within, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below." My own feeling is that the MWI is simply empty of content—thus a dead end for physics.

Let me give you some links to pieces I've written to shore that feeling up a bit—ones written in a less formal, (hopefully) more entertaining fashion. See:

- My pseudo-paper `http://www.arxiv.org/abs/quant-ph/0204146`, particularly Section 4 of it (originally a letter to John Preskill), for my most sweeping discontent with the notion.

- My samizdat `http://xxx.lanl.gov/abs/quant-ph/0105039`. Just look in the index for "Everett" and then flip to the pages. Maybe pages 19–22, 92–93 ("Challenge to the Everettistas"), 356–358 and other letters to Preskill, 461–462 (letter to Spekkens), 472–473.

- And for something a little more serious, maybe the introduction and first couple of sections of this paper `http://xxx.lanl.gov/abs/quant-ph/0205039`.

The main point I try to get at there is that Everett (like most other interpretations) takes Hilbert space, state vectors, unitary evolution, inner products, tensor products, and all that mathematical machinery as the *starting point* of the interpretation, and then *contrives* a story around them. (And in the Everett case, it's the most trivial story—one that could have been written, and as the quote above shows, was in fact written, by 1906.) It should not be that way. It should be the opposite: The story should be written first, and one should be forced to the mathematical formalism from the story alone.

To give you some hint that this is not simply a pipe dream, have a look at the introduction to Rob Spekkens' "toy theory" paper: `http://www.arxiv.org/abs/quant-ph/0401052`. It's a beautiful piece, even though it's not quantum theory directly. What Rob does is start with a simple principle that "maximal information is not complete information" for a toy system and then looks at the properties of these states of information. The lovely thing is that they have *so many* properties that one might have thought were unique to quantum mechanics. And yet, there is no hint of parallel worlds in the theory at all. "Is that a coincidence?," one should ask. No, of course not—the toy theory is explicitly constructed with the idea that there is one true world and what one is speaking of is one's ignorance of it. But yet, as John Smolin points out, one could make a many-worlds interpretation of the toy theory! And he's right. But notice the order of things. The mathematical structure was forced by the information principle alone; the many worlds interpretation of it is a grotesque appendage. (Though John, being a sympathizer of MWI, would not call it grotesque as I did.)

Maybe a better person to ask this of is Howard Barnum. He can give you a detailed account of what he sees as positive in the newest papers, but also, he can pinpoint exactly where he thinks they ultimately fail in their goal (particularly, of deriving the Born rule for quantum probabilities).

That's enough for now, enough to get you started. Let's correspond again Monday or Tuesday about a good time to talk; I'm not quite sure what my schedule is going to be yet.

01-07-07 *First Einstein Quote* (to R. W. Spekkens)

Attached is part of the Einstein quote I was telling you about, from his "Reply to Critics" in the Schilpp volume. Unfortunately, I do not have the whole quote with me here. Where I write "granted" and quote him, that is the part where he describes what he means by "physicist B." What he is grasping to express there is, I'm pretty confident, a physicist like myself or Asher or Carl or Rüdiger, one for whom "unperformed measurements have no outcomes."

The main reason I brought this up tonight is I think you are right that Einstein didn't make a clear distinction between psi-epistemic and psi-supplemented: I would guess for him, that he was simply using the word "incomplete" in the restricted fashion of meaning psi-epistemic only. However, he did at least make one distinction that's pretty important to me: between "psi-epistemic about pre-existent properties" and "psi-epistemic about the consequences of interventions". I think he had that distinction pretty firmly in mind (probably from talking to Pauli), and in the present passages attempted to show that, upon the assumption of "the real factual situation at 2 being independent of what is done at 1," wave functions could not be "psi-ontic about the consequences of interventions." I.e., they could not be objective propensities or some such.

Had a nice long walk with Howard and Marcus tonight, talking about SICs and convex structures. This is already shaping up to be a really good meeting.

The More Pure Einstein

> **Granted:** "The individual system (before the measurement) has no definite value of q (or p). The value of the measurement only arises in cooperation with the unique probability which is given to it in view of the ψ-function only through the act of measurement itself."
>
> **Consider spatially separated systems S_1 and S_2 initially attributed with an entangled quantum state ψ_{12}.**
>
> "Now it appears to me that one may speak of the real factual situation at S_2. ... [O]n one supposition we should, in my opinion, absolutely hold fast: the real factual

situation of the system S_2 is independent of what is done with S_1. ... According to the type of measurement which I make of S_1, I get, however, a very different ψ_2 for [S_2]. ... For the same real situation of S_2 it is possible therefore to find, according to one's choice, different types of ψ-function.

If now [physicist B] accepts this consideration as valid, then [he] will have to give up his position that the ψ-function constitutes a complete description of a real factual situation. For in this case it would be impossible that two different types of ψ-functions could be coordinated with the identical factual situation of S_2."

01-07-07 *Second Einstein Quote* (to R. W. Spekkens)

And below is the second Einstein quote I mentioned tonight, along with a Pauli quote. It's a section from my new samizdat (a letter to Hans Christian von Baeyer). [See 02-01-06 note titled "The Oblique Pauli."] I can't believe I so botched my reporting of it tonight—my brain wasn't working. Anyway, this little quote came soon after the earlier, more technical quote I just sent you.

06-07-07 *Renner Monday* (to R. Schack)

I hope you'll arrive here early Monday morning. The schedule has been revised and Renato Renner will be giving the 10:00 talk before Ben Toner's on de Finetti theorems in the afternoon. The subject of Renato's is supposed to be something like "the de Finetti theorem as a precondition to doing science." Sounds fishy to me. It'd be nice if you'd be in the audience to help me out at those tough moments.

16-07-07 *Question, Possibly Addled* (to A. Wilce)

It was great to see you too. Your clever comments throughout the week were enjoyed as ever. And I'm glad too of your story about Harvey Brown. These guys should stop and think, in the way you suggested, about what actually does compel them *if anything* to the MWI—they rarely do. I think the class of examples in general convex set theories you bring up is great for cornering them on the subject.

About SIC-POVMs, yes, the definition does require that the elements of the POVM be rank-1. You ask, "if so, what advantage does that yield?" Aha, I caught you! The word is that that point was explained at a certain talk at Susquehanna University ... (Or at least I hope it was.) I don't think there was originally any motivation for that particular restriction, other than to keep the problem from being too easy. (Your construction, for instance, is just fine.) But now things are a little more compelling. My favorite reason for the restriction is that, by way of it, one comes geometrically as close to an orthonormal basis as one can on the positive cone. See Section 2 of the attached paper. So a SIC-set is a kind of stand-in for the idea of an orthonormal basis. That's got to be good for something, I think, though I've yet to be able to express completely why.

I think it'll also be the case that if one generalized the idea of minimum uncertainty states we talk about in Section 3 to generally mixed states, one will find again the rank-1 density operators are the only ones that will fulfill the criterion.

Maybe there are other reasons to prefer the symmetric sets of rank-1 states over constructions (like your own) made of mixed states. I should try to think of whether there are other reasons. For instance, would mixed-state constructions like yours wreak any havoc on their equations corresponding to (5) and (6) in the attached paper for defining pure states? Maybe they wouldn't?

Maybe they'd actually make for cleaner equations. That would intrigue me greatly if it could possibly be so.

Can you send me references, by the way, for your papers on topologizing test spaces? If you have any interest in exploring it further, I'd like to come back to your suggestions for generalizing Gleason: I.e., identifying or classifying or finding some interesting characteristic of those test spaces for which the frame functions must be continuous (so that one has no dispersion free states). Once at PI, I'd like to think much harder about that.

31-07-07 *New Address* (to M. G. Raymer)

Raymerism 1: *Thanks for including me on your list. I really enjoy reading your various papers and long, recorded email diatribes on QM.*

In my quantum state, my belief is that quantum states are not simply matters of belief, but I think the idea goes a long way toward dramatizing the matter in the right direction.

Thanks for the kind words. And you shock me that have read any of my samizdats! Very dangerous business …

But one thing to keep in mind about Caves, Schack, and me in our terminology: Try to resist the temptation to think that just because we categorize quantum states as "states of belief" that it means any unique individual can believe anything he wants to believe. I.e., something like, "I'll make up any quantum state I damned well please." If that's part of your aversion to the idea, then try to resist it. In the words of Marcus Appleby, "It's really hard to believe something you don't actually believe" and the same is true of the conception of quantum states as states of belief. When an experimentalist writes down a particular quantum state, he does it for good reason; it isn't made up on the fly, out of nothing. All we are saying is that no part of that "good reason" has anything to with some property intrinsic to the quantum system.

Looking forward to more discussions another day. Steven van Enk had invited me to give a colloquium at U. Oregon this year, but I had to drop out because of too many pressures. Maybe next year you guys will give me a second chance, and we can pick back up then.

01-08-07 *Your Conference* (to F. Topsøe & P. Harremoës)

Topsøe-ism 1: *Does this mean that you (and … ?) may be interested in our workshop "facets of entropy", cf.* `http://facetsofentropy.fys.ku.dk`*?*

Well, I didn't know about your workshop, but now that I do, indeed I am very interested. In fact, I had been meaning to write you and Peter (to whom I'll cc this note) for some time to ask you a question on a rather funky kind of "entropy" (though I dare use that word for this) that has been fascinating me lately. Truth is, I don't actually know what the object is, or whether it has any information theoretic interpretation at all. And I was going to ask you if you've ever seen anything like it *within* standard, classical information theory, i.e., outside of quantum considerations.

In particular, I was going to write up the note a little more formally in LaTeX and send a PDF to you once arriving at PI. But now that I'm writing you anyway, let me pose the question in a little sloppier way. I'll just refer you to the quantity on the left-hand side of Eq. (6) in `http://www.arxiv.org/abs/0707.2071`. Now, divorce it from its original motivating problem, and just imagine the coefficients c_{ijk} are fixed constants handed down from heaven. The question is, does that kind of quantity have any interesting information theoretic (or maybe game theoretic) meaning? Are there any constraints on the c_{ijk} needed in order to make the expression make sense information theoretically?

It strikes me that if I could get a handle on that *style* of quantity (i.e., somewhat independent of the particulars of the c_{ijk}) in a way completely independent of quantum mechanics, then I would have learned something deep.

But a potentially independent question. If you read the paragraph surrounding Eq. (6), you will find that it arises from a consideration to do with a quantum mechanical version of the Daróczy order-3 entropy. But the Daróczy order-3 entropy?? Who ordered that? Have you ever run across any actual use of that particular entropy before?

Now, that I've posed the question, I'll probably be somewhat silent for a while . . . but don't let that stop you from writing me even if I don't reply. It's just that I've got a lot to do to get my house in New Jersey sold and get moved up to Waterloo in the next 9 days. I'll be back in touch for sure, once I arrive at Perimeter.

02-08-07 *Perimeter Visit* (to H. Mabuchi)

Mabuchism 4: *I've been invited for a Perimeter colloquium — when would be a good time for me to come, in terms of your schedules?*

God knows with me. Particularly, until I fulfill my 50,000 mile quota for the year with American Airlines (so that I keep my platinum status), I'm likely to be a wild card . . . selling my soul to travel whenever the opportunity arises. And I'm running behind this year, only 32,000 so far. Pathetic, huh? Anyway, it means at the least I may set up a trip to Beijing before the year is out. Not quite sure when yet.

Here's a couple of blackout dates: [. . .] At the moment, Spring is wide open for me.

Clearly, the strategy ought to be that you just give me your dates, and I try my best to be at PI while you're around.

Congratulations on inheriting Ed Jaynes' office. Now here's a real task for you: Find the desk that Ed Jaynes actually sat at. If you can dig it up, you'll have to let me sit at it, at least for just a minute. Once upon a time, I had a chance to sit at Niels Bohr's desk while no one was looking, and I didn't take the opportunity. I've been kicking myself since.

But more congratulations than for the office, congratulations for the new position! Is it a fancy named professorship? If so, what's your name?

02-08-07 *The One-Belly Theory of the Universe* (to S. J. van Enk)

By the way, I started to compose a note to you several months ago, and then never came back to it. It was titled "The One-Belly Theory of the Universe." When I get to Waterloo, I'll finish it I promise—I think it's actually an important idea. Here's the upshot of it. Alice and Bob are at two ends of a lab, checking for Bell inequality violations. The question arises, are the detectors at both ends of the lab going "click" in this process. My answer is, it depends upon one's perspective. From Alice's perspective, her detector is certainly going click, as she collects the outcomes into her awareness. But also from her perspective, Bob's is NOT going click at all. As far as Alice is concerned, Bob has simply become entangled with the particles. The reason I make these remarks has to do with the last technical discussion we had—I think it may be the key to understanding . . . well, everything. In the next note, I'll at least send you the beginning of the note I had started.

Then you pressure me some in two weeks and I'll finally finish it. But you're so clever, I'm sure you'll see the idea right away … invalidating my needing to finish it after all. Still, I will.

"The One-Belly Theory of the Universe", first paragraphs, to be completed

van Enkism 11: *I'm still not sure how I would violate Bell inequalities with only positive probabilities and extra hidden variables in my belly. After all, the hidden variables of the system on Alice's end and in her belly, and the variables of the system on Bob's end plus his belly are still local hidden variables, aren't they?*

That's the wrong way to think about it. I don't know whether thinking about it in the right way will give rise to a Bell inequality EITHER, but at the very least I can point out that the above is the wrong way to think about it. Here goes.

What I'm going to say is prompted by this thought about the actual quantum situation. Go to any exposition of the Bell set up, and one will see the whole affair laid out in terms of *two* devices going click-click-click, one on Alice's side and one on Bob's side. Then one asks if the data can be "explained" by a local hidden-variable model. What that is taken to mean is whether the actually-observed frequencies are the ones predicted with near certainty by various i.i.d. distributions over the sequence of experimental runs (conditioned by the A and B devices' settings). Then one focusses on the single-shot distributions (for the various settings) and asks whether they can be derived as the marginals of a certain variety from a single deeper probability distribution. Thus arises the Bell inequality, and the conditional probabilities given by quantum mechanics violate it.

Then a sweeping claim is almost always made: That the Bell inequality was derived in a *theory-independent* fashion (aside from the assumptions of locality and the existence of a grand joint distribution that could be marginalized), and quantum mechanics—a particular theory—violates it.

But I'm now starting to think that the premises of the Bell derivation don't even fall within the framework of a quantum description of the whole thing. Thus, perhaps, it is no wonder that quantum theory violates it. Think Wigner's friend.

07-08-07 *Last Email from Bell Labs* (to A. E. White)

I'm just signing out, and I wanted to send you a short note to say thanks for "tolerating me" at the Lab for so long: I know that my research didn't exactly fit in with the present incarnation of Bell Labs and the other departmental interests. But the company nurtured me nevertheless, and I am grateful for my years there. I wish you all the best of luck in the future. If you're ever near Waterloo, Ontario, please visit us at the Perimeter Institute for Theoretical Physics.

15-08-07 *Title* (to L. Hardy)

"Quantum States as Uncertainty, pure and simple. But, Uncertainty about What?"

Probably the longest title you have, but I like it. Subjects include: Einstein's pre-EPR argument, contrast between frequency and Bayesian interpretations of probability, Dutch book argument, no-cloning and no-broadcasting theorem, example of classical no-cloning theorem and yes-broadcasting, standard (non-Spekkens) Kochen-Specker theorem, words on the Paulian ontology in light of KS, ending with fiducial measurements (SICs and MUBs) and the shape of quantum state space.

Does that fit well (or contrast well) with the other talks? I hope so.

20-08-07 *Rényi Order-3 and the Weird Object* (to F. Topsøe & P. Harremoës)

I never heard back from either of you concerning the note below. So, now that I am physically at Perimeter, I'll take the opportunity to send it again, just in case you didn't receive it. It is pasted below. [See 01-08-07 note "Your Conference" to F. Topsøe & P. Harremoës.] (Maybe you didn't get it because I sent it out too close to my departure time on the Bell Labs server.)

If you don't understand the question, let me know, and I'll write it up more formally for you. But also, while I'm here, maybe I can also ask the following. Do either of you know any properties of the Rényi and Daróczy order-3 entropies that set them apart from the pack of other Rényi and Daróczy entropies? Do the order-3 entropies have any *interesting* special properties that the others don't have?

21-08-07 *Rényi Order-3 and the Weird Object, 2* (to P. Harremoës & F. Topsøe)

Thanks for the replies. In case it helps to understand my main question, I attach a slightly more detailed description than my previous. Maybe it helps to set the question outside the context of my previous paper.

Peter wrote:

Harremoësism 1: *The result that* $\mathrm{tr}M^2 = 1$ *and* $\mathrm{tr}M^3 = 1$ *implies that* M *is a 1-dimensional projection can be derived as follows: ...*

For any odd $n > 3$ *one can replace the condition* $\mathrm{tr}M^3 = 1$ *by* $\mathrm{tr}M^n = 1$ *and get the same conclusion. Therefore I think that there is nothing special about the power 3 in the setup.*

Yep, I had understood that derivation and your caveat at the end for quite some time. Still, I call the Jones/Linden result a "remarkable theorem" because it strikes me as being like the no-cloning theorem: namely, trivial in a mathematical sense, but—I think—deep in physical meaning. Plus you would be surprised at the caliber of some people who were not aware of it until it was brought to their attention. For instance, neither Carl Caves, nor Dan Gottesman, nor Rob Calderbank, nor Beth Ruskai, nor Elliot Lieb!!! knew of that simple result—I checked with each one of them. Just one of those simple things overlooked by history (much like the no-cloning theorem until 1981).

Anyway, about the caveat, I'm not sure it makes such an impact on me. It may be the case that there is nothing special about the power 3 in the setup where "quantum states" are already assumed to operators M whose extreme points are characterized as above. But part of the goal for me is to jettison the starting point of operators and instead start at the level of a probability simplex. Then the question is to add interesting (or information theoretically motivated) constraints so that the extreme points of some convex set within the simplex are isomorphic to 1-dimensional projectors. Thus my focus on a function of the form W.

Of course, that question may have nothing to do with the (classical) Rényi order-3 entropy. But I had just wondered whether there's anything special about the order-3 one. Certainly the order-2 entropy has been studied to pieces. But why has no one ever moved up the chain to the next higher power? Why is interest lost at power-2?

Harremoësism 2: *I have attached a paper discussing the use of entropies of integer order in a quantum setting.*

Actually, you didn't attach it. Could I ask you to send it this time around?

Finally Flemming wrote:

Topsøe-ism 2: *As for my part, I will have it in mind, but guess it is the kind of thing you will have a greater chance of getting good comments to at precisely a workshop as the one we plan (VERY strong persons expressed an interest but I hesitate to say who as one never knows, except for the keynote speakers who already accepted to come, if they will really come. Sorry to disappoint you, hoping to see you for the workshop.*

Now that brings up a problem. It now looks like I won't actually be able to come ... so what you are saying is torture. I wish I could be there. The trouble is my colleague Appleby will have his Fall break October 20–28, and we had planned on his visiting PI during that time, for a rather intense exchange on SIC-states existence. Thus, I hope I can ask you and Peter, as friends, to pose the attached question to some of your knowledgeable colleagues and send me any feedback you get!

> We will consider probability vectors $\vec{p} = [p(1), p(2), ..., p(d^2)]$ on an event space of d^2 events, $d \geq 2$ a positive integer. Let c_{ijk} represent a given 3-index "table" of d^6 real numbers—the numbers need not all be positive, some may be negative in fact. We will assume that c_{ijk} is symmetric under interchanges of pairs of indices, and also invariant under cyclic permutations. (Eventually, as needed to make the question more interesting or well-defined, one may assume some other requirements on the c_{ijk}, but at the moment I'll leave that open.)
>
> Now let us define the following function on our probability simplex:
>
> $$W(\vec{p}) = \sum_{i,j,k} c_{ijk}\, p(i)p(j)p(k) \; .$$
>
> I chose the letter W for the function, in order to emphasize that it is a "weird object." The question is, does an object of this variety—or perhaps under specific, interesting choices for the c_{ijk}—obtain any kind of information theoretic meaning? It is a purely classical question as far as I can tell; nothing to do with quantum mechanics. That is my question. Can one find any information theoretic motivation or use for a function of the form $W(\vec{p})$? Has it ever been seen before?

21-08-07 *Your Bayesian vs. Frequentist Talks* (to R. Schack)

I just wrote the abstract below for the quantum foundations summer school here, coming up next week. I wonder if you can give me some inspiration on how to present the flaws in frequentism? Could I ask you to send me some sample presentations (or send me a pointer where I can download them) from your repertoire where you particularly bash the frequentists?

> Quantum States as Uncertainty, pure and simple. But, Uncertainty about What?
>
> David Deutsch implores us to "take quantum mechanics seriously." In these lectures we will take quantum mechanics *deadly* seriously, but not in a way that would please Prof. Deutsch. Here we lay the groundwork for viewing quantum mechanics as a branch of decision theory, specialized to decision-making agents *immersed* in an objective world of some particular characteristic—for need of a name, the quantum world. That is to say, the view presented here is that quantum mechanics is less a direct picture of the

world, and more a method of survival in it. Its statements about the world are therefore oblique, but nonetheless firm and, from some points of view, more exciting because of the creative ontology they seem to hint at.

Topics of the lecture will include: Einstein's pre-EPR argument for the incompleteness of quantum states, contrast between frequency and Bayesian interpretations of probability, Dutch book arguments for the structure of probability theory, the no-cloning and no-broadcasting theorems of quantum mechanics, classical no-cloning and yes-broadcasting examples, the quantum de Finetti representation theorem, the Kochen-Specker theorem, words on a Wolfgang Pauli'an style ontology in the light of Kochen-Specker, fiducial measurements for defining quantum states (for instance, SICs and MUBs), and the shape of quantum state space.

22-08-07 *Facts, Values and Quanta* (to D. M. Appleby)

I'm re-reading your "Facts, Values and Quanta" in preparation for my summer school lectures advertised below (which I have to give Tuesday and Wednesday). It's been a while since I've thought about these things! I'm learning a lot from you. I'm going to make the paper or the other version required reading for my tutorial group.

23-08-07 *Robin* (to M. Sasaki)

I understand that the time for your meeting in Boston is drawing near. I must apologize for doing this to you, but my early days here at PI are turning out to be so hectic that I am realizing I am going to have to cut some things from my travel schedule. I hope you will accept my apology.

On the other hand, I did not want to leave your conference high and dry, without any representation of "maximally quantum" sets of states! So I have arranged for an excellent speaker to be in my place, if you will accept that. He is Dr. Robin Blume-Kohout, one of the very founders of the SIC states, and he would talk on that subject, reporting some results from my recent paper with Appleby and Dang, and some results from his own papers on SICs; also he is hoping to have new numerical results to report before then. Robin has an impressive resume: He did his undergraduate honors thesis with Ben Schumacher at Kenyon College, he did his Ph.D. with Wojciech Zurek at Los Alamos, then he was a postdoc at Caltech with John Preskill. Now he is a postdoc with us at Perimeter Institute.

Please let me know if this is acceptable to you. He has his own travel funding from PI and will be able to pay his own expenses: He is just looking forward to the opportunity to meet some of your other speakers and introduce the subject of SIC-sets.

24-08-07 *Are You Nuts?* (to G. L. Comer)

Comerism 16:

> By the way, you need to read more about Alchemy. One of my big projects for the upcoming year is to familiarize myself much more with alchemical thought.

Are you nuts?!?

Changing lead to gold per se is not the interesting part of old alchemy. It's this: "The release of the substance by the man who transmutes it … is seen by the alchemist … as identical with the saving transformation of the man by the work." Transforming the metal transforms the soul—that was the real goal. In place of soul, put quantum state, and you can start to see where I'm coming from.

Yes, I am nuts. That's why the only place that would have me is PI (and even they aren't so sure).

Actually see the note below, that I wrote you two years ago! I've been nuts for a long time. [See note titled "Philosopher's Stone," dated 19 June 2005.]

24-08-07 *Leifer* (to G. Brassard)

I hope you're doing well. I'm finally in Canada and loving it! It's my fourth day in the office at PI, and it's just fantastic here. Let's hope I put these years to good use.

But the reason I'm writing right now is that I was just talking to Matt Leifer and he got me sad. He was telling me about his take on the hazards of doing foundational work before obtaining a faculty position—it's true, and it always brings sadness. Particularly because, in my eyes, the stuff we've all been involved with is just doing good physics. Doing good physics, period. And why should a physicist be ashamed of doing that?

24-08-07 *My Email* (to R. Schack)

No, I didn't get it. So, thanks for sending something, but try again! Frankly, I'm a little scared about presenting in any detailed way why one should drop frequentism—it's so ingrained in everybody, it strikes one as an almost lost battle to try to sway the masses. I gain new respect for your efforts over the years, as I try to figure out how on earth I'm going to do this.

25-08-07 *How Pleased!* (to R. Blume-Kohout)

How pleased I was to stumble across the poster of the students you had tutored and overhear a student debating the merits of a Bayesian approach to probability over a frequentist approach! It was the greatest feeling. Even some of the phrases in the poster were just gems. "Measurement: An action that tells you something about something." I'm probably reading more into it than I should, but I was particularly pleased by the choice of the word *action* in the definition. For the idea of measurement as action has taken on a much bigger role in my own thinking lately. In case you're interested, have a look at the letters "'Action' instead of 'Measurement'" and "Questions, Actions, Answers, and Consequences" and "Canned Answers" (all to Bas van Fraassen) in this file: `http://perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`.

Action's where it's at!

26-08-07 *Your Note to Ingemar* (to D. M. Appleby)

Yes, I had a hard time following the point you really wanted to make to Ingemar. Also, I wasn't sure how I fit into the context of the discussion.

My reply to this particular part of Ingemar's note

> I expect that if I did settle the MUB question in $N = 6$ (say), then my feeling would be that I had found the correct way to look at an existing structure. *And* at a structure that existed also before Hilbert was born.

might simply have been this: That may be your feeling; it has a very strong tendency in everyone (including me). But it is only a feeling.

There is some (small, but I think instructive) analogy with issues surrounding the EPR criterion of reality:

> If, without in any way disturbing a system one can [gather the information required to] predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity.

I already modified the language of EPR a little bit (adding the stuff in square brackets) in order to make it make any sense at all, but let me go a little further and change the language so that the discussion is more in line with the CFS-Bayesian take on certainty:

> If, without in any way disturbing a system, one can become *certain* of the value of a physical quantity (i.e., assigning probability equal to unity for the value), then there exists an element of physical reality corresponding to this physical quantity.

Well, CFS reject that. It is a category error—or more accurately an illegitimate move to marry distinct categories (epistemic and ontic) at a kissing point. Having the feeling of certainty does not make an element of reality so. Having certainty for the outcome of a quantum mechanical measurement does not make the outcome pre-exist the process of measurement, CFS say. That is the little point of quantum mechanics, I think. It is a species of the big point of pragmatism. And it is that which more directly impinges on Ingemar's feeling.

Now, I feel more secure in quantum mechanics than pragmatism in general, and to that extent am less secure in my assertion about $N = 6$ above. But some flavor of this discussion, or perhaps some careful tempering of it, will, I think, continue to stand with me ultimately. I thought briefly about cc'ing this note to Ingemar, but I think I'll just keep it between us for now (for no particular reason that I can identify other than that, for once, I'd like to think a little more before speaking more).

Back to work on my lectures.

05-09-07 *QF Seminars* (to L. Hardy)

My joke at the beginning of the note was going to be this:

> "Of course, I'd be very happy to organize the seminar. You know I always enjoy an opportunity to bias the random walk when I can."

... Or something like that. It takes a long time to hone a joke.

07-09-07 *Diploma Work on Gleason's Theorem* (to H. Granström)

Ingemar Bengtsson told me that you returned from the PI summer school "full of enthusiasm". If true, I'm glad to hear that we served a good purpose.

One thing that I do regret is that I didn't more diligently pursue an understanding of your ideas on how quantum measurements should be viewed in relational terms. My head was just too full of all the noise around me to think on deeper subjects. But, in fact, one of my first-year goals here at PI is to get a better handle on what I think about such accounts of measurement and how they compare and contrast with the "quantum Bayesian" account that colleagues and I have been trying to construct. So, I'd like to hear more about what you're thinking if you've got the time during my upcoming visit to Stockholm for Åsa's defense. I hope you do. I arrive in Stockholm (the airport at least) 9:30 AM Friday Sept 14 and depart the morning of Sept 19. I don't have a clue where I'll be during that range of time, but Ingemar should know how to put you in contact with me. As far as I know, my weekend is free.

At the moment, I'm not inclined to believe that a relational account can capture the real essence of quantum measurement—it seems to me too static and doesn't seem to capture the "birthy-ness" of quantum measurement outcomes, which is the deep point I think the KS theorem signifies (i.e., that they are little acts of creation in their own right)—but it's hard for me to really judge until I know more. Attached is one of my crazier pieces where I try a little to build a picture of what I mean by all this. [See "Delirium Quantum" `arXiv:0906.1968v1`.] At this stage, of course, everything is vague and just defines a direction of research, but maybe the document will give you some sense of what I'm thinking: That the universe is still under construction and the structure of quantum measurement is our biggest clue for that.

07-09-07 *Consciousness Essay* (to A. Kent)

Here's a transcript of the James essay I was telling you about, "Does 'Consciousness' Exist?". And here's the quote from it that I was trying to repeat:

> To deny plumply that consciousness exists seems so absurd on the face of it – for undeniably thoughts do exist – that I fear some readers will follow me no farther. Let me then immediately explain that I mean only to deny that the word stands for an entity, but to insist most emphatically that it does stand for a function. There is, I mean, no aboriginal stuff or quality of being, contrasted with that of which material objects are made, out of which our thoughts of them are made; but there is a function in experience which thoughts perform, and for the performance of which this quality of being is invoked. That function is knowing. Consciousness is supposed necessary to explain the fact that things not only are, but get reported, are known. Whoever blots out the notion of consciousness from his list of first principles must still provide in some way for that function's being carried on.

Here's a link to the "Are We Automata?" essay:

`http://en.wikisource.org/wiki/Are_We_Automata%3F`

07-09-07 *That Wheeler Quote* (to M. A. Nielsen)

My memory blurred yesterday. I was thinking of some quotes I had used in a later letter. Anyway, the quote was actually this.

From J. A. Wheeler (with K. W. Ford), *Geons, Black Holes, and Quantum Foam: A Life in Physics*, (W. W. Norton, New York, 1998):

> Many students of chemistry and physics, entering upon their study of quantum mechanics, are told that quantum mechanics shows its essence in waves, or clouds, of probability. A system such as an atom is described by a wave function. This function satisfies the equation that Erwin Schrödinger published in 1926. The electron, in this

description, is no longer a nugget of matter located at a point. It is pictured as a wave spread throughout the volume of the atom (or other region of space).

> This picture is all right as far as it goes. It properly emphasizes the central role of probability in quantum mechanics. The wave function tells where the electron might be, not where it is. But, to my mind, the Schrödinger wave fails to capture the true essence of quantum mechanics. That essence, as the delayed-choice experiment shows, is *measurement*. A suitable experiment can, in fact, locate an electron at a particular place within the atom. A different experiment can tell how fast the electron is moving. The wave function is not central to what we actually know about an electron or an atom. It only tells us the likelihood that a particular experiment will yield a particular result. It is the experiment that provides the actual information.

Or the single nugget of John's thinking that I wanted to extract: Unitarity "fails to capture the true essence of quantum mechanics."

08-09-07 *The More Complete Quote* (to A. Kent)

I just read James's "Are We Automata?" and am now moving on to "Does 'Consciousness' Exist?" I noted that the first of the two was written in 1879, while the second was written in 1904. Twenty-five years between the two. That, I note, is a little important because of these lines I just read, preceding the quote starting "To deny plumply ..." which I sent you yesterday:

> For twenty years past I have mistrusted 'consciousness' as an entity; for seven or eight years past I have suggested its non-existence to my students, and tried to give them its pragmatic equivalent in realities of experience. It seems to me that the hour is ripe for it to be openly and universally discarded.

Thus presumably in the first article, he was still treating "consciousness as an entity"—whatever that means. I suppose this was, at least in part, what you were referring to yesterday.

08-09-07 *Finished It* (to A. Kent)

I just wanted to record one thought, having finished the second of the two James essays. It is true that he argues in it that consciousness is a function, rather than an entity, but I suspect the resemblance between his doctrine and the functionalism Wallace speaks of ends at the selection of the word 'function' itself—i.e., there being no resemblance beyond that.

For instance, Wikipedia says this:

> An important part of some accounts of functionalism is the idea of multiple realizability. Since, according to standard functionalist theories, mental states are the corresponding functional role, mental states can be sufficiently explained without taking into account the underlying physical medium (e.g. the brain, neurons, etc.) that realizes such states; one need only take into account the higher-level functions in the cognitive system. Since mental states are not limited to a particular medium, they can be realized in multiple ways, including, theoretically, within non-biological systems, such as computers. In other words, a silicon-based machine could, in principle, have the same sort of mental life that a human being has, provided that its cognitive system realized the proper functional roles. Thus, mental states are individuated much like a

valve; a valve can be made of plastic or metal or whatever material, so long as it performs the proper function (say, controlling the flow of liquid through a tube by blocking and unblocking its pathway).

For James, as I understand him, the whole point of the article was to try to argue that consciousness does not reside on a material substrate. For material and mental, for him, are two distinct aspects of something that is neither. His philosophical playground wasn't the kind of physicalism that Wallace, for instance, seems to presuppose in his thought.

So I apologize for my quick judgment the other day, when I said, "He's probably talking about the same thing that Adrian called functionalism the other day."

10-09-07 *Several Agents* (to S. Hartmann)

Now I've read your note! Thanks for updating me on all. I'm glad you like your new home; I had expected you would. And your position sounds ideal. Use it as a chance to change the world!

Hartmannism 3: *Bayesianism always assumes that there is just one agent who has beliefs which are then updated. But what if there are several agents who have beliefs and one wants to combine these believes to a collective belief? Maybe it would be interesting to see how this works in a quantum setting.*

I agree that would be a very, very interesting topic. We have done a very little bit (infinitesimal, but nonzero) in that direction with this paper: `http://xxx.lanl.gov/abs/quant-ph/0206110`.

11-09-07 *Title and Abstract* (to A. Kent)

Will this fit the bill?

Title: 13 Quotes from Everettian Papers and Why They Unsettle Me

Abstract: 101 years ago William James wrote this about the Hegelian movement in philosophy: "The absolute mind which they offer us, the mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below." With some minor changes of phrase—for instance "mathematical structure" in place of "absolute mind"—one might well imagine morphing this into a remark about Everettian quantum mechanics. This point, coupled with the observation that the Everett interpretation has been declared complete and consistent for the selfsame number of years that its supporters have been trying to complete it, indicate to me that perhaps the Everett approach is more a quantum-independent mindset than a scientific necessity. So be it, but then it should be recognized as such. In this talk, I will try to expand on these suspicions.

11-09-07 *Arrival, Plans* (to I. Bengtsson)

This is just to let you know that I should be arriving at the Stockholm airport ARN at 9:30 AM, Friday September 14. . . .

First question: What should I do after arriving at the airport? Where should I proceed to? And by what means?

Second question: Have you drawn me up any kind of schedule? If so, if possible, I'd like to insure a little time to talk to Prof. Lindblad. Not long necessarily, I just wanted to meet him, as I have long respected him. Also, I'd like to build in a little time to talk to Helena Granström. She said some things at the summer school about "relational" ideas of quantum measurement that intrigued me, and I would like to have a chance to get her to expand on that. Beyond that, you can do with me as you will. Though I would like some time during the weekend to work and think on the talk I must give at the *50 Years of Everett* conference. That is going to be a very tough talk for me, as I am sure to be attacked most roundly while attempting to give it. (Abstract below.) [See 11-09-07 note "Title and Abstract" to A. Kent.] Thus, perhaps not too heavy of a schedule over the weekend, but also I'm not averse to interactions during it either.

11-09-07 *The World Is Made of Catalysts* (to L. Hardy)

Here's that Feynman quote I was telling you about:

> If, in some cataclysm, all of scientific knowledge were to be destroyed, and only one sentence passed on to the next generation of creatures, what statement would contain the most information in the fewest words? I believe it is the atomic hypothesis ... that all things are made of atoms ...

I came across it a few minutes ago as I was organizing my lecture for Tuesday.

18-09-07 *Shakespeare in Sweden* (to P. G. L. Mana)

Here's that Shakespeare quote I was trying to remember last night:

> These our actors,
> As I foretold you, were all spirits, and
> Are melted into air, into thin air:
> And like the baseless fabric of this vision,
> The cloud-capp'd tow'rs, the gorgeous palaces,
> The solemn temples, the great globe itself,
> Yea, all which it inherit, shall dissolve,
> And, like this insubstantial pageant faded,
> Leave not a rack behind. We are such stuff
> As dreams are made on ...

At least that's the part of it that John Wheeler always used when talking of quantum mechanics (though the stanza is a little bit longer).

20-09-07 *Further Reading* (to H. Granström)

Here's the further reading I was going to suggest to you. Go to pages 411 to 416 of [my samizdat] and look at the notes I wrote to Bas van Fraassen.

I would appreciate it if you would ultimately write me back with some commentary both on 1) the previous "delirium quantum" piece I gave you, and 2) these notes to van Fraassen. Tell me where I need to clarify. Tell me where you disagree. Pinpoint those places where my conception of measurement seems to disagree with yours, and also where they seem to agree instead. It would be valuable for me to get this information.

The two notes below, incomplete in comparison to our discussion the other day though they are, are the ones I had written to Steven van Enk on the Belly Theory. [See 10-02-07 note "Inside and Outside" and 02-08-07 note "The One-Belly Theory of the Universe" to S. J. van Enk.] Let me know if you get any good ideas. In a couple weeks I should have the time to collaborate on this if you wish (unless you find a quick counterexample!!).

Helena's Reply, "Some Random Comments on Delirium Quantum"

Enclosed is a document with some of the reflections I've come up with so far. It does not contain much of any expansions on my view (to the extent that I have such a thing as a 'view') of the interpretation of QM, but it does however contain a few loose thoughts provoked by the reading.

As I understand it, the Darwinist idea would apply to all worldly phenomena including immaterial ones as the laws of physics. This would mean that the Bayesian rule that quantum mechanics constitutes, is functional in the current context, but that may very well change.

I must admit I am not quite sure about the implications of this idea. Take gravitation as an example. If it one day turns out that the concept of gravitation is no longer functional as a guide about how to be in and interact with reality, would that necessarily be due to a change that had taken place in the minds of human beings, or could it be because of a gradual, evolutionary-type change in the factual interaction between massive bodies? (I regret using the word "factual" here, but I'm hoping that you get the distinction that I'm after, at least well enough to decide if you think that it's a meaningful one.)

Also, it would be interesting to know how the view of theories as "extensions of our biological brains" put forward on page 13 related to the statement quoted on page 12, that the laws of physics should be applicable to creatures "sharing none of our sensory modalities". If one were to take the idea that all that theories formulated by human beings do (an can possibly do) is to describe that way that a human being will experience and interact with her surroundings, the consequence would be that our theories are applicable only in relation to a very specific class of agents. What qualifies an agent to be a member of this class is not clear, though: just referring to some idealized humanity would probably be tending towards religion.

One aspect about the Darwinian idea that I find worth considering (given that I've understood correctly the sense in which you're using the term), is its inherent contextuality. That is, our eyes cannot be said to be a mechanism for seeing independently of context. Indeed, they have gotten their form in dialogue with caribou, mayfly and nuthatch; our tongues are a product of clear spring water and ripe plums—same thing with all our ways of perceiving and thinking about the world. All that we experience as meaningful, even the most abstract construction, we experience using thought that took form during millions of years on vast savannas, and under wet rocks.

As you also point out, this view provides great reason to question the idea of observer non-detachedness, and the idea that striving to find things out about the world, rather than about the nature of our interplay with the world, holds meaning.

Concerning the issue of abandoning the idea of truth as a symbolic correspondence, I fully agree that this is a reasonable direction to take. I would, however, like to see the casual interpretation of truth more expanded upon. I guess that this relates to my earlier question: exactly in what way do you consider the concept of justification to be, as you put it, temporal? From what I understand, though, the basic idea is one very much in accord with the Bayesian view, i.e. that "laws of physics" are rather guidelines for building a functional relationship with the world around us.

As for your interpretation of the index of POVM elements, I just printed out your further references. As was probably more than clear during our conversation, something about your statement about the indexes puzzles me, but I'll have to get back to you on that.

One of the ideas in the paper that I find particularly intriguing (and that I have thought about some myself, although not really getting anywhere with it as far as the theory of science is concerned) is what you refer to as radical pluralism. One of the most important qualities of the scientific method is probably its systematic reduction, its radical neglection of all aspects of reality that are not immediately relevant. Relevancy, it should also be said, is largely measured according to some criterion determined by the current world view and thereby to a large extent value laden.

I would say, however, that this categorizing is central just not to science but to western civilized thought in general. And many people would be prone to say that it has been quite successful. I guess that the problem is just one of ambition: Just as QM need not necessarily be discarded as a theory, but most likely has to be abandoned or at least severely revised as a theory about the way the world is, so does categorizing and neglecting not necessarily have to be abandoned as a scientific strategy, but we have to be aware what are the flaws and consequences of this approach and the limitations it puts on the scientific quest.

Another, more general, remark on the Bayesian view: I agree that several quantum "paradoxes" are lifted when the quantum state is regarded as a state of belief rather than an objective state of reality. I cannot say, however, that I've fully grasped it, more than superficially. That is, I think I understand most of what I've read on the subject (most of it written by you), but some things still trouble me, even though I cannot at the moment quite pin them down. I think it has something to do with the "updating of information" taking place during a measurement procedure. From my point of view, that is not a satisfactory description.

As for the asymmetry that I was complaining about during our talk. I think, again, that it comes down to a questions of aspiration, and maybe I am confusing the issues. There are, as I see it, (at least) two, namely:

1) How does one formulate a theory that captures the reciprocity and symmetry of interaction? Is this even possible within the current framework of what is considered a physical theory? (I ask this question for example in relation to the above comment about radical pluralism.)

2) How does one interpret quantum mechanics?

It is in no way obvious that the two are intimately related. That is, even though I would prefer to see a theory such as described in 1) formulated, it might be the case that QM does have a fundamental asymmetry built into it. This would mean that QM satisfactorily captures enough of some aspects of our (MY!) interaction with reality to be able to give accurate predictions, but is also in a fundamental way flawed, which may be part of what is causing all the confusion leading to emails such as this one.

I guess these notes sum up some, if not all, of my thoughts on your paper. Hopefully you will be able to get something out of them. Reading your text gave me several associations and ideas (not all of them relevant to expand on here) that I will try to take further, and I very much appreciate getting the opportunity to read it. On my way home from work I will pick up the two books by James that you referenced, for a start.

20-09-07 *Easy Questions* (to P. W. Shor)

I have a few small questions for you, and I hope you have the time to write me back fairly quickly. I'm preparing a talk for the "50 Years of Everett" meeting at PI. And I wanted to say a few words on the influence and/or lack of influence of the Everett view on various aspects of quantum information. I intend to be fair and balanced, and faithfully represent what I am told. With that in mind, I wonder if you would mind answering the following:

1) Did you know of the Everett interpretation before starting work on your factoring algorithm?

2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

I hope you have the time to answer me. I've got to give the talk Sunday!

Peter's Reply

Question 1) Did you know of the Everett interpretation before starting work on your factoring algorithm?

Yes.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

No, the idea was really more to use periodicity, and inspired by Simon's algorithm.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

You should have heard my after-dinner talk in Japan. Both the Everett view and the Copenhagen view are misleading in thinking about quantum computation (although misleading in quite different ways).

20-09-07 *Easy Questions* (to D. R. Simon)

I just sent the note below to Peter Shor. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I know I don't know you as well as Peter, but I wonder if I might ask you the same questions. Just in every place where I ask about Peter's algorithm, let me instead ask you about yours. Thanks so much if you can answer these.

By the way, do you have any interest in visiting us at the Perimeter Institute in the coming year? If so, let me know.

Dan's Reply

No problem–I'll do my best to answer them. Thanks very much for the invitation, but my interests have drifted about as far away from quantum physics as they can get by now, and having a toddler in the family makes travel less attractive than it used to be, so a visit to your neck of the woods would be pretty hard for me to justify. Hope you're having fun, though . . .

Question 1) Did you know of the Everett interpretation before starting work on your quantum algorithm?

Who's Everett, and what's his interpretation?

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

I was approaching the problem purely from a computer scientist's perspective. I learned the absolute bare minimum of physics I needed to be able to understand the computer science question, which (as I saw it) was, "these crazy people are claiming that if you add these very-weird-yet-theoretically-physically-implementable functions to a computer, then you should be able to do amazing things with them. Prove them right or wrong." I actually started out trying to prove that quantum computing was useless, and eventually narrowed down the difficult, unsimulateable part to, "rotate, compute, rotate". That helped guide my search for a computationally interesting quantum algorithm.

From my perspective, though, the "interpretation" of the quantum mechanical operations I was given was irrelevant—I was told that a quantum computer could do these things, and it was my job to figure out how computationally useful "these things" were.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

As a non-physicist, I'm perfectly comfortable living with only the haziest mental model of quantum reality. Distinctions between different mental models that are experimentally indistinguishable are altogether lost on me. For that matter, I wonder why physicists, whose mental models have (or at least should have) all the precision of the mathematical formulae underlying them, would care about fuzzily unquantifiable comparisons among fuzzily unquantifiable mental models.

(I'm assuming here that the "interpretations" in question here aren't experimentally distinguishable–else they'd be "theories", right?)

20-09-07 *Easy Questions* (to L. K. Grover)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar ones—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your search algorithm and any of the other algorithms you developed? If you can do this, it'd be much appreciated!

Lov's Reply

Sorry, I had missed your message — only just got it — hope your talk went well.

20-09-07 *Easy Questions* (to D. Gottesman)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your work on the stabilizer formalism (or anything else you want to mention)? If you can do this, it'd be much appreciated!

Daniel's Reply

Question 1) Did you know of the Everett interpretation before starting work on stabilizer codes?

Yes, certainly.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

There wasn't imagery, per se, although there was intuition. Mostly it came about by accident. I was trying to study degenerate codes, and it seemed to make sense to think about codes whose degeneracies were characterized by Pauli identities – e.g., Z_1 and Z_2 act the same, so $Z_1 Z_2$ acts as the identity. I tried writing down a code with lots of degeneracies, and was surprised to see that it corrected a variety of different errors without any extra effort on my part. I had some intuition that anticommutation was the right thing to look at – I'm not completely sure why. It's possible the intuition had some genesis with "incompatible observables" à la Copenhagen, but certainly I was not thinking about that explicitly.

I can't say that any Everettian idea was at all relevant.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Ev-

erett view? Or do you think the developments in QI are relatively neutral toward it?

Really wrong isn't an option?

In any case, as I believe I have told you before, I subscribe to the pantheistic interpretation, that all interpretations are valid (excluding those which have experimental differences from standard QM). I think work in quantum computation supports that – sometimes it is helpful to think about one interpretation to understand a QI development, at other times a different interpretation is most helpful. In that sense, I would say QI has been mostly neutral towards Everett.

There's one Everettian idea, however, that I think has been very strongly supported by quantum information, which is that basically everything is unitary. I tried to think of supporting arguments for this view, though, and had trouble. The "Church of the larger Hilbert space" supports it in the sense that says we can always think of evolution as unitary, but also opposes it in the sense that we can do this whether the system has a real-world purification or not. Quantum error correction kind of supports it in the sense that it says we should be able to build arbitrarily large subspaces which, with appropriate control, behave arbitrarily close to unitary, but again that's not really the same as saying the underlying dynamics without control are unitary. Perhaps the strongest argument is the study of decoherence mechanisms in real systems, where interaction with the environment always seems adequate to account for the decoherence.

On the other hand, in a quantum computer, while we frequently think of a standard computational basis for convenience, there really is no preferred basis most of the time, and this creates a large regime that seems resistant to a "many-worlds" interpretation.

I think one big impact of QI work for foundations is that it changes the list of what are considered signature quantum behaviors. People outside of QI frequently seem to think that Planck's constant and the Schrödinger equation are the be-all and end-all of quantumness, and that view seems totally incompatible with QI work, since we get lots of quantum behavior without ever mentioning either. This is very harsh on some interpretations (like Bohmian, which depends on a wave equation), but Everett doesn't seem to depend on those much.

20-09-07 *Easy Questions* (to J. Preskill)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest at PI this weekend. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your work on security proofs in quantum crypto (or anything else you think worth mentioning)? If you can do this, it'd be much appreciated!

John's Reply

In my zeal to reply, I seem to have inadvertently deleted your message, but not before reading the questions.

1) Did you know about many worlds when . . . ?

I've known about it since 1973. I was a junior at Princeton and read a book I saw in the university bookstore, edited by DeWitt and Graham, called *The many-worlds interpretation of quantum mechanics*. I think the book was quite new at the time. It included a reprint of Everett's paper. (I was working on a "junior paper" about Bell inequalities, which seemed timely because the Freedman-Clauser experiment had been published just months earlier.)

2) Do you think the many-worlds interpretation influenced your work on . . . ?

No.

3) Does recent work on quantum information strengthen the case for the many-worlds interpretation? Weaken? Is it neutral?

Neutral.

[Yes, I don't usually answer you so promptly, but I'm waiting to board an airplane and I'm kind of bored.]

20-09-07 *Easy Questions* (to A. Kent)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest at PI this weekend. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your work on security proofs in quantum crypto (or anything else you think worth mentioning)? If you can do this, it'd be much appreciated!

Adrian's Reply

1) Did you know of the Everett interpretation before starting work on quantum information theory?

Sure did.

2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it wasn't part of your main imagery, can you say what was?

Nope. As for mental imagery, I'm not sure there's any one big thing — lots of confused images of vectors and spheres and squashings and twirlings and things.

3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

Entirely neutral.

20-09-07 *Easy Questions* (to A. K. Ekert)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your entanglement-based quantum key distribution protocol? If you can do this, it'd be much appreciated!

Artur's Reply

Question 1) Did you know of the Everett interpretation before starting to work on your key distribution protocol?

Yes and I was intrigued by it.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

Sometime in the mid 1989 I read the EPR paper and my attention was drawn to the sentence "... If, without in any way disturbing a system, we can predict with certainty ... the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity." This was a definition of perfect eavesdropping. I guess I was lucky to read it in this particular way. This was my starting point. Thinking about locality, reality and security was in a way going in the opposite direction than Everett but it was very productive nonetheless.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

Yes, QI enhances my predilection for the Everett view.

20-09-07 *Easy Questions* (to A. C. Yao)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest at the Perimeter Institute this weekend. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to any of your early work on quantum Turing machines and communication complexity? If you can do this, it'd be much appreciated!

Andrew's Reply, "Easy Questions on Early Quantum Computing"

Quick answers to your questions:

1. Yes, I was aware of the Everett interpretation since college days.
2. No, the Everett interpretation was not on my mind when I did my work on quantum Turing machines and quantum communication complexity.
3. I am inclined to think that QI developments are relatively independent of the interpretations of quantum mechanics.

Hope this found its way to you in time!

20-09-07 *Easy Questions* (to H. J. Briegel)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your work on cluster-state computation? If you can do this, it'd be much appreciated!

I will send Robert a separate note, as I would like your answers to be independent of each other.

Hans's Reply

> I guess it is far too late to answer this e-mail, which I have just stumbled over, searching in my mailbox for something else ...
>
> I was aware of the many world interpretation long before I started working on quantum information and in particular on measurement-based quantum computation.
>
> I never liked the Everett interpretation and it certainly did not play any role in our studies on cluster states and what they could be used for.
>
> Later however, once the one way model had been established, I thought indeed that the one-way quantum computer might be a good playground to challenge different ontological preferences, i.e. interpretations of q.m., and in particular the many-world interpretation. I think we even discussed this at some point.
>
> As you know very well, Andrew Steane then wrote a note entitled "A quantum computer needs only one universe" which pretty much expressed my views on the subject, too. So I did not find any strong need to elaborate on this.
>
> Did anything new come out of the 50th anniversary party?
>
> I hope you had an interesting meeting, but I also hope that the number of many-world believers has not increased by too many.

20-09-07 *Easy Questions* (to P. A. Benioff)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest at the Perimeter Institute this weekend. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your own work on unitary quantum computation? If you can do this, it'd be much appreciated!

Paul's Reply

Here are my answers to your questions:

1. I knew of Everett's work before my work on quantum computation.
2. His work was not integral to mine and did not play a role.
3. I do not think developments in QI show anything about the validity or nonvalidity of Everett's work.

Hope your talk goes well. Also I hope to see you sometime soon.

20-09-07 *Easy Questions* (to D. P. DiVincenzo)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest at PI this weekend. I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to any of your early work in quantum information (say, the gates papers or unextendable product bases)? If you can do this, it'd be much appreciated!

David's Reply

Question 1) Did you know of the Everett interpretation before starting to work on your algorithms?

Yes.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

No.

I think that I am relatively "interpretationless", the rules of quantum mechanics (viz., Charlie's stone tablets) speak for themselves, to me. Or perhaps I prefer the Schrödinger 1935-6 interpretation, summed up by, "poor pussy".

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

In my view, neutral.

20-09-07 *Easy Questions* (to R. Cleve)

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest at PI this weekend. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, would you answer similar questions pertaining to your work on communication complexity (or anything else you think worth mentioning)? If you can do this, it'd be much appreciated!

Richard's Reply

This might be too late to be useful to you. My answers are below:

Question 1) Did you know of the Everett interpretation before starting to work on your algorithms? [communication complexity]

I had a vague idea of "many worlds" interpretations as corresponding to many universes in superposition.

My viewpoint of the quantum information framework is more along the lines of "probability theory with minus signs". (Or "probability theory on steriods" ☺.)

Probability theory already gives a notion like "superposition", but without interference occurring.

I know that PT is different in that one can choose to think of a probabilistic state as actually being in a definite state, about which we are ignorant. Quantum states cannot be viewed that way.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

It wasn't part of my imagery. My imagery was based on Bell inequality violation stuff.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

Neutral. Whatever works intuitively to help inspire is fine with me.

20-09-07 *Easy Questions* (to D. Deutsch)

I know you've written about this adequately, but Adrian Kent suggested I ask you too nevertheless. So, I think I will.

I just sent the note below to Peter Shor, trying to compile something interesting to say at the Everettfest. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar questions—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, I wonder if you would answer similar questions pertaining to your own participation (and any other algorithms you were involved with)? If you can do this, it'd be much appreciated!

David's Reply

My replies to the questions, mutatis mutandis, would be:

Question 1) Did you know of the Everett interpretation before starting to work on your algorithms?

Yes.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

Yes.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

In a sense. I think the case for Everett was already watertight before quantum computation. Quantum computation provided new, more dramatic, forms of the same arguments, and also provided some better tools for understanding the multiverse.

20-09-07 *Easy Questions* (to R. Jozsa)

I just sent the note below to Peter, trying to compile something interesting to say at the Everettfest. [See 20-09-07 note "Easy Questions" to P. W. Shor.] I wonder if I might ask you some similar ones—I hope you've got a little time to answer these, as whatever your answers may be, I think they'll bring an aspect to the meeting that no one else will be representing.

Simply, in place of where I ask Peter about the factoring algorithm, I wonder if you would answer similar questions pertaining to your own participation (and any other algorithms you were involved with)? If you can do this, it'd be much appreciated!

Richard's Reply

Nice to hear from you after such a long time! And congrats on your move back into rarefied academia! — I'm sure it much better suits the nature of your main research inclinations. I'm not sure my answers to your questions will be of much interest but here they are:

Question 1) Did you know of the Everett interpretation before starting to work on your algorithms?

I've known of the Everett interpretation since the mid 1970's and never really adopted/liked it, even from outset. It always was (and still is) a very vague and incomplete framework to me.

Question 2) If so, was the many-worlds view or the idea of "parallel computations via parallel worlds" something that was integral to your thinking for finding the algorithm? If it was part of the main imagery that steered your mathematics, can you say in what way? If it wasn't part of your main imagery, can you say what was?

I'm not aware that the Everett ideas have ever played any significant role in my thinking on quantum things. I don't have a clear impression of any particular imagery that I could name, underlying or guiding my quantum thoughts.

Question 3) Would you say that the developments in quantum information and computation are evidence that something is really right about the Everett view? Or do you think the developments in QI are relatively neutral toward it?

I do not see that any quantum comp/info developments particularly support the Everett view in any way compared to any other prospective interpretations.

Hope you have a good talk on Sunday!

20-09-07 *Free Will* (to Å. Ericsson)

For Renouvier's argument that I was telling you about, see . . . [15-08-08 note titled "Free Will and Renouvier" to R. Schack].

Also, here's another little passage I happen to have in my files. (Read it slowly and three times over; I think it's worth it.) LaTeX it up if you want slightly easier reading.

From W. James, "Some Metaphysical Problems Pragmatically Considered," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

Let me take up another well-worn controversy, *the free-will problem.* Most persons who believe in what is called their free-will do so after the rationalistic fashion. It is a principle, a positive faculty or virtue added to man, by which his dignity is enigmatically augmented. He ought to believe it for this reason. Determinists, who deny it, who say that individual men originate nothing, but merely transmit to the future the whole push of the past cosmos of which they are so small an expression, diminish man. He is less admirable, stripped of this creative principle. I imagine that more than half of you share our instinctive belief in free-will, and that admiration of it as a principle of dignity has much to do with your fidelity.

But free-will has also been discussed pragmatically, and, strangely enough, the same pragmatic interpretation has been put upon it by both disputants. You know how large a part questions of *accountability* have played in ethical controversy. To hear some persons, one would suppose that all that ethics aims at is a code of merits and demerits. Thus does the old legal and theological leaven, the interest in crime and sin and punishment abide with us. 'Who's to blame? whom can we punish? whom will God punish?'—these preoccupations hang like a bad dream over man's religious history.

So both free-will and determinism have been inveighed against and called absurd, because each, in the eyes of its enemies, has seemed to prevent the 'imputability' of good or bad deeds to their authors. Queer antinomy this! Free-will means novelty, the grafting on to the past of something not involved therein. If our acts were predetermined, if we merely transmitted the push of the whole past, the free-willists say, how could we be praised or blamed for anything? We should be 'agents' only, not 'principals,' and where then would be our precious imputability and responsibility?

But where would it be if we *had* free-will? rejoin the determinists. If a 'free' act be a sheer novelty, that comes not *from* me, the previous me, but *ex nihilo*, and simply tacks itself on to me, how can *I*, the previous I, be responsible? How can I have any permanent *character* that will stand still long enough for praise or blame to be awarded? The chaplet of my days tumbles into a cast of disconnected beads as soon as the thread of inner necessity is drawn out by the preposterous indeterminist doctrine. Messrs. Fullerton and McTaggart have recently laid about them doughtily with this argument.

It may be good *ad hominem*, but otherwise it is pitiful. For I ask you, quite apart from other reasons, whether any man, woman or child, with a sense for realities, ought not to be ashamed to plead such principles as either dignity or imputability. Instinct and utility between them can safely be trusted to carry on the social business of punishment and praise. If a man does good acts we shall praise him, if he does bad acts we shall punish him,—anyhow, and quite apart from theories as to whether the acts result from what was previous in him or are novelties in a strict sense. To make our human ethics revolve about the question of 'merit' is a piteous unreality—God alone can know our merits, if we have any. The real ground for supposing free-will is indeed pragmatic, but it has nothing to do with this contemptible right to punish which has made such a noise in past discussions of the subject.

Free-will pragmatically means *novelties in the world*, the right to expect that in its deepest elements as well as in its surface phenomena, the future may not identically repeat and imitate the past. That imitation *en masse* is there, who can deny? The general 'uniformity of nature' is presupposed by every lesser law. But nature may be only approximately uniform; and persons in whom knowledge of the world's past has bred pessimism (or doubts as to the world's good character, which become certainties if that character be supposed eternally fixed) may naturally welcome free-will as a *melioristic* doctrine. It holds up improvement as at least possible; whereas determinism assures us that our whole notion of possibility is born of human ignorance, and that necessity and impossibility between them rule the destinies of the world.

Free-will is thus a general cosmological theory of *promise*, just like the Absolute, God, Spirit or Design. Taken abstractly, no one of these terms has any inner content, none of them gives us any picture, and no one of them would retain the least pragmatic value in a world whose character was obviously perfect from the start. Elation at mere existence, pure cosmic emotion and delight, would, it seems to me, quench all interest in those speculations, if the world were nothing but a lubberland of happiness already. Our interest in religious metaphysics arises in the fact that our empirical future feels to us unsafe, and needs some higher guarantee. If the past and present were purely good, who could wish that the future might possibly not resemble them? Who could desire free-will? Who would not say, with Huxley, 'let me be wound up every day like a watch, to go right fatally, and I ask no better freedom.' 'Freedom' in a world already perfect could only mean freedom to *be worse*, and who could be so insane as to wish that? To be necessarily what it is, to be impossibly aught else, would put the last touch of perfection upon optimism's universe. Surely the only *possibility* that one can rationally claim is the possibility that things may be *better*. That possibility, I need hardly say, is one that, as the actual world goes, we have ample grounds for desiderating.

Free-will thus has no meaning unless it be a doctrine of *relief.* As such, it takes its place with other religious doctrines. Between them, they build up the old wastes and repair the former desolations. Our spirit, shut within this courtyard of sense-experience, is always saying to the intellect upon the tower: 'Watchman, tell us of the night, if it aught of promise bear,' and the intellect gives it then these terms of promise.

Other than this practical significance, the words God, free-will, design, etc., have none. Yet dark tho they be in themselves, or intellectualistically taken, when we bear them into life's thicket with us the darkness *there* grows light about us. If you stop, in dealing with such words, with their definition, thinking that to be an intellectual finality, where are you? Stupidly staring at a pretentious sham! "Deus est Ens, a se, extra et supra omne genus, necessarium, unum, infinite perfectum, simplex, immutabile, immensum, aeternum, intelligens," etc.,—wherein is such a definition really instructive? It means less than nothing, in its pompous robe of adjectives. Pragmatism alone can read a positive meaning into it, and for that she turns her back upon the intellectualist point of view altogether. 'God's in his heaven; all's right with the world!'—*That's* the real heart of your theology, and for that you need no rationalist definitions.

Why shouldn't all of us, rationalists as well as pragmatists, confess this? Pragmatism, so far from keeping her eyes bent on the immediate practical, foreground, as she is accused of doing, dwells just as much upon the world's remotest perspectives.

See then how all these ultimate questions turn, as it were, upon their hinges; and from looking backwards upon principles, upon an *erkenntnisstheoretische Ich*, a God, *a Kausalitätsprinzip*, a Design, a Free-will, taken in themselves, as something august and exalted above facts,—see, I say, how pragmatism shifts the emphasis and looks forward into facts themselves. The really vital question for us all is, What is this world going to be? What is life eventually to make of itself? The centre of gravity of philosophy must therefore alter its place. The earth of things, long thrown into shadow by the glories of the upper ether, must resume its rights. To shift the emphasis in this way means that philosophic questions will fall to be treated by minds of a less abstractionist type than heretofore, minds more scientific and individualistic in their tone yet not irreligious either. It will be an alteration in 'the seat of authority' that reminds one almost of the protestant reformation. And as, to papal minds, protestantism has often seemed a mere mess of anarchy and confusion, such, no doubt, will pragmatism often seem to ultra-rationalist minds in philosophy. It will seem so much sheer trash, philosophically. But life wags on, all the same, and compasses its ends, in protestant countries. I venture to think that philosophic protestantism will compass a not dissimilar prosperity.

21-09-07 *Easy Questions, RPWS1* (to P. W. Shor)

Thanks very much. What you've already said was very useful.

But could I ask you to expand on this (just for me personally):

Shor Thing 1: *You should have heard my after-dinner talk in Japan. Both the Everett view and the Copenhagen view are misleading in thinking about quantum computation (although misleading in quite different ways).*

I hate I missed that! Could you expand briefly on what you said? Or was the talk recorded and available somewhere? I am really, really curious.

Peter's Reply

I believe I said that the Bohm interpretation was like the man that the man in the balloon asked directions from, that Deutsch's intelligent quantum computer would have to answer "I forget," and that the Copenhagen interpretation was responsible for my success (as this is the reason that nobody discovered the factoring algorithm before me). I don't believe that I mentioned the spiders in the basement of the castle in the forest.

Maybe you should ask Charlie what I said. I'll expand if I can find my notes.

22-09-07 *Easy Questions, RPWS2* (to P. W. Shor)

What I was hoping particularly was that you'd expand on this statement:

Shor Thing 2: *Both the Everett view and the Copenhagen view are misleading in thinking about quantum computation (although misleading in quite different ways).*

Since this is an Everett meeting, do you mind relating at least that one. How do you see it as misleading when it comes to quantum computation?

Peter's Reply

Computer scientists who hear about the Everett interpretation construct a mental model that a quantum computer is many worlds in parallel that can all interact, so that you should be able to do polynomial-depth exponential number of processors classical computation. This is the exponential analog of the complexity class NC, and is much, much more powerful computationally than the real class BQP.

The Copenhagen interpretation, on the other hand, leads people to think about collapse of the wave function as some kind of actual real process, which is also very misleading.

27-09-07 *Easy Questions, RDG* (to D. Gottesman)

Thanks for the detailed reply. I didn't get a chance to use it in my talk—I ran out of time—but I had a good chunk of your Answer to #3 inserted into a transparency. Sorry I didn't get to it. There is indeed a certain methodological analogy between purifying states and introducing scalar and vector potentials in E&M. And by my own criterion in the talk, it's something I should take note of. I was going to say that, but then ran out of time.

27-09-07 *The Old Foil* (to J. Preskill)

I didn't get a chance to roll in many of the replies to my Everettian questionnaire in my talk (other than Shor, Simon, Deutsch, and Jozsa's replies), but I did nonetheless use you as a foil in my discussion on quantum cosmology in it. In case you'd be interested to see it, here's the link: `http://pirsa.org/07090068/`. I fell a little flat in my presentation of how the "external" system can be extended all the way around the observer—and how the very reason we use quantum mechanics is because we are within the universe, not external to it, and that nothing about that changes when we get to cosmology—and so, I have every bit as much right to write down a wavefunction for the universe as an Everettian (more so really), but at least the points are all there. I think I need some drama lessons. Anyway, thanks for being the foil again, 11 years later.

27-09-07 *The Joint-Stock Society* (to H. R. Brown)

Thanks for your remark just before Adrian's talk. I enjoyed it, and would ultimately like to pursue your point in a (relatively, never too) serious discussion.

Here are two further quotes of James that I had prepared for the talk, but didn't get a chance to actually show. They have to do with the way James is thinking about "chance"—not in a Lewisian sort of way, as a synonym for objective numerical probabilities—but as a statement of an ultimate pluralism in things and a rejection of the block-universe conception. At the moment, I tend to think that does indeed seem to capture the endpoint of my quantum research program—and it is something your joke pretty accurately reflected!

> [Chance] is a purely negative and relative term, giving us no information about that of which it is predicated, except that it happens to be disconnected with something else—not controlled, secured, or necessitated by other things in advance of its own actual presence. As this point is the most subtle one of the whole lecture, and at the same time the point on which all the rest hinges, I beg you to pay particular attention to it. What I say is that it tells us nothing about what a thing may be in itself to call it "chance." It may be a bad thing, it may be a good thing. It may be lucidity, transparency, fitness incarnate, matching the whole system of other things, when it has once befallen, in an unimaginably perfect way. All you mean by calling it "chance" is that this is not guaranteed, that it may also fall out otherwise. For the system of other things has no positive hold on the chance-thing. Its origin is in a certain fashion negative: it escapes, and says, Hands off! coming, when it comes, as a free gift, or not at all.
>
> This negativeness, however, and this opacity of the chance-thing when thus considered *ab extra*, or from the point of view of previous things or distant things, do not preclude its having any amount of positiveness and luminosity from within, and at its own place and moment. All that its chance-character asserts about it is that there is something in it really of its own, something that is not the unconditional property of the whole. If the whole wants this property, the whole must wait till it can get it, if it be a matter of chance. That the universe may actually be a sort of joint-stock society of this sort, in which the sharers have both limited liabilities and limited powers, is of course a simple and conceivable notion.

And

> The more one thinks of the matter, the more one wonders that so empty and gratuitous a hubbub as this outcry against chance should have found so great an echo in the hearts of men. It is a word which tells us absolutely nothing about what chances, or about the modus operandi of the chancing; and the use of it as a war cry shows only a temper of intellectual absolutism, a demand that the world shall be a solid block, subject to one control,—which temper, which demand, the world may not be found to gratify at all. In every outwardly verifiable and practical respect, a world in which the alternatives that now actually distract your choice were decided by pure chance would be by me absolutely undistinguished from the world in which I now live. I am, therefore, entirely willing to call it, so far as your choices go, a world of chance for me. To yourselves, it is true, those very acts of choice, which to me are so blind, opaque, and external, are the opposites of this, for you are within them and effect them. To you they appear as decisions; and decisions, for him who makes them, are altogether

> peculiar psychic facts. Self-luminous and self-justifying at the living moment at which they occur, they appeal to no outside moment to put its stamp upon them or make them continuous with the rest of nature. Themselves it is rather who seem to make nature continuous; and in their strange and intense function of granting consent to one possibility and withholding it from another, to transform an equivocal and double future into an unalterable and simple past.

28-09-07 *Easy Questions, RHJB* (to H. J. Briegel)

Thanks for the belated reply. Too late for the talk, of course, but still enlightening. The funniest chain of replies I had were from Peter Shor and Dan Simon. Shor said, "No I wasn't thinking about parallel worlds, I was thinking about periodicity and Simon's algorithm." Then Simon said, "Everett? Who's Everett? And what's his interpretation?" Another interesting dualism came from the separate replies of Deutsch and Jozsa on the Deutsch-Jozsa algorithm—you can imagine how that played out.

28-09-07 *Truth?* (to G. L. Comer)

Comerism 17: *I teach Intro to Physics 111. It's for physics majors; a one credit hour course just to get freshmen into the flow of the program. Today we talked about the nature of TRUTH! A few were distressed when I said I don't believe in truth.*

> I don't believe in Elvis
> I don't believe in Zimmerman
> I don't believe in Beatles
> I just believe in me
> Yoko and me

Apt. You made me think again on the pragmatist conception of truth. "I just believe in me."

01-10-07 *The Joint-Stock Society* (to A. Wilce)

Thanks for the encouragement about my talk. It came off a little flatter than I had hoped, so it was nice at least to hear that you thought it was fun. Particularly, I think I could have done much better in getting the point across that quantum cosmology presents no problem for the quantum Bayesian, but you live and you learn . . . and I have a proclivity for saying things over and over, so eventually I'll get it right.

You might enjoy two of the further quotes that I ran out of time to present. They start to make the point of what James had in mind when he used the word "multiverse"—it certainly wasn't the big block universe the Everettians have in mind. [See 27-09-07 note "The Joint-Stock Society" to H. R. Brown.]

01-10-07 *The Joint-Stock Society, 2* (to A. Wilce)

Wilce-ism 5: *What do you make of Adrian's "real world" branching-histories proposal?*

Do you remember my transparency where I made fun of "speculative ontologies (before the very last moment)". Adrian's is well before the moment.

01-10-07 *Shakespeare in Sweden, 2* (to P. G. L. Mana)

Thanks for relieving the mystery of PIPPO.

Manalogue 12: *Speaking of Wheeler, I read that he once said "Philosophy is too important to leave to the philosophers". Do you know if he was joking or meant that seriously? I was cordially disappointed when I read that.*

Yes, he certainly said that, and I think he certainly meant it. But what do you think he meant by it? Why did it disappoint you?

I don't know if you will enjoy my new talk, but it is on PIRSA: `http://pirsa.org/07090068/`.

01-10-07 *A Quote You May Like* (to W. C. Myrvold)

Myrvoldism 6: *I arrived back in London to find a copy of David Mermin's new book* Quantum Computer Science *waiting for me. Browsing through it, the following caught my eye (p. 38):*

> Before drawing extravagant practical, or even only metaphysical, conclusions from quantum parallelism, it is essential to remember that when you have a collection of Qbits in a definite but unknown state, *there is no way to find out what that state is.*
>
> If there *were* a way to learn the state of such a set of Qbits, then everyone could join in a rhapsodic chorus. (Typical verses: "Where were all those calculations done? In parallel universes!" "The possibility of quantum computation has established the existence of the multiverse." "Quantum computation achieves its power by dividing the computational task among huge numbers of parallel worlds.") But there is no way to learn the state. The only way to extract any information from Qbits is to subject them to a measurement.

I do like it! Thanks. I'm sorry I didn't get a chance to talk to you more this meeting. I'm also sorry my talk came out a little flatter than I had wanted it to (there were several points that I didn't think I emphasized correctly).

Since you're an aficionado of "chance" let me forward a note I had written to Harvey Brown in the aftermath of the meeting. It contains two quotes that I didn't get a chance to read in my talk. I think they decently capture the contrast between James's notion of chance and a David Lewis style chance, and the contrast between James's multiverse (his usage should get priority in any case, as he invented the term) and the limp, pale "multiverse" of the Everettians (a big block universe, whose 'multi' aspect is nothing other than the possibility of viewing it from any of its facets)—a dead, lifeless place.

The week that Brian Skyrms is in London, I'd like to come visit you all during some of it. Keep me abreast.

03-10-07 *Nonlocality Again?* (to J. Barrett)

If you have time, I'd really like to follow up on the discussion we started at lunch … in private, so we can hear each other think. The reason is, I think statements like "Bell inequalities have nothing to do with quantum mechanics" and "their derivation can be posed independently of whether the world operates according to quantum mechanics, and one sees then that they are simply violated by quantum mechanics" or some such forms (as I think you were saying), are incorrect if one is already taking a quantum-Bayesian-like stance. That is, I think I can argue that the usual derivation is simply inoperative from that world view. It takes a completely different world view to get a Bell dilemma going. This is something I've been thinking about a while but I've never tested it flesh and blood on anyone, nor have I written much about it. So, you're the perfect candidate and I would enjoy talking about it with you.

Below is a sketch of what I'll say. (Clearly I never finished my promised notes to van Enk.) [See notes to van Enk titled "Inside and Outside" and "The One-Belly Theory of the Universe".]

03-10-07 *Wave Function of the Universe and Sipe* (to S. J. van Enk)

van Enkism 12: *Last night we had dinner with Sipe (Toronto) and he was saying how during a talk you had written down a wave function of the universe, something like:*

$$|\psi_{\rm universe}\rangle$$

I maintained you'd never assign a pure state to the universe, in fact not even to the best ion in Wineland's ion trap. Please tell me I'm right! In other words, I'm sure you must have used some qualification when you wrote down $|\psi_{\rm universe}\rangle$. *What was the context here?*

I've been meaning to write you. We have voted that we would like you to give a quantum foundations seminar here (to talk on your "toy model"). They're Tuesdays at 4:00. Take your pick, most slots are open. The sooner you can come the better, and of course we would pay for everything.

I did write down a pure state, but that was just meant to be symbolic of a quantum state as a whole. In practice, no, *most* people who "accept quantum mechanics" (in the Fuchsian sense) would not write down a pure state for the external universe (to themselves)—but, one cannot forget that that statement is ultimately dependent upon a prior. I can imagine an extreme case where 1) one accepts QM (i.e., as an addition to decision theory), but 2) has an extreme belief of certainty about some particular question one can ask of the external world. Then that hypothetical agent might well indeed write down a pure state for the external world.

Note the use of "external world" when I'm talking about writing down $|\psi_{\rm universe}\rangle$. The point I was trying to make in the lecture is that that is all one needs for doing quantum cosmology. And that really is all one wants. In the Quantum Bayesian view, all the Born rule signifies in any case is a statement of "coherence" in a sense closely analogous to de Finetti's. The agent writes down his beliefs about baryon number, say. He writes down his expectations for the matter distribution in this era of the universe. He writes down his expectations for the Hubble constant. And so on and so on. Then he distils all those expectations into a quantum state assignment. All that is doing for him is telling what he should expect for all the other questions that he might later ask. For instance, what does he expect the mean inhomogeneity in the cosmic microwave background to be? His belief about that should be coherent with all of his other beliefs.

I tried to make the whole point dramatic in the talk, but I think it kind of came out a flop. I'll send a link in a minute. I drew a picture of an agent and a quantum system near him. Then I made the quantum system bigger, and pointed out that nothing changes. Then I made it bigger and bigger, all the while saying nothing conceptually changes. Finally, I had the system completely surrounding him, and made the point that even there nothing changes. Here's the way I wrote John Preskill the other day:

> I fell a little flat in my presentation of how the "external" system can be extended all the way around the observer—and how the very reason we use quantum mechanics is

> because we are within the universe, not external to it, and that nothing about that changes when we get to cosmology—and so, I have every bit as much right to write down a wavefunction for the universe as an Everettian (more so really), but at least the points are all there. I think I need some drama lessons.

Here's the link to the talk `http://pirsa.org/07090068/`.

03-10-07 *Getting the Word Out* (to S. Aaronson)

Aaronsonism 10: *Well, from my perspective, it's about information, probabilities, and observables, and how they relate to each other.*

Congratulations! With one case of corporate abuse, you've made more of an imprint of that idea on the world than all my years of evangelizing combined! And exponentially more so. You're a dream come true. Thank Darwin's soup for Scott Aaronson!

I sure wish you had stayed at PI.

03-10-07 *Getting the Word Out, 2* (to S. Aaronson)

Aaronsonism 11: *I'm so happy you like the infamous quote (though I think it worked better in its original context). Your talks and papers are a large part of what gave me the courage to talk about quantum mechanics this way. Indeed, if you scroll down to the "Further Reading" section of the plagiarized lecture (*`http://www.scottaaronson.com/democritus/lec9.html`*), you'll see "Pretty much anything Chris Fuchs has written."*

That was very sweet of you. I hadn't looked at your lecture before, but I already like it. Particularly, this phraseology comes through to me:

Aaronsonism 12: *So, what is quantum mechanics? Even though it was discovered by physicists, it's not a physical theory in the same sense as electromagnetism or general relativity. In the usual "hierarchy of sciences" – with biology at the top, then chemistry, then physics, then math – quantum mechanics sits at a level between math and physics that I don't know a good name for. Basically, quantum mechanics is the operating system that other physical theories run on as application software (with the exception of general relativity, which hasn't yet been successfully ported to this particular OS). There's even a word for taking a physical theory and porting it to this OS: "to quantize."*

I think that's right on the mark, particularly if one particularizes math to "decision theory". I hope that comes through about halfway through this lecture, `http://pirsa.org/07090068/` (starting around page 28 in the PDF transcript, at least).

"It is not a physical theory in the same sense ...." It's hard to get that across at foundational conferences!

03-10-07 *Aspects of Lunch* (to M. A. Nielsen)

Thanks for telling me about Scott's blog yesterday. I looked it up last night during a fit of insomnia and saw the commercial on YouTube. It blows my Capt. Stabbin story away by miles and miles (and miles and miles)! Of course, I had a general disgust with the business practices of the ad company (and the CEO's ridiculous denial of plagiarism in the *Sydney Morning Herald*). But, in contrast, I was also quite tickled by the exact message that they did copy—that quantum mechanics is about information and probabilities. The creeps couldn't have had a better accident! To get that idea into pop culture would be so wonderful—it won't happen easily, of course, but this commercial at least gave it a new epsilon.

So, I wrote Scott with my pleasure (though I didn't tell him about the captain!), and he wrote me back the nice note below. It took me quite by surprise to learn that even I had an epsilon role in that too. ... And that one really is an epsilon. Still, these are my lucky days, Capt. Stabbin and Capt. Scott!

I then looked at the actual lecture. It is nice, particularly the philosophy. A much better quote from it—one that I think strikes particularly deep—is from the paragraph just before the infamous one:

> So, what is quantum mechanics? Even though it was discovered by physicists, it's *not* a physical theory in the same sense as electromagnetism or general relativity. In the usual "hierarchy of sciences" – with biology at the top, then chemistry, then physics, then math – quantum mechanics sits at a level *between* math and physics that I don't know a good name for. Basically, *quantum mechanics is the operating system that other physical theories run on as application software* (with the exception of general relativity, which hasn't yet been successfully ported to this particular OS).

I think that's a very good way of putting it. I'm sure I'll try to say these kinds of things to Alain Aspect (again), and he'll think I'm just as crazy as he has the last couple of times.

03-10-07 *Feynman and Bell* (to C. M. Caves)

Once upon a time you told me a story about the reason Feynman does not cite Bell in his early quantum computing paper—namely, at the spot where he derives a Bell inequality. As I recall you said it was because Feynman claimed that he had told Bell of the argument sometime before his own publishing of it. Is that the story you told me? Can you fill in details? If you got that second hand, who did you get it from?

I ask because I related it to Alain Aspect at lunch and he gave several reasons to doubt it. So I just want to get the facts straight hereafter. It could be that Aspect is accurate and you are too, given Feynman's proclivity for simply making stories up that made him look better than he already was. (I've compiled several of these—so I know they exist.)

If you could write me back before dinner time when I have to eat with Aspect again, that would be great!

Carl's Reply

> Maybe you're having a late dinner.
>
> This was in a talk Feynman gave at Caltech in the early 80s. He presented the hidden-variable model for a qubit that colors the Bloch sphere and then showed you couldn't do this with two qubits. Feynman would never have paid attention to what Bell did, but I believe he did say that he told Bell about this before Bell did his stuff.
>
> There is a chance that this talk was published in Caltech's *Engineering and Science* magazine, in which case you might find it relatively easily at that web site. It is almost certain that some transcript of the talk exists in the Feynman archives at Caltech, so if you're really serious, you could track it down, I think.

Read Gleick's biography. It's really about how Feynman defined himself in terms of stories that had a grain of truth, but were always embellished to put him in what he perceived as the best possible light. Ralph Leighton transmitted these stories unaltered in his Feynman books. Murray got into trouble in the *Physics Today* Feynman issue precisely because he made this point.

04-10-07 *Feynman Question* (to J. Preskill)

Yesterday, I had this conversation with Carl Caves (and Alain Aspect). [See 03-10-07 note "Feynman and Bell" to C. M. Caves.] I wonder if you can shed any further light on it. Did you happen to be at the same talk? Do you have any recollection? Also, do you have easy access to the magazine or archives Carl mentions; if not could you give me a name of someone I can contact?

Thanks for the help.

John's Reply

Well, I remember hearing Feynman give a talk in the early 80's on the foundations of quantum theory, called "Negative probability." A version of this was later published in a book, but I don't have a copy handy. I don't recall the talk well, except that I remember that I was not impressed by it. (I heard it in a filled auditorium at MIT, so maybe it was before I left Harvard in 1983.) Maybe Carl heard a similar talk, or maybe he is thinking of a different one.

Anyway, I don't regard Feynman's failure to cite Bell as evidence that Feynman thought he had the idea first. He often did not cite people in those days. I talked to him often about confinement in QCD, and pointed out to him that his ideas about magnetic disorder had been anticipated by 't Hooft, Polyakov, and others (whose work was actually deeper in my opinion), but he still did not cite the earlier work when he wrote about the subject. He just didn't want to bother.

By the way, I have a more vivid recollection of a talk that Alain gave at Caltech in the mid-80s, in which he quoted Feynman's statement: "I cannot define the real problem, therefore I suspect there's no real problem, but I'm not sure there's no real problem." For some reason this seemed very funny, and it got a big laugh. But no one laughed harder than Feynman (I was sitting next to him in the front row.)

04-10-07 *Renewing Interest in Aristotle* (to P. Goyal)

Reviewing my Pauli notes, I do indeed see that I should be a little more interested in Aristotle and his potentia, and try to figure out what he is getting at with the idea. See Pauli's letter to Jung (dated 27 Feb '53), starting on page 142 of the collection I gave you (and reprinted below). Also see the entry from Heisenberg's "Wolfgang Pauli's Philosophical Outlook," on page 62.

In case you want to follow up on our lunchtime conversation (on how I want I want to view quantum measurement), I might suggest my samizdat *My Struggles with the Block Universe*, where I wrote a few letters on the subject to Bas van Fraassen.

Letter from Pauli to Jung, 27 February 1953

I cannot anticipate the new *coniunctio*, the new *hieros gamos* called for by this situation, but I will nevertheless try to explain more clearly what I meant with the final part of my Kepler essay: the firm grip on the "tail"—that is, physics—provides me with unhoped for aids, which can be utilized with more important undertakings as well, to "grasp the head mentally." It actually seems to me that in the *complementarity of physics*, with its resolution of the wave-particle opposites, there is a sort of *role model or example of that other, more comprehensive coniunctio.*[119] For the smaller *coniunctio* in the context of physics, completely unintentionally on the part of its discoverers, has certain characteristics that can also probably be used to resolve the other pairs of opposites listed on p. 3. The analogy is on these lines:

Quantum physics.	Psychology of the individuation process and the unconscious in general.
Mutually exclusive complementary experimental setups, to measure position as well as momentum	Scientific thinking – intuitive feeling.
Impossibility of subdividing the experimental setup without basically changing the phenomenon.	Wholeness of man consisting of consciousness and unconsciousness.
Unpredictable intervention with every *observation.*	Change in the conscious and the unconscious when consciousness is acquired, especially in the process of the *coniunctio.*
The result of the observation is an irrational actuality of the unique occurrence.	The result of the *coniunctio* is the *infans solaris*, individuation.
The new theory is the objective, rational and hence symbolic grasping of the *possibilities* of natural occurrences, a sufficiently broad framework to accommodate the irrational actuality of the unique occurrence.	The objective, rational, and hence symbolic grasping of the psychology of the individuation process, broad enough to accommodate the irrational actuality of the unique individual
One of the means used to back up the theory is an abstract mathematical sign (ψ), and also complex figures (functions) as a function of space (or of even more variables) and of time.	The aid and means of backing up the theory is the concept of the unconscious. It must not be forgotten that the "unconscious" *is our symbolic sign for the potential occurrences in the conscious, not unlike that (ψ).*
The laws of nature to be applied are statistical laws of probability. An essential component of the concept of probability is the motif of "the One and the Many."	There is a generalization of the law of nature through the idea of a self-reproducing "figure" in the psychic or psychophysical occurrences, also called "archetype." The structure of the occurrences that thus come into being can be described as "automorphism." Psychologically speaking, it is "behind" the time concept.
The atom, consisting of nucleus and shell.	The human personality, consisting of "nucleus" (or Self) and "Ego."

I should like to add just a few epistemological remarks to this provisional schema. By allowing for occurrences and the utilization of possibilities that cannot be apprehended

[119] I had interesting discussions about these matters with Mr. M. Fierz, to whom I am most grateful.

as predetermined and existing independent of the observer, the type of interpretation of Nature characteristic of quantum physics clashes with the old ontology that could simply say "Physics is the description of reality,"[120] as opposed to "description of what one simply imagines."[121] "Being" and "nonbeing" are not unequivocal characterizations of features that can be checked only by statistical series of experiments with various experimental setups, which in certain circumstances are mutually exclusive.

In this way, the confrontation between "being" and "nonbeing" that was begun in ancient philosophy sees its continuation. In antiquity, "nonbeing" did not simply mean not being present but in fact always points to a *thinking problem.* Nonbeing is that which cannot be thought about, which cannot be grasped by thinking reason, which cannot be reduced to notions and concepts and cannot be defined. It was along these lines, as I see it, that the ancient philosophers discussed the question of being or nonbeing.[122] And it was especially along these lines that the process of becoming and the changeable, hence also matter, appeared in a certain form of psychology as nonbeing—a mere *privatio* of "Ideas." By way of contrast, Aristotle, evading the issue, created the important concept of *potential being* and applied it to *hyle.* Although hyle was actually "nonbeing" and simply a *privatio* of "form" (which is what he said instead of "Ideas"), it *was* potentially "being" and not simply a *privatio.* This is where an important differentiation in scientific thinking came in Aristotle's further statements on matter (he clung firmly to the Platonic notion of matter as something passive, receiving) cannot really be applied in physics, and it seems to me that much of the confusion in Aristotle stems from the fact that being by far the less able thinker, he was completely overwhelmed by Plato. He was not able to fully carry out his intention to grasp the potential, and his endeavors became bogged down in the early stages. It is on Aristotle that the peripatetic tradition and, to a large extent, alchemy is based (*vide* Fludd). Science today has now, I believe, arrived at a stage where it can proceed (albeit in a way as yet not at all clear) along the path laid down by Aristotle. The complementary characteristics of the electron (and the atom) (wave and particle) are in fact "potential being," but one of them is always "actual nonbeing." That is why one can say that science, being no longer classical, is for the first time a genuine theory of becoming and

[120] *Einstein's* words.

[121] *Einstein's* words.

[122] You got involved in this old discussion when you came across the Neoplatonist formula that evil is "nonbeing," is simply a *privatio boni.* Your characterization of this statement as "nonsense" [*Answer to Job*] I attribute more to the bad habit of modern theologians of using old words whose meaning they have long ceased to understand rather than to the original statement itself. For me personally, modern theologians are totally uninteresting, but on the other hand it seems to me *imperative* in such discussions to go back to the original roots of the words and expressions used.

What the ancients meant when they said "nonbeing" was what we would more accurately describe today as "irrational" or "dark."

Now ever since Socrates and Plato, Good has been understood and considered as *Rational* (the virtues are even teachable!), unlike Evil, which does not lend itself to any conceptual definition—a great idea, or so it seems to me. According to this interpretation, the latter regards Good in the same way that matter regards the ideal ("being") mathematical object. With Plato, matter is actually *defined* as that which distinguishes the empirical object from the ideal geometrical object. What they both have in common is the Comprehensible, the Positive, the Good in the empirical body; what makes them different—matter—is the Incomprehensible, later Evil. Hence, matter has only the *passive* function of adopting the geometrical ideas hypostasized as "being" (it is the "receptacle" or "wet nurse" of these ideas). Thus, in later Platonism the *privatio boni* means: Expressed in general terms and understood from the point of view of the "*one*," unchangeable "being" idea, *like Euclid's geometry*, Evil can be *rationally* characterized as the absence of Good, the lack of ideas.

(It is odd how reading your books always transports me back to antiquity. It is obviously a personal effect you have on me; before reading *Aion*, I was not all that interested in antiquity.)

no longer Platonic. This accords well with the fact that the man who is for me the most prominent representative of modern physics, Mr. Bohr, is, in my opinion, the only truly non-Platonic thinker[123]: even in the early '20s (before the establishment of present-day wave mechanics) he demonstrated to me the pair of opposites "Clarity-Truth" and taught me that every true philosophy must actually start off with a *paradox*. He was and is (unlike Plato) a *dekranos*[124] *kat exochen*, a master of antinomic thinking.

As a physicist familiar with this course of development and this way of thinking, the concepts of the gentlemen with the stationary spheres[125] are just as suspect to me as the concepts of "being" metaphysical spaces or "heavens" (be they Christian or Platonic), and "the Supreme" or "Absolute."[126] With all of these entities, there is an essential paradox of human cognition (subject-object relation), which is not expressed, but sooner or later, when the authors least expect it, it will come to light!

For these reasons I should like to suggest also applying the Aristotelean way out of the conflict between "being" and "nonbeing" to the concept of the unconscious. Many people still say that the unconscious is "nonbeing," that it is merely a *privatio* of consciousness."[127] (This probably includes all those who reproach you with "psychologism.") The counterposition is that of placing the unconscious and the archetypes, like ideas in general, in supracelestial places and in metaphysical spaces. This view strikes me as equally dubious and contradictory to the law of the Kairos. This is why I have opted for the third road in my analogy schema in interpreting the unconscious (as well as the characteristics of the electron and the atom) as "potential being."[128] It is a legitimate description by man for potential occurrences in the conscious and as such belongs to the genuine symbolic reality of the "thing in itself." Like all ideas, the unconscious is in *both man and nature*; ideas have *no* fixed abode, not even a heavenly one."[129] To a certain extent, one can say of *all* ideas *"cuiuslibet rei centrum, cuius circumferentia est nullibi"* (the center of all things—a center whose periphery is nowhere), which, according to ancient alchemistic texts, is what Fludd said of God; see my Kepler article, p. 174. As long as quaternities are kept "up in heaven" at a distance from people (however pleasing and interesting such endeavors, seen as omens, may be), no fish will be caught, the *hieros gamos* is absent, and the psychophysical problem remains unsolved.

The psychophysical problem is the conceptual understanding of the possibilities of the irrational actuality of the unique (individual) living creature. We can only come close to dealing with this problem when we can synthetically resolve the pair of opposites "materialism-psychism" in natural philosophy. When I say "psychism," I do not mean "psychologism" nor something peculiar to psychology[130] but simply the opposite of

[123] The English philosopher A. N. Whitehead once said that the whole of European philosophy consisted of footnotes to Plato.

[124] "Double-head"—nickname for disciples of Heraclitus given by disciples of Parmenides.

[125] I have Parmenides and Kepler in mind.

[126] This is an allusion to Indian philosophy. Even those Indian philosophers who, like Prof. S. Radhakrishnan, avoid applying the word "illusion" to the empirical world have no other way of commenting on the Mysterium of the connection between "ultimate reality" and the empirical world, except to call it "Maya."

The Absolute always has the tendency to place itself at an immeasurable distance from man and nature. I happily quote your own words [*Answer to Job*]: "Only that which affects me do I acknowledge as real. But what does not affect me may as well not exist."

[127] Cf. also *Psychologie and Religion*, p. 153.

[128] Cf. ibid., p. 186 below: archetypes as formal possibility.

[129] This point of view was also put forward by Mr. Fierz in the discussion referred to.

[130] As a psychologist, you have an understandable aversion to all forms of reality that are not just psychic. And just as everything that King Midas touched turned to gold, everything you looked at seemed to me to turn psychic

materialism. I could also have said "idealism," but that would have restricted it in time to the famous currents of philosophy prevailing in the 19th century after Kant. These currents (including Schopenhauer), as well as the whole of Indian philosophy, fall into this category of "psychism."

But as the alchemists correctly surmised, matter goes just as deep as the spirit, and I doubt whether the goal of any development can be absolute spiritualization. Sciences made by man—whether or not we wish or intend it and even if it is natural sciences—will always contain *statements about man.*[131] And that is also precisely what I was trying to express with the analogy schema in this section.

Thus the aim of science and of life will ultimately remain man, which is actually the note on which your book *Answer to Job* closes: In him is the ethical problem of Good and Evil, in him is spirit and matter, and his wholeness is depicted with the symbol of the quaternaty.

It is today the archetype of the *wholeness* of man from which natural science, now in the process of becoming quaternary, derives its emotional dynamics. In keeping with this, the modern scientist—unlike those in Plato's day—sees the rational as both good and evil. For physics has tapped completely new sources of energy of hitherto unsuspected proportions, which can be exploited for both good and evil. This has led initially to an intensification of moral conflicts and of all forms of opposition, both in nations and in individuals.

This wholeness of man[132] seems to be placed in two aspects of reality: the symbolic "things in themselves," which correspond to "potential being," and concrete manifestations, which correspond to the actuality of "being." The first aspect is the rational one, the second the irrational one[133] (I use these adjectives analogously, as you did in the typology theory for the characterization of the various functions.) The interplay of the two aspects creates the process of becoming.

Is it in keeping with the Kairos and the quaternity to call these fragments of a philosophy "critical humanism"?

04-10-07 *Feynman Question* (to K. S. Thorne)

Yesterday, I had the conversation below with Carl Caves (and Alain Aspect). [See 03-10-07 note "Feynman and Bell" to C. M. Caves.] I wonder if you can shed any further light on the issue. Carl thinks that you were at the same lecture. Do you have any recollection? Does your memory bear Carl's remark out, or do you have a dissenting memory? Now that I've brought the issue up with Aspect, I feel obligated to try and get it straightened out.

Thanks for any help you can give.

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and only psychic. This aversion to the nonpsychic was probably also one reason why you did not mention the psychophysical problem in your book *Answer to Job*. However, in the passage already quoted in *Aion* (p. 372), you put forward a point of view on the ultimate unity of physis and psyche that coincides with mine. See also note below.

[131] Cf. my Kepler essay, p. 163, n. 7.

[132] This gives rise to the question very closely connected with the psychophysical problem: Is the archetype of wholeness restricted to man, or does it also manifest itself in nature? See your essay "Der Geist der Psychologie," Eranos Jahrbuch 1946, p. 483f, where you treat the archetypes as not just psychic.

[133] The older ancient philosophers since Parmenides have correspondingly described concrete phenomena as "nonbeing." By way of contrast, all general concepts and ideas with unchangeable characteristics ("form" in Aristotle), especially *geometrical* concepts, were "being." There are ancient astronomical papers that set themselves the task of *"saving" phenomena* $\sigma\omega\ \zeta\epsilon\iota\nu\ \tau\alpha\ \varphi\alpha\nu\iota o\mu\epsilon\nu\alpha$. Apparently, they did not use the word "explain." I am not going into the question of pure mathematics here.

Kip's Reply

My memory is really lousy. I do remember Feynman's lecture, but I have no memory of what he may or may not have said about the relationship of his own thoughts about this to Bell's.

It is true that there might be an audio tape of Feynman's lecture in the Caltech Archives, but it was not routine to tape lectures in those days. If there is a tape, it is because it was Feynman speaking.

04-10-07 *The Surprise* (to G. Brassard)

It was such a pleasant surprise to see you last night—it really shocked me. And I had a great time just before Aspect's talk [finally!] getting a chance to discuss some of the deeper issues about what is ontic and what is epistemic within quantum mechanics.

We didn't get a chance to get anywhere near this far, but I think if I had to put into a slogan what's going to be ultimately found and quantified in this research program of mine, it's this: That the ontic of quantum systems is that they are CATALYSTS. That is their conceptual role. The thing that is intrinsic to quantum systems themselves (and the thing dimensionality is ultimately a quantification of) is that they bring about transformations in things external to them. Quantum systems are transformers. There is an element of Mermin's "correlation without correlata" in this idea, but it is a much more active thing, and it is careful to relegate quantum states to the epistemic (which Mermin does not do). Also, it shares a small piece of similarity to Rovelli's relationalism, but ditto what I just said about Mermin.

Anyway, there are deep things to discuss, and I hope we'll get a much longer chance soon. I'll work hard to get to Montréal in the spring if we can home in on a good time for both of us.

05-10-07 *Easy Questions, RLKG* (to L. K. Grover)

Groverism 2: *Sorry, I had missed your message — only just got it — hope your talk went well.*

You might answer anyway if you get the chance. I got replies from many; it'd be nice to know your answers too ... for the day when I become a historian.

05-10-07 *My Sick Questions* (to D. M. Appleby & S. T. Flammia)

See attachment. I'm sending it to you too Steve, because it gives a little more explanation (though not much more than epsilon) for why I'm asking some of these questions.

Now, it's time for PI's Friday Wine and Cheese. I love this place!

SICk Questions?

A Small Attempt to Deepen Our Understanding of These Bases

Preliminaries

I will alternatively call a set of normalized states $|\psi_i\rangle \in \mathcal{H}_d$ or the set of projectors associated with them $\Pi_i = |\psi_i\rangle\langle\psi_i|$, with index i running from 1 to d^2, "SIC" or "a SIC" if and only if

$$\mathrm{tr}\,\Pi_i\Pi_j = |\langle\psi_i|\psi_j\rangle|^2 = \frac{1}{d+1} \quad \forall i \neq j \; . \tag{69}$$

In all questions that follow I am NOT restricting myself to Weyl-Heisenberg covariant SICs. (Though I hope that the Weyl-Heisenberg SICs will come out as the answer to one or more of these questions.)

Needless to say, we don't even know if SICs exist. But the spirit of all the following is, "*Supposing* they do exist, can one show properties X, Y, and Z?"

A SIC-Schmidt? 1

Recall the usual Schmidt decomposition theorem. Given a state $|\psi_{AB}\rangle$ on a bipartite system with Hilbert space $\mathcal{H}_A \otimes \mathcal{H}_B$ (each component with dimension d), there always exists an orthonormal basis $|i\rangle$ on $\mathcal{H}_A$ and an orthonormal basis $|i'\rangle$ on $\mathcal{H}_B$ such that,

$$|\psi_{AB}\rangle = \sum_{i=1}^{d} \alpha_i |i\rangle|i'\rangle \ . \tag{70}$$

QUESTION: Under what conditions does a bipartite state $|\psi_{AB}\rangle$ have a decomposition of the form

$$|\psi_{AB}\rangle = \sum_{i=1}^{d^2} \beta_i |\psi_i\rangle|\psi_i'\rangle \ , \tag{71}$$

for some SICs $\{|\psi_i\rangle\}$ and $\{|\psi_i'\rangle\}$? Like with the usual Schmidt decomposition theorem, is it always possible? If not, give a characterization of the special cases where it can be done.

A SIC-Schmidt? 2

But maybe a Schmidt-like theorem is not best posed at the state-vector level. Maybe instead it lives most naturally at the density operator level.

Thus, QUESTION: Under what conditions will a bipartite density operator ρ_{AB} have a decomposition of the form

$$\rho_{AB} = \sum_{i=1}^{d^2} \gamma_i \, \Pi_i \otimes \Pi_i'; \ , \tag{72}$$

where $\{\Pi_i = |\psi_i\rangle\langle\psi_i|\}$ and similarly $\{\Pi_i'\}$ are two SICs? Is it always possible? If not, give a characterization of the cases where it can be done.

Give Me an ONB

Given an orthonormal basis $|i\rangle$, how can one characterize the closest SICs? (How can one characterize the farthest?) Here's one idea that comes to mind. Given a SIC $\{|\psi_i\rangle\}$, introduce a partition of it into d sets of d vectors. Relabel them $\{|\psi_{ij}\rangle\}$, i and j now both running between 1 and d. Then tabulate this number

$$F = \sum_i \left(\sum_j |\langle i|\psi_{ij}\rangle|^2 \right) . \tag{73}$$

The larger this number, the closer the SIC to the original orthonormal basis. How large can this quantity be? Could it be that, *by a miracle*, the absolute max of F is achieved by a Weyl-Heisenberg SIC, i.e., $|\psi_{ij}\rangle = X^i Z^j |\psi\rangle$, for some fiducial state $|\psi\rangle$.

A SIC von Neumann Entropy

The von Neumann entropy of a density operator ρ can be defined by the following procedure.

1. Introduce an orthonormal basis $|i\rangle$, with $\Pi_i = |i\rangle\langle i|$, $i = 1, \ldots, d$.
2. Tabulate $p(i) = \mathrm{tr}\rho\Pi_i$, and then consequently
3. Tabulate the Shannon entropy $H = -\sum_{i=1}^{d} p(i) \log p(i)$ of the distribution.
4. Finally minimize H over all orthonormal bases $|i\rangle$.

The resulting minimal such entropy is the von Neumann entropy $H(\rho) = -\mathrm{tr}(\rho \log \rho)$, and the orthonormal basis that achieves it is the eigenbasis of ρ.

However, we can ask a similar question of SICs. What is the SIC-entropy of a density operator ρ? It would be defined by the following procedure.

1. Introduce a SIC $\{|\psi_i\rangle\}$, with $\Pi_i = |\psi_i\rangle\langle\psi_i|$, $i = 1, \ldots, d^2$.
2. Tabulate $p(i) = \mathrm{tr}\rho\Pi_i$, and then consequently
3. Tabulate the Shannon entropy $H = -\sum_{i=1}^{d^2} p(i) \log p(i)$ of the distribution.
4. Finally minimize H over all SICs $\{|\psi_i\rangle\}$, to define a function $S(\rho)$.

Does the SIC-entropy $S(\rho)$ have any interesting functional form? Could it be a multiple of the von Neumann entropy? What is the relation between the optimal SICs for this procedure and the eigenbases of ρ?

Counting Common Elements

Suppose one has two nonidentical SICs $\{\Pi_i\}$ and $\{\Pi'_i\}$. What is the maximal number of common elements between the two sets? If we can't give the number precisely, can we bound it in any interesting way?

Really, Just Give Me an ONB

Going back to the question in "Give Me an ONB." Actually it was motivated by the following question. Suppose one is given an arbitrary SIC. Is there any method of deriving a canonically interesting (or interesting class of) orthonormal basis from it $|i\rangle$ from it? Question "Give Me an ONB" was an attempt to tackle this in one way. Maybe there are others. The over-riding reason for this question is that I think we are accustomed (at least subconsciously, if not logically) to thinking of Hilbert spaces as coming equipped with a standard orthonormal basis. Think of the phrase, "computational basis." I want to break that mentality down somewhat, by first equipping the cone of positive operators with a SIC and then taking it from there. But there's no denying that orthonormal bases are useful. So I'd like a standard construction of one or more to fall out of a SIC ... in something of an analogy to the way the Schmidt orthogonalization procedure gives us a way to go from an arbitrary basis to an orthogonal one.

A SIC-Gleason Theorem

Let $d \geq 3$ and $\mathcal{P}$ be the set of one-dimensional projectors $\Pi = |\psi\rangle\langle\psi|$. We will call a function $f : \mathcal{P} \to [0,1]$ a SIC-frame function if for any SIC $\{\Pi_i\}$,

$$\sum_{i=1}^{d^2} f(\Pi_i) = 1 \; . \tag{74}$$

Can one show that for each SIC-frame function, there is a unique density operator ρ such that

$$f(\Pi) = \frac{1}{d} \mathrm{tr}\, \rho \Pi \; ? \tag{75}$$

This question was on my mind a long time ago, and ultimately led to `quant-ph/0306179`, where we showed that it does not work if $d = 2$. However, I have recently regained hope that it may work in $d = 3$ or greater.

05-10-07 *Start of an Answer* (to P. Goyal)

Goyalism 1: *Since the operational approach to QT is so important to my thinking, I would like to ask you to give some thought to the question of how someone without knowledge of the quantum formalism (QF) might discover/implement a SIC.*

But maybe this is simply impossible. You see, I would be quite keen on the possibility of trying to derive the quantum formalism from a set of postulates where SICs are taken as basic primitive givens, for this has a strong built-in attraction, namely that the outcome probabilities of a single measurement completely determine the state of the system one is measuring. However, to proceed along this direction in a well-motivated way, the challenge would be to motivate SICs purely operationally (which is of course easy with PVMs since we have paradigmatic examples like SG measurements to abstract from).

Thanks again for the question. It's helped focus my thoughts. Here's a little start of an answer maybe.

What does it mean to be a quantum system? Here's one way I might tackle the question. It means that there is some *special* action I can take upon a system that has d^2 consequences for my experience, and for which I feel I should structure my uncertainties for those consequences in the following way.

1) If I perform the action once, and get outcome i, then if I perform the same action again, the uncertainties I find reasonable for a second outcome j are

$$p(j|i) = \frac{1}{d} \qquad \text{for} \quad j = i \tag{76}$$

$$p(j|i) = \frac{1}{d(d+1)} \qquad \text{for} \quad j \neq i \; . \tag{77}$$

2) More generally, I (subjectively) accept the following theory of priors $p(i)$ for the consequences of this kind of action. That valid priors form the convex hull of these two equations:

$$\sum_i p(i)^2 = \frac{2}{d(d+1)} \tag{78}$$

$$\sum_{i,j,k} c_{ijk}\, p(i)p(j)p(k) = \frac{d+7}{(d+1)^3} \; , \tag{79}$$

where the coefficients c_{ijk} have been hard won through previous experience, and, of course, have some very remarkable properties. (We shouldn't know this yet, but they are the structure coefficients for the Jordan algebra generated by a SIC.)

And that's pretty much it. If one can give a better motivated argument for those wacky equations—that would be the hard part!!—then one would at least have the start of an operational theory based on SICs. The main reason I wanted to spell it out like this, however, has to do with our conversation yesterday. Mathematically at least, ingredient 1) above is really just a minor modification of your postulate of repeatable measurements. As we discussed yesterday, in the worldview of "quantum measurements revealing pre-existent properties" the postulate of repeatable measurements is pretty well motivated. But then I don't find that worldview tenable because of the Kochen-Specker construction (in conjunction with locality). So the question that should be thrown back at me is: What, then, from the alchemical worldview—that's what I'll call my view, for want of a pithier name at the moment—what from the alchemical worldview motivates this looser kind repeatability hypothesis? *I don't know.* I don't know, but at least I did say this is only the start of an answer.

Now, you may ask, the above certainly can't be all of quantum mechanics? But it comes pretty close, and this something I'll probably need to explain to you at the board. For instance, one might say, we'll you've only hypothesized one kind of action you can take on a system. What are the other ones? (The answer should be all other POVMs, but how does one get there?) In fact the answer is not so difficult once one has this much of the framework. One can answer: Actions are in one to one correspondence to the refinements one can make to a general (non-boundary) $p(i)$. And in that way, one recovers all POVMs. I'll explain that at the board.

07-10-07 *Pragmatism* (to J. E. Sipe)

Sipesm 10: *This seems to me the real strength of a realist approach (i.e., one in which the abstract elements of the theory refer to what actually exists in the world), even if it is something as weak as Putnam's "internal realism." It gives a narrative in the strong sense (i.e., not a "just-so story").*

I thought what I have been talking about *is* something as weak as Putnam's "internal realism"? (Though I would call it powerful and liberating, rather than weak.) It is "pragmatism," and a lot of learned people out there say that internal realism is a species of the same—I think I even learned that from Putnam!

10-10-07 *The Writing Side of My Brain* (to H. R. Brown)

I enjoyed the conversation today. You've challenged me, and I hope to be able to eventually answer your questions in a convincing way. ... At least in a way that's convincing for me, if not for you. At the moment, I am not even to that first stage yet!

In the meantime, let me give you a link to some letters I wrote to Bas van Fraassen expressing my point of view on the word "measurement." As you get a chance, I hope you will look at the letters to him in `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`. The writing side of my brain is, I believe, a little clearer than the speaking side of my brain, and since I'd like to continue this conversation with you, I'd like to make our starting point as clean as possible.

10-10-07 *When No One is Looking* (to H. R. Brown)

Let me also give you a link to this paper of Matt Leifer's "Conditional Density Operators and the Subjectivity of Quantum Operations": `http://xxx.lanl.gov/abs/quant-ph/0611233`. He does a better job of doing what I was trying to argue to you the other day (and a little bit today).

11-10-07 *Modernizing James* (to J. E. Sipe)

Sipesm 11: *Hmmm.... OK, but internal realism does allow for narratives. Pragmatism, at least in a certain sense, does too. I don't see how the Bayesian approach to quantum mechanics that you and co-workers are developing will result in a narrative for the universe.*

Well, somehow I see our q-Bayesian program as a beautiful example (and better justified species, in fact) of Jamesian pragmatism! Do a search in this document on the word "James". Some of the stuff in the XX instances that pulls up might surprise you.

The narrative is that the world is a creative, plurality. That is what I want ultimately to come out of the q-Bayesian program. Two sketches of a manifesto below. [See 17-06-04 note titled "Preamble" and 12-02-07 note titled "Lord Zanzibar".]

11-10-07 *Hi!* (to R. E. Slusher)

I've been missing you too. Particularly as I've been plunging into trying to refine the idea that what is ontic of a quantum system is that it is a catalyst. It is a little core that transforms the world external to itself. Your grimaces when I say these silly things would be much appreciated—you helped keep me on the straight and narrow! I would have written you sooner, but a) you never gave me your email address, and b) despite my looking, I could never find it on the web! I'm glad you finally sent it to me.

Here are three of my recent talks that you might enjoy:

- `http://pirsa.org/07080042/`
- `http://pirsa.org/07080043/`
- `http://pirsa.org/07090068/`

I hope Georgia's ultimately living up to your expectations, and that you and your wife are both happy there. Drop me more notes as you get a chance.

12-01-07 *Critchley* (to me)

- Frank Critchley, Paul Marriott, and Mark Salmon, "On Preferred Point Geometry in Statistics," J. Stat. Planning & Inference **102**, 229-245 (2002).

18-10-07 *Copyrights?* (to N. D. Mermin)

Good to hear from you! I was just thinking about you today. I'll tell you why, but first: I'm glad to hear about your recovery from knee surgery. I never imagined you were going through that.

Here's why I was thinking about you. Do you think one can get a noncolorable set (in the Kochen-Specker sense) of quantum states ALL of which are maximally entangled states on some bipartite system?

A consistent set of histories (in the Griffiths sense) is *just* a POVM (the elements being the histories). (Not an answer to your question, but something I felt like saying.) So all of the hullabaloo G makes over consistent sets is simply to prefer certain kinds of POVM.

I'm glad to hear you'll come visit us, and I'm glad I've got that in writing!

18-10-07 *Noncolorable Histories* (to N. D. Mermin)

Merminition 168: *OK, I see what you mean. But you would never say that the results of measuring a POVM were there (modulo a framework), whether or not the information was actually acquired. So I take it as a purely formal remark that, however, misses what is essential for Griffiths.*

Well, I suppose my take on it is that Griffiths is just deluded. The histories formalism is full of sound and fury, but in the end signifies nothing. That thing which is "essential" for Bob is, in the end, just of no consequence for quantum interpretation. For me the essential question is whether there are "noncolorable" sets of consistent sets of histories. Sets of sets. Essentially the KS question, just now applied to histories. If so, there is no good sense in which one can even say that within a consistent set one history is TRUE (using Bob's way of writing it). TRUE is only relative to the consistent set. And what determines the consistent set under attention? The agent—nothing more and nothing less than that which sets an orthonormal basis in the usual formulation of quantum mechanics. It is just that the agent now is turning his attention to the class of POVMs that Bob prefers to single out (i.e., sets of "histories") rather than the POVMs that Bohr was concerned with (i.e., sets of orthonormal bases).

I tried to say this better in `http://xxx.lanl.gov/abs/quant-ph/0105039`, in the single (long) letter there to Griffiths, starting on page 178. Looking back at it, it is a horrible job, but maybe I still massaged some issues the right way there.

On your more technical question, yes I think one can generalize consistent sets to sequences of POVMs with a Lüders collapse rule in between. Carl Caves did it once upon a time—as I recall—but never published, and then I think someone else published something similar. I'll try to find that reference for you tomorrow.

So good to have you back! Keep thinking about the new KS question I asked you. I am hoping it is useful for readdressing the issue of the objectivity of unitaries.

18-10-07 *Digging Up Bones* (to N. D. Mermin)

In case it is useful to you, here is Bob's reply to that old letter of mine. I really had to dig to pull this one up in my system. It was dated June 13, 1997.

Most relevant to your present query are these words of Bob's:

> Attempts to work out consistency conditions for POVMs has been attempted by Oliver Rudolph in Hamburg. His earlier work was enormously complicated and the results uninteresting. He claims to have made further progress, but I haven't looked at it. My own opinion is that POVMs (and density matrices) represent calculational tools which can certainly be useful for certain purposes; however, I don't see any reason to employ them as "basic events" in a sample space, and hence no point in working out consistency conditions for them.

Have fun in Oregon. I think I give a colloquium in Eugene in February (either before or after Australia). I go to China for the first time Nov 1. When I first went to Japan you told me not to eat the fugu. Any advice for China?

30-10-07 *Talk Still?* (to J.-Å Larsson)

Larssony 2: *I would like to spend* <u>*some*</u> *time before arriving reading and thinking about SICs.*

OK, I'll change the schedule then. Of course, the *best* single paper to look at is `http://xxx.lanl.gov/abs/0707.2071`. A whole lot of interesting things happened while Appleby was visiting. For instance, he classified all Weyl-Heisenberg SICs in $d = 3$. Lane Hughston has also thrown in looking at algebraic geometry aspects in $d = 3$. He'll give a talk on that today, and in a couple of days, you should be able to find it on PIRSA if you want to see it. Steve Flammia has had some good number theoretic ideas too (which would be a little difficult for me to explain). And me, when I'm not learning from everyone else, I've just been plodding along trying to see if my reduction in equations to $3d$ (instead of the original d^2) doesn't fail in $d = 24$. There is a fear in me, since a related conjecture explored by Joe Renes fails there. Any idea how to make the attached *Mathematica* code "Old Notebook" more efficient? Basically all I'm trying to do is minimize the function `EC` and then check that the vector that minimizes it makes the matrices `TT` and `SS` look right. (The matrix `SS`, for instance, should have 1 in the top left entry and $1/(d + 1)$ in all the rest.) The first cell just basically finds me a good random seed, and then in the second cell I use that seed to verify that my vector has all the properties it should have. Anyway, this method can be pushed to work all the way up to about $d = 18$ if one has patience. But there's probably a much better way to do it that would squeak out at least 10 more dimensions. For instance, Renes's notebook which checks the other conjecture pushes out to $d = 23$ without problem. On the other hand, I've been too stupid to modify mine to work like his (I just cannot figure what's wrong), and Renes himself has gone missing!

Anyway, so glad you're interested in thinking about SICs! (The "official" pronunciation now being Sikhs . . . also to connote their religious aspect.)

05-11-07 *Holevo Hirota* (to D. Gottesman)

I just got this letter from Holevo this morning,

> If I am correct, next June Hirota will be 60. This is an important age, notably in Japan. I am not sure how better to mark this date, perhaps with a collection of papers in a dedicated journal? What is your opinion, perhaps you have better or more concrete suggestions. In any case I would be happy to support any reasonable initiative.

and it creates in me the following idea. (I'm trying to be creative, not having it in me to be an editor or any similar thing at the moment.) What would you think about PI hosting a small meeting (birthday party) in honor of Hirota for his 60th birthday? He has been an indefatigable defender of quantum information in Japan, and has brought significant funding to the field for the QCMC conferences, and supported the likes of Holevo in Russia's troubled financial times. Anyway, I'm just thinking something small (like 10 people or less) composed of some of his best scientific friends, like Holevo, Bennett, Yuen, etc. In fact it would be a good excuse for finally getting Holevo to visit us, and similarly with Charlie. And maybe we could even unearth Helstrom, etc.

What do you think? Does PI have an easy mechanism (and a small budget) for making such a thing happen?

07-11-07 *Listening in Beijing* (to J. W. Nicholson)

After hearing of your eating the fugu, I felt a new urgency to listen to your music. That first one (i.e., two) is a lot of fun! It sort of put me in the same mood as when I was watching one of those old, cheesy light-hearted Italian flicks, the kind one used to find on TV in the early days of HBO. I don't know how to articulate the feeling better than that. The second seems to represent your *Rubber Soul* transition. How do you hold so many styles in your head, man? I only got one, and it alone gets burdensome at times.

Now I be off to the office. I'm gonna crack this problem of quantum mechanics, I tell you. Visiting the Temple of Heaven yesterday and the Chinese apothecaries, and thinking about their practices at both, gave me renewed strength to think about alchemy and the analogy in it of quantum state preparation and measurement. When I can make the point precise, people will listen.

… OK, I probably shouldn't have listened to that *Rubber Soul* stuff so early in the morning.

08-11-07 *Hirota's Birthday* (to A. S. Holevo)

Holevo-ism 2: *If I am correct, next June Hirota will be 60. This is an important age, notably in Japan. I am not sure how better to mark this date, perhaps with a collection of papers in a dedicated journal? What is your opinion, perhaps you have better or more concrete suggestions. In any case I would be happy to support any reasonable initiative.*

I'm not opposed to your suggestion—I think it is a good idea—but I just don't have the stomach myself to be an editor (or co-editor) of yet another special issue for a while. On the other hand, if you would like to pursue an issue as editor, I'm quite sure that QIC (*Quantum Information and Computation*, the journal I am associated with) would be more than happy to have you edit a special issue, and I think I could easily help facilitate that for you.

But here's another idea that could be concurrent with that, or separate from it, either way, that I'd be happy to pursue personally. How about a small scientific "birthday party" for Osamu at PI? The sort of thing I am thinking is a small meeting of maybe 10 or so colleagues, some of Osamu's best friends or greatest influences, "Osamu Hirota, between Distinguishability and Noncausality". For instance, you, Charlie Bennett, Yuen, and some others? You and I could be co-organizers, but the administrative staff would take care of almost all the mundane affairs of the meeting like travel details for the participants, etc. And we would have a pleasant banquet with some nice wine in PI's Bistro. Our main duty would be in choosing the participants and contacting them with invitations, etc.

What do you think of such an idea? I have already contacted Daniel Gottesman and he was happy with the idea of something like that (I thought it best to get his opinion before pursuing things further). If it is a kind of thing you too find worth pursuing, I will write an official proposal for PI. If the conference committee agrees, then we should be able to cover most (if not all) of our participants' expenses. Let me know what you think, and if you are in agreement, also send me a list of suggested participants so that we might starting thinking about how to structure my proposal.

I am in Beijing, by the way, and my connection into PI's intranet is so slow, that I'll probably have to wait until getting back next week before pursuing anything further. But it'd be nice to get your commentary before then.

08-11-07 *Uncle Fierz Needs You* (to H. C. von Baeyer)

Greetings from Beijing! I have been meaning to write you for ages, but one thing would always lead to another, and I would always end up remembering just at a time when I was not at my computer. Anyway, having a long, contemplative walk around the Temple of Heaven yesterday and visiting a Chinese apothecary apparently made my mind disciplined enough that I'm writing you now.

The subject is, I would very much like you to give us a visit at PI, and when you're not just imbibing the excitement and atmosphere, use the time to start hitting your Pauli-Fierz project in a detailed way. By being together physically for discussions, I think it could be a great experience for both of us. More than ever I feel like I've got to get my understanding of quantum measurement into shape—measurement as release of the stone—and the best starting point, I am really convinced, must be the Pauli – Fierz – Jung – von Franz discussions.

I have some visitor funds I need to spend before the end of December, and so I think that is the perfect spur for trying to get together for some long conversations and scholarship. I know this is short notice, but I hope you can come (and be interested in coming for the project I propose!). I would be able to pay your travel expenses, put you up in a PI apartment, and provide a per diem for your meals. Come 1 week, 2 weeks, 3 weeks, or even 4, whatever amount of time you can afford. The more the better, the more we could get done.

So please do think about it. If it helps, have a look at a typical dinner menu for the Bistro and its wine list and browse the cultural venue and maybe take a virtual tour of the facilities. Also take note of the Friday afternoon wine and cheese socials, and that's not even to mention all the physics, physics, physics that'll be all around you all the time.

- W. Heisenberg, "Wolfgang Pauli's Philosophical Outlook," in his book *Across the Frontiers*, translated by P. Heath, (Harper & Row, New York, 1974), pp. 30–38.

> The elaboration of Plato's thought had led, in neo-Platonism and Christianity, to a position where matter was characterized as void of Ideas. Hence, since the intelligible was identical with the good, matter was identified as evil. But in the new science the world-soul was finally replaced by the abstract mathematical law of nature. Against this one-sidedly spiritualizing tendency the alchemistical philosophy, championed here by Fludd, represents a certain counterpoise. In the alchemistic view "there dwells in matter a spirit awaiting release." The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only 'Deo concedente.'" The governing symbol for this magical view of nature is the quaternary number, the so-called "tetractys" of the Pythagoreans, which is put together out of two polarities. The division is correlated with the dark side of the world (matter, the Devil), and the magical view of nature also embraces this dark region.

11-11-07 *A Question on Gleason's Theorem for SICs* (to Z. Ji)

Yes, maybe you do not understand the statement of the problem. For, as you state it below, the standard Gleason theorem would not be correct either—for that theorem too can be posed solely in terms of rank-1 operators alone. Then your argument would have gone:

> The point is, von Neumann measurements consist of rank-one operators only, the function could be $f(A) = \mathrm{rank}(A)/d$, and $\sum f(\Pi_k) = 1$ will trivially hold for all $\{\Pi_k\}$ being an orthonormal basis. But the rank function is not linear and cannot be written as required.

The key is that I am only considering functions whose domain of definition is on the rank-1 projectors. After one gets a characterization of them, perhaps one will want to extend their domain (and it will be doable uniquely if they turn out to be linear), but at the outset the domain of definition is the set of rank-1 projectors.

The statement is: Let $d \geq 3$. Suppose f is any given function from the rank-1 projectors to the range $[0, 1]$ with the property that $\sum f(\Pi_k) = 1$ whenever the $\{\Pi_k\}$ form a SIC. Then can one show that there always exists a density operator ρ such that $f(\Pi) = (1/d)\mathrm{tr}(\rho\Pi)$?

About your example, when restricted to the proper domain of definition there is indeed a density operator with the desired property, namely the maximally mixed state. And then that function f will have a unique linear extension to other operators.

Perhaps it would be helpful for you to look at this old paper of ours, where we considered a precursor question, but with qubits: `http://xxx.lanl.gov/abs/quant-ph/0306179`. The section relevant to what we are talking about above is Section IV.

12-11-07 *Autumn in Beijing* (to D. B. L. Baker)

One day last week, as I was walking through a park watching the leaves fall and the Chinese children play with them—a sentimental scene from every year and every place—I thought I would write you a note with this title. Imagine Frank singing that. Of course, I thought I would write the note to you that very day, but as of now I'm making my way from Hong Kong to Anchorage. That's the way I write now, always in delayed mode. [See 03-02-06 note "Red Spears" to D. B. L. Baker.]

I hope you're doing well. I thought you might enjoy a few pictures I took during my stay in Beijing. For the first time in my life this trip, I actually took a camera with me. Pay close attention to what's in the right hand of the Chairman Mao talisman. After leaving that market, and thinking about it a little, I decided I could kick myself for not trying to buy that as a little decoration for my new house. How many opportunities I've missed in life!

27-11-07 *PI QI Group Meeting Today (Yesterday)* (to M. Mosca & L. Sheridan)

Here a couple of those old van Enk papers I was talking about:

- `http://xxx.lanl.gov/abs/quant-ph/0207142`
- `http://xxx.lanl.gov/abs/quant-ph/0410083`

It would be good to get together and talk about issues to do with "self-calibration." I'd like to see how it fits in my worldview that "quantum operations are Bayesian too". You probably have no clue what I'm talking about, but you can read about it here (in the form of debate with Caves, Mermin, Schack, etc.): [some unknown pages in this samizdat]. The banter there still remains better than any of the more formal stuff I've written.

By the way, what does research on "the fundamental object of conscious existence" mean?

27-11-07 *Tables of Contents* (to M. Mosca & L. Sheridan)

… just thinking again about yesterday's talk. For fun I just looked in both Nielsen and Chuang and David Mermin's books on quantum computing. I did an index search on "energy conservation" and "conservation laws". Didn't find anything in either book on either topic.

29-11-07 *Adam's Conversation* (to J.-Å Larsson)

See page 83, note titled "I See Why Bit Commitment," in:

`http://arxiv.org/abs/quant-ph/0105039`.

29-11-07 *Much Better* (to W. G. Demopoulos)

I just finished reading the final version of your paper and actually feel better about it this time around. I'm not sure what you changed, but I remember being bothered by more aspects before … and today I detected almost nothing that got in the way of my appreciation. I will forward on the paper to John Sipe, for I think it clarifies some aspects of my ongoing debate with him. I will also forward it on to Carl Caves because I think page 15's discussion of indeterminacy exposes some dangers in his ways of thinking about the timeless properties of quantum systems.

Where I do take issue with what you write is the very difficult issue of probability 1. If I understand you correctly, I disagree with the first sentence of the first full paragraph from page 22:

> The representation of an elementary particle as a function which, when presented with an experimental configuration, yields an effect, is interchangeable with its representation as a class of propositions only when the effects are predictable with 0-1 probability.

For, I would say, not even then. That is the point of the attached paper, `quant-ph/0608190`, which you didn't seem to get the point of the last time I sent it to you. It is also the point of my discussions with Bas van Fraassen on pages 411 to 417 (and in much greater detail elsewhere in the collection) of `nSamizdat-2.pdf`.

30-11-07 *Relationalism vs Pragmatism at PI* (to B. C. van Fraassen)

I've been meaning to write you for a long time, and the note below from my colleague Nielson on Perimeter Institute's sabbatical program has helped spur me to finally take action. (Our last conversation was interrupted by the great April Nor'easter floods in New Jersey; my basement took 23,000 gallons of water just as I put my house on the market—you were at the Bubfest that very same weekend. That on top of the market bubble bursting led to a lot of pain between then and now. … Anyway, that's my excuse for the silence.)

But I'm established in Waterloo now, and I'd like to increase the rate of our understanding of what a relational view of QM is all about. Thus I had been meaning to invite you for some visits if you've got the time. For short terms, I could pay all your expenses from my visitors budget. But something like the sabbatical program below would be much better still!

In case you're not familiar with PI, I'd say, take a look at a typical dinner menu for the Bistro and its wine list at […], browse the cultural venue at […], and maybe take a virtual tour of the facilities at […]. Also take note of the Friday afternoon wine and cheese socials. Beyond that, of course, are all the people. With regard to philosophy Harvey Brown will be here for the coming year, and there's everyone in London and Toronto. So, I think you could have some good fun.

Think about both propositions and let me know.

05-12-07 *The Misak Book* (to R. W. Spekkens)

Spekkensism 41: *I saw this book while browsing the Oxford University Press website, and thought that you might be interested.*

> **New Pragmatists**
>
> Edited by Cheryl Misak, University of Toronto
>
> Pragmatism is the view that our philosophical concepts must be connected to our practices — philosophy must stay connected to first-order inquiry, to real examples, to real-life expertise. The classical pragmatists, Charles Sanders Peirce, William James, and John Dewey, put forward views of truth, rationality, and morality that they took to be connected to, and good for, our practices of inquiry and deliberation. In this volume, some of our very best contemporary philosophers explore this and develop the pragmatist project, showing that pragmatism is a strong current in philosophy today.
>
> Contributors: Jeffrey Stout, Ian Hacking, Arthur Fine, Cheryl Misak, Huw Price, David Macarthur, David Bakhurst, Terry Pinkard, Danielle Macbeth.
>
> March 2007 — 208 pages — Clarendon Press
>
> 978-0-19-927997-5, HARDBACK £25.00/$45.00

Thanks for pointing the book out: It definitely needs to be in the collection. (Look at the beautiful bookshelves, in fact, that I just had built in my house. PICT0380 particularly, captures the part of the library devoted to pragmatism.) Also, it was nice to learn from the advert that Misak is in Toronto—I hadn't realized that.

10-12-07 *SICs, FQXi, & the Way Chris Thinks* (to D. M. Appleby)

Applebyism 15: *Are you available to talk? I am getting really hung up on this 500 word summary. My mind has got jammed. I don't like what I have written so far. But I can't think of anything to put it in its place. And I badly want to get back to SICs. Perhaps you could help me to unblock myself.*

Applebyism 16: *The proposed research will form part of a larger project, to develop a novel way of looking at quantum mechanics. Classical physics is based on the correspondence theory of truth: it is supposed that each symbol in the theory is the token for an element of reality (for instance, the symbol E, as it occurs on paper, is the token for an actually existent object, the electric field vector). Wave function collapse makes it very difficult to reconcile that idea with quantum mechanics. However that has not stopped people trying. In fact most work in the foundations of quantum mechanics consists in a variety of ingenious attempts to preserve the classical assumption, that the constructions of mathematical physics in general, and the quantum state in particular, should be conceived as being in one-one correspondence with mind-independent realities. It appears to us, however, that the real message of quantum mechanics is that we need to abandon this rather simple-minded way of thinking about physical theories in favour of something richer, and subtler, and potentially much more fruitful. Instead of thinking of the quantum state as a peculiar kind* *of physical object, we take it to have a fundamentally probabilistic significance. Furthermore we take an epistemic, or Bayesian view of probability. So although the quantum state encapsulates our expectations regarding the world, and to that extent makes a statement about the world, it is not the depiction of something in the world (it cannot be such a depiction because of the way it changes discontinuously consequent on learning a measurement outcome). Preliminary work along these lines has been reported in previous publications by two of the investigators. However, a great deal more remains to be done.* [...]

I saw two movies this week, one titled "Dr. Goldfoot and the Bikini Machine" and one titled, "Dr. Goldfoot and the Girl Bombs." The first (1965) was starred by Vincent Price and Frankie Avalon; the second (1966) was starred by Price and the singer Fabian. Anyway, the fun thing was that Avalon and Fabian were employed by the "Secret Intelligence Command" and were known as SIC-men.

I'm tinkering on your draft to see how it would come out in my own voice. I'll send you the result tomorrow. You're under no obligation to keep any of those changes; I'm just doing it to give further ideas, and perhaps making the hard right turn a little more dramatic. ... ...

Below is the result of my tinkering with your draft proposal. I'm sorry to get this back to you so late, but the quantum gravity meeting actually turned out to be interesting and distracting! (Have any doubts that SICs are important? Patrick Hayden today presented a proposed toy-model solution to the so-called black-hole information paradox by him and John Preskill that made use of 2-designs on finite dimensional Hilbert spaces. He said, "imagine the black hole enacts a random unitary." I said, "Imagine instead a demon in the hole that measures a SIC and outputs a quantum state corresponding to his result." The two expressions are equivalent.)

Anyway, the passages below are what your draft inspired me to write. They may be indicative of nothing but my own take on the deeper reasons for the research. I tried to express that as best I could. The count in this version stands at 556 words. The main thing I tried to capture a little better was the hard right turn we talked about on the phone the other night. What I really wanted to say is that our understanding of QM is really doomed without SICs. And that what we're sikhing is a true bending of our worldview. I wanted to say to FQXi, "you're really called for here!" I don't know that I really succeeded at those goals, but that's the sort of thing that was on my mind, as I modified emphasis away from density operators and made it a little more toward probabilities.

I'll send it to you now without tinkering further. By the way, I didn't incorporate any of Lane's last suggestions, as I didn't know what they were when I started this project. Anyway, as I told you last night, feel free to completely trash this version. I have no emotional attachment (mostly it was an exercise in clarifying my own thinking), and ultimately want whatever will turn out with hindsight to have been the best move! I.e., I want you to win at all costs!

> The proposed research forms part of a larger project to develop a novel way of looking at quantum mechanics. Classical physics is based on the correspondence theory of truth: It is supposed that each symbol in the theory is a token for an element of reality. Wave-function collapse upon the acquisition of information, however, makes it difficult to reconcile this idea with quantum mechanics. Nonetheless, this has not stopped many researchers from trying—much work in quantum foundations consists in a variety of ingenious attempts to preserve the classical assumption that quantum states should be conceived as being in one-one correspondence with mind-independent realities. In contrast, it appears to us that the real message of quantum mechanics is that we need to abandon this way of thinking in favour of something richer, subtler,

and potentially of great significance for our worldview. Instead of thinking of quantum states as peculiar kinds of physical objects, we take them to have a fundamentally epistemic significance, particularly along Bayesian lines. So although quantum states encapsulate our expectations regarding the world, and to that extent make statements about the world itself, they are not the depiction of something in the external world. Preliminary work along these lines has been reported in a number of earlier publications by the investigators.

The deepest development of this idea, however, is significantly impeded by the present formulation of quantum mechanics. In fact, from our perspective, the usual Hilbert space formulation of quantum mechanics is a good part of the whole interpretational problem, as it insinuates a fundamental distinction between quantum states and the probability assignments they entail. Thus we seek a mathematically elegant formulation of quantum mechanics in which the main objects are probability assignments, rather than quantum states. This calls for a deeper understanding and classification of the operator bases allowed within the set of quantum states. Based on several arguments, we believe the so-called "symmetric informationally complete" sets of states (SICs) are the most fundamental such bases and thus should be our starting point. There is only one catch: Do SICs exist? As it turns out, this is an extremely nontrivial mathematical problem, with an origin 35 years ago in coding theory. Recently though, much progress has been made on the issue, with deep connections to other areas of quantum information theory and algebraic geometry—so much so that with concerted effort, one imagines light at the end of the tunnel. For instance, SICs are now known (numerically) to exist in Hilbert spaces up to dimension 45, and analytic constructions have been given up to dimension 12.

The first phase of our project is to settle the existence question and to acquire much needed mathematical insight into the geometrical and group-theoretical structures exhibited by SICs. The second phase will involve a number of applications, particularly getting back to the motivational roots of this problem, as well as issues in quantum tomography, cryptography, and a variety of other problems in quantum measurement theory. The Investigators all have well-established track records for innovative work in quantum theory, and have variously undertaken a number of successful preliminary investigations that are relevant to the proposed research. As a consequence we feel our chance of success is high, and even if only partial success is achieved the results obtained would still amply justify the investment.

11-12-07 *Last Night – Seminar – Teleportation* (to M. Knechtel)

Thanks for the interest.

Yes, you do have to have a "stack of raw building blocks" at both ends of the teleportation scheme. For instance, if one wants to teleport the quantum state of an electron, then one needs 1) the original electron situated near Alice, 2) a "raw" electron also situated near her, and 3) finally a further raw electron situated near Bob (that is "entangled" with Alice's raw electron). At the end of the protocol, the quantum state that was associated with the original electron will now be associated with the electron in Bob's possession.

What this means quite literally is that any information one had about the original electron is now relevant to the other electron instead. It's not nearly as exciting as the word "teleportation" might have indicated. "My information is no longer about this, but instead about this other thing far away."

Hope that helps.

Marguarite's Preply

> Good round table last night. Thank you for your input and your humour. I just wanted to clarify teleportation. It sounded to me like the only way teleportation works is if there is, for example, a stack of building blocks at this end resembling a structure of some sort and a stack of building blocks in a pile at a distance. The structure does not teleport from one place to another, but the information about how to assemble the building blocks at the other end, or how the original structure is built, is what gets transported and then the building blocks can be formed into a similar looking structure. Is that correct? My brain needs to process info in examples I am familiar with.

11-12-07 *Any Comments on This as a Proposal?* (to L. Hardy)

It's almost 500 words, the question is even at this stage, how do you think it would fly for Marcus?

> The proposed research forms part of a larger project to develop a novel way of looking at quantum mechanics. Classical physics is based on the correspondence theory of truth: It is supposed that each symbol in the theory is a token for an element of reality. Wave-function collapse upon the acquisition of information, however, makes it difficult to reconcile this idea with quantum mechanics. Nonetheless, this has not stopped many researchers from trying—much work in quantum foundations consists in a variety of ingenious attempts to preserve the classical assumption that quantum states should be conceived as being in one-one correspondence with mind-independent realities. In contrast, it appears to us that the real message of quantum mechanics is that we need to abandon this way of thinking in favor of something richer, subtler, and potentially of great significance for our worldview. Instead of thinking of quantum states as peculiar kinds of physical objects, we take them to have a fundamentally epistemic significance, particularly along Bayesian lines. So although quantum states encapsulate our expectations regarding the world, and to that extent make statements about the world itself, they are not the depiction of something in the external world. Preliminary work along these lines has been reported in a number of earlier publications by the investigators.
>
> The deepest development of this idea, however, is significantly impeded by the present formulation of quantum mechanics. In fact, from our perspective, the usual Hilbert space formulation of quantum mechanics is a good part of the whole interpretational problem, as it insinuates a fundamental distinction between quantum states and the probability assignments they entail. Thus we seek a mathematically elegant formulation of quantum mechanics in which the main objects are probability assignments, rather than quantum states. This calls for a deeper understanding and classification of the operator bases allowed within the set of quantum states. Based on several arguments, we believe the so-called "symmetric informationally complete" sets of states (SICs) are the most fundamental such bases and thus should be our starting point. There is only one catch: Do SICs exist? As it turns out, this is an extremely nontrivial mathematical problem, with an origin 35 years ago in coding theory. Recently though,

much progress has been made on the issue, with deep connections to other areas of quantum information theory and algebraic geometry—so much so that with concerted effort, one imagines light at the end of the tunnel. For instance, SICs are now known (numerically) to exist in Hilbert spaces up to dimension 45, and analytic constructions have been given up to dimension 12.

The first phase of our project is to settle the existence question and to acquire much needed mathematical insight into the geometrical and group-theoretical structures exhibited by SICs. The second phase will involve a number of applications, particularly getting back to the motivational roots of this problem, as well as issues in quantum tomography, cryptography, and a variety of other problems in quantum measurement theory. The Investigators all have well-established track records for innovative work in quantum theory, and have variously undertaken a number of successful preliminary investigations that are relevant to the proposed research. As a consequence we feel our chance of success is high, and even if only partial success is achieved the results obtained would still amply justify the investment.

11-12-07 *Why I Care* (to S. T. Flammia)

Below is a draft of a "500-word" research summary that I helped Marcus write yesterday for his FQXI application. [See 11-12-07 note "Any Comments on This as a Proposal?" to L. Hardy.] Of course I know you'll disagree with some of it (the part about light at the end of the tunnel, particularly), but maybe the first paragraph and the first half of the second paragraph will shed some light on why I actually care about this problem. We are doing important things, even if we spin our wheels 98 percent of the time, and I hope you never lose sight of that. I feel very lucky that I arrived at PI at the same time as you. Seriously.

12-12-07 *For Lane Hughston* (to me)

> On the windowsill of my home on an island in Maine I keep a rock from the garden of Academe, a rock that heard the words of Plato and Aristotle as they walked and talked. Will there someday arise an equivalent to that garden where a few thoughtful colleagues will see how to put it all together and save us from the shame of not knowing "how come the quantum"?
>
> — John Archibald Wheeler

14-12-07 *No Subject* (to C. Snyder)

I know you'll never be able to capture it on paper the way you did in conversation, but I thought your description of your morning coin tossing contained a particularly deep idea. Could you, would you, please, take a shot at writing it down? I also liked that distinction—if I remember it correctly—between "expressing yourself" (in the form of a coin toss) and "expressing what you think of yourself" (in the form of an action with a predictable consequence, like pushing the pint across the table). Say it all again!

If you're interested, you can find my description of the inveterate gambler and the nurturing-wives and bad-girlfriends penal colonies in this talk: `http://pirsa.org/05070097/`. I think I tell the story of the Jewish mystic in that talk too (couldn't figure out the right way to spell "moyal", so I just called him "mystic"). Also, a couple of stories you might enjoy (and ones that potentially indicate why I liked your story last night) can be found in the attached pseudo-paper.

It was good seeing you again!

Christian's Reply, 11-04-12, "Data Sets and Who We Think We Are"

> Long time no chin wag.
>
> I came across a piece of ephemera, a napkin to be exact, with your handwriting on it the other day. It reminded me that I have neglected (for five years) to get back to you about my thoughts on creating a random binary data set (dropping coins on the shitter) and how that informs the self. To be honest, I have nothing too earth-shattering to report (for further reading on "not too earth-shattering" see my reports on chicken vs. egg and tree in forest). That being said, through the skein of ones and zeros I have managed the kernel of an idea that might be worth some beer if you're around in the next bit.

21-12-07 *Do You Know This Guy?* (to G. L. Comer)

`http://en.wikipedia.org/wiki/Rick_Norwood` ?? Ran across him while reading about Nietsche, Schopenhauer, and "the will"—"the will" being the only part of those three that I really care about. Maybe I should title my book, in contrast to Schopenhauer, *The World as Wills and Representation*. Be sure to take note of the additional S.

22-12-07 *Postdoc Positions* (to H. Price, G. Bacciagaluppi, and others)

It looks like I missed a lot of activity the day I was away from home last week. Luckily, Lucien says I'm absolved of my sins. Anyway, I promise to be more active during the next level of culling. That's the real purpose of this message.

But I also wanted to comment on one line of Huw's, because it seems like a fun thing to do. Namely, this:

Pricey Quote 2: *I agree with Guido that foundations and French philosophy is a very unpromising mix.*

For, I would say, "Ah, but if the French philosophers were Renouvier, Boutroux, and Bergson, that might just be a different story! ... Because ... ahem ... I actually do think they're a promising mix for quantum interpretation problems." But alas, I looked at the guy's CV and found no Renouvier or Boutroux. So, he fell off my list too.

22-12-07 *One More Comment!* (to W. C. Myrvold)

Sometimes it takes me a long time to answer an email. But until I do, my correspondent's mail sits faithfully in my inbox with me from time to time reading over it again. Such was this morning. Anyway, after all this time, I find I've not much to say: I didn't find a lot in your note that I did (or could) disagree with. Maybe in fact my only point of departure from you would be with the penultimate sentence, where you write "assumption – that spatially separated systems have independent physical states — is wrong." For, I would go further and say "the assumption that systems have physical states is wrong." More accurately, I would say the lesson of the conjunction of EPR, Bell, and Kochen-Specker is that the assumption that systems have physical states is perhaps not wrong, but rather so limp that it's not worth propping up if one desires real progress in physics.

However, by this rewriting I only mean that the assumption that systems have *intrinsic dynamical* (i.e., time-changing) states is off track—"intrinsic" being the most important word. The main point about this, is that it still leaves the door open for an ultimately realist construal of quantum systems—though one, I think, along more exotic lines than the usual realist has a taste for. For instance, I can still imagine some kind of relational account of quantum mechanics like Spekkens desires as a possibility. I myself don't think that's the right direction, but I see it as a remaining possibility. My own favorite imagery at the moment has to do with a metaphor of catalysts—I want to see quantum systems as something like catalysts, or when I'm feeling particularly poetic, "philosopher's stones." That timeless property to enact change on that which they interact with is their only intrinsic property. Catalyst, philosopher's stone, hyle (as that which underlies change), words like that, are an attempt to express the ontic character I toy with as underlying quantum mechanics.

By the way, for the fun of it, let me record some quotes of William James that give his take on indeterminism. I feel they capture my own take on indeterminism in quantum theory. (So, when you see that Aristotelian word above, don't think Peirce, but think James ... at least in relation to me.)

> [Chance] is a purely negative and relative term, giving us no information about that of which it is predicated, except that it happens to be disconnected with something else—not controlled, secured, or necessitated by other things in advance of its own actual presence. As this point is the most subtle one of the whole lecture, and at the same time the point on which all the rest hinges, I beg you to pay particular attention to it. What I say is that it tells us nothing about what a thing may be in itself to call it "chance." It may be a bad thing, it may be a good thing. It may be lucidity, transparency, fitness incarnate, matching the whole system of other things, when it has once befallen, in an unimaginably perfect way. All you mean by calling it "chance" is that this is not guaranteed, that it may also fall out otherwise. For the system of other things has no positive hold on the chance-thing. Its origin is in a certain fashion negative: it escapes, and says, Hands off! coming, when it comes, as a free gift, or not at all.
>
> This negativeness, however, and this opacity of the chance-thing when thus considered *ab extra*, or from the point of view of previous things or distant things, do not preclude its having any amount of positiveness and luminosity from within, and at its own place and moment. All that its chance-character asserts about it is that there is something in it really of its own, something that is not the unconditional property of the whole. If the whole wants this property, the whole must wait till it can get it, if it be a matter of chance. That the universe may actually be a sort of joint-stock society of this sort, in which the sharers have both limited liabilities and limited powers, is of course a simple and conceivable notion.

and

> The more one thinks of the matter, the more one wonders that so empty and gratuitous a hubbub as this outcry against chance should have found so great an echo in

the hearts of men. It is a word which tells us absolutely nothing about what chances, or about the modus operandi of the chancing; and the use of it as a war cry shows only a temper of intellectual absolutism, a demand that the world shall be a solid block, subject to one control,—which temper, which demand, the world may not be found to gratify at all. In every outwardly verifiable and practical respect, a world in which the alternatives that now actually distract your choice were decided by pure chance would be by me absolutely undistinguished from the world in which I now live. I am, therefore, entirely willing to call it, so far as your choices go, a world of chance for me. To yourselves, it is true, those very acts of choice, which to me are so blind, opaque, and external, are the opposites of this, for you are within them and effect them. To you they appear as decisions; and decisions, for him who makes them, are altogether peculiar psychic facts. Self-luminous and self-justifying at the living moment at which they occur, they appeal to no outside moment to put its stamp upon them or make them continuous with the rest of nature. Themselves it is rather who seem to make nature continuous; and in their strange and intense function of granting consent to one possibility and withholding it from another, to transform an equivocal and double future into an unalterable and simple past.

Wayne's Preply, from 09-02-07

Woke up this morning and had a thought: I don't think I adequately expressed *why* I don't think that your view commits you to using probabilities only for bets whose outcome has an effect on you.

The worries about Wigner's friend and associated paradoxes, which you expressed as saying you don't find events in the density matrix, come from construing the quantum state ontologically. But for you, a density matrix is a compendium of probabilities regarding all the experiments that might be performed on the system. Use it to extract the probabilities of outcomes of experiments that are actually performed (whether or not the outcome affects you); the rest — probabilities of outcomes of experiments that could have been performed, but weren't — are irrelevant to any decisions you make.

However: that means that the response that you started to make to Brian's question about EPR-Bell correlations — which, I think, was that as far as you are concerned the correlations don't exist until you compare the results (at which time the experiments are all in your backwards light cone) — is not available to you. Which is good, I think, because it permits you to use such correlations as clues to what the world outside the purple line is like.

We associate with EPR-Bell experimental setups certain credences about the outcomes of the measurements. These are credences that we, as Bayesians, have learned through experience (by conditionalizing on the results of experiments) are more appropriate than certain other credences (such as those yielded by the toy theories that Bell constructs in his original paper) for such setups.

These credences have an interesting feature: they can't be mixtures of probability assignments on which the outcomes at the two ends are probabilistically independent. That is, these correlations are different from classical correlations. We can imagine a world in which the experiments had turned out differently — in which classical correlations sufficed. But we don't live in such a world.

The degrees of belief that we learn, through experience, to associate with a given physical situation, tell us something about that situation (that is the lesson, I think, of the two islands). I think that the conclusion we ought to draw from EPR-Bell experiments is that Einstein's separability assumption — that spatially separated systems have independent physical states — is wrong. This is perfectly compatible with an absence of action-at-a-distance, and compatible with special relativity. It is of course not the only possible conclusion, but it's the one that I think is best supported by all available empirical evidence.

27-12-07 *A Question in Two Keys* (to H. Price)

1) If you have a preference, how would you spell "Price-ian"? Pricean? Price-ian? Prician? (I only give the last as a possibility, even though I really dislike it, because I recently read about the views of Duns Scotus and saw it called Scotism … completely bastardizing the last name. So, I don't know, maybe it's common in philosophy to go so far.)

2) Where do you have a more elaborate description or working out of your key metaphor than in the research proposal you sent me?

> The project begins with the hypothesis that science and philosophy have often been dominated by a shallow and misleading conception of the relation of the information we use and process to the world we inhabit. Thought, language and scientific theory are often viewed as passive mirrors, which (when all goes well) offer a kind of image of some facet of that world. On this view, the factual content of thought is simply what the image reveals about the world. This project aims to show that this passive, one-way, conception of factual information is both mistaken in itself, and a major obstacle to a proper understanding of seemingly independent matters of great importance in philosophy and physics. The project offers an alternative view of factual information, as an active, practical resource, shaped not only to the contours of our environment, but also, crucially, to a range of different aspects of our own needs, natures and situations. In place of the dominant metaphor of information as a *mirror* or *picture*, the project substitutes the metaphor of a *key*: a practical, two-ended device, shaped at one end to the outlines of some part of the environment, and at the other end to the shape and needs of its users.

I'd like to read more about it in preparation for what I'll say in Sydney.

Hope you had a nice holiday. Attached are two pictures of the new Waterloo Library of Pragmatism! I'll be thinking about your keys in there.

Huw's Reply

> Did I ever send you the draft Introduction to my planned collection of essays? It doesn't have the key metaphor explicitly, I think, but does have some working out of the ideas – copy attached.
>
> Re #1, I guess "Pricean" makes most sense! There's "Pricey", too, but while I'd like to think my views are valuable, I also like to emphasize how economical they are, compared to their metaphysical rivals – so I'm torn on that one ;-)
>
> Looking forward to seeing you! I've got my head down trying to finish a written version of my Everett talk – I promised the editors I've had it done by the end of the month.

27-12-07 *A Little Christmas Pragmatism* (to W. G. Demopoulos, J. E. Sipe, & R. W. Spekkens)

For Christmas fun, I've been reading Edward C. Moore's book, *American Pragmatism: Peirce, James, and Dewey*. And I came across a passage on pages 35 and 36 that made me think of all three of you. Therefore, for a little more Christmas fun, it seemed worthwhile to type it into my computer. The result is attached.

As I perceive it, there is an affinity between this description of Peirce's form of realism and Bill's take on a quantum system as a "function." Though Bill seems to want to de-anthropocentrize (or de-agentize) the idea a little more than the pragmatist might. Anyway, it has been to the extent of this overlap that I have been endorsive of Bill's work (if I can coin a term). So, I'm not reporting anything particularly new here, I just liked Moore's articulation better than my own.

To put this passage in the context of my discussion with John, I would say: Part of the message here is that Peirce is analogous to a "consequence Bayesian" when it comes to percepts, whereas he is analogous to a "traditional Bayesian" when it comes to concepts. The two doctrines peacefully coexist for him, as I believe they peacefully coexist for me.

The Orientation Toward Pragmatism

> With the above statement of Peirce's realism as a background it is not difficult to see what he means by saying that pragmatism "could scarcely have entered a head that was not already convinced that there are real generals [['universals' in today's terminology, Chris]]" (5.503). Pragmatism is a method for defining general concepts (5.8). If one is a nominalist and believes that there is no such thing as "triangularity" anywhere—that triangularity is only a fiction—then there is no place that he can look to see what triangularity really is. But if he is a realist and believes that triangularity may be found in any triangle, then he knows how to define it; it may be defined as part of what one will experience when one examines a triangle.
>
> It is at this point that we must get a firm grasp on the elusive doctrine of immediate perception. Triangularity may be defined as a part of the experience of a triangle because "the percept is the reality" (5.568). "The experience of a triangle" and "a triangle" are epistemologically identical. Therefore, if we list all of the possible experiences one might have of a triangle, these experiences *are* the triangles.
>
> Now how would we go about it if we wanted to list these experiences? We could just state the bald list of experiences, but the list by itself might not suffice to enable the individual for whose benefit the definition is being made actually to obtain those precise experiences. To insure that he would have the proper experiences, perhaps the best approach would be to prescribe for him a certain action such that, if he accomplished it, he would then be confronted by the required experience. Such a prescription would be a plan or a guide for action. One who performed the prescribed action would have the requisite experience and would then know—by experience of it—the property being defined. Of course, such a plan for action would necessarily be complex, but if it is sufficiently detailed so as actually to give a perceptual acquaintance with the property being defined, then it would serve as a definition. Peirce gives an example of this procedure:
>
>> If you look into a textbook of chemistry for a definition of *lithium*, you may be told that it is that element whose atomic weight is 7 very nearly. But if the author has a more logical mind he will tell you that if you search among

> minerals that are vitreous, translucent, grey or white, very hard, brittle, and insoluble, for one which imparts a crimson tinge to an unluminous flame, this mineral being triturated with lime or witherite rats-bane, and then fused, can be partly dissolved in muriatic acid; and if this solution be evaporated, and the residue be extracted with sulphuric acid, and duly purified, it can be converted by ordinary methods into a chloride, which being obtained in the solid state, fused, and electrolyzed with half a dozen powerful cells, will yield a globule of a pinkish silvery metal that will float on gasolene; and the material of *that* is a specimen of lithium. The peculiarity of this definition—or rather this precept that is more serviceable than a definition—is that it tells you what the word lithium denotes by prescribing what you are to *do* in order to gain a perceptual acquaintance with the object of the word (2.330).

One might generalize this approach by holding that a concept may be defined by prescribing that: If you act in a certain manner, then you will have certain experiences, and the sum of the ideas resulting from these experiences constitutes the meaning of the concept being defined. The development of this thesis leads to Peirce's version of pragmatism.

Such a theory of meaning can only be accepted if one believes that concepts are real, that is, if he believes that the concepts have a real external counterpart. If he believes this, and wants to know where to look for this counterpart, then a pragmatic definition will give him a practical guide for actions that will result in an experience of the counterpart. But if he does not believe that concepts are real, then when he follows out the pragmatic definition he will not believe that what he experiences will be the external counterpart, or the referent of the concept, for he does not believe that the concept has a referent. In short, to accept pragmatism is to accept metaphysical realism with reference to concepts.

As Peirce says (1.27), the realist-nominalist controversy is a question to which only two answers are possible: yes or no. If one admits that concepts are general ideas [[i.e., universals again]] and then asks, is there anything in reality that stands in a one-to-one relation to the concept, an affirmative answer is only possible on a realist position; a negative answer relegates concepts to the realm of fictions. It follows from this that pragmatism could not be accepted by anyone who does not also accept metaphysical realism, and that the former could scarcely have entered the head of anyone who did not already understand the latter (5.503).

28-12-07 *A Little Christmas Pragmatism, 2* (to W. G. Demopoulos)

Demopoulosism 32: *I found the quote from Peirce very suggestive. I think Moore's commentary is not so good though. Perhaps he has elsewhere explained metaphysical realism, but it strikes me that one can understand what Peirce is saying without using that notion.*

Perhaps I was a little over-enthusiastic for the passage—it is true that I don't really know what one (and, say, Peirce in particular) means by metaphysical realism. Attached are three things I dug up in light of your points. 1) Moore's original article, from which the chapter I read yesterday appears to be only a minor modification (save for the stuff about Quine and Carnap). [Edward C. Moore, "The Scholastic Realism of C. S. Peirce," *Philosophy and Phenomenological Research* **12**, 406–417 (1952).] In it, he says a little of what he means by "metaphysical realism" with respect to Peirce.

2) An interesting rebuttal by Ralph Bastian. [Ralph J. Bastian, "The 'Scholastic' Realism of C. S. Peirce," *Philosophy and Phenomenological Research* **14**, 246–249 (1953).] 3) A review of Moore's book (with particular emphasis on the same passage as yesterday) by Richard Rorty. [Richard M. Rorty, *Ethics* **72**, 146–147 (1962).]

Demopoulosism 33: *I also think that you're right to see an affinity between Peirce's remarks on lithium and the idea of an effect. The atomic weight is an eternal property of lithium, but what Peirce emphasizes is not this but the way it interacts with things that may be taken to test for it. It is relevant but incidental to this account – the emphasis on effects – that we have knowledge of lithium by performing such tests. All of which is what I think you mean when you say I would de-agentize Peirce while still enjoying your endorsiveness.*

No, the issue isn't "knowledge" as a point of conflict between you and me (I, like you, do consider that incidental, though I would couch the issue in terms of 'belief' rather than 'knowledge' ... being a good follower of Richard Jeffrey), but rather that there is an agent behind the actions Peirce is speaking of. That is to say, you want to define "effect" without reference to one of my favored terms, "consequence for the agent."

In any case, thanks for keeping me honest! I'll comment on your earlier note to me on "certainty" once I return to the real world from holiday mode.

28-12-07 *Qbit* (to N. D. Mermin)

In case you haven't seen it yet, Scott Aaronson wrote a review of your quantum computing book on his blog: `http://www.scottaaronson.com/blog/?p=296`. I found it fun that he raised the standard complaint that I've now heard (probably) ten times over.

31-12-07 *A More Intelligent Model* (to J. B. Lentz & S. J. Lentz)

Here's the YouTube link to the commercial:

- `http://www.youtube.com/watch?v=saWCyZupO4U`

Here's the *Sydney Morning Herald* story:

- `http://www.smh.com.au/news/technology/professor-claims-ad-agency-cribs-lecture-notes/2007/10/03/1191091161163.html`

Here's Scott's original blog:

- `http://scottaaronson.com/blog/?p=277`

02-01-08 *Conference* (to A. Y. Khrennikov & J.-Å. Larsson)

I was just looking at the conference venue and noticed that Cabello is in your invited speaker list, along with Jan-Åke and myself. Thus the following idea came to mind. If there's interest, maybe one of the themes of the meeting could be a special focus on the import and implications of the Kochen-Specker theorem, and maybe the latest technical developments with it too. I also happen to know that Ingemar Bengtsson and Helena Granström are working on issues to do with the KS theorem themselves. So that's two more. If there's any room in the speaker list (and if you're interested in the idea), maybe some of the remaining speaking slots could be tilted in that direction. If you want any suggestions for other speakers in that regard, I could probably develop a few further ideas for speakers in that specialty. (You both know I'm always more than eager with ideas!)

03-01-08 *Jerusalem Probability Workshop December 2008* (to M. Hemmo)

I apologize for taking so long to get back to you. When your first note came I was deeply under the gun on many things and fell behind in my correspondence. But then when your second one came I had already moved into the Christmas vacation with my family. Anyway, I am here now, and I want to thank you for the invitation. It would be an honor to honor Itamar! So, yes, please do count me in. I haven't been to Israel in some years, and can already feel a visit may lead to great inspiration.

Please keep me up to date as the planning proceeds.

03-01-08 *20 Years* (to G. L. Comer)

Didn't we meet in Fall of '89?

Comerism 18: *So, what's this about there being no spacetime?*

You provoked this retort in me. "Yeah, there's only coordinate charts. Sometimes they can be stitched together, in which case we call it spacetime. Sometimes they can't, and then we call it quantum mechanics." I'm sure that's way off the mark in formulation, but there's a little of me that thinks there's a grain of truth in it.

04-01-08 *Screech!* (to G. Bacciagaluppi & H. Price)

Well, I had purchased the tickets!!! I apologize, I had completely spaced on the Feb 4 meeting! But at least I've emended things now. I changed my flight to leave on the same schedule but on Feb 5.

Bacciagaluppism 2: *Is that a live option?*

Now, you know I like that language. Very Jamesian of you Guido! I immediately thought back to beginning of James's essay "The Will to Believe":

> [L]et us call the decision between two hypotheses an *option*. Options may be of several kinds. They may be — 1, *living* or *dead*; 2, *forced* or *avoidable*; 3, *momentous* or *trivial*; and for our purposes we may call an option a *genuine* option when it is of the forced, living, and momentous kind.
>
> 1. A living option is one in which both hypotheses are live ones. If I say to you: "Be a theosophist or be a Mohammedian," it is probably a dead option, because for you neither hypothesis is likely to be alive. But if I say: "Be an agnostic or be a Christian," it is otherwise: trained as you are, each hypothesis makes some appeal, however small, to your belief.

About a year ago, Rüdiger Schack, Carl Caves, and I had a heated debate on whether a probability assignment of unity (i.e., certainty) could be made for a *dead* option (i.e., one that has already passed and thus couldn't have been otherwise than it actually was). I.e., whether talk of probability was even sensible in that situation. For Huw's potential entertainment, I'll paste an excerpt from my take on the issue below. [See 13-12-06 note titled "Offline Discussion" to R. Schack.]

Huw, by the way, I finally nabbed a copy of *Facts & the Function of Truth*. It looks like the presentation will be a little tough-going for me (my lacking the proper vocabulary at this point), but I'll try to get my head around it some nonetheless.

06-01-08 *21 Years Ago* (to H. J. Bernstein)

Finally, finally after all these years, I ran across a copy of the book you edited with Marcus Raskin, *New Ways of Knowing*. In it I read your article "Idols of Modern Science", and as always, felt much in tune with your way of thinking. And you were already thinking those things more than 21 years ago! It was a great article.

Accordingly, I've updated my long-in-the-making document *The Activating Observer* to reflect what I most liked in your article. You can see the result starting on page 9 of the attached (all lovingly typed in with my own two hands). In connection with those passages, you might also enjoy reading or re-reading Pauli's letter to Markus Fierz, dated 10 August 1954, starting on page 96. [See 02-01-06 note "The Oblique Pauli" to H. C. von Baeyer.] Pauli's words in that letter are very reminiscent of your own.

Thanks for stimulating my mind once again old friend.

Quotes from:

- H. J. Bernstein, "Idols of Modern Science and the Reconstruction of Knowledge," in *New Ways of Knowing: The Sciences, Society, and Reconstructive Knowledge*, edited by M. G. Raskin and H. J. Bernstein, (Rowman & Littlefield, Totowa, NJ, 1987), pp. 37–68.

> In their quest to extract consistency from nature, scientists relentlessly pursue very particular and specialized knowledge.

and

> As a physicist, I began this essay with critical self-examination of science as a particular form of organized human behavior. In this light, one aspect stands preeminent. The scientific study of the world upholds experience as the final arbiter of knowledge. Not any experience, but only the specially controlled and preconditioned experience of experiment. Science entails as much creativity, originality, and as many "free inventions of the human spirit" as other intellectual activities (like art, poetry, philosophy, or social studies), but always inventions which can be checked against the experience of nature; not direct, emotional experience like smelling a rose or hiking to exhaustion in the High Sierras or "psyching out" the answer by intuition, but rather as stylized, conventionalized and, indeed, bizarre behavior which traps nature and emphasizes its regularity. Think of the most ordinary scientific operation, such as finding the length of this page with a ruler. You use an instrument of great complexity, with a straight edge supplied by a brass strip held in the fine groove of a specially stamped and printed piece of wood, calibrated against an arbitrary standard. For more detailed knowledge

of the world, science obviously goes beyond the simple rule, but note how value-laden even that device is: could any but an industrialized and commercialized society provide so many millions of brass and wooden copies of a central arbitrary definition of length, or the training and attitudes needed to use it correctly?

and

Physics shows as much variety and method as any science. Indeed, the first attack on a new problem is to try all the tricks that previously worked; conversely, every new trick is quickly applied to all the currently interesting problems if it succeeds on any one of them. There are always new techniques and new approaches being invented. But some form of mathematics always appears, so we might attempt to deduce a general method from this fact. At least one eminent theoretical physicist, Eugene Wigner, has marveled at the "unreasonable effectiveness" of mathematics very extensively, and that mathematical elegance and newly created mathematics are often guides to new physical theories. It is as if Nature herself were secretly, somehow, mathematical ... and man's creation of mathematics mimics nature's creation of phenomena.

But from the present point of view, it is rather bizarre behavior of experiment which projects consistency onto nature. We are, in effect, making phenomena appear rational for our own intellectual and emotional pleasure. Current scientific data are the result of manipulations of natural objects with the specifically numerical outcome of measurement. While doing scientific experiments we do not assemble those rare substances and produce those unusual conditions in order to "groove on the vibrations." Men's and women's minds invent facts of science through their intellectualized, and mathematized, perceptions of phenomena. It is not at all surprising that the highly similar invention of mathematics—with its emotional, nonrational, and unconscious motivation, its own search for consistency as opposed to Truth, and its insistence on producing a rational outcome—has great power and relevance in science. Out of the vast range our minds possess, mathematics and science seem to have tapped the self-same mode.

Asking why the world turns out to be so mathematical is almost like wondering how vegetables come to be so well-suited to feeding animals. Nature's construction of all the organic forms (we now know) is of a piece; of all the various ways to combine elements of earth, only certain carbon-based molecules, controlled by extraordinarily similar genetic codes, form the compounds of all life. Compatibility is no mystery. What sunlight streaming through the biosphere has done for animals and plants, nonrational inspirations flowing into the human mind have done for science and mathematics. What seems most marvelous in this is the "unreasonable effectiveness" of emotions in producing rationality both in mathematics and in physics.

and

To Einstein, the good of physics resided precisely in its objective ability to comprehend reality, not to manipulate it. Only contemplative rationality could serve the morally commendable goal of pure knowledge. In the light of Forman's analysis, Einstein's choice was to reject the standard science, with its hidden acceptance of political pressure, in favor of older values of objective realism. He would not have put it this way; he did not identify the Copenhagen (acausal) interpretation with world politics, but simply with an epistemological error in science itself—namely, confusing study of what we know with a study of what actually is. But most physicists could conveniently relegate such issues to philosophy. They substituted their own construction, knowledge of the universe, in the place of the real world itself as object of investigation *without* taking full responsibility for the power of making real that which they would choose to know. The quantum physicists hoped to banish unverifiable analogies and fictions. But moral visions are also fictions, images, and analogies. In bypassing them (as in ignoring the question of *what* to make real) they excluded this whole realm of thought. Without addressing any moral implications, they took quantum mechanics and its interpretation as the very definition of disciplinary excellence.

The consequent events were extraordinary. Quick to apply the "best" new theories to nuclear puzzles (once they solved those of atomic electrons), physicist hurtled us willy-nilly into a new age, fraught with powerful new dangers. In 1928, barely two years after Born invented the statistical interpretation, Gamow applied quantum mechanics to nuclear physics; in fact, his data on alpha decay happened to start from Uranium-238. In the thirties, experimentalists discovered fission, presaging nuclear energy; the Manhattan Project, triggered by a letter from Einstein to Roosevelt, eventually made World War II our first nuclear war.

and

In the case of quantum mechanics, the defining shift from "it explains" of classical physics to "it works" inspired a different sort of debate. For many years Einstein argued that quantum mechanics was physically incomplete, that there were elements of physical reality which it could not represent. Bohr argued that only the actual results of experiments could be considered real—and these results all obey the probabilities given by quantum mechanics. Both Bohr and Einstein realized that physics had discovered a limit to its own knowledge of reality. Neither of them realized that this limit posed questions of moral dimensions: not whether we were slipping from an old morality of objective (and omniscient) knowledge but new questions, questions about knowledge that works: "Works for what?" "For whom?" "Knowledge to what end?"

All of these questions arise because the quantum revolution entailed not merely abandoning a priori definitions of reality (upon which Einstein seemed to insist) but adopting a new direction of gaze as well—one which is self-reflexive of the fact that it involves a choice. Jauch compares the phenomena of nature to random messages, waiting to be deciphered by scientists:

> But since the code is not absolute, there may be several messages in the same raw material of the data, so changing the code will result in a message of equally deep significance in something that was merely noise before and *conversely*: In a new code a former message may be devoid of meaning. Thus a code presupposes a free choice among different, complementary aspects, each of which has equal claim to *reality*, if I may use this dubious word.

That physics has found itself selecting a reality may seem striking. For our archeological approach, however, the cross-linkage of parallel developments in several fields is even more important. We might even say the analysis could only be correct if the realization that we create reality were reached simultaneously in many aspects of modern culture.

and

> Consider again quantum mechanics, where the problems of measurement theory remain unresolved after more than fifty years: How much does the experimenter control reality? What attributes of the experimenter are relevant for the exercise of this control? How different would physics be if *more* of the experimenter were allowed into the science, if more of the motivation and social connections were considered part of the experiment itself? These are some of the questions that can now be posed within the discipline of physics.

07-01-08 *What You've Written* (to B. C. van Fraassen)

Could you send me all that you've written so far on relational quantum mechanics, à la Rovelli etc.? The title of my talk at the PIAF meeting in Sydney is "Relational Quantum Mechanics, Jamesian Pragmatism, Pricean Pragmatism, and What I Want from This Collaboration," and I want to use the better part of the time to give a survey of the first proper noun in that.

Please send me everything you have!

Bas's Reply

> I really have only the paper I gave at the conference, it was the end of an evolution involving two mss. and a day with Carlo and others in France last year, but I have improved it a bit more since the conference, and attach the version that is to appear in Foundations of Physics.
>
> But fyi I am attaching the paper by Groenewold that I think presages Rovelli's — as I think is visible as soon as we compare the equations — amazingly early for a discussion of information theory in this context!!

08-01-08 *What You've Written, 2* (to B. C. van Fraassen)

Thanks. I've printed them out now. You might be interested in Nancy Cartwright's '72 opinion of Groenewold. I'll attach the pdf. [See N. Cartwright, "Review of *Quantum Theory and Beyond* by Ted Bastin," Phil. Sci. **39**, 558–560 (1972).]

08-01-08 *Many Worlds Note* (to B. C. van Fraassen)

Whatever happened of this note from 2006?

van Fraassenism 12 : *I read a couple of student papers about recent work on many worlds interpretations, and talking with them and others I kept thinking about just what the appeal could be. Late one night I wrote up the worry, it's just two pages — what do you think?*

Did you modify it, publish it, get any devastating feedback on it?

Let me bring this paper by Alex Wilce to your attention. Attached. [See A. Wilce, "Formalism and Interpretation in Quantum Theory,"] The discussion on page 3, I think, captures the crucial issue for me:

> Thus, we have two problems, somewhat in tension with one another. If we view quantum mechanics as a linear dynamical theory, in which physical states are wave

functions, evolving according to the Schrödinger equation, then the theory's analytical apparatus is not especially problematic (Hilbert spaces were, after all, *invented* to describe just this sort of thing); but its probabilistic content seems mysterious and *ad hoc*. If, on the other hand, we accept the theory's minimal probabilistic interpretation as unproblematic, then it is the theory's formal apparatus that seems mysterious and *ad hoc*.

For purposes of this paper, I'll refer to the former problem as the *problem of interpretation*, and to the latter problem as the *problem of the formalism.* Both have proved remarkably refractory, withstanding decades of sustained, and often brilliant, effort by physicists, mathematicians, and philosophers of science, and, in the process, generating substantial technical literatures. It is quite remarkable, therefore, that these two obviously related problems have been pursued somewhat in isolation from one another. Superficially, perhaps, this is understandable, as each problem begins where the other wishes to end; nevertheless, when a tunnel is being dug through a mountain, it is usual for those working from opposite sides to coordinate their efforts.

In this paper, I want to urge that each project has something to contribute to the other. It has become increasingly clear in recent years that many of the most puzzling "quantum" phenomena—in particular, phenomena associated with entanglement, and including, as I'll show, a version of the measurement problem—are in fact quite generic features of essentially all non-classical probabilistic theories, quantum or otherwise. This suggests that many of the interpretive ideas that have been advanced in connection with quantum mechanics can be carried over to a much more general setting. This exercise has something to offer to both foundational projects. On the one hand, an interpretation of quantum mechanics that can't be made sense of absent certain special structural features of quantum mechanics, is potentially a source of fruitful ideas with which to approach the problem of the formalism. On the other hand, if an interpretation can be kept aloft even in the thin atmosphere of a completely general non-classical probabilistic theory, then perhaps it has little to tell us about the physical content of quantum theory. To compress this idea into a slogan: a completely satisfactory interpretation of a physical theory should be capable of yielding (or at least, constraining!) its own formalism.

Namely, despite all the posturing of the Everettians, the pages and pages of elaborated formalism, I simply see *nothing* uniquely implied by quantum mechanics in the ontology they offer us. Quantum mechanics makes the view no more compelling than it was 101 years ago, when William James wrote:

> [I]f you are the lovers of facts I have supposed you to be, you find the trail of the serpent of rationalism, of intellectualism, over everything that lies on that side of the line. You escape indeed the materialism that goes with the reigning empiricism; but you pay for your escape by losing contact with the concrete parts of life. The more absolutistic philosophers dwell on so high a level of abstraction that they never even try to come down. The absolute mind which they offer us, the mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below. And the theistic God is almost as sterile a principle. You have to go to the world which he has created to get any inkling of his actual character: he is the kind of god that has once for all made that kind of a world. The God of the theistic writers lives on as purely abstract heights as does the Absolute. Absolutism

has a certain sweep and dash about it, while the usual theism is more insipid, but both are equally remote and vacuous.

I.e., I'm willing to call Many Worlds little more than a modern species of the genus.

09-01-08 *Wednesday, just before Toronto* (to D. M. Appleby)

I haven't said hi in a while either. Some people rise to the holidays; myself I was buried by them. I'm only now working my way back out. I drop the in-laws off at the Toronto airport this afternoon.

It's great to hear that you are ploughing on in $d = 5$. Myself I spent a little bit of the holidays working on $d = 6$ finally. I got three independent eigenvectors out of your happy unitary and started to work out the equations for the fiducials. Two levels of elimination finally gave me some linear relations between the coefficients, but I haven't gotten a chance to finish that up yet. I want to get that before Wednesday, because I have to give the colloquium here, and I want to close with Steve's "constructible number" hypothesis if it's true. One thing's for sure, though, the Grassl "solution" (quotes because I only presently trust the 3rd and 6th components of it) is definitely not an eigenvector of happy unitary. It had been my hope that it would be, and that that would help me eliminate a couple of equations on its own.

The title and abstract for the colloquium are below. I'm also giving it in Bristol the following week. One thing I wanted to ask you. Would you mind if I advertise your tomographic result showing that SICs are sometimes optimal there? You never did publish that, did you? If you don't mind my saying something, could you send me a precise statement of the theorem?

I'm glad to hear there's a small number of parameters in the triple products of $d = 5$. There's no chance that the parameters can be adjusted so that *all* triple products (of distinct projectors) have the same real part, is there? How I would love that to always be the case! In $d = 3$, looking at your old notebook though, it looks like "all but nine" is the best one can do. So this leads to another question that perhaps can be explored in $d = 5$. Might it possible to make all but 25 have the same value (the real parts that is), and the remaining 25 have all the same of another value?

In general, one can ask can any useful bound be put on the maximal number of Re(triple products) all obtaining the same value? What does that tell us about the shape of Hilbert space?

09-01-08 *Pragmatism at the Perimeter* (to L. Smolin)

Well, the Waterloo Library of Pragmatism is finally open for business! Look at these beautiful solid-oak bookshelves I had built in my house! Sadly, I've only now ordered Unger's book for it, but I *have* ordered it, and I pick it up in Toronto today after dropping my in-laws at the airport. I'll come to your seminar tonight—but to warn you—I might be a little late.

Going back to the theme of the library, I'd like to discuss a couple of things with you when you get a chance. What I'd really like to get straight in my mind is how to ultimately shake off any last vestige of a "block-universe" conception in (the next) physical theory. You know that I think pragmatism has its heart in the right place, but it doesn't have the technical brawn; on the other hand, I feel strongly that quantum mechanics shows some of the technique, but it's only a first hint of the direction we need to go.

Thus, I have wondered whether:

1. You might have any interest in a little semi-periodic discussion session on the confluence of subjects, with emphasis on the pragmatism side of things? It might consist of us two,

probably John Sipe and Philip Goyal, maybe a couple of others. Maybe Appleby when he's visiting, etc. I haven't thought about the format too much yet, but if you have some interest, maybe we could talk about this.

2. Might it be interesting to have a small meeting at PI this year on the subject? The title I've played with in my head is "Pragmatism at the Perimeter." I'm not sure who I'd invite, but Ian Hacking and Cheryl Misak from U. Toronto phil. dept, Unger, Michel Bitbol, Chris Timpson, Rüdiger Schack, Marcus Appleby, and Huw Price come to mind pretty quickly. And you probably have some ideas too. Would you be interested in co-organizing something along these lines with me?

Those are the questions. As I say, as you get a chance, I'd like to talk about these things some more.

10-01-08 *Philosophy Books To Get Rid Of* (to W. G. Demopoulos)

Do you have any interest in having any of these books?

1. Harald Høffding, *A History of Modern Philosophy*, Vol. 2, paperback, Dover.

2. Donald W. Sherburne, *A Key to Whitehead's Process and Reality*, paperback, Indiana University Press, 1966.

3. John Dewey, *The Philosophy of John Dewey: The Structure of Experience*, edited with intro and commentary by John J. McDermott, paperback, G. P. Putnam, 1973.

4. John Dewey, *The Philosophy of John Dewey: The Lived Experience*, edited with intro and commentary by John J. McDermott, paperback, G. P. Putnam, 1973.

I had double copies in my library. If you want any, I'll reserve the ones until I see you again. Otherwise they'll be taken to The Old Goat for 50 cents credit or so. (They're all in good shape, it's just I know I won't get anything for them.)

Høffding, in case the name doesn't ring a bell, had been Bohr's philosophy mentor in Copenhagen. Some historians make a big to-do about his influence on Bohr.

11-01-08 *Tomorrow? Sunday?* (to S. T. Flammia)

Are you going to be in at PI tomorrow or Sunday? I think I've got a good method for bounding the max number of identical triangle areas. A little cubing of the identity, a little combinatorics, a little Schwarz inequality (or maybe arithmetic-geometric mean inequality), and poof! Or so it seems. Anyway, it'd be nice to talk it through with someone. Unfortunately, I've got a social engagement this evening, and then housework preparing for Emma's party tomorrow, but sometime in the afternoon I'll become free.

15-01-08 *Brain Pulp: Quotes on Planck* (to M. A. Nielsen)

> Planck traced the discovery of his vocation to the teaching of an instructor at the gymnasium, Hermann Muller, who awakened an interest, which became a passion, to 'investigate the harmony that reigns between the strictness of mathematics and the

multitude of natural laws.' In 1878, at the age of twenty, Planck chose thermodynamics as the subject of his doctoral dissertation, which he wrote in four months. He recalled that his professor at the University of Munich, Philipp von Jolly, had counselled against a career in physics on the ground that the discovery of the principles of thermodynamics had completed the structure of theoretical physics. That had not dissuaded Planck, who had his compulsion and also an objective far removed from the principal ambition of today's physicists. He had no wish to make discoveries, he told Jolly, but only to understand and perhaps to deepen the foundations already set. — J. L. Heilbron

Many kinds of men devote themselves to Science, and not all for the sake of Science herself. There are some who come into her temple because it offers them the opportunity to display their particular talents. To this class of men science is a kind of sport in the practice of which they exult, just as an athlete exults in the exercise of his muscular prowess. There is another class of men who come into the temple to make an offering of their brain pulp in the hope of securing a profitable return. These men are scientists only by the chance of some circumstance which offered itself when making a choice of career. If the attending circumstance had been different they might have become politicians or captains of business. Should an angel of God descend and drive from the Temple of Science all those who belong to the categories I have mentioned, I fear the temple would be nearly emptied. But a few worshippers would still remain—some from former times and some from ours. To these latter belongs our Planck. And that is why we love him. — A. Einstein

15-01-08 *What I Think I Mean by Pluralism and Indeterminism* (to L. Hardy)

It's the idea of a joint-stock society, rather than a universe. Quotes below. [See 27-09-07 note titled "The Joint-Stock Society" to H. R. Brown.] Don't forget to forward to Nielsen.

15-01-08 *Potentest of Premises* (to L. Hardy)

I never did send you that note on the potentest of my premises, did I? The note I just sent you on the joint-stock society conception of nature reminded me of that. [See 15-01-08 note titled "What I Think I Mean by Pluralism and Indeterminism" to L. Hardy.] Anyway the short of an answer to you is below. [See 17-06-04 note to H. Mabuchi titled "Preamble."] It is that, the key physical element of quantum theory—that is, that in our interactions with the world there is creation—must ultimately be distilled, and then, to it, added a Copernican principle that there is nothing particularly special about us, other than our perspectives. We have evidence of the creation because we partake in it, but we cannot be the whole story. I can't think of a more exciting and more positive conception of nature than that it always, everywhere, and without exception, creates.

20-01-08 *Q&Q* (to T. Slee)

Thanks for the compliments, but I'm not fooling myself: I thought the radio show came out pretty boring honestly. I hope the Rogers Cable version worked better, with its 40 minutes of more material and untrimmed explanations. Also the visuals should have helped on some jokes.

Slee-ism 1: *I think I'd like to try reading some of your papers at some point and see if I can understand what you are on about if that's possible. I didn't quite see how the "quantum mechanics describes the information in a system" rather than "quantum mechanics describes a system" is more than a linguistic sidestep, but I know there's physics behind what you do and if there is something accessible I'd be interesting in reading it.*

Well, I wouldn't use the phrase "QM describes the information in a system." I only make (louder and, I hope, more consistent) the old time-honored observation of Einstein: That a quantum state cannot be a property inherent in a system—owned by it and it alone—but rather must be something else, of the flavor of one's information. Else QM would imply action-at-a-distance and a load of other conceptual difficulties (Wigner's friends and so on). The research program I push, and which I think has had decent success, is to build up the structure of quantum mechanics from that idea alone, along with an adequate answer to the deeper question, "Information about what?"

I'd be flattered if you'd read a paper of mine or two. Let me give this one as maybe the best starting point:

`http://xxx.lanl.gov/abs/quant-ph/0205039`.

For a study in the sociology of my developing views, you might have some fun with

`http://xxx.lanl.gov/abs/quant-ph/0105039`.

Particularly the correspondence with Mermin and Peres, might be useful. And finally further fun can be found (if you like Sudoku) in this paper by Rob Spekkens:

`http://xxx.lanl.gov/abs/quant-ph/0401052`.

He doesn't reconstruct quantum mechanics by any means, but he does demonstrate that many of the phenomena touted in quantum information studies are simply generic to the idea of information (a better word would be "simple uncertainty for whatever the reason"). As Rob puts it in his abstract, "The diversity and quality of these analogies is taken as evidence for the view that quantum states are states of incomplete knowledge rather than states of reality." The only place where Rob and I differ is that he would ultimately like to revert back to hidden variables in his hoped for reconstruction of QM, whereas I argue vigorously that it's time to move on. Reality is trying to tell us something really interesting with quantum phenomena—something far more interesting than hidden variables—and we should listen.

21-01-08 *Value Added!* (to H. C. von Baeyer)

If you do indeed arrive on March 24, it'll be a great day for alchemical ideas in QM! It looks like Marcus Appleby—one of my two toppermost intellectual soulmates at the moment—will be arriving the very same day. Marcus also has a deep interest in Pauli, Jung, Fierz, alchemy, ... and SIC-POVMs! So, we'll have a wonderful time, I'm sure.

21-01-08 *Potential Topic of Discussion* (to R. Schack)

I'll let you know Thursday or so when I plan to arrive in Egham. In the mean time let me send you this little drunken thing I just wrote up (a consequence of my disappointment with Kiki's $8,000 counters in the kitchen). If you've got nothing on the agenda for us to talk about when

I visit, then maybe this is a worthy subject. The point is, I think I'm homing in better on the idea that the Born rule is "an addition to coherence," in contrast to the usual idea that "it is a probability setting rule."

Looking very much forward to seeing you!

Hidden Assumption in Coherence Arguments?
(21 January 2008)

Suppose there is an agent who believes that if he takes a certain action it will result in one of n consequences for him, and he deems the probabilities of those consequences $p(i)$. The $p(i)$, of course, are all nonnegative and $\sum p(i) = 1$, as demanded by coherence.

Alternatively, consider some *other* action that the agent might perform with consequences j (drawn from a potentially different number m of consequences). Furthermore suppose the agent has a firm set of beliefs for which consequence j he would find if he had first found i in the other action previously considered. That is, suppose the agent has a family of probability distributions $p(j|i)$ that he feels are acceptable degrees of belief. To say it again, $p(j|i)$ is the probability of consequence j for the second action, supposing the agent finds consequence i for the first.

Question: Given all that was described above, does coherence make any requirements on the agent's unconditioned probability assignment for j? That is, can we infer what the agent ought to assign for $p(j)$ straight out from the $p(i)$ and the $p(j|i)$? If we had dropped the language of "actions" and "consequences," and substituted "observed" and "found," I think most people familiar with Bayesianism would say we could: If i were some "event" in the usual language, with probability $p(i)$, and $p(j|i)$ were the probability of another event j, given knowledge of i, then

$$p(j) = \sum_i p(i)p(j|i) \tag{80}$$

would be the assignment demanded by coherence for j itself.

But consider i the outcome of some SIC-POVM measurement, and let j denote the outcomes of some *other* SIC-POVM. Then one might well be in possession of $p(i)$ and $p(j|i)$, without anything initially further. If so, what should one assign for $p(j)$? That depends. If one really is making the measurement of i intermediately (but somehow not becoming aware of the outcome), then one should assign a $p(j)$ as in Eq. (80) above. But suppose one has all the information above and goes straight for the jugular—i.e., one wants to calculate the probability of a j result, knowing full well that one is not going to perform the intermediate measurement of i. Then quantum mechanics still gives a means for calculating the $p(j)$, but it is an interesting modification of the above. That one will make the intermediate measurement or not actually makes a difference for how one should calculate one's ultimate probabilities! Also—and this is *quite* surprising—it makes a difference in a very controlled way. Taking the $p(i)$ and the $p(j|i)$, one just modifies the formula above to

$$p(j) = (d+1)\left(\sum_i p(i)p(j|i)\right) - \frac{1}{d}\,, \tag{81}$$

where d is the dimensionality of the Hilbert space. That is, the probability one *should* assign is given by the one would imagine as coming about by a normal coherence argument $p'(j)$, but then "stretching" it according to $(d+1)p'(j) - \frac{1}{d}$. It is as if quantum mechanics gives a *reward* for not actually performing the first measurement i. One can take advantage of the conceptual device of using the $p(j|i)$ for calculating and ultimate $p(j)$, but one is rewarded in comparison to the classical standard if one does not bother to actually take the action to collect a value for i. I mean by "reward" that one is allowed more certainty generally for one's answer than would be expected classically.

All of this makes me wonder if there is a hidden assumption of "reality" in the standard Dutch book argument based on conditional bets that I had not previously appreciated. I don't have an answer, but that's the sort of thing I'd like to discuss in my brief time in Egham.

In contrast, suppose the measurement of j was of the character of a von Neumann measurement consisting of rank-1 projection operators (while i still denotes the outcome of a SIC-POVM). Then the modification to standard coherence would be

$$p(j) = (d+1)\left(\sum_i p(i)p(j|i)\right) - 1\,, \tag{82}$$

Again, this is a deviation from the classical formula, but in a controlled way.

Why such simple modifications to coherence!?!? Why does QM reward us for *not* performing the intermediate actions we so freely imagine for use in our calculations?

Wilder idea: Is this the source of the power of quantum computation? That we have more certainty about the consequences of our actions than a classically honed intuition expects we ought to have?

21-01-08 *Talk and Diverse* (to P. G. L. Mana)

As long as you send me your abstract two weeks in advance of your talk, it will be enough time for me. What are the precise dates you'll be visiting?

Manalogue 13: *I am studying Schrödinger's equation and its 'second quantized' form in this period. There are some nice results which can be easily derived from Holevo's convex framework (we soon need to find an appropriate name for this framework, since 'convex', 'vector', or 'r-p' are misinformative or uninformative names).*

First, it can be shown that any physical ('ontic') theory behind the wave function cannot be founded on particle-like entities only, but needs field-like entities as well. More precisely, the physical objects behind the wave function must have a continuum of degrees of freedom, hence cannot be just particles. I know of Beltrametti and Bugajski's extension (which is a special case of a theorem by Holevo from the '80s), but the above is really a mathematical proof. Which is also interesting, because what is proven is partly 'metaphysical'!

This sounds similar to Lucien's "excess ontic baggage" result. He never posted the paper on the archive. If you haven't heard of it before, you might ask him for a copy of the paper.

We will have many things to talk about during your visit. Unfortunately, I can have no extra mind now, as I am preparing to give a PhD exam in England this week, then I come home for one day before departing for the Sydney meeting, where I will talk on "Relational QM, Jamesian Pragmatism, and Pricean Pragmatism." Particularly, wish me luck for the latter!

23-01-08 *In Town* (to R. Jozsa)

I should let you know I'm in town. I just had one of those kinds of sleeps that one can only have in a foreign hotel in an upside-down time zone after a sleepless flight. They're the best sleeps of the year! Now I think I'll slowly wake up, get a shower, and wander out for some food. I've still got a lot of reading to do in the thesis, so most of the rest of my day ought to be taken up with that. Did you have any particular plans for me that I should be aware of, though? Or should I just plan to be a free agent today?

23-01-08 *Old Books* (to R. Jozsa)

While I'm thinking about it, I don't want to forget: Are there any used or antiquarian book stores within walking distance of the hotel? I'd particularly appreciate ones with good philosophy sections. Maybe I'll tool around there while you're busy tomorrow morning. There's a running joke at PI, that Daniel Gottesman won't read anything that's not published electronically, and I won't read anything that's not at least a hundred years old.

24-01-08 *Counter-Bayesians!* (to J. A. Smolin)

You've got to have a look at this article:

> "When Several Bayesians Agree That There Will Be No Reasoning to a Foregone Conclusion," by Joseph B. Kadane, Mark J. Schervish, Teddy Seidenfeld, *Philosophy of Science*, Vol. 63, No. 3, Supplement. *Proceedings of the 1996 Biennial Meetings of the Philosophy of Science Association*. Part I: Contributed Papers (Sep., 1996), pp. S281–S289.
>
> **Abstract:** When can a Bayesian investigator select an hypothesis H and design an experiment (or a sequence of experiments) to make certain that, given the experimental outcome(s), the posterior probability of H will be lower than its prior probability? We report an elementary result which establishes sufficient conditions under which this reasoning to a foregone conclusion cannot occur. Through an example, we discuss how this result extends to the perspective of an onlooker who agrees with the investigator about the statistical model for the data but who holds a different prior probability for the statistical parameters of that model. We consider, specifically, one-sided and two-sided statistical hypotheses involving i.i.d. Normal data with conjugate priors. In a concluding section, using an "improper" prior, we illustrate how the preceding results depend upon the assumption that probability is countably additive.

24-01-08 *Bohm-Biederman Correspondence* (to R. Jozsa)

The name of the artist was Charles Biederman. Here's the Amazon.com link:

`http://www.amazon.com/Bohm-Biederman-Correspondence-Vol-Creativity-Science/dp/0415162254`.

25-01-08 *Bristol, 3 AM* (to D. Gottesman)

The other day, after talking to you about how the usual Bayesian "law of total probability" gets modified in the context of quantum measurements when one uses a SIC representation of it all, I wrote this little bit of poetry in my notebook:

> WILD SPECULATION: This little reward for a physical step not taken is ultimately responsible for the power of quantum computation.

What it's referring to is this. Suppose $p(i)$ gives a quantum state in a SIC representation, i.e., it is the probability for getting outcome i for some imagined SIC measurement. Now consider an arbitrary PVM measurement consisting of rank-1 projection operators, labeled by j—this is the measurement that one is *actually* going to perform. Finally consider the probability for an outcome j that one would write down if one *imagines* first measuring the SIC and finding i; I'll write that as $r(j|i)$. I emphasize "imagines" because one is not really only going to do the i measurement; we're just going to use these numbers as conceptual pieces in deriving a number $q(j)$ representing the probability for the outcomes of the j measurement that we're actually going to perform. Putting it all together, the answer turns out to be

$$q(j) = (d+1)\left(\sum_i p(i)r(j|i)\right) - 1,$$

where d is the dimension of the Hilbert space. That is to say, one can calculate $q(j)$ by an ever slight change of the law of total probability. Slight change mathematically, but big change conceptually. For the upshot of the transformation $x \to (d+1)x - 1$ is that it spits out a probability distribution that is a little more pure (a little less mixed) than one would have imagined classically. That's the reward I was talking about.

Anyway, I'm writing all this down mostly because I feel like writing something down at this funny hour. But I've also been rereading the Steane paper that I had told you about. It is: `http://xxx.lanl.gov/abs/quant-ph/0003084`. I find that I had pretty severely misquoted it when talking to you. I'd be curious to get your reaction to the real thing, if you feel like reading it.

25-01-08 *Morning Report* (to J. E. Sipe)

Well, I'm up to page 148 in Morris's book now. [Charles Morris, *The Pragmatic Movement in American Philosophy*.] My opinion seems to be different than what I thought you expressed last week: I think it's mostly a throw-away book; pretty worthless really. It is hardly "the clearest explanation of American pragmatism ever written" that the attached review claims it to be!!! [See R. Ginsberg, "Review of *The Pragmatic Movement in American Philosophy* by Charles Morris," *Annals of the American Academy of Political and Social Science* **396**, 184–185 (1971).] Still, at least I did get a little thought out of pages 128–136 on Mead's cosmology, which I hadn't appreciated before as having some similarity to James's "joint-stock society" conception of nature. To the extent that I understand it, I like it.

I've been thinking, maybe a much more relevant thing for us to tackle would be to jointly read, H. S. Thayer's massive history of pragmatism and its roots, *Meaning and Action*. I have it on my shelf at home and have read bits throughout, though not a complete linear reading yet. I think it is excellent and much more worthy than the present junk.

28-01-08 *Thinking about Unitarity* (to K. Wiesner)

I looked at some of your papers with Crutchfield over the weekend. I see why you might be interested in the things we talked about at dinner the other night. We should have you come by PI to give us a talk sometime, maybe next Fall if you've got the time.

Let me point out the two papers of my own that might be most relevant for further thinking about the stuff we discussed the other day. I'd say, this:

`http://xxx.lanl.gov/abs/quant-ph/0205039`

and this

`http://xxx.lanl.gov/abs/0707.2071`.

After meeting with Schack right after Bristol, I'm even more hopeful that the representation of unitarity I showed you will help unlock some doors in thought.

In all, good meeting you!

29-01-08 *Knowing Me, Knowing Huw* (to H. Price)

… God, that's a bad ABBA pun, and you must have already heard it hundreds of times in your life … but given my excitement, it's the best I can do! (Ouch, I did it again.) No need to send me more reading for my flight, I've already got plenty of it, and it's all by Huw Price!

Seriously, I am very excited since reading your "Naturalism without Mirrors" on my flight back from Bristol the other day, and I've got about 10 more of your papers printed to read (and reread). At least at this stage, I couldn't find a single thing in the said article that I didn't like. I was very, very impressed: You are an amazingly clear thinker, and I am sorry I had not appreciated your depth of thought before. I knew that I was attracted to some of your ideas previously, but I think my mind wasn't quite prepared the last time I was looking at your stuff (on naturalism and truth).

Particularly I'm excited by this: I now think you far outstrip me in clarifying what it is *I* really want out of quantum mechanics. It was probably captured best by this passage:

> Very crudely put — ignoring, for example, all the obvious grounds for holism — the explanatory project goes something like this. We find our speakers disposed to say "P" (i.e., 'P' appears in the list of statements on the left of the model). We now ask, "Why do they say that?"; and in general (without pretending that this distinction is sharp) we look for an explanation that refers both to features of the speakers, and to features of their natural environment. Note that in our own case, this attitude always looks sideways on, or ironic. We say that P, and then wonder why we said so, how we came to be making a claim of that kind — looking for something deeper as an answer, of course, than merely "Because we realised that P". (This is the kind of irony characteristic of practitioners of the human sciences, of course, who cannot help but view themselves as examples their own objects of enquiry.)

For I would say what I have been on about the last few years, is thinking of the interpretational issues of QM as a *laboratory* for this very kind of thought:

> Very crudely put, the explanatory project goes something like this. We find our agents disposed to accept the structural features of quantum mechanics into their catalogue of probability assignments. We now ask, "Why do they say that?"; and in general (without pretending that this distinction is sharp) we look for an explanation that refers both to features of the speakers, and to features of their natural environment.

So, indeed, I'm really looking forward to fleshing out some serious ideas with you in this collaboration. It's a much more specific project than the one you have in mind, but I think it'll be a very instructive laboratory, and maybe you would get something out of my prods.

Anyway, that paper went down fairly easily for me (probably because it was only an introduction, rather than a technical article), and was enough to hook me. I've since reread "Naturalism without Representationalism" and again come away with the feeling that I am indeed a subject naturalist. But there I understood your arguments far less well. You will have to walk me through some. At the moment, I'm on the paper with Quasi-Realism in the title, which you co-authored with someone. I hope to have two or three more papers read by the time we see each other in Sydney.

29-01-08 *Wittgenstein, Etc.* (to R. Jozsa)

Jozsa-ism 3: *It was certainly a pleasure to catch up last week and I hope you had a good trip back!*

I just wanted to send you a link to the must-read book I mentioned about Wittgenstein/Popper (titled "Wittgenstein's Poker"): [...] (the American edition appears to have a really ghastly poker picture on the cover! ... unlike the UK edition). Also the recent Beatles compilation CD I mentioned (titled "Love" put together by George Martin), most recommendable.

So, hope you like these, and look fwd to maybe seing you again in Waterloo sometime this year.

Thanks for that—I'll certainly be getting both. It was good seeing you too. I'm on my way to Australia now; very busy week!

I enjoyed talking with your student Ashley too. But watch out, the guy has Everettian tendencies. (He said it's his favorite interpretation.) Remember it's part of your duty as his mentor to disillusion him of that! Be sure to give him a complete education!

31-01-08 *Tracking Last Night* (to H. Price)

It dawned on me that the part of last night's discussion centering on whether there might be two possible threads to follow when focussing your 'naturalism without mirrors' project onto quantum mechanics ... It dawned on me that that issue tracks pretty well with Chris Timpson's discussion on pages 26–28 of the attached paper [C. G. Timpson, "Quantum Bayesianism: A Study"], where he discusses potential ontologies that might underly the quantum Bayesian effort of Caves, Schack, and me. It's easy reading, and just three pages: Tell me whether you think whether I got it right that it tracks our conversation, or whether I'm still missing something.

31-01-08 *Tracking Last Night, 2* (to H. Price)

It doesn't look like you read far enough. I would think the paragraph starting with, "It would seem that the cleanest setting for the proposal is ..." on page 28 captures a good bit of what you really have going on in the back of your head: the universe is just a big block of facts (with no easy local description), and it is our perspectival limitations that give rise to the use of QM and potential explanations of the need for that use in terms of retrocausation. The retrocausation is an artifact of our perspectival situation; the ontology—the end result of the Copernican project—though is that of a block.

31-01-08 *Potentest of My Premises* (to E. G. Cavalcanti)

Hi (I'm sorry I've already forgotten your first name again),

Below are the relevant quotes about a Copernican Project based on agency. [See 15-01-08 note "Potentest of Premises" to L. Hardy.] There's also a little more context for them in the attached pseudo-wacky-paper-thing. [See "Delirium Quantum" `arXiv:0906.1968v1`.] Sorry, it's not much to go on yet. You'll see some resemblance—I think—between the James quotes below and the remark you made in Vienna that impressed me.

02-02-08 *Practicing French* (to R. Schack)

Here's an article that you and Friedemann might use as a focus for your morning French practice ["Bruno de Finetti: l'Origine de Son Subjectivisme," by Simona Morini]. I just stumbled across it as I was preparing for my PIAF talk. Is there anything of interest in the paper? See also attached quotes for my own talk. I'm going to talk on the modified coherence stuff this morning. I think I put together a pretty nice talk on it. I hope it stands up in the face of Howard Wiseman … and Carl Caves!

> *Very* crudely put … the explanatory project goes something like this. We find our speakers disposed to say "P" (i.e., 'P' appears in the list of statements on the left of the model). We now ask, "Why do they say that?"; and in general (without pretending that this distinction is sharp) we look for an explanation that refers both to features of the speakers, and to features of their natural environment. Note that in our own case, this attitude always looks sideways on, or ironic. We say that P, and then wonder why we said so, how we came to be making a claim of that kind - looking for something deeper as an answer, of course, than merely "Because we realised that P". (This is the kind of irony characteristic of practitioners of the human sciences, of course, who cannot help but view themselves as examples their own objects of enquiry.)
>
> — Huw Price, "Naturalism without Mirrors"

> *Very* crudely put, the explanatory project goes something like this. We find our agents disposed to accept the structural features of quantum mechanics into their catalogue of probability assignments. We now ask, "Why do they say that?"; and in general (without pretending that this distinction is sharp) we look for an explanation that refers both to features of the speakers, and to features of their natural environment.
>
> — CAF, stealing from Huw Price

03-02-08 *Tsallis and Daróczy* (to O. J. E. Maroney)

Tsallis's 1988 entropy can be found here:

`http://en.wikipedia.org/wiki/Tsallis_entropy`

A little report on Daróczy's 1970 entropy can be found on pages 30–34 of here:

`http://arxiv.org/abs/quant-ph/9601020`

cf. Eq. (2.130).

03-02-08 *Decoherence* (to M. Schlosshauer)

It was good meeting you. I was just looking at some of your papers, and noticed various lines with the sentiment of this one (from your comment on Wiebe and Ballentine), "This leads the authors to the general conclusion that decoherence is not essential to explanations of the classical behavior of macroscopic systems. We show that these claims are not warranted ...." I would be curious to know your reaction to Johannes Kofler's two most recent entries on the archive: `http://xxx.lanl.gov/find/quant-ph/1/au:+Kofler/0/1/0/all/0/1`. Is it really being done without decoherence, or is it, as you were saying in yesterday's conversation, there in a hidden way?

04-02-08 *Tsallis, Daróczy, Gibbs & Shannon* (to O. J. E. Maroney)

Maroneyism 1: *Fair 'nuff!*

Of course, if I wanted to be pedantic, I could always say that if information theorists insist on calling the Gibbs entropy, Shannon information, there's no reason condensed matter theorists can't call the Daróczy information, Tsallis entropy!! ☺

Have a look at Eq. (3) on page 6 of

`http://arxiv.org/abs/quant-ph/9601025`.

Notice which name comes first in the line below that.

05-02-08 *Q&Q, 2* (to T. Slee)

In LA at the moment, on the way back to Waterloo from Australia.

Slee-ism 2: *I confess the bit that puzzles me comes right in the first three words of that section "When two systems ..." I just don't understand what "two systems" means.*

I could see that in the earlier blog entry you gave me. It's true: That's where your lack of familiarity with quantum information shows. One wavefunction does not mean one system. The notion of system is prior to any talk of wavefunctions. Two spins are two spins: They may be assigned a single entangled wavefunction (to describe one's knowledge of them), say, but they remain two spins. Operationally, a system is defined by the parts of the whole that can be manipulated separately. Formally, that is captured by writing down a tensor product Hilbert space $\mathcal{H}\otimes\mathcal{H}$ when describing the whole. One $\mathcal{H}$ belongs to one component, one to the other, and $\mathcal{H}\otimes\mathcal{H}$ belongs to the whole. A unitary operation on the left-hand system, but no action on the right-hand system, would be captured by a bipartite unitary $U\otimes I$ — U being the local unitary on the left, and I being the identity operator on the right. An action on the right, but not on the left, would be denoted by $I\otimes U$. "Local operations and classical communication (LOCC)" is the bread and butter of all quantum information theory. "Bipartite" is a common adjective in the field, not of my invention.

Slee-ism 3: *A single closed system has a state vector, no?*

No. A good Copenhagenist wouldn't say that (after a flavor, I am no more than a Copenhagenist myself). Wavefunctions are not properties systems possess independent of considerations of what an observer knows about them. Consequently a perfectly isolated system may be described by a pure state by one observer, but a density operator by another observer, and still a different density operator by another. It simply depends upon what one knows. This sort of situation arises all the time in quantum crypto. Alice, the preparer, writes down a pure state for her system; Eve, the eavesdropper, writes down a density operator. Nothing to do with ensembles: There's only one copy of the system. Instead, it has everything to do with knowledge or information.

Slee-ism 4: *But the main frustration I felt was that, while you repeated in a couple of places that you reject hidden variable theories (hooray!) I couldn't get what you were putting in their place (boo!). . . . didn't get the answer to "information about what?"*

All I can say is have patience. That is the research program. The whole bit about information is only the first part of the program: It's the bushwhacking to clear the weeds—to make clear that it's not the quantum state that is the objective property of a system. In the end, the "information about what" will be of the flavor of Bohr's answer, but finally made precise. And as far as the ontology, what does the system have when there is nobody considering it? That's neither the information, nor what the information is about. But something else. My presently favorite image is that—at a very deep level—quantum systems are catalysts. That is their ontological role.

By the way, I now have a DVD of the public performance. It's way way better than the radio program. If you want to see Tony again, I could lend it to you.

Now, I fly.

08-02-08 *Chipping at the Block* (to J. A. Vaccaro)

It was good meeting you last week. I hope my personality didn't come off as too audacious the one day we talked . . .

But I've been having another crazy idea lately. I'm thinking about organizing a meeting at PI on issues in pragmatist philosophy (perhaps with Lee Smolin), and I've been thinking about titling it, "Pragmatism at the Perimeter: Chipping at the Block Universe." In that regard, I've become quite taken with your painting "Broken Block". Three (not necessarily intersecting) questions about it:

1) Would you send me a higher resolution .jpg of it, so that I might view it more closely?

2) Are you interested in selling it? I'm starting to fancy it in my home library of pragmatism. See attached photos (though wall color subject to change).

3) If said conference materializes, would you be averse to its being used in some way as a logo for the meeting?

Just exploring the issue.

Joan's Reply

> It was a pleasure to meet you as well. I seem to have the impression that you are fearless in your research so I didn't find you too audacious in person!
>
> Regarding the painting, I am rather chuffed that you are interested in it. And I am delighted about the connection with chipping away at the block universe. At the moment it is sitting in a pile in a cupboard somewhere so you are most welcome to have it, for free of course. I would be happy to send it to you. Just let me know of

the address. It is not very big, perhaps 10in x 8in. It's not framed either. I would be extremely chuffed if you happened to use it for a logo. Golly.

The workshop idea sounds interesting.

08-02-08 *Chipping at the Block Universe* (to L. Smolin)

I'm back from Australia now if you want to pick up talking about pragmatic things. Particularly, I'm thinking harder about that conference idea I mentioned to you. In fact I think I'd like the running theme of it or its focus to be on arguments con (and for fun, a couple of talks pro) the "block universe" conception. Thus I've tweaked the title a little bit to "Pragmatism at the Perimeter: Chipping at the Block Universe," (and a few variations thereof) and have found myself staring at this painting of Joan Vaccaro's as a potential logo

`http://members.optusnet.com.au/j.vaccaro/gallery/`.

PI conference proposals are due Feb 15. If you have an interest, we should talk very quickly.

I read the first half of Unger's book, btw. It was hard for me to see precision in a lot of it, but I did pick up a couple of ideas that were novel to me and I liked in the chapter on time.

10-02-08 *Invitation and Potential Anti-Block Universe Conference, 1* (to C. Misak)

We have never met, but I have two friends, Huw Price and Harvey Brown, who recommend you very highly and suggested that I contact you. I am a researcher at the Perimeter Institute for Theoretical Physics in Waterloo (see our website at `http://perimeterinstitute.ca/` if you've never heard about us), who has a great private interest in pragmatism—particularly Jamesian, Deweyian, Rortyan—believing that the interpretational issues of quantum mechanics lead us straight up that path. Witness the library I just had built for the subject in my new home in Waterloo! (See attached photo; two books edited by you can be seen in the center section, fourth shelf from the top.)

Anyway, I write to you for two reasons.

1) Harvey and I would like you to give us a visit sometime this Spring, and, if you will agree, we'd very much like to you give us a seminar on Peirce and his ideas on chance, indeterminism, tychism, and such stuff.

2) Lee Smolin (also at PI) and I are thinking hard about organizing a conference here, tentatively titled "Pragmatism at the Perimeter: Chipping at the Block Universe". The reason is that a few us here think our studies in quantum foundations and quantum gravity lead in a natural way to abandoning the block-universe picture. However, we're somewhat at a loss about how to directly incorporate that insight more deeply into our physics. Thus it seems like a good time to build a larger community of colleagues we could talk to in this regard, and we're thinking an event like this might help kick off the effort. PI has a large budget for visitors and conferences.

The question is who might we invite as speakers? I have a decently strong knowledge of classical pragmatism and thinkers, but I am not so familiar with the crowd of *living* (non-emeritus) pragmatists (and Bergsonians)—i.e., ones who might best contribute to a meeting a like this. Do you have any recommendations? Or, can you point me to someone who would be in a better position to make recommendations? . . . keeping in mind that the main topic of the meeting will be what's wrong with the block-universe conception and how one might formalize the idea of the opposite.

So, 1) I hope you will give us a visit, and 2) any suggestions you can give regarding the conference will be most appreciated!

10-02-08 *Invitation and Potential Anti-Block Universe Conference, 2* (to C. Misak)

Misakism 1: *As for people you might invite to the conference (apart from Huw), I'd consider Arthur Fine and Ian Hacking. Both of course can talk physics. Arthur is a self-described pragmatist, while Ian is only described as a pragmatist by me and others.*

That is already helpful. I had, by myself, thought of Hacking (whom I met at a "pragmatism and quantum mechanics" meeting in Paris last February, a wonderful guy!), but I had not thought of Fine. Would he be a pragmatist who waves the banner of the universe being on the make? Or would he be a mushy pragmatist like Huw, who accepts the methodology and the anti-representationalist point on truth, but *rejects* the "joint-stock society" conception of nature (as James put it)?

Misakism 2: *Someone who is less well-known, but is nonetheless excellent is Randy Dipert at SUNY Buffalo. He knows Peirce's mathematics very well.*

I will look into Dipert right away.

I'm not necessarily looking for people who know anything of the details of quantum mechanics. Particularly, too much thought on the mysteries of QM might be a bad thing on their resume! Clouding the view, so to speak. If the meeting materializes, I would like several of the participants to indeed be immersed in the classical literature. For instance, an ideal participant might be Ralph Barton Perry, if it weren't too late for that. And Richard Rorty would have been wonderful. I have wondered whether we might still be able to attract Hilary Putnam, or whether he's too old now. It would be great to have an expert on Peirce's metaphysics (that seems easy, given the input you've already given), but also one on James's, one on Dewey's, and one on Mead's. One on Bergson's? One on Deleuze's? . . . Maybe I shouldn't push it too far! But certainly a good grounding on all the classical pragmatists would be most helpful.

About your talk here, just pick a few possible dates: our regular quantum foundations seminar is every Tuesday at 4:00, and you could take one of those slots. The sooner the better! We'd love to talk to you.

The attached document won't reveal too much of my particular interests, but it may help you to see how I see pragmatism as fitting into the bigger game (for me) of quantum issues. It is a teaching proposal I wrote several years ago, for a potential joint position between physics and philosophy.

Thanks for the prompt response. I'm grateful, and will let you know how things develop wrt the conference, and if you have more ideas please do let me know.

10-02-08 *Dues Request, and a Question* (to D. M. Hester)

Thank you for letting me know that my William James Society membership has expired. I will renew right away.

In the meantime, I'd like to ask you a question. I am now a researcher at the Perimeter Institute for Theoretical Physics in Waterloo, Canada (see our website at `http://perimeterinstitute.ca/`), and a colleague, Lee Smolin, and I are thinking hard about organizing a conference here, tentatively titled "Pragmatism at the Perimeter: Chipping at the Block Universe". The reason is several of us here think our studies in quantum foundations and quantum gravity lead in a natural way to abandoning the block universe picture. However, we're somewhat at a loss about how to directly incorporate that insight more deeply into our physics. Thus it seems like a good time to build a larger community of colleagues we could talk to in this regard, and we're thinking an event like this might help kick off the effort. PI has a large budget for visitors and conferences.

The question is who might we invite as speakers? I have a decently strong knowledge of classical pragmatism and thinkers—witness my home library on the subject in the attached photo—but I am not so familiar with the crowd of *living* (non-emeritus) pragmatists (and Bergsonians)—i.e., ones who might best contribute to a meeting a like this. Do you have any recommendations? Or, can you point me to someone who would be in a better position to make recommendations? ... keeping in mind that the main topic of the meeting will be what's wrong with the block universe conception and how one might formalize the idea of the opposite.

Thanks for any help you can give!

11-02-08 *Potential Anti-Block Universe Conference* (to D. M. Hester)

Sorry to continue bothering you with this, but you're my only connection to the world of William James. Let me expand on the question I sent you yesterday: Below is part of a conversation I had with Cheryl Misak yesterday. [See 10-02-08 notes "Invitation and Potential Anti-Block Universe Conference, 1 & 2" to C. Misak.] It says a little more about the cross-section I'm thinking of for the proposed meeting.

What are your thoughts on Tom Burke? I was favorably impressed by his book *Dewey's New Logic*. Might he fill the shoes of an expert on Dewey's metaphysics I mention below? Or are their better choices?

Micah's Reply

> Hacking and Fine are good choices. Putnam is too, but he is so busy, and his wife does not ambulate as well as she used to. You can try, but I suspect you will get a polite decline.
>
> Burke is good; Ray Boisvert at Sienna wrote a book on Dewey's metaphysics (though he hasn't written much on it since). John McDermott would have something to say, but I' don't know that he would be able to make given his schedule.
>
> What about someone like Christopher Hookway? Russel Goodman at New Mexico might be good as well.
>
> I'm just brainstorming ... as for James's metaphysics, Bill Gavin at Southern Maine might be a good choice, and Richard Gale (now in Knoxville) is always a kick.

11-02-08 *Anti-Block Meeting Invitees (working list)* (to L. Smolin)

Here's where I've gotten so far in my thinking about who might be invited to this meeting. I'll go ahead and send this malleable list even at this stage, so that we might have a starting point to discuss things tomorrow.

I'm open to loads of suggestions and dollops of advice. Please give. The point of the meeting, as I see it, is to get PI people to stop thinking in block-universe ways. If we can surpass that barrier, the thought is, our theory-making intuitions will be opened to grander vistas. But the first step is to get a glimpse of what's over the barrier, and that's what this meeting should be mostly about. We add a little talk on quantum mechanics (witness Bitbol, Fine, Shimony, and Price) to bring the focus back in—to give some sense that the issue is relevant to existing physics—but the bigger point is peering over the barrier, so that we can get to the next physics.

I hope we can get together tomorrow.

- Michel Bitbol — director CNRS, École Polytechnique, Paris. Philosopher of physics, organizer of recent international meeting "Pragmatism and Quantum Mechanics."

- Raymond Boisvert — Siena College. Author of *Dewey's Metaphysics*; see attached review. [See William M. Shea, "Review of *John Dewey's Theory of Art, Experience and Nature: The Horizons of Feeling* by Thomas M. Alexander and *Dewey's Metaphysics* by Raymond D. Boisvert," Am. J. Edu. **98**, 83–88 (1989).]

- Arthur Fine — University of Washington. Philosopher of science with a long history of influential research in quantum foundations; author of *The Shaky Game: Einstein Realism and Quantum Theory*; pragmatist credentials stated vividly by Richard Rorty, who called him "my favorite philosopher of science".

- Richard Gale — University of Pittsburgh. Expert on James's metaphysics. Vita found here: `http://www.pitt.edu/~rmgale/vita.htm`. See also attached review of one of his books. [See Gerald E. Myers, "Review of *The Divided Self of William James* by Richard M. Gale," Philosophy and Phenomenological Research **64**, 491–494 (2002).] He looks fantastic actually; comes recommended by a contact in the William James Society.

- Russell Goodman — University of New Mexico. Author of *Wittgenstein and William James*. Vita found here: `http://www.unm.edu/~rgoodman/Vitae.html`.

- Ian Hacking — University of Washington. Renowned philosopher of science, with a deep interest in the history of probability and chance. Author of 11 books. In a recent paper, "On Not Being a Pragmatist: Eight Reasons and a Cause," he tries to fend off his fellow philosophers who classify him as a pragmatist, but regardless of the reasons for them he has well explored a set of thoughts quite relevant to this meeting.

- Louis Menand — Harvard University. Author of the Pulitzer prize winning *The Metaphysical Club*—ostensibly a biography of Peirce, James, Dewey, and Oliver Wendell Holmes Jr., but one of the deepest and far ranging explorations of pragmatism that I have read.

- Cheryl Misak — University of Toronto. Influential scholar of Peircean pragmatism.

- Huw Price — Sydney University. Perhaps the most influential Australian philosopher of science, and our partner in the PIAF (PI–Australian Foundations) collaboration. Deeply pragmatist about methodology, philosophy of language, and the meaning of truth, he is a surprise in his ideas about time (on which he has published extensively): He may be the one pragmatist on earth who believes in the block universe picture!

- Hilary Putnam — Harvard University. The most famous pragmatist in this list without question! Deeply influential in all branches of philosophy, and a philosopher who has given a significant amount of thought to quantum mechanics.

- Abner Shimony — Boston University. His credentials in quantum mechanics are well known. But also he is a great admirer of Peirce's metaphysics.

- Roberto Unger —
- Another expert on William James's metaphysics —
- Expert on George Herbert Meade's metaphysics —
- Expert on Bergson's metaphysics —
- ??? Who else ???

12-02-08 *Conference, 2* (to A. Y. Khrennikov)

I apologize!! I long since forgot about this. Below are the few lines you requested:

> We will also encourage a discussion of the significance and meaning of Kochen-Specker style theorems and the issue of contextuality in quantum mechanics, as recently there has been a resurgence of interest in these issues in the quantum information and foundations communities. To this end, there will be a special session on the topic organized by C. A. Fuchs. Speakers will include J. Emerson, C. A. Fuchs, I. Bengtsson, J-Å. Larsson, J. A. Smolin, and others.

I'm so sorry, I was late on this. I hope you will still add it to you conference webpage.

12-02-08 *Anti-Block Meeting Invitees (working list), 2* (to L. Smolin)

Lee Smolinism 1: *These all sound like good people, but the problem is that this is sounding like a philosophy conference and the people whose minds you want to change are not likely to go to pure philosophy talks. You can go ahead anyway and make it as interesting as possible, and at least form a network of alliances of people and ideas. To that end I would broaden it in the direction of other sciences with analogous or homologous issues like biology and economics. Or you can focus it on the relevant physics issues concerned with time and invite people with the full spectrum of views from pro-blockers to believers in evolution of laws and make it a conference to discuss and argue differences of opinions.*

I recognize the truth in what you say. On the one hand, I worry that my working list is too overtly philosophical, but on the other hand, I worry about fostering once again the usual stalemate that one finds at foundational meetings. I guess I was imagining something different at this conference: One that would lay the groundwork for people not too familiar with the subject of a different worldview from the usual one presented in our physics training.

At some point, one just has to jump off the diving board, I think to myself. I'm reminded of the diary entry William James wrote when he was on the brink of suicide in 1870. [See 05-08-02 note "The Spirit that Breathes Life" to G. L. Comer.] At some point, one simply must *choose* to start physics afresh, to obdurately pursue the anti-block picture. It, like free will, is a *choice* one must make before one can have it.

I'm just coming in to the office now (once I get my car out of the snow). And I'm sure to be in a more sober-talking mood by then. We can hammer this out!

12-02-08 *Chipping at the Block, 2* (to J. A. Vaccaro)

Well, I'm very flattered that you'd honor me with your painting. I'll take it! Simply send it to my PI address below; it'll be better protected from the weather that way (rather than chancing the mail man leaving it on my porch in the snow). It'll look great near all my volumes on William James (the philosopher who invented the very phrase "block universe"):

> What does determinism profess? It professes that those parts of the universe already laid down absolutely appoint and decree what the other parts shall be. The future has no ambiguous possibilities bidden in its womb; the part we call the present is compatible with only one totality. Any other future complement than the one fixed from eternity is impossible. The whole is in each and every part, and welds it with the rest into an absolute unity, an iron block, in which there can be no equivocation or shadow of turning. ...
>
> Indeterminism, on the contrary, says that the parts have a certain amount of loose play on one another, so that the laying down of one of them does not necessarily determine what the others shall be. It admits that possibilities may be in excess of actualities, and that things not yet revealed to our knowledge may really in themselves be ambiguous. Of two alternative futures which we conceive, both may now be really possible; and the one become impossible only at the very moment when the other excludes it by becoming real itself. Indeterminism thus denies the world to be one unbending unit of fact. It says there is a certain ultimate pluralism in it; and, so saying, it corroborates our ordinary unsophisticated view of things.

He also invented the word "multiverse", though he used it in a very different sense than the Everettians do.

I keep my fingers crossed that the conference will materialize. The difficulty is that I want it to be particularly philosophical, and PI may not be the right venue for that. Despite Lee's warnings, I'm finding it difficult to give up on the idea. Thus I'm starting to half toy with the idea of seeing if a philosophy department might throw in with us. We'll see.

14-02-08 *What To Do About Lewis?* (to R. Schack)

Now, I've got a new urgency! Sorry to worry you. I just haven't been able to keep up with my obligations, in the face of all this damned construction around me. It was worse when I got back from Australia than when I left.

To your question: I'm not sure that apples and apples are being compared. The argument you gave me a few days ago, is essentially the one of Lewis's paper, if I understand correctly. And it's the one Wayne Myrvold has spit back at us on several occasions: That the hypothesis of a chance is a hypothesis like any other. Consequently, one can have a (subjective) "credence" about it and update accordingly in the light of new evidence.

Schackcosm 100: *What saves her from an infinite or finite regress is the fact that she is a good subjectivist Bayesian. Her belief is her belief and does not need to be justified. There is thus no need for her to give an unambiguous description of the experimental setup that she is repeating. Consequently, my whole anti-Everett argument falls apart.*

There is no need to give an unambiguous description of how to produce the chance if she is treating it like a hypothesis, of which she is gaining confidence by finding the truth values of various ancillary propositions (i.e., the outcomes of those experimental trials). This is no different from any (valid) conditioning. By finding the truth value of *this* proposition, I update my degree of belief for *that* proposition, but I am under no obligation to give an exact description of the origin of that proposition. I am under no obligation to further discuss the mechanics of what might physically make it true or false.

This is why I think you are comparing apples to oranges. Your earlier argument—the one you attempted at the Everett meeting—was an attempt to attack elsewhere. It was an attempt to attack the logical coherence of the "truthmaker" for the chance. ("Truthmaker" is actually a technical term in some nonpragmatist philosophies; see attached paper of Huw Price. See also attached plays-on-words by me; I found much in the Price article that would be of value to us.) The usual "truthmaker" for a Lewisian chance is "identical chance situation," whatever that might mean. Your attack was on the "What means identical?" part of it, and the argument went like this. To specify identical situations, one must supply a precise description of those situations. But it is not good enough to give a classical description for a preparation device. For, despite Bohr's exhortations, a classical description is not a precise description of a preparation. One would have to supply the machinist with more than classical drawings if one wants him to build the device one truly has in mind. And that more precise description would require quantum states. And so one has an infinite regress in journey to find a precise description of a preparation. That is to say, the only coherent truthmaker available for a chance is a quantum state, and the only coherent truthermaker for a quantum state is another, more all encompassing quantum state. And so on, and so on. So goes your argument.

But that has nothing to do with the Myrvoldian-style distraction expressed above, which is just a species of the original Lewis argument: That chances are something we can have subjective credences about. Your argument is not in contestation of that, but rather, "What would make a chance true in the first place?" I.e., even supposing Lewis and Myrvold are not flawed, what are these things we imagine we're building up credences for? Answering that question leads to the infinite regress you point out.

The only thing I see potentially as a flaw in the argument is the Everettian's move to simply stop the regress by declaring a quantum state for the universe. I think that might be partially why Simon Saunders is so bold as to say only the Everettian view solves all the deep issues in the interpretation of probability. He is implicitly asserting that probability will ultimately only make sense if chance makes sense.

But that said, you know my own opinion is that there is simply nothing at all in the Everettian view; it really is "sound and fury signifying nothing". They act as if it hangs together, but I don't even see it rise at all from the flat ground. Thus, when they declare, "See, there's no problem at all," they're not saying anything that's actually consistent.

Anyway, that's my thought. If you spot a flaw in it, let me know.

18-02-08 *Hirota Meeting Proposal* (to Perimeter Institute)

Conference Proposal: Osamu Hirota, between Distinguishability and Noncausality
Principle Organizers: Christopher A. Fuchs and Alexander Holevo

Synopsis

Osamu Hirota has been a key figure in bringing two generations of quantum information theorists together—old QI and new QI—who might not have otherwise been so easily aware of each other. His tireless conference organization, particularly the major semi-annual QCMC series, fundraising for visitor programs to Japan, and establishment of the QCMC awards for theorists and experimentalists in quantum information and computing, have been no small components in making this community what it is. Thus, in honor of his 60th birthday (an important birthday in Japan), we would like to hold a small conference at PI, the participants being some of his best friends. It so happens that those friends are some of the key figures in quantum information, and this is a very good excuse to attract them to PI to report on what they are thinking.

By old QI, we mean the first era of studying the classical-information carrying capacities of quantum channels and the distinguishability of quantum states in the early 1970s. The key papers during that time being written by Levitin, Holevo, Belavkin, Helstrom, Yuen, Kennedy, Lax, and others. By new QI, we mean everything after the discovery of quantum teleportation in 1993, the time when it was realized that the successful transmission of quantum states could be an end in itself. In the first days of the newer era, these were completely separate communities. But the international Quantum Communication, Measurement, and Computing conferences organized by Hirota saw an end to that. Perhaps a personal anecdote will help convey the importance of these meetings. In 1996, a topic of great interest with the newer generation was the classical-information carrying capacity of a set of nonorthogonal quantum states, for which the answer was not known. In fact, an amazing quantum effect—the superadditivity of capacities—had only recently started to be understood in this connection in that community. But at the 1996 QCMC meeting, to which Hirota had drawn Alexander Holevo to after his not being involved in QI issues for many years, it became clear that this "new" effect was in fact quite old. It was reported in a 1978 paper by Holevo, but the paper somehow fell off the map and was simply unknown to those of us in new QI. Soon after this meeting, progress was rapid, and we can thank it for bringing Holevo and several others back to the field. Stories like this abound and make Osamu Hirota's contribution to the quantum information community very honorable indeed.

Research Areas Benefited

A broad range of quantum information studies at PI should benefit from the meeting. The invited speakers—Charles Bennett, Peter Shor, Alexander Holevo, etc.—will virtually ensure that. But, the focus is likely to lean toward capacity and superadditivity issues in their various guises. There may also be discussion on the foundations of quantum information itself (particularly in D'Ariano, Ozawa, and Fuchs's talks). One long-standing open problem that many of the participants have contributed to is the issue of the minimum entropy-making inputs to tensor product quantum channels: That will most certainly be a hot topic of discussion. Several researchers at PI and IQC, I am quite sure, will be poised to build on what they learn from the talks and discussions.

Relevance and Timeliness of Event

The timeliness of the event is dictated by Hirota's 60th birthday, which is in June. The relevance is that their deep friendship with him will bring some of the world leaders in quantum information to PI to share their thoughts.

To emphasize the caliber of the invitees, we list their associations here in more detail than can be given in the space set aside for it on page 6. Charles Bennett—one of the very fathers of new-QI, having been a co-discoverer of several of the most important effects in the field: 1) quantum teleportation, 2) super-dense coding, 3) the B92 quantum crypto protocol, 4) entanglement distillation, 4) the entanglement assisted capacity, and the list goes on. John Smolin—also from IBM Research, seminal contributions in all aspects of quantum information theory, especially through the power of his numerical work (it would be hard to count the number of interesting quantum information items first found by his "minimizer", for instance the five-qubit code). Alexander Holevo, played a role much like Charles Bennett, but in old-QI. Jeffrey Shapiro is director of MIT's historical Research Laboratory of Electronics. Masanao Ozawa and Horace Yuen were the first to prove that the so-called "standard quantum limit" to measuring successive positions of a particle could be exceeded; since both have had long distinguished careers in many aspects from the very theoretical to the quantum optical sides of quantum information. Masahide Sasaki is the director a major quantum information laboratory in Japan. Peter W. Shor, founder of the quantum factoring algorithm and the first to discover the existence of quantum error correcting codes.

Desired Outcomes

A report of the state of the art in various quantum channel questions—from the IBM Research team (represented by Bennett and Smolin), the Pavia team (represented by D'Ariano), the efforts at MIT (represented by Shor and Shapiro), the Tohoku team (represented by Ozawa)—should be of benefit to all. A worthwhile goal would be to make good progress on one or more of the additivity issues.

19-02-08 *These Eyes* (to D. M. Appleby)

Applebyism 17: *It is true: I had forgotten about Hans, and yes I would really like to see him. As you say, my purview, like yours, is much much bigger than SICs.*

And I should keep you up to date on what I've been thinking: It is what I think is maybe THE ultimate reason for our studying SICs. I now have a good sense in which a SIC representation of quantum states gives the very best unentangling possible between Bayesian probability theory and what is different in quantum mechanics. It is clean and simple, and had been right in front of me for over a year. Below is a weak summary of it. [See 25-01-08 note to D. Gottesman titled, "Bristol, 3 AM."] Sorry that it's not more detailed, and I don't want to take the time to write it more properly now (showing the sense in which this is the best possible disentanglement, etc.). But in my mind, this (and also the similar representation of unitarity) are now the real reasons I use for justifying the study of SICs.

At the PIAF lecture in Sydney, I put all this in Dutch book terms, but unfortunately there's only an audio file of the lecture here: `http://idisk.mac.com/centre.for.time-Public?view=web`. I think one really needs visuals. We can talk in detail when you get here.

There's all kind of poetry and prose that little equation evokes in me:

C: "World, I need your help factoring this large number."

W: "Fine. Push on me the right way, and I'll cough up the answer for you."

C: "But be warned, I'm in a hurry."

W: "Then you too be warned. Be gentle, don't push on me more than you have to; I'll give you a reward in the end."

19-02-08 *These Eyes, 2* (to D. M. Appleby)

Applebyism 18 : *Also, am I right to assume that the reduction in entropy (if that is the measure of "mixedness" you are using?) as compared with what you would have classically is greatest possible for a SIC?*

No, it's that the quantum "residue"—the last remaining hint of quantum mechanics, thought of as a deviation from the law of total probability—is the smallest. I.e., when using a SIC in the Bureau, one gets an expression for the j measurement in terms of the i measurement that is as close in form as possible to the law of total probability. There is still a deviation from it, but that deviation is functionally as small as possible—and that I presume in some shape, form, or fashion, represents the core of quantum mechanics. It is a hidden expression, I think, of the source of the Kochen-Specker theorem: unperformed measurements have no results.

Applebyism 19 : *I find this interesting for another reason as it touches on a question I discussed with Lane, possibly in a note I forgot to copy to you. We were speculating whether SICs are really and genuinely what they superficially seem to be: namely, the maximally symmetric rank d^2 POVM. The reason I thought they might not be is the triple products, which seem to be quite nasty for $d > 3$. I was wondering if there might be a POVM in which, the squared moduli of the overlaps are not all the same, but in which the triple products are nicer. Perhaps even a POVM in which the triple products are all the same? If it is true (as I think it might be) that the triple products are more fundamental than the overlaps such a POVM might have a better title to the term "symmetric" than SIC. At any rate it might have an equally good title.*

I find that question interesting too. But for the present problem, it is the symmetric measurements in our usual sense (of any rank)—rather than the tri-SIC measurements if they exist—that touch closest to the Bayesian representation. I show why in the colloquium I gave here: `http://pirsa.org/08010004/`. It's only the symmetric measurements (of any rank) that give a quasi-diagonal transformation between the P and Q functions, so to speak. And then, the rank-1 symmetrics further minimize the constants as much as possible.

So, the most important layer for the questions that titillate me—the ones about this issue of total probability—is that we first satisfy SICness completely. Thereafter, we try to satisfy tri-SICness as close as possible in one or another senses.

It would have been lovely if we could have had them both at once, SICness and tri-SICness! But there's certainly something to be learned here by finding that our easiest fantasies are being blocked by something.

19-02-08 *Speaking of "Blocked by Something"* (to D. M. Appleby)

Here's a lovely quote I ran across this weekend in William James's 1904 review of F. C. S. Schiller's book *Humanism*. (Humanism was Schiller's word for pragmatism.)

> But humanistic empiricism will have many other steps forward to make before it conquers all antagonisms. Grant, for example, that our human subjectivity determines *what* we shall say things are; grant that it gives the "predicates" to all the "subjects" of our conversation. Still the fact remains that some subjects are there for us to talk about, and others not there; and the farther fact that, in spite of so many different ways in which we may perform the talking, there still is a grain in the subjects which we can't well go against, a cleavage-structure which resists certain of our predicates and makes others slide in more easily. Does not this stubborn *that* of some things and not of others; does not this imperfect plasticity of them to our conceptual manipulation, oppose a positive limit to the sphere of influence of humanistic explanations? Does not the fact that so many of our thoughts are retroactive in their application point to a similar limit? "Radium," for example; humanistically, both the *that* and the *what* of it are creations of yesterday. But we believe that ultra-humanistically they existed ages before their gifted discoverers were born. In what shape? There's the rub! for we have no non-humanistic categories to think in. But the *that* of things, and their affinity with some of our *whats* and not with others, and the retroactive force of our conceptions, are so many problems for Humanism over which the battle is sure to rage for a long time to come.

19-02-08 *Octahedra in Higher D* (to S. T. Flammia & D. Gottesman)

Are there any standard names for higher dimensional analogs of regular octahedra? Analogs in any sense, I suppose. But particularly, polytopes with $d(d+1)$ vertices whose ambient space is a d^2 dimensional linear vector space? Does that kind of object have a name?

19-02-08 *Octahedra in Higher D, 2* (to S. T. Flammia & D. Gottesman)

Flammia-ism 1: *I don't know of any such object. The only generalization of the octahedron that I know of is called the cross polytope, and it is simply the polytope of the vectors having unit 1-norm. If the ambient space has dimension d, then it has $2d$ vertices, so it isn't what you are looking for.*

Well, it might be, and I'm just asking the question in a confused way. What I'm really asking is this. Take a complete set of MUBs and connect the projectors to make a polytope. Does that kind of polytope have a name? Bengtsson and Ericsson called it "the complementarity polytope" when they discussed it in their paper, but I was wondering whether there is a more common name for the structure, if one strips away considerations about it being embedded in the set of quantum states.

19-02-08 *Lunch at 12:10?* (to S. T. Flammia)

Flammia-ism 2: *I'm at UNM until Friday, then I'm going on the ski trip Monday. I'll be in next Tuesday, the 25th.*

OK, I'll see you next Tuesday. You're probably going to wince when you see this, but here's the conference proposal I ended up submitting for us. Lucien got a chuckle from my verbal report of it, and told me he'd let me know before it goes to committee if there's anything too outrageous in it.

Conference or Workshop Program Proposal

Name of Conference or Workshop:
Seeking SICs: An Intense Workshop on Quantum Frames and Designs at PI

Principal Organizers:
Steven T. Flammia and Christopher A. Fuchs

Topic and Research Area:
Quantum Foundations, Quantum Information Theory

Synopsis (Please make this accessible to non-experts. See sample proposal.)
What is the shape of Hilbert space? More precisely, what is the geometry of the set of quantum states, thinking here of quantum states as a subset of the Hermitian operators? What symmetries does this set have? This question is of deep importance to several issues in quantum information theory, and it is of deep importance to a surging school of thought in quantum foundations, the quantum Bayesian approach. We propose an intense workshop to settle, or at least make significant progress with regard to a seemingly simple, but extremely recalcitrant, version of this question: The question of the existence of minimal symmetric informationally-complete (SIC) sets of pure quantum states.

The question is simply this: Take the set of pure quantum states (one-dimensionsal projection operators) for a d-level system; these operators span the d^2 dimensional space of Hermitian operators. Can one create a regular simplex (the higher dimensional analog of a regular tetrahedron) of d^2 vertices with elements drawn from this set? This is seemingly an almost trivial question—one of the most basic questions one can ask of a convex set—but it has a long unsolved history, ranging back, in one guise or another, at least 35 years. A related question, and a kind of scratchpad for the more pressing problem, is the question of existence of "complete sets of mutually unbiased bases (MUBs)"—a kind of higher dimensional analog of regular octahedra—in composite dimensions. In all, these are examples of so-called "2-designs" and "tight frames" with very specialized properties, hence the subtitle of our workshop proposal.

SICs and MUBs, if and when they exist, are already known to have some remarkable properties, both for quantum information processing and quantum foundations. For instance, thinking of them as measurements that can be performed in the laboratory, they are known to be optimal for certain ways of quantifying efficiency in quantum tomography. And SICs in particular may be used to form "maximally sensitive" alphabets for eavesdropping detection in quantum cryptography. For foundational purposes, SICs, if and when they exist, satisfy a sense of being as close to an orthonormal basis in the space of density operators as that structure will allow, and give rise to phase-space representations of quantum states and unitary evolution that are as simple as they are allowed to be in such representations. Furthermore, both SICs and MUBs may provide deeper insight into and new techniques in quantum computation, as both structures appear to have very interesting connections to the Clifford group. Thus there is ample (and ever growing) reason for trying to settle this question of existence.

The idea of the meeting is to gather the best people in quantum information theory who have given this problem significant thought, put them all in one place, and see progress made, damned be, by any means! We'll start off with a fiery rendition of Henry V's St. Crispin's day speech:

> This story shall the good man teach his son;
> And Crispin Crispian shall ne'er go by,
> From this day to the ending of the world,
> But we in it shall be remembered—
> We few, we happy few, we band of brothers;
> For he to-day that sheds his blood with me
> Shall be my brother; be he ne'er so vile,
> This day shall gentle his condition;
> And gentlemen in England now-a-bed
> Shall think themselves accurs'd they were not here,
> And hold their manhoods cheap whiles any speaks
> That fought with us upon Saint Crispin's day.

roll up our sleeves, brace for the scars, and get to work. Each day (save the first) will consist of two morning talks, each on some aspect of the problem, with the afternoons reserved for round-table/chalkboard working sessions in the Alice Room. The simple idea is to run the troops into the breach opened by these last few years of research, and finally defeat this cantankerous problem!

What research areas at Perimeter Institute will benefit from the event and how? How does this event benefit your own research?

Both Quantum Information and Quantum Foundations research at PI will benefit for the reasons described in the synopsis. In the case of Flammia, the problem has been a significant part of his research in the last few years. In the case of Fuchs, it's now a religious quest—being a significant ingredient in his sought-for interpretation of quantum mechanics.

Please include a justification of the relevance and timeliness of the event (feel free to explain in technical terms).

The earliest known posing of the SIC problem was in 1973 by Lemmens and Siedel in the context of the maximal number of "equiangular lines" that can be supported by a vector space. In real vector spaces, an upper bound of $d(d+1)/2$ on the answer could be proved, and in complex spaces a similar bound of d^2 could be similarly proven. Early on, however, it was understood that the upper bound generally could not be achieved in the real case; in fact, the actual maximal number turned out to be a very complicated function of d. Because of this, perhaps, it was thought to be similarly so in the complex case, and interest in the problem languished—until 1999 it was only known that the complex-case bound could be achieved in dimensions 2, 3, and 8. With the study of Renes et al. in 2003, however, all that changed: Numerical work now indicates that SICs exist in dimensions 2 through 47, and analytic constructions (most via the help of computer algebra packages) exist in dimensions 2 through 14. Thus progress has been very rapid. At least 46 papers on the `quant-ph` archive cite the original Renes et al. paper, and something like 10 of those have been devoted in an substantial way to the existence problem. Of the related problem with MUBs, over 100 papers on the archive have the words "mutually unbiased bases" somewhere in the record.

Here at PI, at least three residents have devoted a significant amount of brain pulp to the SIC existence problem (Blume-Kohout, Flammia, and Fuchs) in the last couple years, with brief forays by some others (Gottesman, Hardy). Also we can list several visitors of PI that have given a significant amount of time to the problem (months to years in some cases): D. M. Appleby, L. Hughston, J.-Å. Larsson, H. Barnum, C. M. Caves, M. Roetteler, A. J. Scott, and W. K. Wootters. The point is the problem has interested many who have walked in these corridors. It is simply time to settle it if at all possible.

What are the goals and desired outcomes of the conference or workshop?

To amass as much new information about SICs and MUBs as possible, and possibly to prove existence of SICs in all finite dimensions (or at least an infinite class of dimensions).

For the invited participants and speakers please include their full name and affiliation.

D. Marcus Appleby	Queen Mary University of London
Howard Barnum	Los Alamos National Laboratory
Ingemar Bengtsson	Stockholm University
Robert Calderbank	Princeton University
Carlton M. Caves	University of New Mexico
Steven T. Flammia	PI
Christopher A. Fuchs	PI
Markus Grassl	University of Innsbruck
David Gross	Imperial College
Lane Hughston	King's College London
Andreas Klappenecker	Texas A&M University
Martin Roetteler	NEC Laboratories
Andrew J. Scott	Griffith University
Peter W. Shor	MIT
Neil J. A. Sloane	AT&T Research
William K. Wootters	Williams College

20-02-08 *Your 60th Birthday* (to O. Hirota)

Alexander Holevo made me aware that your 60th birthday will happen soon. It is in June, if I am not mistaken? Thus, in your honor, we would like to organize a small "birthday party" for you here at the Perimeter Institute.

In that regard, we have just written a detailed conference proposal and submitted it. It is titled, "Osamu Hirota, between Distinguishability and Noncausality." If it is accepted by the conference committee at PI, then we would be very much honored by your attending the meeting. I should know shortly whether we will get the funding.

Under the assumption that it will be approved, let me tell you a little bit about the proposal. Preliminarily, I have listed the following colleagues as invited speakers:

Charles Bennett
Mauro D'Ariano
Christopher Fuchs
Osamu Hirota
Alexander Holevo
Debbie Leung

Masanao Ozawa
Masahide Sasaki
Jeffrey Shapiro
Peter Shor
John Smolin
Horace Yuen

We probably have room for 1 or 2 more invited speakers if you would like to add anyone to the list. Perhaps further colleagues or important students from Japan? Or others that we have forgotten?

If the proposal is approved, we should be able to cover everyone's travel and local expenses, and we would also be able to have a nice banquet for you here at PI's Black Hole Bistro.

An important question for you to be thinking about, is what dates you would like it. The meeting should be 3 days long, but when? If you approve, please also send me a range of dates for which it would be possible. Summer is a very busy time for the facilities here at PI, so I may have to fight for an empty slot in the planning. Thus, the more possible dates you have the better.

I hope you will accept our proposed birthday gift for you! You have been a wonderful friend over the years.

25-02-08 *Newsletter Submission* (to E. Goheen)

Goheenism 1: *We would like to prepare an article on the recent PIAF workshop and what this new partnership/project means for PI for the March edition of our newsletter, Inside the Perimeter. Lucien suggested I might be able to ask you a few questions about it.*

Here are my answers finally. I'm sorry for the delay.

Goheenism 2: *What is the partnership, who is involved?*

It's called PIAF (and pronounced as the name of the great French chanteuse Edith Piaf) but stands for the PI-Australian Foundations partnership. The participants on the Australian side are Sydney University, the University of Queensland, and Griffith University, with principle investigators Huw Price and Stephen Bartlett at Sydney, Gerard Milburn at Queensland, and Howard Wiseman at Griffith. The partnership will fund three postdoctoral positions based in Australia, all with significant visiting periods at PI. Thus, between faculty, existing postdocs, new postdocs, and graduate students, there's probably on the order of 15-20 people involved.

Goheenism 3: *How did this project come to be?*

It was the brain-child of Howard Burton, really. Howard recognized that the PI quantum foundations effort cannot live in a vacuum if it is going to achieve its potential. This is because quantum foundations is unique among the research areas pursued by PI in that, outside our walls, there simply isn't much of an infrastructure for the field. This contrasts with string theory, for instance, where there are several internationally recognized research centers for it (PI, Princeton, Stanford, Caltech, etc.). Thus a flow can be established for incoming and outgoing postdocs at those institutions. But quantum foundations is isolated in comparison—PI might just be the end of the career for many a good quantum foundations researcher. Thus, if we really want the field to flourish, part of our effort should be of the outreach variety, making physics department around the world aware of the return one can get from quantum foundations research, and to convince them it's worth investing some of their own resources to this field. PIAF represents our first effort in that direction.

Goheenism 4: *How will PI benefit from these types of partnerships?*

The benefits should be manifold, and not only for the reasons already mentioned. PIAF also establishes a vigorous visitor program between our institutions, and we can all imagine the benefits of that. Sometimes a calculation long processed in the back of one's mind simply needs a jolt of crisp, wintery Ontario air to bring it to life. Sometimes too a heated mid-Winter (i.e., mid-Summer!) discussion at a sidewalk cafe in Sydney might be just the thing to thaw loose a key idea previously frozen away. Thus we expect many good things to come from this collaboration. Finally, PIAF intends to establish the premier annual international meeting in our subject. In general relativity, there are the famous Marcel Grossman meetings; in quantum information, there were the famed Torino meetings; in quantum foundations, there will be PIAF!

Goheenism 5: *How did the first workshop go? Are there more planned in the future?*

It was fantastic, far exceeding expectations. The original intent for this meeting was for it to be a kind of "kick-off meeting", establishing what the various researchers would like to get out of this collaboration. In the end, it somewhat-spontaneously turned into a full-fledged research conference with about 35 participants (17 speakers), with a lively round-table discussion one session. Our next meeting will be here at PI and will be the first of the international series described above. This year's theme will be, "Time and Quantum Foundations." But the theme will change yearly, so stay tuned! Particularly, as it has been noticed that Hawaii lies exactly halfway between Waterloo and Sydney on the geodesic ...

I hope these were the sorts of answers you were looking for.

26-02-08 *Great News* (to A. Wilce)

That's great news indeed about your tenure! Well deserved and all that old boy, harrumph. ... But, for a serious note: You are now in a position of security, and that is a call to do something great with your life. Don't pass up the opportunity. If you were hammering away at quantum mechanics already, it's time now to pull out the jackhammers. We've got to understand this damned theory! We've got to understand it by any means!

So, indeed, very great news from you! Give my regards to your wife.

I'm soon off for a whirlwind tour of Eugene, Oregon. One day flying there, and one day flying back, all for the opportunity of spreading the word of SICs for one day in the middle.

28-02-08 *Even a Title* (to D. Gottesman)

And here's the title I'll give the talk, whenever I do give it: "Dressing Richard Feynman in Bayesian Clothes". It just came to me. Despite the title, it'll be a chalk board talk with plenty of equations.

29-02-08 *Seattle Note* (to H. B. Dang)

One of our postdoc candidates (Piero Mana) impressed me very much in many ways during his visit here. Particularly, I learned that he carried a sheet of paper in his pocket marked off with 365

$\times$ 75 squares — each square was to represent a day in his expected lifespan. He told me he kept it with him to remind him of the urgency of life and so that he would not forget to prioritize.

29-02-08 *Past the Halfway Mark* (to R. Schack)

Schackcosm 101: *Exchangeability is not a property of the world, but it depends on the agent's prior judgement.*

I would say "rather," rather than "but".

Schackcosm 102: *The concept of chance is therefore simply redundant in a classical deterministic theory.*

That's not quite what you want to say. You've already pointed out how the idea of chance in a deterministic theory is simply inconsistent. "Unneeded" perhaps, rather than "redundant".

Schackcosm 103: *It turns out that the attempt to define such a preparation procedure leads to the same regress as in the classical case. The state prepared by any preparation device depends on the quantum state of the preparation device itself. To guarantee that the same system state is prepared in each trial, one has to make assumptions about the quantum state of the preparation device, which means that one has simply moved the problem up one level. This leads to a regress as claimed.*

Why are you always so terse? This is a significant point of the paper. It seems to me that you could expand it to the level of our verbal discussions.

Schackcosm 104: *From the perspective of a full quantum Bayesian theory the answer is that objective quantum states, and therefore the objective wavefunction of the universe, have no useful place in quantum theory.*

And in fact cause great trouble.

29-02-08 *Readers' Rights* (to R. Schack)

Schackcosm 105: *Instead of two fundamental concepts—objective quantum states and decision-theoretic preferences—and their awkward connection via the principal principle, we now have a single fundamental concept. This translates into much greater conceptual and mathematical simplicity.*

I think it would be within the reader's rights to ask: Now why, really, is that an awkward connection? It still looks very natural to me. How have you convinced me that it's awkward? So much of what you think and feel is left silent in your papers. Is it really that I'm carried away by your oratorical skills in our personal conversations, and when it all boils down, the content is what you have recorded on these pages?

Schackcosm 106: *In the quantum Bayesian approach, the prior is a single density operator on $\mathcal{H}_n$. In the many-worlds approach, this prior is a probability distribution over all density operators on $\mathcal{H}_n$, a much more complex mathematical object*

I like something of the flavor of this, but it is the same mathematical object (the representation theorem gives the isomorphism). What are you really trying to say when say that it's more complex?

I doubt I was very helpful but those are the most thoughts I've been able to muster this morning. (I'm in Seattle now; just finished with a long talk with a soldier going back to Iraq. I would love to see George Bush tarred and feathered.)

Send me the next iteration of the paper; I'll try to think a little more deeply once I'm back in Ontario.

03-03-08 *t.tex* (to R. Schack)

Your use of "t" for the names of almost all files is as mysterious to me as the word "magma" for the most all inclusive algebraic structure in Bourbaki. (Any idea why they use that name?)

I read the appropriate areas; sorry for the delay. It's better now. Send it off—not much more to do. But I do continue to fear that the regress argument will still not be accepted by most. Why *must* one assign a quantum state to a preparation device? At that point it loses its role as a preparation device, a Bohrian would say. But this paper is not written for the Bohrians, I suppose.

08-03-08 *Chipping at the Block, 3* (to J. A. Vaccaro)

Dear JV, (just looking at your art page again, where I see you call yourself that),

I don't know how I can thank you enough. The piece is just gorgeous, and as I said before I'm very flattered that you'd let me have it. It'll hold a place of honor in my library.

Things are starting to look good for the conference too. John Sipe contributed a crucial idea that I think will make it start to look favorable in the eyes of PI, and Lee Smolin now seems to be pretty on board after hearing that. So, soon, I'll put a formal proposal in with PI. The idea is to have the philosophers read formal papers on some issue exposing cracks in the block universe conception, and then have physicists reply. The papers would be prepared well in advance of the meeting, so that the chosen physicists would have to time to develop a relevant response, explaining how something the philosopher said might be relevant (or not) with respect to a given issue, or rather arguing that the philosopher is only spouting some antiquated 19th century idea that has since been made very unlikely by modern physics, etc. If at all possible, I'll get your painting into the conference poster.

I'll write again when the painting arrives.

10-03-08 *Stabilizer States* (to M. G. Raymer)

My, it takes me a long time to reply to emails, doesn't it?! I'm sorry. But thanks for the hospitality in Oregon; I really enjoyed the trip. I was also impressed by your sensitivity to "pragmatic" issues. Attached is a picture of the Waterloo Library of Pragmatism, aka Chris's study—you'll see I'm quite serious!

Thanks also for the note. It sounds exciting! Is it true? Like Caroline Thomson said of me, I really do not have an intuition. Honestly, I didn't understand why you say "the answer is independent of the angle." Have you tried passing it by the van Enk test? If it gets that far, then it'll probably be a hop skip and a jump from a formal proof, and I'll try to weigh in on that. In the meantime, I have learned of a result saying that in finite dimensions, there are no SIC fiducial states with equal amplitudes, nor even such states with periodic amplitudes. So, if your ansatz does work out in the infinite limit, that will be quite interesting. For it would mean the infinite limit comes about in a nontrivial way.

10-03-08 *That Paper* (to M. G. Raymer)

Also here's the paper I was telling you about on the eight-port detector being able to measure all Weyl-Heisenberg covariant POVMs (making use of an appropriate "parameter" state in one port): `http://lanl.arxiv.org/abs/0708.4094`.

12-03-08 *Reality Check* (to A. Kent)

Kentism 8: *I was really calling just to be quite sure we've reached closure on the possibility of you contributing to the many worlds book.*

I held off on replying to you because I had hoped there was some chance I'd pull through and have something relevant written by now. But reality rears its face again. I think, honestly, there's not a chance I'd have something finished by "end of March (absolutely final firm deadline)". So, thanks for encouraging me, but I will disappoint you again.

In the end, these foundational wars—weighing one stale point of view against another stale point of view—just get old. What new physics comes from the effort? What new vista opens before our eyes? A complete rewrite of quantum mechanics is called for, and none of those guys see it. Nor will they ever. So, I think it is better for me to keep going on my own way.

17-03-08 *0803.1264* (to C. H. Bennett, G. Brassard, and J. A. Smolin)

Bennettism 32: *This looks like the kind of thing you (and CBH) were looking for. Do you understand what their convex framework of theories is, and how they define entanglement within it without automatically limiting the theory to standard quantum theory? Also, is their convex framework broad enough to encompass theories like Smolin's Pangloss universe, or Intelligent Design?*

To answer Charlie's question, the convex framework certainly encompasses a Pangloss universe (even if it's not exactly John's version), simply because it contains quantum mechanics. And the quantum world—we're bound to eventually learn—is surely the best of all possible worlds.

18-03-08 *Application* (to H. B. Dang)

I am presently in the Austin airport, delayed—perhaps indefinitely—by thunderstorms. Makes for a very miserable day. But Appleby will arrive at PI Sunday for a week, and then he's coming again for two months in the summer. So, I have something to look forward to.

Dangism 3: *I guess what I'm still missing is how probabilistic quantum mechanics can result in a more intuitive formulation.*

Intuitive is not necessarily the watchword. Reread my `quant-ph/0205039`. I just want to get at the core of what QM is telling us. And that core, by conventional standards, might appear to be the conjunction of two or three *seemingly* contradictory statements (like, for instance, is the case with special relativity). The point of a probabilistic representation is that it most directly gets at the (correct) meaning of the quantum state and the correct meaning of the Born rule. With the underbrush cleared away, then one should be able to more easily see the true forest.

18-03-08 *Needing a Little Jung Myself* (to D. M. Appleby)

I should record this snippet of a dream while I happen to be thinking of it again. (Emma and I were just talking about the importance of dreams.) I remember little of last night's dream except that, in it, I thought I was giving a particularly eloquent exposition of what I was thinking about quantum mechanics. And included in that exposition was a phrase that went like this: "The structures that *act* on Hilbert space—density operators, unitaries, and such—are the ones that represent the epistemic. The things that *live* in Hilbert space are the ones that represent the ontic."

I have no idea what that means, particularly as I cannot think of anything that "lives in Hilbert space". Clearly I'm making a distinction between $\mathcal{H}$ and $B(\mathcal{H})$, and saying the ontic/epistemic cut resides there. But what "lives" in $\mathcal{H}$?

By the way, our flights were indeed canceled. We are having to stay in Austin tonight. I've been amused by your latest notes on $\sqrt{3}$ and such.

19-03-08 *More Spam* (to D. M. Appleby)

Just reading the Barack Obama's speech from today before knocking off to bed. Came across these lines, which I like:

> Understanding this reality requires a reminder of how we arrived at this point. As William Faulkner once wrote, "The past isn't dead and buried. In fact, it isn't even past."

... particularly if you read them out of context and think about QM.

20-03-08 *Jungian Analysis* (to D. M. Appleby)

Applebyism 20: *I have managed to find a few more minutes. Your dream is interesting because it seems on the face of it to represent almost the opposite of what you consciously think. At least it does if one reads the word "live" the way I would naturally read it. As I would understand it the only things that "live" in Hilbert space are vectors. But in your waking life you would never say that the state vector has ontic significance (curiously, though, the density matrix is said to have epistemic significance: so to that extent the dream seems to agree with what you consciously think).*

Yeah, that's exactly what troubles me. What beside vectors "lives" in Hilbert space? I thought to myself, "Well, does dimensionality 'live' in the space?" If so, that seems like a forced usage of the word. On the other hand, for instance, one might say that maximal sets of equiangular lines do live in the space. Is that part of what the dream was trying to bring to the surface? This much is for sure: I have long (many years) puzzled over why quantum mechanics seems to be built on a two layer structure: the states (density operators) are not just in a vector space, but in a vector space $B(\mathcal{H})$. ($\mathcal{H}$ being the bottom layer, and $B(\mathcal{H})$ floating above it.) What is the significance of that very basic statement? My interpretation of the dream is that it was aiming at an answer to that question. And if so, there may be a little progress here. For, I had never thought of the ontic/epistemic cut as perhaps having anything to do with the $\mathcal{H}\,/\,B(\mathcal{H})$ cut before. But now I find myself taking this as a direction worth exploring.

20-03-08 *Group Meeting* (to D. Gottesman)

I should give something of an abstract since my title is so uninformative. It is below:

> In this talk, I'll add some details on why I'm so interested in the existence of SIC (symmetric informationally complete) sets of quantum states. Some *leisure* readings relevant to the talk, in case you're interested, are this old paper by Feynman,
>
> `http://projecteuclid.org/euclid.bsmsp/1200500252`.
>
> Also Chapter 1 of Volume III in his *Lectures on Physics*, and finally, his original article on quantum computing, Int. J. Theor. Phys. **21** (1982), page 467.

24-03-08 *Those Pages* (to H. C. von Baeyer)

Could you send me an electronic version of the translation you made of the Pauli lecture? I.e., the one you gave me a paper copy of when we first spoke yesterday. I had only read half of that this morning, but I forgot to put it in my backpack so I could read the rest of it tonight.

See you tomorrow at about 11:00. I'm really enjoying your being around here; I'm already hoping we can lure you back.

Hans's Reply, 24-03-08

> Chris, "those pages" sounds a bit scurrilous, like "that woman", but here they are.
>
> I'm having a great time. My conversations, including dinners with Marcus, are helping me to formulate questions and projects. I'm thinking very hard!
>
> > **Wolfgang Pauli: The influence of archetypal concepts on the formation of Kepler's scientific theories.**
> >
> > (In *Wolfgang Pauli: Das Gewissen der Physik*, Charles P. Enz and Karl von Meyenn, eds., Vieweg 1988, p. 509)
> >
> > *[Pauli reports in a passive voice on a lecture he delivered to the Psychology Club of Zurich in 1947/48. He poses the question: What is the bridge between a scientist's intuition and the development of scientific concepts and theories? He uses Kepler as an example. He cites Plato's notion that the feeling of satisfaction evoked by the understanding of nature stems from the "coming into congruence" of pre-existing, innate images in the human psyche with the observation of external objects and their behavior. Kepler calls such images archetypal. This notion agrees largely with Jung's introduction of the same word into psychology. Archetypes could also be called symbols, and furnish the sought-after bridge.*
> >
> > *Pauli analyzes Kepler's work, starting with the identification of the three-dimensional sphere as symbol of the trinity of the Christian deity and of the Sun as symbol of God the father. Thus the insistence on the Sun as the*

center of the universe turns out to be an expression of religious fervor. Pauli emphasizes that there is no hint of quaternity in Kepler, and relates this omission to the absence of the concept of time from most of his system.

Kepler's work was attacked vehemently by the respected Oxford physician and Rosicrucian Robert Fludd, who tried to restore the quaternity. For Pauli this is a symbol of the completeness of experience, which includes emotional components in addition to material ones, and is thus superior to the scientific point of view. What follows is my translation of the three concluding paragraphs of Pauli's essay.]

Finally there is the attempt to connect this problem originating in the 17th century with today's commonly expressed wish for a more unified *Weltbild* or world picture. First it is suggested that the significance of the pre-scientific stage of thinking for the development of scientific understanding should be accounted for. This step can be accomplished by complementing the study of scientific insights on the external world by the study of the inner meaning of these insights. While the former aims to adapt our understanding to external objects, the latter should illuminate the archetypal images used in the development of scientific concepts. Complete understanding would seem to be achieved only by combining these two directions of investigation.

Secondly, it is pointed out that modern microphysics has led to the result that today we have natural sciences, but no longer a scientific world picture. This could be alleviated by progress toward a unified total world picture in which the sciences are only a part. Indeed, modern quantum physics has begun to return toward the quaternary point of view that opposed the nascent science of the 17th century by treating the role of the observer in a more satisfactory manner than classical physics. In contrast to the "detached observer" of the latter, the former postulates an uncontrollable interaction between observer or apparatus and the observed system in the course of every measurement. In this way the deterministic description of phenomena becomes impossible. According to modern physics an observation, which makes specific choices and interrupts the game that is proceeding according to predetermined rules, is in essence not an automatic process, and can be compared to a creation in the microcosm, or with a transformation with unpredictable outcome.

The reaction of the discovery on the discoverer [or of observation on the observer][134], which leads to transformative religious experiences, and for which alchemy as well as the heliocentric idea furnish useful examples, transcends science. It can only be captured by symbols which simultaneously express the emotional side of experiences through images and furnish a vivid relationship to all human knowledge and to the actual process of discovery. Precisely be-

[134]The German words are *Erkenntnis auf den Erkennenden.* (I trying to translate them, I found out that currently there is no online etymological dictionary in German.) Ordinary dictionaries yield *Erkenntnis* = knowledge, perception, recognition, realization, insight, discovery and cognition. A hint comes from *Erkenntnistheorie* which is simply epistemology or theory of cognition.

In ordinary conversation, *erkennen* is to recognize something or someone, with an overtone of recovering something you knew beforehand (Plato). But I think that element is not dominant here. Since Pauli writes in the preceding about *Beobachtung* and *Beobachter*, which are simply observation and observer, I put those in square brackets. Intuitively I still go with discovery.

cause the possibility of such symbolism has become estranged in our time, it might be of interest to go back to another time which did not yet know about the mechanics we now call classical, but which enables us to provide proof of the existence of symbols with simultaneous religious and scientific functions.

[Finally I translate the epigraph attached to this piece by the editors of the memorial volume. It is from a letter written three years after the lecture and exemplifies the unique role of Pauli's correspondence with Markus Fierz.]

"I came upon Kepler as a trinitarian and Fludd as a quaternarian, and felt a resonance between their polemic and my own inner conflict. I have certain traits of both, but now, in the second half of my life, I should switch over to a quaternary attitude. The problem is that the positive value of the trinitarian attitude may not be sacrificed in this move ...

By the way, I would like to remark that back in Hamburg my journey to the exclusion principle had to do with just this difficult switch from 3 to 4: namely the necessity of ascribing to the electron, besides its three translations, another, fourth degree of freedom. To struggle through to the understanding that contrary to the naïve attitude a fourth quantum number is a property of the same electron — this was actually the principal labor ..."

[Fierz replied modestly that he himself was not as far along in his journey from three to four, that he didn't know whether he was a trinitarian or a quaternarian, and that he might never know. But by this very admission Fierz reveals himself as the ideal mediator between the divine Pauli and the rest of us.]

25-03-08 *Those Pages, 2* (to H. C. von Baeyer)

> But I want to say one thing to the quantum information community. I want you to listen to me; I'm going to say this again. [finger wagging] I did not have intellectual relations with those pages [pause] Mr. Pauli's. I never told anyone to venture into alchemy, not a single time, never. These allegations are false, and I need to go back to work for the quantum information community ...

Thanks, I got them last night and finally read them. There is some good stuff there. I'm particularly intrigued by his remarks (of a flavor I have seen before, but this brings it up again) that, "The reaction of the discovery on the discoverer [or of observation on the observer], which leads to transformative religious experiences, and for which alchemy as well as the heliocentric idea furnish useful examples, transcends science." The reaction of the discovery on the discoverer? The Bayesian would say, yeah, that's just the transformation of one's subjective beliefs from $P(h)$ to $P(h|d)$. But he means something more or something else than that.

On my end, I became very frustrated looking for the originals of the Bohr-Pauli letters. They should have been in my files under B, P, or F (for Folse, who gave them to me). But they're simply missing—very unusual for me.

25-03-08 *Funny Thing I Found on the Web* (to H. C. von Baeyer)

While trying to find Henry Folse's website (so I might recover my missing Bohr-Pauli correspondence), I came across the following article by Robert Pirsig, "Subjects, Objects, Data and Values":

http://www.moq.org/forum/Pirsig/emmpaper.html. Kind of funny, given that I just bought the 25th anniversary edition of *Zen and the Art of Motorcycle Maintenance*.

25-03-08 *Pauli-Bohr Exchange* (to H. C. von Baeyer)

See, I did have them once! Below is a letter from Henry Folse, dated 2 April 2001, explaining that they were written in English and had some handwritten marginalia by Pauli.

How on earth could I lose these gems?!?

Letter from H. J. Folse, dated 2 April 2001

> You're quite right that this an interesting exchange between Bohr and Pauli. I suspect that many have ignored them because of the late date.
>
> The letters are in English and typed, but at least on one Pauli inserted several comments in handwriting. Since it's about 15 pages or so, I've photocopied and mailed all four of them to you at your Bell Labs address. The copies are too poor to scan very easily. The Pauli letters are photocopies of the originals; the Bohr letters are photocopies of Bohr's carbon copies.
>
> Hope you enjoy reading them.

25-03-08 *That Fierz Paper* (to H. C. von Baeyer)

Here's a link to the Fierz article I was telling you about that had such an influence on me. It's reprinted in full on pages 405–407 of http://www.perimeterinstitute.ca/personal/cfuchs/SamizdatSE.pdf.

In this case I've pieced together why I can't find the original. It's because I possessed it before the fire and haven't replaced it. But where the correspondence went (which I obtained after the fire) still eludes me.

See you later this morning.

25-03-08 *Another Reference* (to H. C. von Baeyer)

The chapter by Inga R. Gammel, "Mission Impossible? Wolfgang Pauli's Idea of a Neutral Language," in *Towards Otherland: Languages of Science and Languages Beyond*, edited by R. E. Zimmermann and V. G. Budanov:

http://www.upress.uni-kassel.de/online/frei/978-3-89958-107-2.volltext.frei.pdf

looks potentially relevant to our discussions. (I haven't read it yet; just noting it.)

25-03-08 *The Coin Tosser* (to D. M. Appleby)

Looking over the note [to Christian Snyder titled "No Subject," dated 14-12-07], I see I missed something crucial in my report to you tonight. An action with a predictable consequence, he told me, "expresses what you think of yourself." I said I thought it was particularly deep, but really, I guess, I just thought it was particularly interesting. It certainly does have an affinity with my general orientation that any action initiated by one's bodily motions, and with a predictable consequence, might as well be thought of as an extension of one's body to begin with it.

26-03-08 *(Purely) Measurement-Based Quantum Computation* (to H. C. von Baeyer)

Here a set of good articles on the subject.
Here is Nielsen's elementary exposition of the idea:

`http://arxiv.org/abs/quant-ph/0504097`

Here is a kind of review article by the inventors (Robert Raussendorf and Hans Briegel), but with prettier pictures than Nielsen's:

`http://arxiv.org/abs/quant-ph/0301052`

and a shorter version of the same:

`http://arxiv.org/abs/quant-ph/0207183`

Here is Steane's important conceptual paper that makes use of this kind of computation to put a nail in the many-worlds idea:

`http://arxiv.org/abs/quant-ph/0003084`

Here's a Zeilinger paper on making a 4-qubit cluster state:

`http://arxiv.org/abs/quant-ph/0507086`

And here's one doing an experimental demonstration of Deutsch's algorithm with such states:

`http://arxiv.org/abs/quant-ph/0611186`

There's plenty more out there to read on the subject, but this will get you started.

27-03-08 *Fantastic Quote* (to H. C. von Baeyer)

That was a fantastic quote you sent to Marcus last night of Pauli comparing evil and acausality. Could you send it to me? I was surprised by Pauli identifying hyle with nonbeing. Very different from F. C. S. Schiller's use of the term, and very different from what I find in my book, *The Concept of Matter in Greek and Medievel Philosophy*. Particularly the chapter, "Matter as Potency."

Hans's Reply, 28-03-08

> Chris, I'll reply as soon as I find a Greek etymological dictionary. Hyle means matter, but Pauli implies that its roots mean something negative. I'll check it out – fortunately I attended a German humanistic boarding school where Latin and Greek were compulsory for six years.
>
> I REALLY enjoyed PI, have banished the Demon, and have already started stocking my library with Pauli material. Thank you for inviting me, and thank you especially for introducing me to Marcus who is clearly a unique person — not only intellectually but humanly as well.

Hans's Further Reply, "Pauli Reassessment," 03-04-08

Pauli died on 15 December 50 years ago. It occurred to me that one might write a reassessment of his legacy in PT or something.

The elements of such a piece would be:

Downplay Jung. The late Pauli gave plenty of evidence that while he owed a great debt of gratitude to Jung, he was fed up with Jungians.

Downplay Pauli effect and Pauli anecdotes in favor of his serious legacy. The principal witness to the idea that Pauli took his effect seriously was Fierz, and he is dead.

Divide the Pauli legacy into two parts: The private part asks how his dreams and beliefs influenced the creation of new scientific concepts. Gerald Holton is the expert on that, and his invention, the "thematic content" of science, deals with it well. The public part is Marcus's concern: the re-connection of the rational with the irrational in our world picture, the "coniunctio" of inner and outer, the healing of the cut between subject and object ...

I feel that your program is a step in the right direction. Consider this good quote from a letter to Fierz (1953):

> Einstein has neither the courage nor the mental agility to admit the essential incompleteness of science within life – *and for this reason* speaks erroneously about the incompleteness of quantum mechanics within physics.

In other words, accept Copenhagen and go on to larger questions.

What do you think about this idea? I feel comfortable about writing everything except the last part – your work.

Hans's Preply to Marcus Appleby, "Pauli's Darkness," 26-03-08

Pauli's own footnote in his letter to Richard von Weizsaecker 5 May 1953:

> I also see certain parallels between evil and the acausal. Both are, after all, the "dark", in a manner of speaking, which escapes the rational ("enlightened") order of things. Just like the acausal, evil must also be recognized as real — in contrast to neoplatonism, which wanted to declare both evil and matter unreal, by calling the latter hylae (non-being) and the former, euphemistically, privatio boni = absence of good. Just like observation in quantum mechanics, there are instances in life where "the calculation doesn't compute" (e.g. Plato in Sicily). One can neither get rid of these instances, nor avoid them by fleeing into the other side of the pair of opposites. One can only approve of them and accept them into a symbolic reality.

In the interest of speed I took some liberties in translating, and I don't know what he is talking about in the second half. Plato screwed up in Sicily and had to return home in disgrace. The point of the passage is that Pauli uses in one passage evil, dark, and acausal.

Hans's Preply to me, "Postprandial Nugget," 26-03-08

Pauli to Jung 27 May 1953: "... It was, if I am not mistaken, in 1931 that I got to know you personally. At that time I experienced the subconscious like a new dimension ... "

27-03-08 *Exposing the Cracks in the Block Universe* (to J. A. Vaccaro)

I got your great painting in the mail yesterday. Thanks again so much; I'm so tickled with it. The painting made it briefly to my house yesterday, but at the moment, I have it propped on my office desk at PI. I started thinking after taking it home that it'd be nice to show it off to more people for a while, before it lives out the rest of *my* life in my quiet library.

Some questions if you don't mind my asking. I notice it's signed JV00; do you recall what month you painted it? Can you articulate your intention for painting it? What concept you were trying to capture? (I've already explained my own reason for wanting it. But I'm sure you were trying to express something completely different in painting it. And I'm just curious.)

02-04-08 *Need Flammiazation* (to S. T. Flammia)

When in the hell are you going to be back?

04-04-08 *Hirota's Birthday Party* (to A. S. Holevo & the invitees)

Dear colleague,

Alexander Holevo and I would like to invite you to a three-day scientific birthday party at the Perimeter Institute for Theoretical Physics, June 25–27, in honor of Osamu Hirota's 60th birthday. The meeting, "Osamu Hirota, a True Quantum Communication Channel," plans to be a relaxed venue where we'll be able to interact and convey what we each think is presently the most exciting (or most perplexing) in quantum information theory. Our budget should be able to cover your travel and local expenses, should you need it.

The proposed speaker list includes, along with yourself,

Charles Bennett (confirmed)
G. Mauro D'Ariano
Steven van Enk
Christopher Fuchs (confirmed)
Alexander Holevo (confirmed)
Debbie Leung
Masanao Ozawa
Masahide Sasaki
Jeffrey Shapiro
Peter Shor (confirmed)
John Smolin
Horace Yuen

Please confirm with me whether you will be able to come enjoy Professor Hirota's birthday.

The PI facilities may also be able to accommodate a few of us for a limited time before and after the meeting (up to two weeks) for extended collaboration. When you reply to this note, please let me know whether you might like to stay longer than the meeting, and we will see what can be done. PI can be a very good place for thinking and gaining new ideas!

Best regards,

Chris Fuchs (and Alexander Holevo)

04-04-08 (to T. A. Brun)

On behalf of the GQI executive committee, I am pleased to let you know that your student Bilal Shaw was the winner of the APS March Meeting Best Student-Presentation Award in the theory category. Could you please pass the word on to him?

The judging panel consisted of Prof. John Sipe, chair (Toronto), Dr. Howard Barnum (LANL), Prof. Dagmar Bruss (Duesseldorf), Prof. Barry Sanders (Calgary), Prof. Lorenza Viola (Dartmouth), and Prof. Harald Weinfurter (Ludwig Maximilians, Munich). The candidates were judged on four equally weighted categories: content, clarity, style, and q&a. All the candidates were, in fact, very strong in comparison to many, many more-senior speakers at the meeting, but Mr. Shaw's presentation was deemed the best.

The award will be announced in the next issue of *The Quantum Times*. The award carries a 500 USD cash prize which will be presented to the winner through the American Physical Society, but comes from funds established by the Perimeter Institute for Theoretical Physics in Waterloo, Canada. Also, PI would like to invite Mr. Shaw to Canada to give a longer version of his presentation. Thus, please put him in contact with me so that we can make arrangements for a visit.

06-04-08 *Librarians* (to O. Hirota)

I am glad you are happy.

Holevo pointed out to me that between 6–11 July, there is an IEEE Symposium on Information Theory in Toronto. So, he will stay at PI the two weeks preceding that (roughly 22 June to 5 July), and then he will go to the IEEE meeting.

As I said, I will be happy to show you my library. You know, I have told you how important I think the philosophy of William James, John Dewey, and F. C. S. Schiller is for our understanding of quantum mechanics (it was quantum mechanics that pushed me toward them). That philosophy has the name "pragmatism" and my library has nearly 400 on books on some aspect of the subject. In the middle of the shelves is a volume I've recently purchased titled *Contemporary Japanese Philosophical Thought*, published in 1969. Chapter 3 of it is devoted to pragmatism and related philosophies from 1901–1925. What is interesting is that I found that there was actually a Japanese pragmatist of some renown. His name was Tanaka Odo. Perhaps your son with an interest in philosophy can explain to us the particularities of Tanaka Odo's views!

08-04-08 *PIAF Invitation* (to the invitees)

The Perimeter Institute for Theoretical Physics in Waterloo, Canada and three Australian universities (University of Sydney, University of Queensland, and Griffith University) have recently banded together to seed growth in the field of quantum foundations. In the long run, this PI-Australian Foundations (PIAF) collaboration has as its biggest objective to make it possible for physicists and philosophers to pursue sustainable academic careers in quantum foundations. More immediately though, within our portfolio of activities, we plan to establish a major international conference for the yearly exposition of the best work in quantum foundations. This year's meeting "The Clock and the Quantum: Time in Quantum Foundations", will be held in Waterloo, 28 September to 2 October, 2008, and we are hoping to give the series a very strong start by having an outstanding set of invited speakers.

In that regard, we would be honored if you would be one of those invited speakers. All your travel and local expenses would be covered by PIAF. Would you be able to come? For your reference, here are the other invitees:

Samson Abramsky	(Oxford U.)
Yakir Aharonov	(Tel Aviv U.)
Julian Barbour	(Oxfordshire)
Harvey Brown	(Oxford U.)
Phil Dowe	(U. Queensland)
Brian Greene	(Columbia U.)
Gerard 't Hooft	(U. Utrecht)
Matthew Leifer	(IQC, Waterloo and PI)
Paul Kwiat	(U. Illinois, Urbana-Champagne)
John Norton	(U. Pittsburgh)
Wayne Myrvold	(U. Western Ontario)
Roger Penrose	(Oxford U.)
John Preskill	(Caltech)
Carlo Rovelli	(U. Mediterranee)
Lee Smolin	(PI)
Roderich Tumulka	(Rutgers U.)
William Unruh	(U. British Columbia)
Lev Vaidman	(Tel Aviv U.)
Anton Zeilinger	(U. Vienna)

Please let us know as soon as possible if you will join us (so that we may set the poster printing in motion). We believe this promises to become an important conference series, and your participation would be a great coup for us.

Sincerely,

Chris Fuchs, Lucien Hardy, and Ward Struyve (the local organizers)

Organizing Committee

Guido Bacciagaluppi	(U. Sydney)
Christopher Fuchs	(PI)
Lucien Hardy	(PI)
Ward Struyve	(PI)

Advisory Committee

Stephen Bartlett	(U. Sydney)
Gerard Milburn	(U. Queensland)
Huw Price	(U. Sydney)
Howard Wiseman	(Griffith U.)

08-04-08 *PIAF Invitation* (to M. S. Leifer)

Matt, I'll also add a word of personal encouragement: This conference *needs* you, so I hope you can come. Like I told Lucien, "if his work on conditional density operators ain't about time in quantum mechanics, nothin' is." And you'd be the only representative of that point of view.

09-04-08 *Yo Yo and JFK on Wiki* (to L. Hardy)

Thanks for reminding me. Impressive. I wish I had gotten the opportunity to meet the guy.

The frame of this also reminds me in turn to look up the James bang quote I told you and Vanessa about. Here it is:

> It is a common belief that all particular beings have one origin and source, either in God, or in atoms all equally old. There is no real novelty, it is believed, in the universe, the new things that appear having either been eternally prefigured in the absolute, or being results of the same *primordia rerum*, atoms, or monads, getting into new mixtures. But the question of being is so obscure anyhow, that whether realities have burst into existence all at once, by a single 'bang,' as it were; or whether they came piecemeal, and have different ages (so that real novelties may be leaking into our universe all the time), may here be left an open question, though it is undoubtedly intellectually economical to suppose that all things are equally old, and that no novelties leak in.

It comes from his book, *Some Problems of Philosophy: A Beginning of an Introduction to Philosophy*, published soon after his death in 1910.

14-04-08 *Your Dad* (to L. Wheeler Ufford)

I learned the news of your father's passing away today. It has been a very sad day for me. But I have him very literally to thank for my career: The questions I have worked on these many years were the questions he asked so many years before that. I was lucky enough to meet him in 1983 and absorb those questions. He was a very great man.

14-04-08 *John Wheeler's Death* (to G. L. Comer)

In case you hadn't heard about it yet:

"John A. Wheeler, Physicist Who Coined the Term 'Black Hole,' Is Dead at 96"

`http://www.nytimes.com/2008/04/14/science/14wheeler.html?scp=1&sq=Wheeler&st=nyt`

14-04-08 *Sad Day* (to D. Overbye)

Thank you for the nice article on John Wheeler today. It is a sad day for me. His thoughts have pushed me for years.

One minor inaccuracy, I believe; I don't know if it is too late for you to fix it. I believe it was Bryce DeWitt who coined the term "many worlds" rather than John. I believe Wheeler himself always called it "the relative state formulation of quantum mechanics."

14-04-08 *Times and the End of Time* (to W. P. Schleich)

I became very disorganized, and I apologize for keeping you waiting. But today, with the sad news of John Wheeler's passing away, I was shaken back to business. John was a very great man, and how lucky I was to meet him in 1983. He provided the questions that have framed my career ever since. Mostly at the moment, I feel like I should work harder: It is what I owe him for his wonderful gift.

Thanks again for inviting me to Ulm. The MPQ has kindly offered to pay for my transatlantic flight; so I'll make those guys the major part of my visit. But I would still like to come to Ulm and give a talk if you will have me ... and if I can synchronize my family's schedule with the window of time you'll be in town. It'd be nice to spend a night in Ulm and get you guys revved up about the SIC representation of quantum states.

I should have a precise schedule worked up in a couple of days. I'll get back in touch then if it looks like I'll be able to come to Ulm July 21 or July 22, say.

I know this day has probably been rough on you. I could see how close you were to John in Princeton, Feb 2006.

14-04-08 *Epistemic Meeting* (to H. Westman)

Westmanism 1: *There has been some interest circulating about getting together and talk about the epistemic interpretation of QM (à la Spekkens) and whether or not is it possible. It could be that one could construct a no-go theorem or perhaps a concrete working model.*

It struck me that "the" is an awfully singular word. So I thought it would be fun to revisit my own early uses of the word. Here are a couple of instances I dug from my `quant-ph/0105039`. Thus the word did have some currency before Spekkens, and particularly before it was hijacked to the purpose of hidden variable theories.

Still, yes, it would be fun and informative to discuss these things. It's just a question of me finding the time in these coming weeks.

12 December 1999, to John Preskill, "Freedom"

> **Preskillism 11:** *Free will usually means the ability of conscious beings to influence their own future behavior. Its existence would seem to imply that different physical laws govern conscious systems and inanimate systems. I know of no persuasive evidence to support this viewpoint, and so I am inclined to reject it.*
>
> Can't agree with your second sentence. When you think of physical law, you should say the chant epistemic, epistemic, epistemic. Then you can chime in with MLK: free at last, free at last, thank god, I'm free at last!
>
> Had fun talking to you the other day.

20 March 2000, to Paul Benioff, "Small Addendum"

> I agree with you wholeheartedly that in quantum mechanics the "randomness" of measurement outcomes is NOT epistemic. However that does not preclude my view that all probabilities (including quantum mechanical ones) are epistemic in nature. They quantify how much we can say about a phenomena based upon what we know. It so happens in the quantum world that we cannot tighten up our knowledge to the point of removing all ignorance (about the consequences of our interventions), and in that sense the randomness is ontological—it is a property of the world that was here long before we ever showed up on the scene. But it takes epistemic tools to describe that property. That's the direction I'm coming from.

26 January 2001, to Jeremy Butterfield, "Worlds in the Everett Interpretation"

Anyone who knows me knows that I am rather down on attempts to interpret quantum mechanics along Everett-like lines. I think the most funny and telling statement of this in the present context is that, whereas Mr. Wallace speaks of "Everettians," I often speak of "Everettistas." Thus, I am almost surprised that you sent me this paper to referee.

My difficulties come not so much from thinking that an Everett-like interpretation is inherently inconsistent or that parallel worlds tax the imagination too much. It's more that this line of thought strikes me, at best, as a complete dead end in the physical sense. At worst, I fear it requires us to tack on even more ad hoc structures to quantum theory than we already have. (Here, I'm thinking of a preferred basis for the Hilbert space and a preferred tensor-producting of it into various factors.) For these reasons, among umpteen others, I have always been inclined to an epistemic interpretation of the quantum state. Doing this has helped me (personally) to focus the issue to asking, "What is this *property* of the quantum world—i.e., reality—that keeps us from ever knowing more of it than can be captured by the quantum state?" To that extent, I consider myself something of a realist who—just as David Deutsch—takes the wavefunction absolutely seriously. BUT absolutely seriously as a state of knowledge, not a state of nature. I do well believe we will one day shake a notion of reality from the existing theory (without adding hidden variables, etc.), but that reality won't be the most naive surface term floating to the top (i.e., the quantum state). When we have it, we'll really have something; there'll be no turning back. Physics won't be at an end, but at a beginning. For then, and only then, will we be able to recognize how we might extend the theory to something bigger and better than quantum mechanics itself.

21-04-08 *Born's Rule?* (to R. Laflamme)

Rüdiger told me last week that you're planning to do some experimental test of the Born rule. I'd like to learn more about that, particularly as it impinges on our quantum Bayesian program. If you've got some time this week or next, I wouldn't mind an introduction to the ideas and what's being done.

22-04-08 *Ariana Margaret Blume-Kohout* (to R. Blume-Kohout & M. E. Blume-Kohout)

Congratulations. It takes me back years. It's a truism that your life will change, and it is a truism that it will change for the better. Myself, I believe that life and creation is the big principle in the universe. Tell Ariana Margaret that—that her birth played a crucial part in making the universe fly. She should understand that about herself.

23-04-08 *Dylan Quote on Time* (to S. Weinstein)

The first thing you notice about New Orleans are the burying grounds—the cemeteries—and they're a cold proposition, one of the best things there are here. Going by,

> you try to be as quiet as possible, better to let them sleep. Greek, Roman, sepulchres—palatial mausoleums made to order, phantomesque, signs and symbols of hidden decay—ghosts of women and men who have sinned and who've died and are now living in tombs. The past doesn't pass away so quickly here. You could be dead for a long time.

24-04-08 *PIAF Abstract* (to L. Hardy)

Hardyism 2: *Does time exist between measurements?*

Pauli would be very proud of you for including his question. Here was his own version of it (from a 1947 letter to Markus Fierz):

> I'm more and more expecting a further revolutionizing of the basic concepts in physics. In connection with this particularly the manner in which the space-time continuum is currently introduced into it appears to me to be increasingly unsatisfactory. ... Something only really happens when an observation is being made, and in conjunction with which, as Bohr and Stern have finally convinced me, entropy necessarily increases. Between the observations nothing at all happens, only time has, "in the interval", irreversibly progressed on the mathematical papers.

More serious comments on all manner of things later.

24-04-08 *Topos Theory Today* (to L. Hardy)

Here is my own *present* feeling about the research program of Doering, Isham, and associates. It seems to be a significant amount of mathematical obfuscation for saying, in the end, something that has been tried, tested, and debated since 1935, but generally in more accessible language: It is that something ascribed probability one should also be ascribed an element of reality. By implication, some aspect of a pure quantum state then corresponds to an element of reality. Here's the way Roger Penrose put the essential point:

> One of the most powerful reasons for rejecting such a subjective viewpoint concerning the reality of $|\psi\rangle$ comes from the fact that whatever $|\psi\rangle$ might be, there is always—in principle, at least—a *primitive measurement* whose **YES** space consists of the Hilbert-space ray determined by $|\psi\rangle$. The point is that the physical state $|\psi\rangle$ (determined by the ray of complex multiples of $|\psi\rangle$) is *uniquely* determined by the fact that the outcome **YES**, for this state, is *certain.* No other physical state has this property. For any other state, there would merely be some probability, short of certainty, that the outcome will be **YES**, and an outcome of **NO** might occur. Thus, although there is no measurement which will tell us what $|\psi\rangle$ actually *is*, the physical state $|\psi\rangle$ is uniquely determined by what it asserts must be the result of a measurement that *might* be performed on it.

What I have been left wondering is what does the category theoretic approach add to that essential point? Particularly, in what way—and for what compelling reasons—is their approach any less ad hoc than any of the other 500 approaches to quantum logic? (And I would guess there really are easily 500 other approaches along those lines, without exaggeration.) And what is the grand plan for where it'll lead (beyond saying we ought to get rid of the continuum)? Neither has ever come across to me. Though admittedly I have never worked hard to let it come across to me either: Instead, because there was no immediate (subjective) mental "hook" to it for me, I treated it as somewhat like a patent clerk receiving another detailed plan for a perpetuum mobile. Complicated, very complicated, interesting move, ingenious, I never thought of it that way ... ... but ultimately doomed to failure. Red stamp.

John Wheeler was too much of an influence on me: "The solution is not in some magical mathematics, but in some magical idea. A magical idea we've yet to see." (paraphrase)

So, there that's what I feel about the research program—as I say, *presently*. But, just because I'm guessing category theory will be barren in this particular usage of it, it does not mean that I deem category theory itself barren. Lee, for instance, has told me more than once how I might need it for my own research program. Maybe he's right. And it is always good to have a table of integrals about.

24-04-08 *An Entry That Made Me Giggle Again* (to L. Hardy)

Just inserted this one. [See 13-05-04 note "10 Lines and MaxEnt" to R. Schack.] Made me think about Moses wandering the desert for 40 years! Just no place we can call our home.

24-04-08 *Which Stern?* (to H. Atmanspacher)

I found a quote in one of your papers from a 1947 letter from Pauli to Fierz (it is a quote I had known from one of Laurikainen's books, but your version is more complete):

> More and more I expect a further revolution of basic notions in physics, where I am particularly dissatisfied with the way in which the spacetime continuum is introduced at present. (Of course it is ingenious to disband time from ordering causal sequences and — 'as once in May' — use it as a romping place for probabilities. But if one replaces ingenious by impudent, this is not less true. In fact, something happens only during an observation, where — as Bohr and Stern finally convinced me — entropy increases necessarily. Between observations nothing happens at all, only time has reversibly proceeded on our mathematical papers!) This spacetime continuum has now become a Nessus shirt which we cannot take off again! (Instead of 'Nessus shirt' you can also say 'prejudice', but this would, first, sound too harmless and, second, shift the mistake too much from a mere conception to a judgment.)

I wonder if you can tell me a couple of things about it?

1) Which Stern is Pauli referring to?

2) What is a "Nessus shirt"?

The main question is 1), as I'm preparing a better-indexed version of my "Notes on a Paulian Idea" and I want to re-post it at the same time as the posting of my new samizdat (another 500 pages, roughly May 10).

By the way, I'll be in Munich July 23 - August 9, letting my children (and wife) visit with the grandparents. I'll take a desk at the MPQ in Garching, but I don't believe I am committed to too much time there. Would there be any interest in having me give a seminar in Freiburg? I could talk about these very symmetric quantum measurements we have been spending so much time on at PI, and relate them to quantum foundations problems.

I hope all is well with you.

Harald's Reply

> Thanks for your note, good to hear from you! Of course, by all means we should organize something while you are in Germany in summer. We can make detailed plans at Paris in June, where you and I will meet anyway. If you want to have a definitive schedule earlier, please let me know.
>
> Stern is Otto Stern (of the Stern-Gerlach experiment).
>
> The Nessus shirt refers to Greek mythology, see
>
> http://en.wikipedia.org/wiki/Shirt_of_Nessus.
>
> A powerful metaphor for a present that brings harm and mischief.

25-04-08 *Which Stern?, 2* (to H. Atmanspacher)

Thanks! I figured it was Otto Stern, but I seemed to have recalled another Stern that Pauli once referred to, and I wanted to make sure I didn't make a mistake.

Yes, let's plan in June; see you in Paris. I think it'd be fun to come to Freiburg for a day or a day and a night.

05-05-08 *Appleby Papers to Look At* (to K. Martin)

- http://arxiv.org/abs/quant-ph/0412001
- http://arxiv.org/abs/quant-ph/0611260
- http://arxiv.org/abs/0707.2071
- http://arxiv.org/abs/quant-ph/0308114
- http://arxiv.org/abs/0710.3013

05-05-08 *Kant Cola* (to M. Friedman)

It was good meeting you. I certainly appreciated your sympathetic ear for Quantum Bayesian ideas.

I thought I'd get your email address from the web so that I'd have it in my address book. Seeing your interest in Kant (and hearing the things you laughed at last night!), maybe you'll get a chuckle from this diary entry of mine: page 140 of http://www.perimeterinstitute.ca/personal/cfuchs/SamizdatSE.pdf (that's actually page 162 in machine counting, if you include preface and title pages etc.). Or you can simply search on the phrase "Kant Cola".

From 27 February 1996 note "Kant Cola" to Greg Comer

> Kiki and I went into the Outrèmont area Friday evening in search of an interesting restaurant ... and what a find we made! Let me tell you a strange little story. During the summer of 1985, I was reading a book by C. F. von Weizsäcker titled *The Unity of Nature*. Most of the book was about quantum mechanics and Kantian philosophy. Apparently it spurred me to have the following dream. I was in a little hole-in-the-wall joint somewhere in Austin; my old friend David was there, also John Simpson and

Marshall Burns. The place really stood out in my mind because of the Bohemian feel to it: dark, smoky, mystical almost. The night wasn't filled with much of interest: David only wanted to talk about getting drunk, John only wanted to talk about finding a girl, and Marshall only wanted to talk about philosophy. In those days John didn't drink alcohol, so, at some point, when he asked for a drink, I thought we'd be out of luck. But upon looking around, I saw a refrigerator in the middle of the bar near the pool table. We walked over to it and took a look. It was filled with all different sorts of vegetable drinks. John grabbed one, and I looked through it for something more interesting. At the very back, I found one lone can of "Kant Cola." That was written on the label, along with a small portrait of Immanuel Kant. I opened the can, took a drink, ... and, for a miraculous moment, I understood all the intricacies of the world—I understood the necessity of quantum mechanics. When I came out of my trance, the can was empty and I knew that I would never see the light again. Then I awoke. I was so taken with this dream that the next day I sketched out the layout of the joint and made a record of the dream. That was over ten years ago.

So back to the restaurant of Friday. The place was called "City Pub"; it was such a strange little place: dark, smoky, mystical almost. The food was excellent—far better than it should have been for the price. Each option in the place was only $4.99. I had steak, fries, and a vegetable. Kiki had potato soup, quiche, fries, and veggie. They had a special on beer, three for the price of one (so we had six). The music was some sort of strange mesh of things that I suspect you'd only hear in some little bar in Germany where everyone wears black. Anyway, we had quite a time there. However, just a little while before leaving I started to note how similar this place was to the place in my dream 10 years ago. I told Kiki the whole story. Then I looked around and—strangely enough—there was a refrigerator in the middle of the room near the pool table! I was so taken by this that upon my way to the restroom, I took a look into it. What a disappointment: it only contained beer. However, the restroom did have a surprise for me. In the middle of all the graffiti (about Québec's hoped for independence) was something written in bold black letters:

De nobis ipsis silemus
— *E. Kant*

That made my evening. I wrote down the words so that I wouldn't forget and went home to look up my old notes on the dream. Sure enough, there were similarities in the layout of the two places, and moreover, I saw that the name of the original place in my dream was "Hole in the Wall Pub." Very strange. I asked Rüdiger Schack to translate the words for me, and he came up with "About ourselves we remain silent."

05-05-08 *The Hilary Had No Clothes* (to A. Wilce and H. Barnum)

Yesterday at the Demopoulos Bash, I saw Hilary Putnam for the first time. My God, how I was looking forward to it. The title of the talk was "Quantum Mechanics and Ontology," and I had been wondering all week what flavor of subtle pragmatic consideration it would end up expressing on that subject. Well, I set myself up for the singular biggest hero-worship crash I ever had in my life. The guy—this year's version of him anyway—is a friggin' Bohmian and deeply under the influence of Tim Maudlin. I was clinically depressed the remainder of the afternoon. I didn't even seek the energy to talk to or meet with him; maybe better to let him pass through my life like a wraith.

He spoke briefly about information-based approaches to quantum foundations, like Clifton-Bub-Halvorson and your own efforts, and maybe my own and Bill Demopoulos's, I don't know. He ended his brief survey by declaring them either "instrumentalism with post-modern sauce" or "positivism with post-modern sauce"—I don't remember which as he was using both terms in the talk, but it was one of the two. Either way, it was equally insulting.

I guess I write you because I remember the little conversation we three had about him in Atlanta at a fast-food restaurant of some sort.

Recovering, but sad again for having recorded this,

05-05-08 *Pages, Coffee!, and Stairs* (to R. Schack)

I just posted an updated collection. Your name appears several more times now. We're up to 623 pages at the moment.

I'm in a London hotel, and I'll drive back to Waterloo as soon as I get a new cup of coffee in me and get a shower. I just discovered my first cup was wasted: It was decaffeinated!

I've been at the Demopoulos Bash the last two days. Yesterday, there was a very fine talk by Allen Stairs titled "A Loose and Separate Certainty." You can guess the subject: our quantum Bayesian program, and particularly our certainty paper. I hadn't realized how photogenic we three are! Anyway, as indicated, the talk was quite good, but what it indicated about our paper was not so: The guy was genuinely confused by the logic/goals/layout of the paper. And in most of the cases, I could really tell that we were the cause. For instance, he spent five minutes arguing that "Gleason's theorem could not be the greatest triumph of Bayesianism" hanging on that dreadful sentence Carl insisted on. Stairs' only sin was that he read the sentence literally, and then pointed out that there was nothing to bar an objectivist about probabilities from the premises of Gleason's theorem. And so it went with several things. I had a long lunch with him, and then sat next to him at the dinner. He strikes me as being very sympathetic to the program (and sympathetic to the idea of preserving locality at all costs); he just wants to get things straight. I invited him to stay a couple or three weeks at PI and encouraged him to write a "critical article" on us. My guess is, if he delves into this further, it won't end up being so critical, as simply clarifying.

By the way, Hilary Putnam was an absolutely huge disappointment. See story below. [See 05-05-08 note titled "The Hilary Had No Clothes" to A. Wilce and H. Barnum.]

08-05-08 *Triple Products in Dimension 3* (to D. M. Appleby)

Now, that is really cool! I think there is indeed no need to bother with a picture. I think I can figure out a good way to present this. It'll be in the context of finding the algebraically simplest possible equations for quantum state space.

As I say, I had intended to present the talk as joint work with you. I hope you won't mind: I know that I can be something of a wildcard in a talk and you may not (or quite probably will not) agree with everything I say. Actually another option is that I can up-front present the technical work as joint work with you, but make a clear statement that you may not agree with anything beyond that: Time will tell. That'd probably better as I intend to soon write this conceptual piece with business about modifications of Dutch-book coherence when it comes to SICs, etc., and I had intended to write that on my own.

Among the things I want to emphasize is the contrast between what I see as my own foundational program and the convex sets approach of Lucien and Barnum, Barrett, Leifer, Wilce, etc.:

1. They at the outset take measurements as linear maps. I, on the other hand, want to first establish a Bureau of Standards and think of measurements as refinements of probabilities w.r.t. the BoS. Linearity comes secondarily when disparate agents come to agreement that they are performing the same measurement.

2. Best measurement for BoS must be the SICs or Super-SICs. But they give no natural tensor product structure. This tracks metaphorically with the de Finettian / Applebyan idea that "probabilities are single case or nothing", and I think is a very important point. This contrasts with the usual convex approaches in that it does not take the composition of systems as a fundamental operation to be axiomatized. Instead one seeks to characterize the state space of a single system, and *when* one wants to think of separate systems, one does it by decomposing not by composing.

3. The essence of the Born rule, when expressed in terms of SICs is conceptually almost the law of total probability. There is a hidden assumption in the usual Dutch book argument connecting conditional and joint probabilities, that is apparently being surpassed here: It is that "unperformed measurements have no outcomes". Fine, but then why is there any connection at all between the Born rule and old law?

Those are maybe the major components.

Thanks, by the way, for the extensive notes on the title page. Interesting the contrasting reactions I've gotten between you, Lucien, Guido, Rüdiger, and Kiki. You and Kiki are all for the "My Struggles"—enthusiastic about it even. Guido and Rüdiger are offended by it, for some dainty European reason as far as I can tell (but one I don't want to discount, because there are a lot of Europeans in my potential reading audience). And Lucien's first thought when he heard it was "Mein Kampf". It was Lucien's initial reaction (and Guido throwing his two cents in at the same time) that sowed a seed of doubt in me. At the very least, I think I'm definitely keeping the "My"; what I'll do with the subtitles I'm still thinking about.

08-05-08 *Wigner Connectives* (to R. Schack)

Thanks for all the advice. I'll explain in Zurich why I probably won't follow it, but I needed to get a clearer sense of what I might be getting myself into.

Schackcosms fixed as requested.

I'm trusting good discussions in Zurich. I think we really broke ground the last time we were together. Your anti-Zurekian diagram is still at the top of my board. And you should have seen Bill Harper's eyes light up when he understood it. (BTW, it was Harper and Skyrms who laid the first criticism on van Fraassen when he claimed that reflection was a requirement of rationality. They pointed out that it only holds when one judges that recognizing the data is "learning something." I.e., not whacking.)

Below is something I dug up as I was compiling the samizdat. Why I called it a review of general covariance, I'm not completely sure, but it seems to fit now. Linearity of the probability rule—if you take this point of view that actions are defined as sets of likelihoods—arises as a kind of covariance between different agents. It comes about as a kind of intersubjective agreement. I.e., Dutchbook coherence is a one-agent phenomenon, but maybe, just maybe, linearity is a two-agent phenomenon? I.e., I think I'm quarreling with your discomfort with the idea again. [See 23-03-06 note titled "A Review of General Covariance" to G. L. Comer.]

09-05-08 *Keye Bayesian?* (to K. Martin)

My god, you have an interest in Bayesian ideas too? (I just dug up an old paper of yours on `quant-ph`.) How on earth did we not meet for so long? Just do a Google search on "Fuchs quantum Bayesian" and you'll get a few hits.

09-05-08 *Change of Plan* (to S. J. van Enk)

van Enkism 13: *How close were we to writing a paper on counterfactuals?? I do remember having fun making up titles ("Had we found a better title we would have used it" still looks nice!), but I remember almost nothing about the possible contents ...*

So far I found no remarks of mine in your samizdat that I regret: probably you edited them so they were not insulting to anyone. Yes, please tell me I did at least insult one person!

I don't know how close we were, but I think we need to do it. It is a point that has not come up in my other writings. Of course you insulted someone, and of course I was careful not to reprint those words ... One of the entries, by the way, is that very title. I must have written the note because you said it, and I didn't want to forget it. [See 04-04-06 note title "Title" to S. J. van Enk.]

How about this for the real paper: "Had we found a better title we would have used it: The Use of Counterfactuals in Quantum Bayesianism"?

Did you read about our new director here at PI? Ever heard of the guy. BTW, I tried to get 't Hooft to bite on the SIC problem today, but he's a slippery fish. (I explained how useful they'd be as a starting point for his hidden variable considerations.)

09-05-08 *Change of Plan, 2* (to S. J. van Enk)

van Enkism 14: [CAF wrote:]

> "Did you read about our new director here at PI?"

Nope ... does he/she like quantum information??

The more important question is does he like quantum foundations! (My guess is cosmologists rarely do.)

09-05-08 *Change of Plan, 3* (to S. J. van Enk)

van Enkism 15: *While biking back from work I was thinking about counterfactuals: do many-worlders have a different view than Bayesians on this subject??*

I guess it'd be hard to even imagine a counterfactual in a deterministic world—in a world that is one "unbending unit of fact".

12-05-08 *Foundations Curriculum* (to S. L. Braunstein)

Braunsteinism 5: *If you were to teach a graduate course in foundations of QM what topics would you cover?*

I need to decide between this and a course in quantum information pretty soon, so a quick response would be very much appreciated.

There's just one delay after another with me. You're right when you say, "sounds like you're doing a lot of traveling." Here's something I wrote another friend a couple of days ago:

> I've adopted a truly insane travel schedule again. I leave for seven days in Zurich tomorrow. Next month a week in Paris. 2.5 weeks in Munich in July. A week in Sweden in August. Innsbruck in September. Sydney in November. Jerusalem in December. And I had already been to England, Australia, Oregon, and Texas in the previous months of this year. I probably listened to the Allman Brothers Band too much when I was young.

Let's see if you get the final allusion in that.

Here are the kinds of topics I myself would cover in a quantum foundations course. (Of course keeping in mind that I have little sympathy for hidden-variables, GRW, or many-worlds: I.e., I have an agenda. An agenda which takes as a rallying point something Asher first drove home to me, "Unperformed Measurements Have No Outcomes." Thus quantum foundations, as I see it, is about exploring the truth in that phrase at the same time as *not* giving up on the more basic agenda that world would still be here if we petty humans happened to drop dead tomorrow. That physics is an attempt to say something about the world as it is without us.) Thus, the topics below, roughly in the order I'd weave them together.

1) As warm up, selections from Mermin's book *Boojums All the Way Through*. and/or his Rev. Mod. Phys. **65**, 803 – 815 (1993) paper, "Hidden variables and the two theorems of John Bell."

2) Keeping with that theme some selections from Appleby's paper "The Bell-Kochen-Specker Theorem" `quant-ph/0308114`. Maybe also for fun, go over Asher's 33-ray state-independent proof, and Clifton's 8-ray state-dependent proof (old papers, not on `arXiv`).

That material sets the stage for the slogan, and leaves the tension of "My god, where do we go from here?". Accept action at a distance? Or accept that quantum states are not elements of reality, but more akin to incomplete information? (Leaving aside for the moment, the question of information about what.) At this point I would start to move down the informational track, with a prelude of showing off various of the phenomena of quantum information theory: no-cloning, no-broadcasting, teleportation, entanglement monogamy, superdense coding, such things. And then backtrack, by hitting the students over the head with:

3) Spekkens' toy model, `quant-ph/0401052`. That should clench any doubt that quantum states are analogous to incomplete information.

But information about what? At that point introduce them to two parallel themes (that are not yet married but to some extent flirt with each other):

4) The quantum Jamesian pragmatism of Fuchs `quant-ph/0205039`, the quantum Bayesianism of Caves, Fuchs, and Schack `quant-ph/0404156`, `quant-ph/0608190`.

and

5) The relational ideas of Rovelli `quant-ph/9609002`.

Those are both attempts to speak to the issue of information about what (in a way that is not hidden variables).

You might bolster that with a side tour on Bayesian probability itself:

6) Appleby's two papers are about the best things out there: `quant-ph/0402015` and `quant-ph/0408058`.

But if all those ideas on the right track, how do they imply the mathematical shape of quantum mechanics? Well we haven't figured that out yet, but there have nonetheless been some valiant attempts to derive the shape of the theory recently and they should be reviewed for the insight they might give:

7) Hardy's Five Axioms `quant-ph/0101012` (Hardy has a new version not yet posted that he'd probably share with you).

8) The convex sets work of Barnum, Barrett, Leifer, and Wilce: `arXiv:0707.0620`, `arXiv:0803.1264`, `quant-ph/0611295`, `arXiv:0712.2265`.

I'd guess you're at a whole course by now. But if you still have time, and want to leave on a positive note, maybe explore what all this might mean for the one-way model of quantum computation. Honestly, I think that's a grand playground for this kind of point of view in quantum foundations: For there it's obvious that by kicking the cat (state), you really do set the world in motion.

Hope all of this is of some value to your thinking.

12-05-08 *Fascination with Words* (to W. G. Demopoulos)

Can you explain in simple English and give an example or two of what Carnap meant by a "functor"? The term came up at PI's wine and cheese Friday as I joked that I'd like to see quantum measurement outcomes not as indicative of properties of the system, or even indicative of a relation between the system and agent, but rather as a functor of the two objects (system + agent). Then I mumbled some things about the sexual interpretation of QM, and finally admitted that I had no idea what a functor is, but just really liked the sound of the word.

On the other hand, just now I read this in wiki:

> Carnap used the term "functor" to stand in relation to functions analogously as predicates stand in relation to properties. [See Carnap, *The Logical Syntax of Language*, p. 13–14, 1937, Routledge & Kegan Paul.] For Carnap then, unlike modern category theory's use of the term, a functor is a linguistic item.

which intrigues me.

Greetings from Zurich. I just arrived a couple of hours ago, and the spirit of Pauli already seems palpable.

12-05-08 *Fascination with Words, 2* (to W. G. Demopoulos)

Demopoulosism 34: *A functor, for Carnap, is just a functional expression—an expression for a function. So this is a case where Wiki is a reliable guide to the truth.*

You may be a distinguished, honored professor, but you don't read instructions very well! Now I'm more confused than before. Trouble is I have no training in philosophy. Thus, what you've written appears to me to contradict wiki. Probably because I don't really know what "functional expression" is, nor do I really know what "predicate" is. If you could draw up a couple of examples, that'd probably really help me.

Bill's Reply

Let me try to keep it simple; I can give you the long answer when we get together.

The linguistic expression

x is even

is used to express a property of integers. Upon substitution of a numeral name or other singular term for the variable, the result is either a true or a false sentence.

But the linguistic expression

x + y

is used to express a function from pairs of numbers to numbers; upon substitution for the variables, the result is a singular term which picks out a particular number:

x is even/x+y :: predicate/functor.

And by implication, predicates go with properties, functors with functions.

I'll be curious to know if this settles your perplexity, which I suspect is not at the level of words.

I don't recall what a category theorist means by 'functor' but I think it's a *special* kind of function, not a general term with the same meaning as 'function.'

12-05-08 *Fascination with Words, 3* (to W. G. Demopoulos)

That helps quite a bit. I'll keep thinking. I like the word so much, I really do want to use it! Homework challenge for you: find a place where a functor arises in your thoughts on effects.

13-05-08 *Incendiary Stuff* (to C. Misak)

Thanks for the abstract. It'll be good to start a dialog with you. But watch out, the charge of postmodernism is incendiary stuff! It is true, that I think quantum mechanics indicates a certain malleability to reality—that nature can be sculpted to some extent—that is indeed a description I like. But it's a far cry from that to the more extreme idea that reality is just a conversation.

See Sections 4 and 5 in the attached pseudo-paper. A reality that gave no resistance to our whims (or conversations) wouldn't be of much use, I would think.

I'll get your abstract into the PI schedule.

Greetings from Zurich, in case I haven't already sent them!

13-05-08 *More on Malleability* (to C. Misak)

This is secondary material – certainly I hope you'll read the stuff in the last note I sent you before touching this. But while I'm bombarding your email box, I thought I'd go all out.

You can find the aspects of William James that I take seriously here in the attached, starting at item 237. Also, in the Menand item 302, you'll see some of the strains of pragmatism I like best. Schiller in 411. The pieces of Rorty I toy with taking seriously start at 395. But you should never lose sight that I am an experimentalist in thought (part of the nature of being a physicist). Please resist lumping me in with a set school.

I guess your abstract made me sensitive; the last thing I need in this serious effort to get quantum mechanics straight is a misplaced charge of postmodernism. It is bad advertising.

13-05-08 *Misak Tuesday* (to J. E. Sipe)

These are interesting thoughts indeed! I've never thought about things like this. What a place for them to hit me, here at the home of Pauli! (I'm in Zurich at the moment.)

The meeting has not been forgotten; it's just been put into the planning of next year. Next week I will start discussing it with Misak. You might be amused by her abstract. Here it is:

> Some theoretical physicists, Chris Fuchs among them, take quantum mechanics to go hand in hand with an anti-representationalist accounts of truth and reality such as that offered by the American pragmatists — William James, Charles Peirce, Richard Rorty, etc. On this view, scientific theories are instruments, rather than mirrors of the real world. In this talk, I'll suggest that if the quantum physicist is to team up with the pragmatist, he'd do best to join not with James and Rorty, who see the world as radically plastic or malleable. He would do best to join with the founder of pragmatism, Peirce, who argued that a regulative assumption of inquiry is that there is a right or determinate answer to the question at hand. It may look as if the anti-representationalist quantum theorist will be unhappy with this suggestion, but I'll argue that this would be a mistake. The trail of the human serpent, as James said, is over everything but, as Peirce saw, this does not toss us into the sea of post-modern arbitrariness, where there is nothing to say about what is true and what is real.

I love the way she talked about "some theoretical physicists" in the plural!

John's Preply

> Sorry I'll miss the talk on pragmatism; it sounds great. But I'm waiting to hear the announcement for that great workshop you'll be running on pragmatism! ☺
>
> A quick thought: I was musing about that workshop and how one of the goals is to confront the the idea of a block universe with a more pragmatic one. That suggests looking for particular instances where 'block universe thinking' and your 'reach out and touch someone thinking' give one different perspectives or different strategies for dealing with various issues. Just for fun, here's a kind of sci-fi issue: Consider the idea of time travel. The usual objections or difficulties in thinking about it is that you might be able to go back in time and inadvertently kill your grandfather, and then how could it be that you would have been there to do that, and so on and so on. These difficulties seem to me to follow from at least implicitly thinking in a 'block universe' way, with the past and present and future all carved out in the block. It's that that seems to lead to the kind of contradictory situations that one might be able to get into were time travel possible. But if one abandons the block universe view completely, and doesn't think of space and time in this way at all, are the problems of time travel so severe? I'm not asking for a technical solution here, of course, but a discussion involving the concepts.
>
> Grist for a philosopher's mill. How is the pragmatic analysis of the possibility of time travel different from the block universe analysis?
>
> If you don't think this is too stupid, you might ask Cheryl.

14-05-08 *Many Thanks for Those Papers* (to C. Misak)

Misakism 3: *Now that I've read the papers you sent yesterday, I've got a much better set of things to say. You really are a Peircean, not a Jamesian or a Rortian. And that's a good thing!*

My guess is that I'm a nuance that's taken elements from all three. And those elements are likely not consistent ... but they strive to become so in a greater unity motivated by quantum foundational considerations. In any case, I'm glad you seem to have enjoyed what you read.

See you Tuesday.

15-05-08 *1:30 AM Note – Decompressing the Day* (to D. M. Appleby)

A very short note. I believe I made some inroads today with the talk. I think it went quite well. Ben Toner called it "a beautiful talk". Mauro got very excited as usual, and reprimanded me (as usual) for talking about things not yet published (not because he cares about my career, I think, but because he momentarily wants to get his students working on the stuff). Reinhard Werner was attentive, and that was flattering enough—I respect Reinhard a lot. Mathias Christandl really liked the stuff on modified coherence. Howard Barnum tolerated it kindly, even though he surely understood all this stuff long ago. Časlav Brukner was pretty happy, though he pointed out that I did not mention about how he and Anton where the first to make a big deal of the quadratic entropy in quantum mechanics—I really should have; the ellipsis wasn't intentional, but I felt very bad. Alex Wilce said he much better understood the foundational importance of SICs and my obsession with them after listening today; and wanted to get the summer students in his REU working on the project. So I was very happy in general. Gisin was dismissive as usual, but I did not expect less. I had an edifying conversation with him. One thing he emphasized was how wrong he thinks my attitude is that one should take the single system as basic in an axiomatization of QM (rather than giving composition rules like Hardy and everyone else). My point was that tensor products should be derived from decomposition, rather than composition. Gisin said I would miss entanglement that way. I explained that I would not, could not. And I explained how the move was much more in line with your dictum, "Probabilities are single case or nothing"; it is a Bayesian move demanded almost by consistency itself. He said, "Well, Jauch and Piron could get the tensor product from their approach too, but then it took out the mystery of entanglement. Of entanglement, all they could say was 'so what?'." And all I could say was, "Exactly!" "You cling to a mystery; whereas I want to get enough understanding to move to the next stage."

One highlight of the day was meeting Ernst Specker. We talked on the bus, and then shuffled very slowly across the campus. He is 88 and an amazing man. He explained to me how the Kochen-Specker theorem (which he had first published by himself five years before the usually cited joint paper) arose from a theological question. At the time, he really wanted to know whether God could know what the world would have been like if Hitler were never born. He had all kinds of insight to share with me in that hour I had with him, and I loved it. He dismissed the nullification of Meyer as not understanding what real numbers are really about (I didn't completely understand him, but the best I could surmise he thought Meyer was committing a category error). The thing he wanted to emphasize is how often we are caught up in thinking we have a good grasp of reality. The way he viewed it, almost all of physics and our usual discourse is just of "surface phenomena." He said we just have no clue of how much more there is to the world than that. In his lecture, he was very funny, but very serious. He said we should always strive to see the world from the perspective of the things around us: For instance, try very hard to imagine the world as it "appears" to a flag wrapped around a flagpole. I think he was very serious on that point, actually. Wonderful man from what I could tell.

OK, not such a short note, but a grand day. Tomorrow, I recover from these too many beers, and get back to work on the grant proposal.

Thanks for all the help getting me here.

16-05-08 *2:35 Happy Blues* (to R. Schack)

I know you're sound asleep at the moment (it's 2:45 AM), and that you probably won't read your email until the weekend is over.

But I thought I'd give you a progress report. I still can't see how to get the triple product constraint, but I have been able to tell this much: The constraint that the probabilities lie on a sphere is not sufficient to insure that the modified coherence formula be nonnegative. Here's a simple example of two probability vectors which will make the quantity go negative:

$$\begin{aligned} p &= (0, x, y, y, ..., y) \\ q &= (x, 0, y, y, ..., y) \end{aligned}$$

Fulfilling the proper constraints:

$$\begin{aligned} x + (d^2 - 2)y &= 1 \\ x^2 + (d^2 - 2)y^2 &= \frac{2}{d(d+1)} \end{aligned}$$

one finds that the Born rule quantity

$$d(d+1)\sum_i p(i)q(i) - 1$$

already goes negative at $d = 3$.

That is very good. Because I think it now much more likely that we really will need the extra triple-product constraint to keep things from going negative.

I hope you have a good trip back to London in four hours.

17-05-08 *Springtime in the Air* (to G. Musser)

Yes, definitely desirable and soon. My writing cap is on again, in fact. I was stirred by John Wheeler's recent death and had started to compose a note to you the day after. It would be wonderful if I could combine a story on quantum Bayesianism with a story of the "law without law" and "it from bit" of John Wheeler.

I'm in Zurich, and traveling back to Canada tomorrow; fighting a deadline to get a grant proposal in by Wednesday (which I absolutely have to get out the door); and sparring with a pragmatist Tuesday who likely will say some bad things about me. See: [`http://pirsa.org/08050041/`, possibly?]. (It might be a cup of tea that'll interest you, when it's broadcast on the web starting Wednesday; follow the link on the page I just gave you.)

I'll get back in touch with you Wednesday. Thanks for the amazing stamina you've shown in trying to get me to write. It is very flattering.

19-05-08 *Plurals, for Your Reference* (to C. H. Bennett)

Bennettism 33: *Umlauting the vowel and appending an -e is one of the commonest German plural patterns, as in Ansatz ⇒ Ansätze or Fuchs ⇒ Füchse.*

I've never been pluralized before. Thanks for letting me know how to do it!

20-05-08 *SICs at PI* (to H. B. Dang)

I thought I'd keep you up to date on my activities. Attached is a proposal that I just sent off to ONR. If you dig around, you'll find your name mentioned in the proposal briefly. If by wild chance I get the money, you would have the first shot at it, if you want it. That would free you from having to do anything with quantum crypto, and you could devote yourself fully to foundations (ahem, the quantum information theory of SICs).

Schack and I made great, great progress last week in Zurich. I believe we are on the verge of having something I would be willing to call "the fundamental equation of quantum theory." It's turning into quite a beautiful story, but the work is not complete yet.

21-05-08 *John Wheeler* (to S. K. Stoll)

Thanks; I wish I could be there to hear it. When John died, my collaborator Rüdiger Schack was visiting, and it was funny how, spontaneously, all week long I would keep coming back to stories of him or some aspect of his thought. He's a big part of my mind. I didn't know you knew that.

I hope all is well with you.

21-05-08 *Determinacy, Determinates and Determinable* (to J. R. Brown)

It was good meeting you yesterday. I was able to dig up the correspondence I had with Demopoulos about the terms above. Sorry my memory was so frazzled last night. You can see what he meant by going to text pages 596 and 601, page numbers at the bottom of the pages (or pages 619 and 624, respectively, by the raw pdf page count on the top) of the following file: `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`. At least my memory had served me that W. E. Johnson was the right character. [In the present document, see 03-02-07 note "Bill's Thoughts on QL and QI Frameworks" and 04-02-07 note "The Painful Ambiguity of Language," both to W. G. Demopoulos.]

The document I link you to is currently under construction, so I apologize for its present lack of an introduction and its incomplete index. (And I apologize if the page numbers have changed by the time you take a look at it; in which case, just use the search tool to search on "determinacy," etc.) I'm somewhat infamous for publishing my foundational emails; this will be a new collection of them, posted officially relatively soon.

Jim's Reply

> Thanks for the passages from Demopoulos. The idea that we should chop up the world in a very different way (different from objects with properties and the probability

of specific instances) is somewhat radical, but may well be right. Certainly worth considering, at the very least.

It turns out I have a copy of Johnson's Logic, though until now I never read it. The determinable-determinate distinction comes up when he's trying to distinguish "Plato is a man" from "Red is a colour". Grammatically they are the same, but (he rightly claims) they are quite different logically. In a type hierarchy it is easy to see: "Plato" is an individual object, "is a man" and "is red" are first-order properties of objects, "is a colour", is a second-order property, ie, a property of properties. For Johnson, a determinable is like a second-order property and a determinate is a first-order property. Eg, Mary (individual) has the property weight (determinable) and the magnitude of her weight (determinable) is 60 Kg. I presume the QM analog would be: An electron (individual) has observable momentum (determinable) and the eigenvalue is p (determinate). This might turn out to be useful terminology. It is certainly better than the standard "observable."

I'm attaching an invitation to Dubrovnik next April when we're doing physics as a topic. There will be lots of people, including many you know. The place is stunningly beautiful and the conference lots of fun. Try hard to make it.

22-05-08 *Struggles with the Block Universe* (to C. Misak)

Misakism 4: *Many thanks for the invitation Chris. I had a great time and learned much!*

No, thank you for coming! Meeting you has made me wish we could get a serious study group going about these things—too bad you're (I'm) so far away. (By the way, my knowledge of Chauncey Wright came from Philip Wiener's book, "Evolution and the Founders of Pragmatism"; have you seen that one? It might be good material for your present study.)

I'm really angered that I seem to have lost the notes I took at your talk. There were a few things I wanted to reply to. I guess I'll just have to listen to your talk again eventually, and take it from there.

But there is at least one issue that stayed in my mind from the lunch conversation that I can potentially reply to. It is that I got the impression you were dubious of mixing Peircean tychism and Bayesian probability into the same pot. Don't let it be so! (If it is so.) They're perfectly compatible, and not getting confused on the subject, as many fans of Peirce are (like Abner Shimony), is the first step to making progress.

Here's a place to read a little bit about the way to think of it. [. . .]

22-05-08 *Contextuality* (to R. W. Spekkens)

Spekkensism 42: *Speaking of the "recent resurgence of interest in [contextuality] in quantum information and quantum foundations communities," I forgot to tell you about the QIP application of contextuality that Ben Toner and I have finally nailed down: parity-oblivious multiplexing. See* `http://arxiv.org/abs/0805.1463`*.*

Your new results sound juicy. I'm surprised: I didn't realize you were an experimentalist. Watch Cheryl Misak's talk on PIRSA [see `http://pirsa.org/08050041/`]:

American Pragmatism and the Construction of the Universe

> Some theoretical physicists, Chris Fuchs among them, take quantum mechanics to go hand in hand with an anti-representationalist account of truth and reality such as that offered by the American pragmatists — William James, Charles Peirce, Richard Rorty, etc. On this view, scientific theories are instruments, rather than mirrors of the real world. In this talk, I'll suggest that if the quantum physicist is to team up with the pragmatist, he'd do best to join not with James and Rorty, who see the world as radically plastic or malleable. He would do best to join with the founder of pragmatism, Peirce, who argued that a regulative assumption of inquiry is that there is a right or determinate answer to the question at hand. It may look as if the anti-representationalist quantum theorist will be unhappy with this suggestion, but I'll argue that this would be a mistake. The trail of the human serpent, as James said, is over everything but, as Peirce saw, this does not toss us into the sea of post-modern arbitrariness, where there is nothing to say about what is true and what is real.

It was quite a good talk. But she made me sound plumb danged like a philosopher. (And that surprised me too.)

22-05-08 *Unstoppable Forces, Immovable Objects* (to H. R. Brown)

Barnumism 17: *Before she left Cheryl [Misak] told me how much she enjoyed the visit to PI yesterday; you must be pleased with the outcome. And thanks for responding to my criticisms (again) with such good grace yesterday!*

Your criticisms are a pleasure, precisely because they're much needed. They are reminders that I don't have as adequate or as pithy of answers for you as I would like to have.

This question of why update probabilities when one's eyes are closed is a good one. I understand your point well. If one supposes ontic variables (hidden or otherwise) underwriting the results of quantum measurement outcomes, then one has an immediate story for why one is updating one's probabilities (for measurement outcomes, that is) when one's eyes are closed and time flows by: One's degrees of belief should change, because one is supposing the ontic variables to be changing by some objective dynamics.

But we quantum Bayesians presumably have no recourse to such a story, as we reject hidden variables Bohm-style, as well the objective wave functions Everett-style. Our probabilities are probabilities *directly* about experience—directly about the consequences of an agent's interaction with a system external to himself. In the vernacular, probabilities directly about measurement outcomes. So, the point is, you surmise, why would we ever write $p_0(h)$ now, and $p_1(h)$ later, when learn no new data in the meantime?

However, as Marcus Appleby emphatically replied (when he was last visiting and we were discussing your point): one writes $p_0(h) \to p_1(h)$, *precisely* because one does believe SOMETHING is changing. And I have to agree with that. The only question is what is changing? What do we q Bayesians believe is changing with the flow of time?

This is where my answer becomes inadequate, even by my own lights. An agent in this quantum setting changes his probabilities because he believes something integral between himself and the system is changing. But to break it down into a change of variables internal and a change of variables external I will not do; that would be a step backward from our point of view.

I would love to have a more precise answer for you, but I do not have it presently. And when I do have it, I won't only have it for you, but the whole research program will have moved forward significantly by the understanding it'll bring. So, I welcome the question, but one final point on it:

Not having an answer to one isolated question is not a blemish on the program in my eyes. It rather means the program is alive, in contrast to the usual way I view the Everettian program as dead: New PHYSICS will come of it. It has to; it's forced to. Physicists are not born into the world with all the answers. Rather the lucky ones are born with a good nose that'll lead them in directions of progress.

The bit of progress we have made in the last few years is that we now understand much better how to view quantum states in purely probabilistic terms (purely Bayesian or subjectivistic terms, I would even say). And it hangs together with a formal coherence that surprised even us. That, we see as an overpowering clue that we're on the right track. Here's the way, Rob Spekkens put it very eloquently (with regard to his particular approach) in his original toy-theory paper:

> Because the theory is, by construction, local and non-contextual, it does not reproduce quantum theory. Nonetheless, a wide variety of quantum phenomena have analogues within the toy theory that admit simple and intuitive explanations. ... The diversity and quality of these analogies provides compelling evidence for the view that quantum states are states of knowledge rather than states of reality, and that maximal knowledge is incomplete knowledge. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with a research program wherein the quantum state being a state of knowledge is the idea upon which one never compromises.

The point is, we know which idea NOT TO COMPROMISE on anymore, and we let that partially lead the way to new physics. Your great question is part of that process.

Now to another issue. It is an important psychological question, I think, as to the roots of why we don't see eye-to-eye. I look 1) to the sheer number of phenomena in quantum information theory (from no-cloning to teleportation to correlation monogamy to 25 other things) that come about immediately for any kind of incomplete knowledge (i.e., without any regard to quantum issues), and 2) to the formal near equivalence of quantum theory and probability theory simpliciter. Those two things together lead me down this research path.

For point number 2), SICs make it particularly obvious. See the attached file, which contains the core of the scientific part from the proposal I just wrote for ONR—it is the best introduction I have at the moment. In that language, the Born rule is nothing other than a mathematically minor variation of the law of total probability. And unitary evolution? Well, it is exactly the same thing—formally, the same expression! Look at the last two equations on page 5 and the first equation on page 6:

> Let us describe a unitary evolution $\rho \longrightarrow U\rho U^\dagger$ in these terms. In terms of probabilities, this is just a transfer from an initial probability distribution $p(i)$ to a final one $q(j)$. What is amazing is the way the formula, to within a sharpening factor and a renormalization, looks exactly like a classical stochastic evolution:
>
> $$q(j) = (d+1)\sum_i p(i)t(j|i) - \frac{1}{d}\ ,$$
>
> where the $t(j|i)$ are matrix elements of a doubly-stochastic transfer matrix, which itself is a SIC representation of the unitary operator U:
>
> $$t(j|i) = \frac{1}{d}\mathrm{tr}\Big(\Pi_j U \Pi_i U^\dagger\Big)\ .$$
>
> In other words, aside from the universal sharpening factor $(d+1)$ and the renormalization $1/d$—both of which are some kind of "quantum magic" independent of the particular

unitary U—a unitary evolution in this language is mathematically identical to a noisy classical channel.

Now, the SIC measurement is only there for the representation, but what we are generally interested in are the probabilities of outcomes for some *actual* measurement, perhaps a measurement of the computational basis at the conclusion of a quantum computation. For instance, suppose the measurement of real consideration is a von Neumann measurement consisting of d outcomes characterized by an orthornormal basis $|j\rangle$. Then the usual calculational tool for quantum probabilities, the Born rule, turns into a mathematically minor variation of the *classical* law of total probability! To see this, let $p(i)$ represent the probabilities of the imagined measurement as before, and let $Q(j)$ represent the probability of outcome j in the measurement that will actually be performed. Furthermore, given an outcome i for the imagined measurement, let $T(j|i)$ represent the probability of an outcome j in the actual measurement, but now thought of as occurring immediately after the imagined one. Then,

$$Q(j) = (d+1) \sum_i p(i)\, T(j|i) - 1 \; .$$

In fact, aside from the differing normalization factor (a 1 instead of a $1/d$), this formula is mathematically identical to the one that represents unitary evolution itself.

My physicist's sense says that points 1) and 2) cannot be coincidence. Your philosopher's sense, as far as I can tell, says that it can be NOTHING BUT a coincidence. Thus you lob back the challenge to me, "But what of the intricate structure of the Schrödinger equation?"

And I have an answer to that: The Schrödinger equation is simply our method for setting the conditional probabilities of the last equation of page 5. In character, in fundamentals, it is a method of no great conceptual distinction from Ed Jaynes' introduction of the maximum entropy principle for setting prior probability assignments in statistical mechanical problems. That, I already feel as an adequate answer. But my relations with you are such as to lead me to be believe that you'll think I'm simply not getting the point.

So, I wonder why *you* don't get the point? And you wonder why *I* don't get the point!?! And with that, I start to speculate that perhaps there is something so deeply ingrained in our individual worldviews that it is blocking translation between the two of us. That is the psychological point.

In any case, all I can say is there's work to be done, and I have not dismissed your question!

I hope you won't mind, but I plan to forward this note to Marcus Appleby, Rüdiger Schack, and Rob Spekkens (by the end of the day), as it should impinge upon their own thinking.

23-05-08 *From Waterloo* (to M. Pérez-Suárez)

You will have discovered that I'm not the email man I used to be: It takes me forever to reply to things now. ...

Lately I have been more excited about SICs than ever. I'm writing a paper at the moment on why they're so very special. I will be sure to send it your way when it's done. If you're interested in seeing some of my recent talks, you can go here: `http://pirsa.org/`. Just do a search on my last name. Another you might find interesting is Cheryl Misak's comparing and contrasting some of the things I've written with C. S. Peirce's writings. Schack by the way, has had some interesting developments in analyzing quantum random number generators from a Bayesian perspective. You might write to him for the latest.

27-05-08 *The Best Time* (to R. Renner & O. C. O. Dahlsten)

Sometimes it takes a while for the things on my mind to come out on paper. I want to tell you guys how much I enjoyed your conference, how much I learned, and how much I was taken with the city of Zurich. Despite my spending so much time locked in my room writing a grant application (which had to be turned in on the day of my return), the week in Zurich turned out to be the best conference of the preceding year for me. Rüdiger Schack and I broke an impasse in our q-Bayesian program while there, and we now have the glimmer of something we think is going to be very, very fruitful. And my moments with Specker were priceless—it was a once in a lifetime chance. So thank you both again. The conference was simply fantastic.

28-05-08 *Unstoppable Forces, Immovable Objects, 2* (to W. G. Demopoulos)

Let me send you this note I wrote to Harvey last week for two reasons. [Sentence parsed as, "Let me send you (this note I wrote to Harvey last week) for two reasons."] The first is because the attached document lays out the formalism I showed you on the board yesterday (though, unfortunately, it is buried in the middle of a grant proposal). Second, it dawned on me overnight, that Harvey, once he's thought about it enough, will surely make the same point with you that he continually makes with me.

The criticism would be this: If every property intrinsic to a particle is eternal and timeless from your point of view, then why would we ever write down dynamical (time evolving) probabilities for effects? What is changing if not the particle itself? That would be his question. Further explication below. [See 22-05-08 note titled "Unstoppable Forces, Immovable Objects" to Harvey R. Brown.]

I got to page 5 of your paper last night; I'll try to come back to the rest of it over the weekend. I can say this much right now: I wish I could see the force of your argument for determinacy better; it still seems elusive to me.

29-05-08 *Unstoppable Forces, Immovable Objects and their Points of Contact* (to H. R. Brown)

Would you give me a reference (or, better, a copy) of your paper with Guido, where you take issue with the EPR criterion of reality?

29-05-08 *Determinacy, Determinates and Determinable, 2* (to W. G. Demopoulos)

Demopoulosism 35: *I copied the Specker paper and put it in the mail today. It's very short and very elementary, but it shows that he had the main theorem before 1960. There are a number of interesting things about it, not least his implicit general understanding of the significance of the theorem as having to do with prediction, things I'd forgotten. Oh and I forgot to say that Allen [Stairs] did the translation.*

Regarding your remark, the title of Specker's talk at the meeting I attended was "Infuturabilia and Quantum Logic." Looking on the web, you won't find any entries on this strange word, but if you look up "futurabilia" you will find plenty. Particularly you'll find things about Leibniz's discussion of God's knowledge of counterfactuals. Kind of fun.

29-05-08 *Determinacy, Determinates and Determinable, 3* (to W. G. Demopoulos)

Demopoulosism 36: *Regarding Harvey's question, no doubt the particle is changing; but (I hope I'm not being too glib here) isn't the point of the effects framework precisely this: we can ignore how the particle changes, how it evolves dynamically, and still recover the phenomena—still recover the effects particles produce in various controlled settings.*

You're not being glib. But also I don't think you are helping yourself by talking that way. To say, "we can ignore how the particle changes" is, linguistically at least, pretty much on the verge of invoking hidden variable imagery. Something much better is called for, I would say, and that is why Harvey's question is a good experimental playground.

29-05-08 *Infuturabilia* (to E. Specker)

It was very good meeting you on the bus before your talk at the "Information Primitives and Laws of Nature" conference a couple of weeks ago. It was also very nice walking with you across the campus. I hope you remember me. Your "colorability" results (as in your construction with Simon Kochen) have long interested me, and I well believe they are the key conceptual component of quantum mechanics.

Thus I wonder if you would entertain an email conversation with me? To some extent I have become something of a historian for my small community within physics—the field of quantum information—and I enjoy recording stories on the origins of this and that all around. An example of some I have recorded can be found here: `http://www.perimeterinstitute.ca/personal/cfuchs/SamizdatSE.pdf`.

In that vein, would you record some of what you told me when we met? Would you tell me again about what was on your mind when you first started thinking about (what we in my field call) Kochen-Specker constructions? What were the years? Were you initially thinking about quantum mechanics, or rather theology? I believe I understood from you that these things were on your mind well before you had met Si Kochen—but I want to get all the facts straight.

If you will share this history with me it would be much appreciated. I learn in many ways—some by logical thought, but some by simply compiling history. (In my own way, I too have been concerned with infuturabilia for a long time; see this collection of thoughts in particular `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`.)

E. Specker's Reply, "Futurabilia"

> Thank you for your mail — I shall try to answer your questions as far as possible, but I need some time, for instance I have no idea when the seminar of Gonseth and Pauli was held. In the mean time you have perhaps a look at my article "Was is ein Beweis", published in a book *Grenzen des Wissens*, where there is a reference to the notion of "double truth" of Ibn Rushed. I hope that you understand German. By the way, we had in Cornell a friend named Wolfgang Fuchs. We liked to discuss in German — and not, as an other emigrè called it, American baby talk. If you do not understand the article, please let me know and I shall stick to English.

30-05-08 *Futurabilia* (to E. Specker)

Thank you much for your note. I look forward to your more detailed reply. I apologize, but I don't speak a word of German. Unfortunately, my ancestors came to the United States (Texas actually) in the 1870s–1880s, so that by the time I stuck my head into the world, there was only English around me. So, I know that this will make it more difficult on you (and you don't deserve that at 88), but German would be completely lost on me.

I am intrigued by your mention of Gonseth and Pauli, and wonder how they fit into your story. Did these two people influence your thinking about infuturabilia? I looked in my own database of Pauli'an things, and found these two items having something to do with Gonseth, but otherwise, I don't know who Gonseth was.

> F. Gonseth, "Remarque sur l'idée de complémentarité," Dialectica **2**, 413–420 (1948). Wolfgang Pauli writes of this article in his introduction to the special issue of Dialectica:
>
> > Many of the articles mention possible applications of the idea of complementarity outside physics, as for instance to questions connected with biology or psychology. I shall not discuss these questions in this introductory survey but wish to draw the reader's attention to the interesting attempts of Gonseth to formulate the idea of complementarity so generally that no explicit references is made anymore to physics in [the] proper sense. This is, of course, only possible by the use of a language to which the physicists are not accustomed, which uses expressions like "horizons of reality," "profound horizon" and "apparent horizon," "events of a certain horizon." The word "phenomenon," however, is used in this article strictly in the above mentioned sense given to it by Bohr. To the "profound horizon" of Gonseth belong the symbolic objects to which conventional attributes can not be assigned in an unambiguous way, while the "traces" of Gonseth are identical with the "phenomena" in our sense. I wish again to stress here the circumstance that the free choice of the observer can produce either the one or the other of two "traces" and that every phenomenon or "trace" is accompanied by an unpredictable and irreversible change in the "profound horizon."
>
> W. Pauli, "Theory and Experiment," Dialectica **6**, 141–142 (1952). Translated in Ref. `\cite{Pauli94}`, pp. 125–126.
>
> > F. Gonseth's dualistic standpoint in regard to the "Dialogue between Experiment and Theory" appears to me to be a special case of the more general relation of internal (psychical) and external (physical). In the situation of *cognition* we are concerned with the relation of the cognizant to the known. The purely empiricist standpoint, which seeks to reduce every "explanation" to a "description" (albeit a general and conceptual one) leaves out of account the fact that in every case the setting up of a concept or system of concepts (and hence also a law of nature) is a *psychical reality* of decisive importance.

30-05-08 *Answers and Stories* (to O. J. E. Maroney)

I'm curious to know whether I answered the question you wanted me to answer yesterday? Or was I off on a tangent on another subject?

Funny story, I don't know if I ever told you. Do you know of how much of a big deal Ed Jaynes made of the original rejection of his first MaxEnt paper? He framed the referee report to show and put it on the wall in his office to show students how inane referees can be. Well, as it turns out, Landauer's first erasure paper was rejected too. It put a great smile on my face when I learned that Jaynes was the referee. If you want, sometime I'll show you the referee report (which I don't think I'm allowed to distribute yet). [See 26-10-04 note "More 'More on Landauer'" to W. T. Grandy, Jr.]

If you're interested in lunch, come get me on your way up.

02-06-08 *Unstoppable Forces, Immovable Objects, 3* (to W. G. Demopoulos)

Demopoulosism 37: *As for instrumentalism, there is certainly this difference: an instrumentalist will refrain from saying that a theory is true—it's merely instrumental for getting predictions. But I think QM gives the true description of the algebraic structure of effects, just as CM sought to give the true description of the algebraic structure of propositions. So in this respect I still think of my view as realist. But it's also realist in a more mundane sense insofar as it holds that the reality of the micro world is an observer independent one; it therefore coincides with the realism of Einstein, which was rejected by Born and others.*

First quick comment. I think you're being too hard on Born. In an important and probably never cited essay, he made it clear that he believed in the same kind of man-independent reality that Einstein believed in. Born was not simply positivist. In the essay (I wish I could remember where I read it), he identified the man-independent stuff with the symmetries quantum theory was built on top of. That is a position, I think, not unlike Carl Caves's that John Sipe tries to capture in the essay I sent you.

I will try to remember the source.

Other comments later as they bubble into existence.

02-06-08 *My Christmas Present?, 2* (to S. T. Flammia)

[See 27-12-06 note "My Christmas Present?" to G. L. Comer, S. J. van Enk, and J. W. Nicholson.] Apparently my reputation has somewhat recovered since that time. Google brings up only two pages of hits now, whereas apparently then it brought up three.

It seems that the CSIQM page is indeed gone now; I guess even porn doesn't help sell some subjects.

02-06-08 *Emailing: 453566c* (to G. L. Comer)

Comerism 19: *What does it mean that the Information is always conserved in Quantum Mechanics? It's in the short "Theoretical Physics" piece in the attached file.*

As far as I can tell, it really means nothing—it is just something that people like to say without thinking very deeply. Information is not a fluid, like phlogiston, with a conservation law. Information is not a fluid.

03-06-08 *Dow Commercial* (to me)

"It is a world that responds to our touch."

04-06-08 *Hammerhead* (to G. L. Comer)

Comerism 20: *There is a number 7 in one of your [SIC] equations in the proposal. Now, doesn't that imply something wrong? I mean, I've never seen a 7 before in a fundamental equation. 8, π, e, are good numbers. But 7?*

I've got a funny story about the 7. I first presented the result at an APS meeting a couple years ago. Charlie Bennett was in the audience and asked, "Is that a 7?" I said, "Yep, it's really a 7." Charlie said, "Well then, it's the first 7 I've ever seen in quantum information." And what else would you expect from a truly fundamental equation?! Indeed it is a 7, and well checked many times by myself and independently by my students. In fact, just the other day by the latest, Ryan Morris, who first found a 6 instead … but then ultimately found a 7.

05-06-08 *Death of Auchentoshan* (to J. W. Nicholson)

Funny that you wrote me this morning. I was just going to write you, to tell you of the demise of the lovely bottle of Auchentoshan you brought me in Dublin. The poor old fellow passed away peacefully about 10:30 yesterday evening. He'll be missed.

Yes, the new Lazarides money was a bit of a shock last night. We knew there was going to be a big announcement, but we didn't know about what. I suspect the money will be used to bolster our expansion plans: Somehow (architectural nightmare) the office space in the building is going to be doubled, add a new bigger lecture hall, double the size of the Bistro, etc. The announcement was made just before Bill Phillips's public lecture—I'm really annoyed that I forgot about it, not because of the announcement, but because I bet it was a heck of a lecture: "Time and Einstein in the 21st Century: The coolest stuff in the universe".

I talked to Lazarides briefly after my own public lecture thing (panel discussion); he had dinner with the group of us, and hung out till midnight. It was funny, kind of like Jon Waskan once described his meeting with Bill Clinton, how he almost melted when he shook his hand. In my own case, I had this overwhelming feeling of gratitude—uncontrollably, somehow the first thing out of my mouth when I had him alone was, "Thank you for this place!" He said, "Physicists are a really good investment; you get a really good return on very little money."

Sciencewise, I'm very happy at the moment. Rüdiger and I have made some serious inroads toward squeezing Hilbert space out of a single inequality. If we make it all the way, this will be the best scientific work I've ever done. I just hope the IF isn't too big of one.

If I discover that Philips mentioned your name in his lecture (when I view it on PIRSA), I'll let you know!

09-06-08 *Pauli Understood Entanglement* (to R. Blume-Kohout)

Points all well taken, of course. The moral is I should have titled my note "Pauli had an inkling of entanglement," which is certainly no insult for a 1927 thought.

Robin's Preply

I presume you mean:

> Now it should of course be emphasized that such reductions first of all are not necessary when all the measuring instruments are included in the system. In order to be able to describe the results of observation theoretically at all, one must ask what can be said about one part of the total system on its own. And then one sees as a matter of course the complete solution—that the omission of the instruments of observation in many cases (not always, of course) may formally be replaced by such discontinuous reductions.

Yah?

I started to write a long analysis, then realized I don't have time to think that deeply! So I'll content myself to observe that Pauli obviously *did* have a grasp on some good stuff ... and then I'll dig at the end bit a little.

He writes ("as a matter of course") that the solution is to replace "omission of the instruments of observation" with "discontinuous reductions" ... while noting that this can only be done some of the time. Now, granted that the first part of his letter implies at least that he understood that it's *possible* to describe the supersystem unitarily ... and that such a description doesn't say anything useful about "the results of observation" ... is there really any content in this last bit? He seems to be saying that the process of disregarding the apparatus is, sometimes, equivalent to collapse. This is a rather unhelpful statement because of the "many cases (not always, of course)" ambiguity. Furthermore, it seems to be Copenhagen ("Shut up and collapse") beyond that ... and finally it seems to conflict with our idea that you represent "omission" of a subsystem by partial tracing.

Perhaps a better way to put that is that we represent omission *either* by partial tracing *or* by collapse, but Pauli provides no guidance on which to use.

Anyway, this vague last sentence makes me wonder how to interpret the earlier sentences. I am not sure what he means by "such reductions first of all are not necessary when all the measuring instruments are included in the system". The best way (IMHO) to interpret that is in a relative-state framework – i.e., that we can describe the joint state as being in an eigenstate of the "One system knowing about the other one" observable, and therefore can confidently answer questions like "Does the observer know about the system?" even if we can't answer questions like "So, what does she know?" But I'm not sure if I should interpret Pauli this way!

25-06-08 (to K. Brading)

By interesting coincidence I discovered that our meeting the other night was our third meeting ... not our second. This memory of mine is becoming so horrible! To see what I mean, download [my samizdat] and search on "Katherine Brading".

Well, anyway, it was great meeting you for real this time: Now you'll last in my memory until there really is none left of it. I wish it had worked out that we had talked earlier in the week.

In the document linked, there are also some tidbits about Poincaré that might interest you. Particularly, I thinking about de Finetti's giving Poincaré credit for coming to within a hair of full-blown Bayesianism (and being de Finetti's greatest influence). And also Poincaré's discussion of Boutroux's philosophy.

Hope you met up in London with your family easily enough. Take care, and I hope to see you again without a lapse of four years.

25-06-08 *Darn!* (to H. C. von Baeyer)

Likewise for me, it was great having you a that meeting: It was nice seeing a mind of sense floating in that sea of contrariety. I wish you could come to PI; the time with Marcus is going to be grand. And as I told him recently, I fear you need a booster shot of Fierz.

Attached is a copy of Bitbol's talk, in case you wanted to review it again. I so wish you could have been there for Rovelli's talk. It struck much deeper, it seemed to me, than his papers on "relational quantum mechanics". I took a long walk with him after the meeting, and felt that he is seriously searching ... even if he over-cautiously fears the agent's introduction into physics. Anyway, I like that in a person.

I'll keep you up-to-date as I respond to others about the meeting with some relevant points. But at the moment, I must go be the MC for the HirotaFest!

03-07-08 *Painful Choices of Language* (to A. Stairs)

Stairsism 1: *Good to hear from you. I'd like very much to work out a way to come to Perimeter, though let me toss in a couple of qualifications.*

The first is that I'm very much more a philosopher than a philosopher of physics. By that I mean my technical skills and knowledge are shockingly limited. (I could easily embarrass myself by elaborating...) So caveat sponsor*.*

In all seriousness, it is almost never the case that a philosopher of physics interests me. The technical skills that you speak of, as far as I can tell, are almost always wasted because of deeper wrong-headednesses that have nothing to do with physics per se. ... Thus maybe why I'm attracted to you.

03-07-08 *PIPPO!* (to P. G. L. Mana)

It should not be my business to pry, but I think you should exercise more control with PIPPO. I am sympathetic to his quantity of self-respect, but there are limits necessary within any civilization—one can only go so far and still expect to maintain an orderly and positive life for all citizens. No one living being can be so singled out at the expense of others.

Concerning the carrion "collapse," I do not know when the word was introduced, but von Neumann made a distinction between Type I and Type II evolutions for quantum states already in his 1932 book. Of course, if one changes the word from "collapse" to "conditionalize" (in the sense of a subjectivistic approach to probability), I myself think we are talking about something completely innocuous.

07-07-08 *Philip Goyal and the Information-Geometric Reconstruction of QM* (to C. H. Bennett)

Bennettism 34: *Does this guy's stuff have anything to do with your goals?*

Unfortunately, no. Philip often says the word "Bayesian" like I do, but that's where the resemblance ends. Look harder, and you'll see that he's proposing that quantum theory is a kind of statistical inference theory about a hidden variable. Roughly, whenever a quantum measurement is performed, two outcomes happen instead of one; it is just that one is hidden from us, and we can't tell which of the two we have. My own program (for instance the flavor you saw with the SICs the other day) is pretty anti-hidden variable.

And by the way, I do not write things really big to compensate for the weakness of my own ideas. I write really big because I assume some in my audience are near-sighted ...

08-07-08 *Two Places to Read* (to A. S. Holevo)

It dawned on me to maybe follow up on one of our lunchtime conversations. With regard to what criterion was used for (imperfect) teleportation in the Kimble experiment, you can go here: `http://arxiv.org/abs/quant-ph/9910030`. With regard to our discussion on how the details of an "instrument" are "washed out" when we consider the state update on the far half of an entangled state, you can look here: `http://arxiv.org/abs/quant-ph/0205039`. The relevant part is the discussion around Eqs. (98)–(101).

I hope you had a safe flight back to Moscow.

08-07-08 *Wednesday Instead* (to R. Schack)

I won't be able to call you this morning after all: I've got this damned talk to give at IQC, which I'm still preparing for.

Appleby and I had another long discussion on the ueberungleichung yesterday. The origin of these α_{ijk} is (remains) just damned confusing. One thing valuable that came though is that maybe ueber is not the best prefix. We toyed with the idea that "Ur" might be better. Can you tell us the precise meaning of "Ur"? Urungleichung, does it work?

09-07-08 *Wednesday Instead, 2* (to R. Schack)

Schackcosm 107: *Wednesday OK. Urungleichung is a much better term. Ueberungleichung has been a joke and shouldn't become the official name.*

Ur, as a prefix, means original or primitive (it is also a noun meaning aurochs). It is a very powerful prefix. Here are a few examples:

> *Ursprung = Ur leap = origin*
> *Ursache = Ur thing = cause*
> *Urbewohner = Ur inhabitants = first inhabitants, aborigines*
> *Urknall = Ur bang = big bang*

Beautiful! Urungleichung it is!

09-07-08 *SIC It Will Be* (to C. H. Bennett)

Bennettism 35:

> [Chris said:] When you were visiting, you seemed to naturally pronounce "sic" as "seek". Is that based on a grammatical rule I can look up? Is everyone else wrong when they pronounce it "sick"?

See attached for my pronunciation of sic*. I assumed you were using it in the sense of the Latin textual annotation, not as an abbreviation for symmetric informationally complete, or any of the other meanings for* sic*. According to my dictionary, even the Latin word may be pronounced like sick in English, but I studied Latin in high school, so I pronounce it like* seek*. By the way, the noun corresponding to* pronounce *is* pronunciation*, not* pronounciation *(sic). Don't ask me how the second* o *got lost.*

Thanks! That settles it.

09-07-08 *Life/Etc.* (to D. R. Terno)

Appleby, Schack, and I are working quite hard to "SICken" the axioms of quantum mechanics, and I think we're making some very nice progress. I have *hope* now that the formal structure of quantum mechanics may be pulled out of essentially a single inequality. It would be the greatest of all my dreams. Pulling all of (the formal structure of) quantum mechanics out of a quantification of the idea that "unperformed measurements have no outcomes". It would be nice to be able to say this with some confidence at Asher's summer school.

09-07-08 *HirotaFest Picture* (to O. Hirota, A. S. Holevo, & the invitees)

Thank you all again for coming to PI last week. It was a fantastic time.

Charlie Bennett's conference photo has now been posted on the web. You can find it by going here: `http://www.perimeterinstitute.ca/images/conferences/hirota_fullsize.jpg`.

09-07-08 *Tables of Contents* (to S. Gharibian & L. Sheridan)

Thanks for the interest.

You can find a better, more careful version of my teleportation example here: [See 25-10-05 note "GOBs, Bobs, Steering & Teleportation" to H. Halvorson and B. C. van Fraassen.] In any case, in the talk version, I was careful to say that when Alice checks the parity, she then randomizes the coins (i.e., she shuffles them so that Charlie no longer knows which is which). Thus from Charlie's perspective, he now knows nothing about the coin in the original box he gave Alice, and would write down a 50-50 distribution for heads/tails in it. It is in that sense that Charlie's original state is "destroyed" for his original coin.

About your second question, my own formalism was just quantum mechanics rewritten. Thus, there is everything in it that there is in quantum mechanics, including Kochen-Specker phenomena and Bell-inequality violations. What I said "must break" is some feature of Spekkens's toy model. As presently written (in terms of epistemic states for local hidden variables), it cannot accommodate these kinds of phenomena. In my own case, it is my belief that my rewrite of the formalism is such that it helps pinpoint the essential difference between the classical and quantum worldviews: It is that "unperformed experiments have no outcomes" (as Asher Peres would say). Something new, that was not there before, comes out of each and every quantum mechanical measurement. That is, even at this very low level of things, there is something analogous to birth in the world. If that is an example of something you would call an "inherent disconnect" between the classical and quantum worlds, then, yes, I believe there is such. It was only that I think SICs help massage the formalism into a form that makes this the clearest yet (by giving the formalism a more direct comparison to classical Dutch book arguments).

But do these damned SICs exist generally? That's the real moneymaker of a question.

09-07-08 *The History* (to G. Gutoski)

Thanks for your comments yesterday. In case you're still curious about some of the prehistory about these lines of thought, let me refer you to the Brassard section of `quant-ph/0105039`.

The idea that the existence of QKD might be a key part of the heart of quantum mechanics is an older one, exhibited for instance in the 6 July 1998 letter to Rolf Landauer titled "Evolution and Physics". But the idea that the nonexistence of bit commitment might also be a potential ingredient in QM was born over a bottle of single-malt scotch with Gilles. I'm still very proud of the allegory I created to go with it. Let me particularly direct you to that. It's in the 23 January 2000 letter to Gilles titled "I See Why No Bit Commitment!" See that and the note "Fuchsian Genesis" it refers to.

I hope I provided some entertainment and some food for thought for your group yesterday.

14-07-08 *Harper, Fuchs, and James* (to R. Blume-Kohout)

I read the Berlinski essay. I didn't much like it actually, or maybe more to the point, him. There was something about the writing that made me feel he's pretty arrogant. As you suspected, I guess, there were indeed points I'm sympathetic to (about "things not seen" and about $M + x$), but then he clouded things up with too much about faith in God (Gods?) for my taste.

You can find my own favorite rebuttal of Clifford here: `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`. Do a search on the phrase "Pragmatism versus Positivism" to get to the right spot. (It'll take you to text page 138 in the present build.) Also there are a couple of other tidbits on Clifford if you search on his name.

14-07-08 *Wheeler Scholars* (to W. P. Schleich, B. W. Schumacher & W. K. Wootters)

I've had a little correspondence with Michael Brooks at *New Scientist*, and he told me about how he bought a copy of Wheeler's booklet *Frontiers of Time* from a mail order place and was pleased to find a little inscription by John in it. It is dedicated to a "Ruth" and dated 3 May 1986. See attached photo.

The question is, who is the mysterious Ruth? Do any of you have an idea?

Wolfgang's Reply

> Many thanks for your email concerning the mysterious "Ruth". This is indeed an intriguing problem. I did call up one of Wheeler's old secretaries, Emily Benett. She did have some interesting information but not concerning "Ruth". She had no idea who she might have been.

However, from the date of that dedication, May 1986, I got some ideas. First of all, the dedication was written during the time when I was in Austin. I remember that exactly at this time John had his heart operation. From the dedication it sounds as if it was a nurse who took care of him in the hospital in Houston. This theory would also explain why the book was resold. Anybody in physics would have recognized the value of such a dedication and would have not sold the book.

I have tried to get in touch with one of his daughters, Allison, but she did not pick up the phone. Most likely they are in Maine on the island right now.

By the way, the only Ruth that I associate with America is Dr. Ruth Westheimer. But I seriously doubt that John would have written such a dedication to her despite the fact that she seems to be very much into "creating islands of time". I wonder if she is still alive.

Bill's Reply

Adrienne suggested that Ruth might be his secretary at High Island. He certainly had a longtime secretary there, and I see that Ruth Bentley is thanked along with his Austin secretary Zelda Davis in the preface of the book *Quantum Theory and Measurement*.

15-07-08 *Paris on the Way to Munich* (to H. C. von Baeyer)

Thank you very much. I reduced the font and the line spacing, and I printed it [your manuscript on the Parisian parks through the eyes of a physicist]! Now I have something to read on our flight to Munich Tuesday. (The family and are staying with Kiki's parents for 18 days ... though I'm taking a desk at the Max Planck in Garching and also slipping off to Freiburg for three days.) I will stay conscious of the absence of Lili's drawings, and be surprised again when I see the final product.

This morning, I found a nice new representation of density operators in terms of SICs. It's embarrassingly simple, but somehow I hadn't noticed it before. Appleby, Schack, and I are working away to try to derive the structure of quantum state space almost solely from the "urungleichung". I think I may have told you about the hope while at the philosophy meeting last month. Everyday, the problem yields a little more. But unfortunately so far, little really means little (it is no hyperbole). Thank God for the integral!

I keep promising more: Eventually you'll get it.

16-07-08 *Title/Abstract for FPP-5* (to A. Y. Khrennikov & G. Adenier)

In case I didn't already send you a title and abstract for FPP-5, let me send you the one below. It's the tweak of a tweak of an earlier abstract (which I may or may not have sent you), and I like it better.

> Charting the Shape of Hilbert Space, or SICkening Axioms of Quantum Mechanics
>
> Abstract: As physicists, we have become accustomed to the idea that a theory's content is always most transparent when written in coordinate-free language. But sometimes the choice of a good coordinate system is very useful for settling deep conceptual issues.

This is particularly so for an information-oriented or Bayesian approach to quantum foundations: One good coordinate system may (eventually!) be worth more than a hundred blue-in-the-face arguments. This talk will motivate and chronicle the search for one such class of coordinate systems—the so-called Symmetric Informationally Complete measurements—which has caught the attention of a handful of us at PI, a handful of our visitors, and a handful of other colleagues around the world. Finally, I will turn the tables and discuss how one might hope to get the formal content of quantum mechanics out of the very existence of such a coordinate system.

18-07-08 *Is Probability 1 Probably Certain?* (to N. D. Mermin)

Merminition 169: *I thought as authorities on $P = 1$ you would like the exchange reported by the AP in this morning's Ithaca Journal.*

> Congressman Gallegly R-Calif:
>
> > Had we not [tortured people] would the probability of another
> > attack not only be a probability but a certainty?
>
> Former Attorney General Ashcroft
>
> > It could well have been.

Funny to hear from you today: Marcus Appleby and I were just having a conversation about you last night! And subject was Mermin, subjectivity, and his dual attractions and distastes of the concept.

But that was a break from other matters. We've been making great progress this week in understanding the "shape of Hilbert space." By that I mean, what density operator space looks like when represented as a subset of the probability simplex. There is serious hope that we can reduce everything to the satisfaction of a single inequality between pairs of probability distributions. This is a program of study Rüdiger and I started a couple of months back and it has continued to surprise us evermore ... by looking ever more right. And the inequality, where does it come from? A direct (but quantitative) expression of the idea that "unperformed measurements have no outcomes." Why the particular quantification (where do the constants come from), I don't know: But it all seems to hang together.

18-07-08 *"Experience", with quotes and without* (to H. J. Bernstein)

Bernsteinism 4: *OK so how come you are not a Whiteheadian? Those throbs of experience which constitute Whiteheadian events (at least as Shimon Malin describes them) are the closest idea I have ever encountered to your famous "Zing!"—attributes of the interaction of quantum stuff with real world observers addicted to classical answers through the force of "white man's" European post Renaissance history, power, hegemony etc etc.*

love out of the blue, herb B

You brightened my evening Wednesday. "Throbs of experience"—I love the phrase, and hadn't made note of it before. It's a deep phrase, isn't it? The quantum measurement mysteries teach us that "experience" can't be ignored as a component of things. But the lessons of the Copernican revolution also teach us that we shouldn't hoard "experience" only for ourselves—"ourselves" being the agents of which the present quantum formalism takes as the localized centers of the universe.

Who knows, maybe I am a Whiteheadian. But I am so slow. Whitehead himself (as he explains at the beginning of *Process and Reality*) takes his starting point in William James and Henri Bergson. That's where I myself am at the moment, but I also recognize that it's only the beginning of a road.

I'm glad you came out of the blue.

21-06-08 *Strawson Quote?* (to me)

From Huw Price at breakfast in Sydney:

> "If you prise off language from the world, you prise off the facts, but you don't prise off the objects." — (or something like that), P. Strawson.

Also look at Ryle – "The Concept of Mind" – relevant to the "a implies b" debate.

28-07-08 *Small Calculation* (to J. I. Cirac)

I think the attached partially answers your question. This is at least one sense in which randomly chosen states are always different from SIC states, even asymptotically.

I enjoyed the style of the questions you were asking today. It was a good dose for me because I do need to think about these things more.

d^2 Random States

Recall the function K I defined in the talk (now specialized to pure states):

$$K = \sum_{i \neq j} |\langle\psi_i|\psi_j\rangle|^4 \,. \tag{83}$$

One has that the states $|\psi_i\rangle$ form a SIC if and only if

$$K = \frac{d^2(d-1)}{d+1} \,. \tag{84}$$

This was the value I exhibited today; I'll leave the proof aside.

Let us compare this quantity with the value that obtains on average if the d^2 states are not SIC, but instead pulled out of a hat according to the unitarily invariant measure. The key ingredient is the value of the integral (which I looked up in one of my papers, and is of course much older and well known):

$$\int |\langle\psi|\phi\rangle|^4 d\Omega_\phi = \frac{2}{d(d+1)} \,. \tag{85}$$

Then

$$\begin{aligned}
\langle K\rangle &= \int\!\!\int \sum_{i\neq j} |\langle\psi_i|\psi_j\rangle|^4 d\Omega_{\psi_i} d\Omega_{\psi_j} && (86)\\
&= (d^4 - d^2)\int |\langle\psi|\phi\rangle|^4 d\Omega_\phi && (87)\\
&= 2d(d-1) \,. && (88)
\end{aligned}$$

The point is, asymptotically min K goes as d^2, whereas $\langle K\rangle$ goes as $2d^2$.

30-07-08 *The Lost Pauli Letter* (to H. Atmanspacher)

Just so I don't forget: Please send me the coordinates of that letter from Pauli to Jung that you told me about today. (The one that Jung published somewhere, but did not appear in the Pauli-Jung correspondence.)

Also, don't forget about the letters to Fierz that you were going to select: Ones on neutral language and neutral monism.

30-07-08 *Seeking Peirce?* (to R. W. Spekkens)

Spekkensism 43: *The collected papers of Charles Sanders Peirce, vols. I – VI. This doesn't happen to be something that you desperately need, does it? They're selling here second hand for 45 GBP for the lot.*

That shows how much Peirce is valued in the UK! If they're in good shape, and it is really *only* 45 GBP for the whole lot (rather than the individuals), then *indeed* get them!!! I would repay you in money, kindness, and brotherly love.

I just looked in my records: Vol 8 alone of the collected papers of Bertrand Russell cost me 20 GBP! (The only reason I bought that one is because he has correspondence in that one concerning pragmatism.)

You are a saint for bringing this to my attention.

03-08-08 *European Tour* (to H. C. von Baeyer)

Good morning. I'm writing you as I take a morning train from Freiburg to Munich, having spent two days with Harald Atmanspacher talking of Pauli, neutral monism, and panpsychism. Actually, we started at his very pleasant home in the beautiful Swiss village of Amden (over Lake Walensee).

Finally I get a chance to write you properly. As I had promised, your *Swing Under the Eiffel Tower* was a companion to me on my flight over. I very much enjoyed it and got a better sense of some of the things I will see and do when Emma and I are in Paris next Spring. (I don't know that I've ever shown you pictures of Emma, by the way. Attached are two: One at the piano just before our leaving Canada, and one taken her first morning in Munich!) I came away from my reading with a sense of envy for your years in Paris, and envying your ability to look at things with a physicist's eye. I think you pulled it off well in your text, mingling these two aspects of your life, neither of which I really have. Frankly, or I should say honestly, I learned some physics from it. Too bad I couldn't have seen Lili's illustrations at the same time as my first reading.

If I didn't think it would only cause more trouble in our relationship, I would send the first two paragraphs in your Introduction to my mother-in-law:

> With this book I return to a question that has been rattling around in the lumber room of my brain for thirty years. Although I have rarely asked it explicitly, the answer informs all of my writing, which the French, with barbed Gallic poignancy, would characterize as *vulgarisation de la science*. The question is this: How does a physicist see the world? As all of us go about our daily tasks, our perceptions of what we see and feel are molded by our professional knowledge in ways we are hardly aware of. Artists, for example, see things in terms of colors and shapes, engineers notice structure, biologists pay attention to plants and animal, and musicians to the

> cacophony of the soundscape they are immersed in. The interest of mathematicians is drawn to numerical and geometric patterns, and that of historians to the traces of the past that are encoded in the way things are arranged in the present. It is said that dogs experience their surroundings as a panorama of smells — in the same way people see primarily that to which they are most sensitive. Accordingly, we physicists perceive the world in the light of the laws of nature that are indelibly etched in our brains. What exactly does that imply about our relationship to the universe? How does it shape our perceptions? In what way do physicists think differently from other people? Many of our non-scientific friends would like to understand what makes us tick, but most of them haven't a clue.
>
> I will try to describe some of the images and associations that flit through my mind as I wander through my daily life. I hasten to admit, though, that generally I am not thinking about physics at all, but about the things everybody else is thinking about too. Today's grocery list, my daughter's dental appointment, the news from Iraq, and the itch on the back of my neck jostle each other in the mundane meanderings of my thoughts — normal human concerns. So my world is infused with meaning not so much by what I *think* as by what I *know* about physics. Newton's laws and Maxwell's equations go with me wherever I go; though they are normally hidden from view, they are powerful in their influence on my world view. I don't think about physics most of the time, but I am aware of it throughout my waking hours. Physics provides the spectacles, as it were, through which I see the world.

They are perfect. She tells me over and over how I need to learn to relax and enjoy life—which of course only means that I do not walk through life in the same way she does. I never think of myself as particularly unhappy—certainly at least when I'm traveling away from the construction at my house!—*except* when I'm being told how unhappy I am. (I unfortunately really blew up at her Sunday morning when she pointed out to me for the hundredth time in only five days how much of life I'm missing. How much of life I'm missing?!) When she sees me as wanting some quiet time to think, or to write, she sees no relaxation in it at all. When she sees me pull out a pad to sketch an equation, then of course that must mean miserable work. ... But I digress.

I'll record some things from your draft that caught my eye while reading.

Les Tuileries. This was my favorite chapter. It struck my sensibilities, I suppose, because of my meanderings in the last two years into the issue of Wigner's friend. Einstein taught us that we each "carry" our own proper time. What does quantum mechanics teach us? In part, I think it teaches us to recognize in a concrete way that we each carry our own "experience". Each "I" is the one who has it; no one else does. The lesson is that there is a public part to the world, but there is also a private part to the world. You say, "[R]elative to the park, I am indeed in motion. But relative to myself, which is to say measured in a reference frame that is attached to my own body, and has its origin at my navel, I am always securely at rest, while the park recedes backward at walking speed. Both these claims, that I am moving relative to one reference frame and stationary relative to another, are correct ..." In quantum mechanics, something like this goes too, though in the opposite direction for its particulars: It's in the agent's own frame where the "motion" so to speak occurs—for that's where the experience is that quantum mechanics gives us the tools for gambling upon.

Some things to read in this regard, in case you're interested. First pick up the latest version of *My Struggles with the Block Universe*. Thereafter, do a search on

1. "Wigner's friend"

2. “Rivers of Nows”

3. “Wootters’ Preply” (that’s actually a note Bill wrote, and probably played some inspiration for my own thoughts)

4. and finally have a look at the quotes of William James in the frontispiece of the document.

In this chapter of yours, I was reminded of how much I would have liked you to have heard Rovelli’s talk in Paris. I thought it was very deep, and certainly within reach of (both from and to) the cluster of ideas mentioned above. I think he misses a lot by trying so hard to never refer to agents and states of belief, but on the other hand, he gives hints of having grasped the personal nature of “experience” (he calls it the “relativity of facts”) in a deeper fashion than I had hitherto.

Parc Monceau. I found your challenge in this chapter inspiring:

> [T]hings can’t go on this way. In 1926, when Erwin Schrödinger was inventing his version of the quantum theory, he wrote to a friend: ‘Physics does not consist only of atomic research, science does not consist only of physics, and life does not consist only of science. The aim of atomic research is to fit our empirical knowledge concerning it into our other thinking.’ We have ignored that wisdom, and remained locked in our secret garden. It’s time we invited the people in. I am confident that in the twenty-first century we will finally heed Schrödinger’s advice, and bring quantum mechanics into the world picture of ordinary people. Creating a more accurate and suggestive atomic logo would be a good start! ...
>
> If I were as rich as the Duke of Chartres, I would sponsor an international competition for the design of a modern atomic logo. The jury would include writers, painters, sculptors, physicists, chemists, and teachers. Somewhere, I feel, there must be a mind creative enough to come close to translating the well-understood mathematical language of quantum mechanics into visual terms ...

I will be your publicist, as well as taking my own shots at entry! First though, a few thoughts, as the challenge simmers in my brain. It’s not quite a public slogan yet, or an image that can be drawn on paper, but it is starting to strike me that perhaps the driving idea behind quantum mechanics might just come down to a single inequality. Schack, Appleby, and I have been calling it the “urungleichung”. And it, to my mind, finally starts to give an image to Hilbert space. It is something like a very symmetric spider web stretched over a sphere.

Let me tell you a little bit about it for the fun of it (and for practice). The story goes back to three transparencies I used in Paris last month. Why should I believe Asher Peres’s phrase, “Unperformed measurements have no outcomes”? And how might I turn it into a quantitative statement? Until recently, I believed that the real essence of Asher’s slogan was in the Kochen-Specker theorem and, better, a variation of Allen Stairs’ use of it (which Carl, Rüdiger, and I review in our last paper together). But now I hope/believe it can be traced back to a deeper level, or at least to a deeper quantitative statement in overtly Bayesian terms.

In a separate email, I’ll send you scans of three of my transparencies, and a little LaTeX document besides (some working notes from my collaboration with Rüdiger and Marcus). The essence of the story is in the diagram. It asks us to compare an apple and an orange. “What probability we should assign for the outcomes of the lower right measurement device *if* we’re going to take our quantum system directly to it.” –Versus– “What probability we should assign for the outcomes of the lower right measurement device *if* we first send the quantum system through the measurement device in the sky.” Apple versus orange. Well, we know how to calculate the orange in terms of the given data in the diagram. We make use of the “law of total probability”, which has a perfectly sound Bayesian derivation in terms of de Finettian coherence for *conditional bets*. But how to calculate the apple? There are no conditional bets in this scenario. A Bayesian not steeped in quantum mechanics would say, "Well then, there's no reason to be probabilistically coherent with any conditional betting. The probabilities in apple are stand-alone entities; they are priors in the proper sense, tied logically to nothing else." That "apple DOES NOT EQUAL orange" is a raw statement that unperformed measurements have no outcomes—unperformed SIC measurements at least. This, any Bayesian who has understood de Finetti's Dutch-book derivation of the connection between conditional and joint probabilities should appreciate.

But quantum mechanics says something that surprises even the well-prepared Bayesian. Though it certainly re-enforces the raw statement above, in the end it *does* let us compare the apple to the orange: For there is a precise relation between the two. Not equality; but seemingly a simple linear relation. Who ordered that?!?!?1

Well, I do not know. But that simple relation seems to contain within it much, if not all, of the formal structure of quantum mechanics. Particularly, by demanding that apple be between 0 and 1, one is forced to constraints on the probabilities for the outcomes of the SIC-in-the-sky measurement. We do not yet know whether the resulting space of allowed probabilities is completely equivalent to the standard quantum mechanical set, but it certainly shares many features with it. And we are guardedly optimistic that it will eventually yield the whole structure. The Introduction to the LaTeXed notes defines the problem more precisely than I have done so here and lays out the tools for getting to the image I spoke of previously.

Coming back to your challenge and the diaphanous web. We start by constructing the extreme points of the convex set it will specify. The inequality says that they will lie on the surface of a d^2 dimensional sphere of a specific radius. Now, we get the rest of the state-space structure by removing points from the sphere. Go to any valid point, the urungleichung tells us, and its antipode will not be in the set. Moreover, a spherical region surrounding the antipode must go too: It must be excised from the original sphere. The result of all this pruning, if our hope turns out to be correct, is a diaphanous manifold of dimension $2(d-1)$ living at the intersection of all these excisions.

Maybe not an emotive image yet. But one that, in some deep way, carries the meaning of quantum mechanics.

But going back to your chapter: Is the phrase "put paid" a typo? Or rather an English phrase that I do not know?

Parc Montsouris. This passage struck me:

> Great revolutionaries don't stop at half measures if they can go all the way. For Newton this meant an almost unimaginable widening of the scope of his new-found law. Not only Earth, Sun, and planets attract objects in their vicinity, he conjectured, but all objects, no matter how large or small, attract all other objects, no matter how far distant. It was a proposition of almost reckless boldness, and it changed the way we perceive the world.

For you are right: That was indeed an amazingly bold move—and I had not appreciated that before. Put in that light, one looks at Newton in awe.

Jardin du Luxembourg. Of course, I was flattered to find my name in this chapter. It teaches me it is good to hobnob with good writers! But how does one translate "Chris Fuchs (rhymes with books)" into French?! On my trip to Amden from Munich the other day, I passed through a town Buchs, and was quite surprised when the conductor announced it (just before I had looked out the window). Finally I learned another German word rhyming with my name!

Contentwise, I liked the paragraph on Zeilinger's phrase "the two freedoms". For I think I agree at some level that the secret of quantum mechanics is to be found in the tension between the two freedoms. I have certainly said things along those lines too at selected times in my life. (Where, by the way, does Zeilinger write of the "two freedoms"; I'd like to get the exact flavors of how he says it.) But also, I think the formulation leaves something out. Certainly at least in my own previous versions. For I imagine there is something truly autonomous in the outcomes of quantum measurement. The agent and the system supply the raw materials so to speak (like parents), but the event thereafter is a life of its own. This is why I have been moved at times to call this view the "sexual interpretation" of quantum theory. (Speaking of flavors, you might want to do a search on that phrase too in link.)

Parc des Buttes Chaumont. It's a funny thing, these multiverse and literal-landscape worldviews. At the surface, the arguments behind them must be nonsense—no deeper or more scientific than the story I tell of the priest in the section titled "Cash Value" (17-11-05 note to H. Halvorson). At the level these guys speak of, we experience one world, but, the argument goes, this world can only make sense (or even exist at all) if it is literally one among many. Yet so many of our colleagues are ready to accept such a statement out of hand, feeling that they are driven to it by science itself.

Parc George Brassens. There are two typos in the penultimate paragraph: "fields are the as essential" and "power of the basic idea not been compromised".

I hope you are enjoying your summer in the parks of Paris! It wouldn't do to stop your time in them now that your book is finished!

Hans's Reply

> "Fuchs se prononce comme foucs." Unfortunately fou means crazy.

11-08-08 *Too Exhausted* (to K. Martin)

Martinism 1: *By the time I convinced the security guy at PI to give me a key to my room, I was also tired . . .*

Did you know that in the room I am in they have some books under the tv for casual reading – one of them is a book that you wrote!

No, I did not know that. I had always wondered what happened to the two copies I sent to PI years ago. I have noted that they are not in the library catalog. But it might just be someone's personal copy that they didn't care to keep. There's not a dedication on the first or second page to someone, is there?

11-08-08 *The Two Freedoms* (to H. C. von Baeyer)

That paper? Actually I'm quite familiar with that one: I liked it very, very much. Here's what I have quoted in my Pauli'an Resource file about it:

> **From:** A. Zeilinger, "On the Interpretation and Philosophical Foundation of Quantum Mechanics," in *Vastakohtien todellisuus, Juhlakirja Professori K. V. Laurikaisen 80-vuotispäivänä*, edited by U. Ketvel, A. Airola, R. Hämäläinen, S. Laurema, S. Liukkonen, K. Rainio and J. Rastas (Helsinki University Press, Helsinki, 1996), pp. 167–178.

When investigating various interpretations of quantum mechanics one notices that each interpretation contains an element which escapes a complete and full description. This element is always associated with the stochasticity of the individual event in the quantum measurement process. It appears that the implications of this limit to any description of the world has not been sufficiently appreciated with notable exceptions of, for example, Heisenberg, Pauli and Wheeler. If we assume that a deeper foundation of quantum mechanics is possible, the question arises which features such a philosophical foundation might have. It is suggested that the objective randomness of the individual quantum event is a necessity of a description of the world in view of the significant influence the observer in quantum mechanics has. It is also suggested that the austerity of the Copenhagen interpretation should serve as a guiding principle in a search for deeper understanding.

But I too don't recall a discussion of the "two freedoms" there. Could it instead have been in the little one page thing he published in *Nature* a couple of years ago?

Hans's Reply

In my frustration I googled "the two freedoms zeilinger" and the first entry hit paydirt:

`http://www.signandsight.com/features/614.html`.

11-08-08 *The Two Freedoms Found* (to H. C. von Baeyer)

Cool! Our emails just crossed each other in mid air! Synchronicity! Synchronicity! I'm not lying your note arrived just as I had hit "send" on mine!

13-08-08 *Reflection* (to V. Hardy)

At the end of our conversation outside the restaurant last night, I sensed that I had crossed the line of acceptable teasing. However, even if the line would not have been crossed, I find this morning that I very much regret the things I said. Birth is to be respected, not feared, and my behavior was no better than a schoolboy's in a playground. In honesty—without the teasing now—Dina's assessment strikes me as about right. But more importantly, and the thing I think one should keep in mind through these months up to the moment, is the flipside. I remember how Charlie Bennett's wife, Theo, put it once: "The births of my three children were the three most intense moments of my life. I was, and have never been since, so alive." It is something men never feel, except in the abstract, and in this way, I know that women's lives are more complete than ours can ever hope to be.

Just wanted to say that …

13-08-08 *The Train* (to K. Martin)

Your note did take me back to the South, and brought to mind those Hank Williams words:

Hear that lonesome whippoorwill
He sounds too blue to fly

The midnight train is whining low
I'm so lonesome I could cry

Notes for my (some future day) write-up of the Christian-Snyder morning coin toss story are below. [See 14-12-07 note "No Subject" to C. Snyder.]

14-08-08 *Physical Theories as Women* (to G. L. Comer)

If you haven't seen this before, have a look: "Physical Theories as Women," by Simon Dedeo, `http://www.mcsweeneys.net/links/lists/physical.html`. I love the description of quantum mechanics particularly:

> Quantum mechanics is the girl you meet at the poetry reading. Everyone thinks she's really interesting and people you don't know are obsessed about her. You go out. It turns out that she's pretty complicated and has some issues. Later, after you've broken up, you wonder if her aura of mystery is actually just confusion.

No wonder I've never been able to get her off my mind.

14-08-08 *Possible Collaboration?* (to T. Duncan)

Duncanism 3: *I've been taking some time this summer to reflect ... kind of connecting the dots to see the theme that drives and underlies my work, so that the next phase will stay on track and help develop this core theme further. I've started using the phrase "Looking for meaning in the modern scientific universe" to describe what I see myself doing. Everything I find myself working on ties somehow to the question, "Is there a perspective or world view one can hold which satisfies the longing for meaning/purpose to our lives that we feel, which is also accurate as far as we can tell (i.e. consistent with all of our current scientific understanding of the world)?" For some reason I still feel rather squeamish about revealing this theme for my research to other physicists ... afraid I won't be understood or won't be taken seriously, perhaps? But from some of our conversations, and rereading your collection of papers and notes, I suspect it's a line of thinking you might share or at least be sympathetic to. Anyway I may have an opportunity to build a more formal collaboration around this theme, so I wanted to share a bit of what I'm thinking and see if you'd like to be involved in the discussions or collaboration in some way.*

A few months ago I submitted a proposal for a research program and book on the theme of "looking for meaning in the modern universe" to the U of Chicago Arete Wisdom Initiative. In the end my proposal wasn't funded, but I was told by the review panel that it was important work and my proposal made it quite far in the process. They encouraged me to submit the proposal directly to the Templeton Foundation. So I'm in the process of reworking and possibly broadening the proposal to submit to Templeton. I've attached a very rough draft of the letter of intent. Please take a look when you have a chance and let me know if you have thoughts, and if it looks like something you'd want to be involved in.

Thanks. I'll look at the proposal, and try to write you with any thoughts soon. There's not a chance you'll be at the Växjö meeting at the end of next week, is there? ...

Actually this is very interesting. Plus I have always been impressed by your thoughts in this regard. Let me think for a few days about how I might fit in. What were the sorts of things you were envisioning for my role?

15-08-08 *Free Will and Renouvier* (to R. Schack)

Attached is the latest version of my quote collection. The thing to do is just search on the term Renouvier (just to make sure you miss nothing). But in particular, that will eventually take you to page 101, Logue's discussion. Then, starting on page 105, you will find Long's discussion. Funny coincidence that they are back to back. Then there is some discussion by Menand on pages 111 and 112. You can find a list of Renouvier's original works on page 158.

Now about Free Will more broadly, you can do a search on that term too. I myself like James's discussions on pages 67-68 and 78-79.

Hope this helps.

W. Logue, *Charles Renouvier: Philosopher of Liberty*, **(Louisiana State University Press, Baton Rouge, 1993):**

> The discussion of political freedom and human rights had become a major theme in Western thought only beginning in the seventeenth century, while the theological and philosophical problem of the freedom of the will, whether limited to the question of salvation or more generally understood, had a long history in Western thought, had indeed been one of the perennial big questions of philosophy.[135] The question of the relations between this freedom of the will and the more concrete issues of political freedom and individual rights had rarely been examined, if only because political and individual freedom were rarely issues before modern times. But even when they became issues, few saw any need to connect these practical questions with the abstruse debates of philosophy.
>
> Unreflective opinion had two choices: Either there was no connection between free will and public freedom, or there was some direct but not very clear connection between the two. The former attitude, common from the beginning of the nineteenth century, enabled thinkers to be simultaneously defenders of political freedom and exponents of deterministic and materialistic philosophies that denied free will. This was a secular counterpart of the traditional Christian attitude that emphasized free will for reasons of practical morality but could not reconcile it with the theological determinism of an all-powerful, all-knowing God.[136] Few thought that there was any serious conflict, and still fewer imagined that the diffusion of deterministic philosophies would serve to undermine the cause of political and individual liberty.
>
> Renouvier was, I think, the first, at least the first major philosopher, to see clearly into this danger and to devote himself to drawing attention to it while proposing a remedy. The development of his neocriticist philosophy was itself a response to what he had come to see as the anti-liberty forces dominant in nineteenth-century thought. Under the influences of discussions with his friend Jules Lequier, he became convinced of the reality of human free will and its central importance for the understanding of everything else.[137] This conviction came to Renouvier while he was still deeply under the

[135] *Deuxieme essai*, xiv-xv. Free will was a problem in religion before becoming one in philosophy but a problem most Christians avoided simply by believing in free will and predestination simultaneously. Greek philosophy, the first to give this question nonreligious consideration, was inclined to psychological determinism, though there were, Renouvier believed, some defenders of freedom—Aristotle, Pyrrhus, Epicurus (*Esquisse*, I, 227-28, 238, 240-43).

[136] Esquisse, I, 249-50; see also I, 248-51.

[137] Renouvier's account of his "conversion" to free will is in the last part of Vol. II of the *Esquisse*. Lionel Dauriac ("Les Moments de la philosophie de Charles Renouvier," *Bulletin de la société francaise de philosophie*, IV [1904], 23) defined the high point of Lequier's influence—the writing of the *Deuxième essai*—as one of four "moments" in Renouvier's philosophical development.

influence of his first contact with the Saint-Simonians. He experienced, not an overnight liberation from their deterministic viewpoint, but a more gradual readjustment of his views, which became a complete detachment from them only after 1851. Perhaps the failure of the socialist movements in 1848, rooted as they were in the would-be scientific philosophies of the preceding three decades, finally persuaded him of the dangers of rejecting free will.[138] Alienated from political life during the Second Empire, he would spend nearly two decades in the construction and elaboration of his philosophy of liberty, establishing its foundations and exploring its consequences.

Renouvier was aware that for a long time the question had been of mainly religious significance: whether man's salvation depended on free will or on predestination.[139] This debate had reached its peak, in both vehemence and subtlety, in the famous exchange between Erasmus and Luther in the sixteenth century. The emergence of a secular debate over free will was a result of the rise of the scientific worldview in the seventeenth century. The ascendancy of the idea that the world was governed by invariable laws, taking the role previously occupied by an all-powerful, all-knowing God, seemed to leave less and less room for the view that man was somehow an exception to the general rule. The most heroic task for the modern philosopher was to find a means of validating science and free will simultaneously, and the most heroic effort of the eighteenth century was that of Immanuel Kant. But for many in the next century, it seemed that Kant had saved free will only at the cost of making it irrelevant.[140] Fichte tried to rescue Kantian philosophy from this unhappy outcome, but in the general opinion ... his effort led to the fairyland of absolute idealism, denying reality to the material world.[141]

Against the rising tide of determinism, Renouvier would try to show that Kant could be the launching pad for a defense of free will that would maintain its practical relevance and demonstrate its compatibility with natural science, properly understood. He did not claim to be presenting any new arguments in favor of free will; he felt they were in any case unnecessary.[142] Renouvier's reasons for coming to the defense of free will were partly shared with Kant and partly his own. As we have seen in the previous chapter, the shared part was the most familiar: a concern for the connection between free will and moral behavior. Free will was for Kant the essential basis of *practical reason*; without it, the whole idea of moral obligation ceased to have meaning. For Renouvier, this consideration remained central. Without moral responsibility, man would not be distinct from the rest of the animal kingdom, and the whole of civilization would be meaningless. But this was not the sole basis for his concern with free will, and this additional concern moved Renouvier beyond Kant and Fichte, bringing him closer to our own time.[143]

[138]Mouy (*Idée de progrès*, 43) argues that the disappointments of 1848 played a key role in shaping Renouvier's idea of liberty. For Renouvier, free will came to be seen as the ultimate basis of political liberty (*Deuxième essai*, 551). See *Histoire*, IV, 431, and especially *Esquisse*, II, 382, and *Deuxième essai*, 371*n*1.

[139]Renouvier saw free will as one of the basic concepts of both philosophy and Christian doctrine (*Histoire*, IV, 277).

[140]What does it matter to man if he has freedom in the world of the noumena if his world of phenomena is entirely determined? Renouvier later felt that Kant held on to free will solely for the sake of morals while not really believing in it (*Quatrième essai*, 35–36).

[141]Renouvier praised Fichte as a defender of freedom and criticized him as a mystic (*Quatrième essai*, 46).

[142]Renouvier was concerned to establish a rationalist and not an empiricist view of science. He saw free will as perhaps the main issue dividing the rationalists and empiricists (*Histoire*, IV, 262). He indicated that there had been no new arguments in favor of free will since Kant and Rousseau (*Esquisse*, I, 280).

[143]Renouvier saw Kant's German disciples as having abandoned liberty for determinism, optimism, and pantheism

It is not just the moral aspect of civilization that hangs on the reality of free will, in Renouvier's opinion, but the whole of our intellectual life. Free will is also the foundation on which philosophy and the natural sciences rest.[144] Without free will, our ability to know anything, whether about man or about nature, is fatally undermined. Scientists do not need to believe in free will, and as he knew, they prefer to avoid this sort of question. In practice, they can legitimately do so because in their narrow spheres of inquiry they have developed techniques of investigation that work even when the scientist is unconscious of the fundamental assumptions on which his method rests. But without free will, the certainty of scientific truths becomes illusory; a consistent determinism must lead to a profound skepticism.[145] Renouvier would never despair of convincing the scientists that just as our concepts of right and wrong depend on free will, so do our concepts of true and false. Indeed, without free will, we could not even talk sensibly about things being true or false.

If, as he pointed out, I hold such and such a view to be true and I am determined by forces outside my control to hold this view, the person who disagrees with me is equally determined by outside forces in his position. If these mutually contradictory positions are equally necessary, what grounds can we have for the certainty that either view is the correct one?[146] If our belief that our ideas are determined is itself determined, so is the other person's belief in free will determined. Under these conditions how could it make any sense to speak of one view as "right" and the other as "wrong"? If, on the other hand, our choices are free, I may freely choose to believe in free will or in spite of the apparent contradiction, to believe in universal determinism. Of the four possible positions revealed by this analysis, the only one that can serve as a foundation for a rational certainty in the truth of our beliefs is to freely believe in freedom.[147] But as Renouvier insists, this means that we must give up any pretension to the absolute certainty of our beliefs.[148] The truth of free will cannot be proved so that no rational person can doubt it. It is a relative truth, like all our other truths, but more important because it plants a relativism at the very core of our thought.[149]

Scientists, Renouvier thought, should have no difficulty understanding and accepting this because science is built on an awareness of the conditional character of our knowl-

(*Histoire*, IV, 467).

[144]See *Deuxième essai*, 227.

[145]*Histoire*, IV, 399; *Deuxième essai*, 327.

[146]*Histoire*, IV, 399; *Deuxième essai*, 306–307 (according to Hamelin, *Système de Renouvier*, 242). Necessity destroys truth: "If everything is necessary, error is necessary just as much as truth is, and their claims to validity are comparable" (*Deuxième essai*, 327). For a restatement of his argument that freedom is essential to the certainty of our knowledge, see *Esquisse*, II, 270–74; see also, *Science de la morale*, II, 377.

[147]The four are (1) we are determined to believe in freedom; (2) we are determined to believe in determinism; (3) we freely believe in determinism; (4) we freely believe in freedom. See *Deuxième essai*, 478; *Histoire*, IV, 399; Hamelin, *Système de Renouvier*, 273–74.

[148]"Certitude is not and cannot be an absolute. It is, as is too often forgotten, a condition and an action of man: not an action or a condition where he grasps directly that which cannot be directly grasped—that is to say, facts and laws which are outside or higher than present experience—but rather where he places his conscience such as it is and as he supports it. Properly speaking, there is no certitude; there are only men who are certain" (*Deuxième essai*, 390). For Renouvier's battle against the idea of evident truths, see *Histoire*, IV, 75, 261; certitude is a sort of "personal contract," "a real contract that a man makes with himself" (Lacroix, *Vocation personnelle*, 114).

[149]*Deuxième essai*, 309–10 (according to Hamelin, *Système de Renouvier*, 242). There were, however, "great probabilities in its [free will's] favor" (*Deuxième essai*, 475). "It ought to be a universally accepted maxim that *everything that is in the mind is relative to the mind*" (*Deuxième essai*, 390). Philosophy needs to take into account the existence of disagreement among philosophers (*ibid.*, 414). Renouvier's approach to the existence of these disagreements is one of the distinctive features of his philosophy.

edge, an openness to the discovery of new truths and the abandonment of old ones.[150] In fact, he had to admit, many scientists were still under the sway of older metaphysical conceptions of truth, except in the conduct of their personal research, and were unaware of any inconsistency in their position.[151] Some who were aware were evidently afraid that to admit that an act of belief was at the base of scientific knowledge would risk undermining the claim of science to objectivity and, even worse, open the way to the proliferation of pseudoscientific beliefs.[152] In reality, pseudoscientific beliefs were already proliferating under the aegis of the belief in determinism. Without a critical analysis of the nature and limits of scientific knowledge, however, our intellectual life is subject to a constant abuse of the name and prestige of science.

The abuse of science takes many forms: the application of research methods to fields where they do not apply, the application of particular concepts to areas other than those where they originated, the confusion of "Science" with the operations of particular sciences. One of the main intellectual trends of the nineteenth century, which Renouvier called *scientisme*, usually rendered as "scientism" in English, was the product of this abuse. Renouvier's relativism does not justify believing in whatever we want to believe.[153] It insists on submitting our opinions to every possible test of logic, experiment, and experience. But we have to admit that our logic, the hypotheses on which our experiments are based, the schemas of thought by which we interpret our experience, all rest ultimately on acts of belief and not on absolute certainties.[154]

If free will is thus essential to both morals and science, just what does he mean by it? Over the centuries, most of the debate over free will has failed to advance our understanding because of the lack of agreement about what is meant by the term. I cannot solve that problem, but I think we will see that Renouvier's view makes the issue more comprehensible.

Free will, for Renouvier, is a capacity possessed by human beings, and only by human beings, that enables them to choose whether to accept one idea or another, whether to perform one act or a different one. It is thus a rejection of the doctrine that holds that all events, mental or physical, are absolutely determined and cannot be other than what they are.[155] Free will is also a rejection of the doctrine of chance, for it is an active power and not the "liberty of indifference" so belabored by determinists.[156] Chance is also hostile to liberty, since it denies man a real power of decision.

The existence of free will requires a measure of indetermination in the universe but could not exist if nature were essentially indeterminate.[157] Our acts of free will are the

[150] On the use of hypotheses in science, see *Premier essai*, 200.

[151] Renouvier credited the English empiricists, following Hume, with freeing science from the metaphysical concept of cause (*Histoire*, IV, 273).

[152] This is the concern of Parodi (*Du positivisme à l'idéalisme*, 184–85), who finds in Renouvier a dangerous fideism. So does Brunschwicg (*Progrès de la conscience*, 625). For Renouvier's praise of Boutroux's argument that the contingency of the laws of nature is not a threat to science, see *Histoire*, IV, 673–74.

[153] Dauriac ("Moments de la philosophie," 30–32) strongly makes this point. It would be interesting to compare Renouvier's conclusion on this point with the similar view expressed by Richard Rorty, coming from a rather different direction.

[154] *Histoire*, IV, 692.

[155] See definition of free will in *Histoire*, IV, 337; on liberty as choice, see *Deuxième essai*, 466; on real alternatives, see *ibid.*, 339. Renouvier is rejecting a causal necessity, not analytic necessity, as in the syllogisms of logical operations (*Premier essai*, 232–36).

[156] *Deuxième essai*, 330–34, 336, 337; Hamelin, *Système de Renouvier*, 242–43,249.

[157] "Liberty does not require the complete indetermination of particular future events, even of those that are directly connected to it" (*Deuxième essai*, 459); see also *ibid.*, 357; Hamelin, *Système de Renouvier*, 244.

> beginnings of chains of consequences and would have no meaning if their consequences were not subject to cause and effect. "Free acts are not effects without causes; their cause is man, the ensemble and fullness of his functions. They are not isolated, but are always closely attached to the preceding condition of the passions and of knowledge. *A posteriori* they seem henceforth indissoluble parts of an order of facts, although a different order was possible *a priori*."[158] The laws that permit us to say this is followed by that do not admit of an infinite regression into the past, according to Renouvier. Therefore, every series of phenomena—and indeed the existence of any phenomena—must have a beginning that we cannot explain in terms of antecedents.[159]
>
> The act of creation of the universe is thus replicated (in a much smaller way!) in every act of free will. Every act of free will is the creation of a new series of phenomena, a series that would not otherwise have existed.[160] These new chains of cause and effect are not simply the product of the intersection of existing but independent series, as A.-A. Cournot argued, for such intersections, though they appear random from the point of view of any one of the colliding series, would be necessary from a higher viewpoint.[161] They must be new beginnings, arising from a conjuncture in which, given the antecedents, more than one consequence was possible: "ambiguous futures," Renouvier called them.[162] Free will is the capacity to opt for one or another of those futures.

and

> Renouvier's assault on the optimistic doctrine of inevitable progress was carried out with both philosophical and historical arguments. The philosophical arguments were probably the most conclusive for him, but he understood that it is possible to differ over philosophical positions. What he found especially difficult to understand was how anyone could look at human history and still believe in continuous and automatic progress. The great system builders were also great oversimplifiers: "Hegel and Comte, as well as Bossuet and Vico, treat history the way Eudoxus and Ptolemy treated astronomy, with their ideal spheres." Rather than looking at all of history, most empirical arguments for progress lean heavily on the history of Western science or in more recent years the advance of material comforts or life expectancy. The progress of the sciences in the past has also favored growth of the illusion that science will in the future be able to solve all our problems.
>
> In the nineteenth century there were also some who argued the reality of moral improvement as well as an intellectual advance, and especially the superiority of the nineteenth century over its predecessors. Renouvier's measure of progress was the level of individual freedom, certainly a good measure from which to criticize the belief in

[158] *Deuxième essai*, 359; see also *Science de la morale*, II, 361–62.

[159] *Premier essai*, 237; *Science de la morale*, II, 360–61. Most scientists today reject the idea that infinite regression is an absurdity; *Esquisse*, II, 378–79. We cannot explain beginnings because they are by definition at the limits of our possible knowledge.

[160] *Esquisse*, II, 196-97.

[161] A.-A. Cournot, *Considérations sur la marche des idées et des événements dans le temps modernes* (Paris, 1973), 9–10, Vol. IV of Cournot, *Oeuvres complètes*, ed. André Robinet.

[162] *Deuxième essai*, 210. "The real indetermination of various phenomena envisaged in the future" (*Premier essai*, 240). "[A determinist] would renounce everything called reflection and reason, for these functions do not work without the consciousness of a *representative self-motivation*, which is itself linked to an awareness of the *real ambiguity of future conditions* before it takes action" (*Histoire*, IV, 769). See also *Troisième essai*, xlvii; Hamelin, *Système de Renouvier*, 230.

inevitable progress but not an easy one to apply in any precise way. Unlike many of his contemporaries, Renouvier was not convinced that individual freedom had made great strides in the nineteenth century, but what worried him most was the illusion that the gains made were secure and that further expansion was certain.

The principal reason why there could be (and had been occasionally) progress was the same reason there could be no inevitable, necessary progress: free will. Even if, as Renouvier admitted, most of the decisions men make are psychologically, socially, or otherwise determined, the presence of even a few free acts prevents any long-term necessary process of development. Free will also makes possible not just failure to progress but also regression, and he found enough historical examples to support his conclusion. Modern optimism about progress, he observed, took root in the eighteenth century, but its most celebrated advocate, Condorcet, did not consider it absolutely inevitable, nor did he hold regression impossible. The domination of historical determinist views in the nineteenth century seemed to Renouvier an abusive extension or corruption of more realistic eighteenth-century views. In philosophy, the main culprit was Hegel, though most of the main schools were also guilty.

W. H. Long, *The Philosophy of Charles Renouvier and Its Influence on William James*, **Ph. D. thesis, Harvard University, 1927:**

In 1834, at the age of nineteen, Renouvier entered l'École Polytechnique in Paris ... Here at the École he met two fellow-pupils, Felix Ravaisson and Jules Lequier, destined to contribute significantly to the life of Renouvier. Ravaisson, in his *Rapport sur la philosophie francaise au XIXe siecle*, was one of the first to recognize the neo-criticist as one of the great thinkers of the age. Jules Lequier, an original mind but too sensitive of imperfection to produce published works, was a direct influence in developing in Renouvier that psychological and ethical voluntarism which came to have such a dominant role in his thought, and hence indirectly also an important influence in the thought of William James.

Jules Lequier was born on January 30, 1814, at Quintin (Côtes-du-Nord), and, having studied at various colleges, entered 1'École polytechnique in 1834. Having remained here for two years, the young man entered military service. In 1838 occurred the death of his father, inspiring in the son an impassioned sense of the problem of life and of reality. Resigning from military service, he gave himself up to an intense study of philosophy, passing through a critical spiritual and mental struggle, winning at last a faith in freedom and the divine destiny of man. Strangely enough, Lequier remained a loyal Roman Catholic through life—"la faiblease de ce grand esprit," in Renouvier's words. He was in spirit, however, protestant, insisting upon the authority of the primitive tradition rather than the unique Roman point of view.

It was an unfortunate loss to the world when Lequier, modern Pascal, burned most of his manuscripts. His sensitive spirit was too keen to imperfection, and too restless to achieve the ideal formulation of his thought, to allow him to produce written work. At his death, in 1862, there remained only some few scattered collections of notes, the major portion of which Renouvier, faithful friend and loyal to the name of one whom he called his master, published in 1865 under the title of *La recherche d'une primière vérité, fragments posthumes de Jules Lequier*. Renouvier's loyalty and devotion is quite striking, for he acknowledges in practically all of his works his indebtedness to his friend, and reaffirmed on his death-bed this debt to the mind which had stimulated in him the voluntaristic point of view in speculation.

For example, a noteworthy statement by Renouvier in the introduction of his *Manuel de la Philosophie Ancienne*, written in 1844, is of value in the history of pragmatic voluntarism. He writes that

> Il est surtout une chose que je ne dois poing négliger de dire: de longs entretiens sur les questions fondamentales de la métaphysique avec un ami qu'il n'est pas temps encore de nommer, et qui en a fait depuis plusieurs années l'objet habituel, de ses réflexions, ont imprimé quelquefois sur mes idées les traces des siennes. Ces communications mutuelles ont été, malgré les différences profondes qui nous séparent, favorisées par un accord frappant dans quelques-uns des principes de la haute logique. Accoutumé, comme il l'était lui-même, à donner une grande place à la croyance dans les fondements de la science, j'ai mis à profit des analyses sur la foi; sur la liberté, sur l'intervention de l'idée de liberté dans celles du savoir et de la certitude, qui sont, pour lui, le résultat d'études sérieuses et de méditations suivies, et qui joueront un rôle important dans ses travaux: la publication n'en tant pas prochaine, c'était une raison de plus pour constater ice que les vues generales qui les dominent lui sont tout à fait personnelles.

Elsewhere Renouvier writes concerning the influence of Lequier upon his doctrines of freedom, certitude, and belief, that he owed to his friend and master,

> tout ce qui concerne d'une manière essentielle, dans mon livre, l'établissement de la liberté et de ses rapports avec la certitude, à un philosophe, M. Jules Lequier .... Il ne dépend pas de moi de donner ... satisfaction à sa mémoire. Je peux du moins reconnaître, encore qu'il soit difficile de l'exprimer en termes vraiment suffisants, l'incomparable obligation que j'ai contractée envers l'homme qui a fait tomber un certain jour l'écaille de mes yeux, qui m'a montré la faiblesse des doctrines dont j'étais l'adhérent, même involontaire, et m'a apris ce que c'est que liberté, ce que c'est que certitude, et qu'un agent moral est tenu moralement de *se faire* des convictions touchant des vérités, dont les penseurs rationalistes ont la mauvaise habitude de mettre la preuve sur le compte de l'évidence et de la nécessité.

For Lequier, in Renouvier's words, a man's philosophy centers around a master thought and an active faith. The true Odyssey of the human mind, according to Lequier, must begin with the Cartesian search for a first verity, for certitude. Certitude requires evidence. What, then, is evidence? "Impossible, évident, légitime, que de rapports mal démêles!" ejaculates this remarkable seeker for truth. What is evidence? That which "est impossible de douter avec bonne foi." But there is apparent and false evidence as well as true. Hence the Cartesian methodical doubt is the only true method; for true evidence and apparence [sic] are not identical. For unknown errors may creep into the most apparent truths. What, first of all, shall we say of the pretended separation of belief and science? What shall determine our choice when the head and the heart, science and belief, are in conflict? "J'aviserai, je verrai: je sacrifierai l'une à l'autre," writes this modern Pascal. "Tu dis vrai, mais je ne veux pas t'entendre; ne l'ai-je pas dit souvent? Ou plutôt je dirai à ma raison: Tu dis vrai et je le voie, mais je ne te crois pas, et m'aidant de ne te croire pas pour m'empêcher de voir que mon coeur me trompe, je trouve que tu as tort et que c'est mon coeur qui a raison. Je préfère la sagesse de mon coeur qui m'élève et me satisfait, à ta lumière qui ne me montre que mon abaissement et mon désespoir. Quand tu affirmes que ce qu'il affirme est évidemment faux pourquoi te croirais-je, puisqu'il affirme que ce que tu affirmes est faussement évident?

"Mais il serait mieux que ma raison et mon coeur eussent raison ensemble."

The search for a first verity, avoiding the snares of prestige and false evidence, ends in a vicious circle, since first truths are begged.

> Les vérités primitives ne peuvent s'établir par l'évidence puisque l'évidence est déductive.

To escape the vicious circle and its accompanying doubt requires an act of will:

> Franchir ce cercle vicieux c'est posséder en quelque façon, c'est créer, c'est faire que ce qui n'était pas soit; c'est faire en moi la lumière, mais la faire en effet ... Agir, c'est commencer. Je le franchis donc en agissant, ce cercle vicieux, dans mon effort qui se produit lui-même; cet effort qui l'instant d'avant n'était pas et qui tout à coup devenant, par lui-même à lui-même sa cause, est, c'est-a-dire s'est produit, s'est fait de rien. C'est là vouloir.

Thus knowledge begins with free act, and destroys the illusory necessity.

Lequier's own spiritual history in similar to that of William James, and it is in fact the former whose doctrine brings the latter out of his slough of despond. The striking mental regeneration of Lequier is so interesting in the history of American pragmatism that it is worthy of quotation. From the whole of the remarkable memoire of this young men seeking freedom the following item is given:

> Une seule, une seule idée, partout reverberée, un seul soileil aux rayons uniformes: Cela que j'ai fait était nécessaire, Ceci que je pense est nécessaire, L'absolue nécessité pour quoi que ce soit d'être à l'instant et de la manière qu'il est, avec cette conséquence formidable: le bien et la mai confondus égaux, fruits nés de la méme sève sur la même tige. A cette idée, qui révolta tout mon être, je poussai un cri de détresse et d'effroi: la feuille échappa de mes mains, et comme si j'eusse touché l'arbre de la science, je baissai la tête en pleurant.
>
> Soudain je la relevai. Ressaisissant la foi en ma liberté par ma liberté même, sans raisonnement, sans hésitations, sans autre gage de l'excellence de ma nature que ce témoignage intérieur que se rendait mon âme créé à l'image de Dieu et capable de lui résister, puisqu'elle devait lui obéir, je venais de me dire, dans la sécurité d'une certitude superbe: Cela n'est pas, je suis libre.
>
> Et la chimère de la nécessité s'était évanouie, pareille à ces fantômes formes pendant la nuit d'un jeu de l'ombre et des lueurs du foyer, qui tiennent immobile de peur sous leurs yeux flamboyants, l'enfant, réveillé en sursaut, encore à demi perdu dans un songe: complice du prestige, il ignore qu'il l'entretient lui-même par la fixité du point de vue, mais sitôt qu'il s'en doute, il le dissipe d'un regard au premier mouvement qu'il ose faire.

Thus by an act of will Lequier asserted his freedom and renounced the power of fate and necessity.

True, Lequier insists, it is impossible to demonstrate freedom; but at the same time it is impossible to demonstrate necessity. In this situation the individual has the right to affirm freedom as a postulate of life, and this affirmed truth receives a new dignity and value, namely in being freely chosen by the individual in a moral act. In the absence of proof there arises a definite dilemma and challenge:

To affirm Necessity necessarily; or

To affirm Necessity freely; or

To affirm Freedom necessarily; or

To affirm Freedom freely.

In this alternative of freedom and necessity, Lequier bids us choose "entre l'une et l'autre, avec l'une ou avec l'autre." He preferred to, choose freedom freely; "j'embrasse le certitude," he declares, "dont je suis l'auteur." And in this he finds the first verity which he sought, namely, "freedom the positive condition of knowledge, and the means to knowledge."

It is thus shown that Lequier was a determining factor in Renouvier's theory of belief, certitude, and freedom, end hence, as it will be shown, the immediate source of William James' "will to believe" doctrine, and his theory of freedom, is to be traced to this little-known modern Pascal, Jules Lequier.

L. Menand, *The Metaphysical Club*, (HarperCollins, London, 2001):

In his own way, and despite Holmes's distaste, William James was a bettabilitarian, too. But he did believe in free will—what would it mean to bet, after all, if we were not free to choose the stakes? He was repelled by Wright's reduction of the world to pure phenomena—he thought Wright made the universe into a "Nulliverse" and he regarded the abyss Wright insisted on placing between facts and values as a fiction. James thought that Wright's decision to separate science from metaphysics was itself a metaphysical choice—that Wright's disapproval of talk about values was just an expression of Wright's own values. Wright was a positivist because positivism suited his character: moral neutrality was his way of dealing with the world—and that, in James's view, is what all beliefs are anyway, "scientific" or otherwise.

Wright was a regular visitor to the James family home in Cambridge long before 1872, and in any case William James did not need the Metaphysical Club to reach his conclusion about the nature of beliefs. He had already arrived there by experimentation on what was always his favorite human subject, himself. When he was living in Germany in the late 1860s, he had got caught up in the speculative frenzy about free will and determinism inspired by Buckle's book. As usual, he found merits on both sides. "I'm swamped in an empirical philosophy," he wrote to Tom Ward shortly after getting back to Cambridge in 1869; "—I feel that we are Nature through and through, that we are *wholly* conditioned, that not a wiggle of our will happens save as the result of physical laws, and yet notwithstanding we are en rapport with reason .... It is not that we are all nature *but* some point which is reason, but that all is Nature *and* all is reason too."

After he took his M.D. from Harvard, in June 1869, James collapsed. He descended into a deep depression, exacerbated by back pains, eye trouble, and various other complaints. His diary for the winter of 1869–70 is a record of misery and self-loathing. Then in the spring, after reading the second installment, published in 1859, of a three-part work called the *Essais de critique générale* by the French philosopher Charles Renouvier, he had a breakthrough.

Renouvier was a French Protestant from a family active in liberal politics, but he had quit political life after the rise of the Second Empire, in 1848, to devote himself to the construction of a philosophical defense of freedom. Renouvier's argument was that "the doctrine of necessity" is incoherent, since if all beliefs are determined, we have no way of knowing whether the belief that all beliefs are determined is correct, and no way of explaining why one person believes in determinism while another person does not. The only noncontradictory position, Renouvier held, is to believe that we freely believe, and therefore to believe in free will. Even so, we cannot be absolutely certain of the truth of this belief, or of anything else. "Certainty is not and cannot be absolute," he wrote in the second *Essai*. "It is ... a condition and an action of human beings .... Properly speaking, there is no certainty; there are only people who are certain."

This was, in effect, Wright without the nihilism, and it was entirely appealing to James. "I think that yesterday was a crisis in my life," he wrote in his diary on April 30, 1870.

> I finished the first part of Renouvier's 2nd Essays and see no reason why his definition of free will—the sustaining of a thought *because I choose to* when I might have other thoughts—need be the definition of an illusion. At any rate I will assume for the present—until next year—that it is no illusion. My first act of free will shall be to believe in free will .... Hitherto, when I have felt like taking a free initiative, like daring to act originally, without carefully waiting for contemplation of the external world to determine all for me, suicide seemed the most manly form to put my daring into; now, I will go a step further with my will, not only act with it, but believe as well; believe in my individual reality and creative power.

As bold as this resolution sounds, it did not release James from his depression. He seems to have been incapacitated by psychosomatic disorders—in particular, an inability to use his eyes for reading or writing—for another eighteen months, and he suffered chronically from depression, eyestrain, and insomnia all his life. Henry's mention of the formation of the Metaphysical Club in January 1872 is one of the first signs, after the diary entry about Renouvier, written a year and a half earlier, that William was socially active again.

Still, James believed that Renouvier had cured him, and he sent him thanks. "I must not lose this opportunity of telling you of the admiration and gratitude which have been excited in me by the reading of your *Essais*," he wrote to Renouvier in the fall of 1872. "Thanks to you I possess for the first time an intelligible and reasonable conception of freedom .... I can say that through that philosophy I am beginning to experience a rebirth of the moral life; and I assure you, sir, that this is no small thing." Renouvier had taught James two things: first, that philosophy is not a path to certainty, only a method of coping, and second, that what makes beliefs true is not logic but results. To James, this meant that human beings are active agents-that they get a vote—in the evolving constitution of the universe: when we choose a belief and act on it, we change the way things are.

- C. Renouvier, *Science de la Morale*, 2 volumes, (F. Alcan, Paris, 1869).
- C. Renouvier, *Essais de Critique Générale. Premier Essai. Analyse Générale de la Connaissance. Bornes de la Connaissance. Plus un Appendice sur les Principes Généraux de la Logique et des Mathématiques*, (Paris, 1854). Second edition: *Essais de Critique Générale. Premier Essai. Traité de Logique Générale et de Logique Formelle*, 3 volumes, (A. Colin, Paris, 1875).
- C. Renouvier, *Essais de Critique Générale. Deuxième Essai. L'homme: la Raison, la Passion, la Liberté, la Certitude, la Probabilité Morale*, (Paris, 1859). Second edition: *Essais de*

Critique Générale. Deuxième Essai. Traité de Psychologie Rationelle, 3 volumes, (A. Colin, Paris, 1875).

- C. Renouvier, *Esquisse d'une Classification Systématique des Doctrines Philosophiques*, 2 volumes, (Paris, 1885-86).
- C. Renouvier, *Essais de Critique Générale. Troisième Essai. Les Principes de la Nature*, (Paris, 1864). Second edition: *Les Principes de la Nature*, 2 volumes, (A. Colin, Paris, 1896).
- C. Renouvier, *Essais de Critique Générale. Quatrième Essai. Introduction à la Philosophie Analytique de l'Histoire*, (Paris, 1864). Second edition: *Introduction à la Philosophie Analytique de l'Histoire, Les Idées; les Religions; les Systeèmes*, (A. Colin, Paris, 1896).
- C. Renouvier, *Histoire et Solution des Problèmes Métaphysiques*, (F. Alcan, Paris, 1901).

18-08-08 *SICs and Partial Orders* (to K. Martin)

Thanks for sending your tutorial. This looks like good stuff. I very much like this idea of a partial order on the probability simplex, and I would like to think about it more. Seeing your example about Grover's algorithm the other day convinces me that there's nothing intrinsically bad about building a partial order on the density operators by introducing a fixed BASIS and then relying on a well-known order on the simplex. Introducing a SIC is doing the same conceptual thing, but the SIC has the added bonus of being informationally complete. Thus all density operators become comparable under it.

Maybe one interesting technical question that could be considered is what properties of the partial order are preserved when one changes from one SIC to another. Maybe it's not the case that everything blows to the wind, but at least some features are preserved.

In any case, it was really good meeting you. And how grateful I am that you're interested in this passion of mine. I genuinely do think that a lot of stuff (even revolutionary stuff) will come out of the study of SICs.

20-08-08 *Want to See a Glacier?* (to D. Gottesman)

Having now gone to Bristol, Sydney, Eugene, Zurich (twice), Paris, and Garching already this year, and leaving again for Sweden next week, I woke up two nights ago with the severe pain of a broken travel bone. I decided I just can't take it anymore. Not for a while at least.

Thus I wonder if you'd be interested in taking my place at this meeting:

> `http://www.uibk.ac.at/th-physik/qics-obergurgl2008/`.

It would be nice for PI to have some representation there, and I think it is a meeting (and a subject) you would enjoy in any case.

If you're interested, let me know, and when I tell the organizers I'm out, I'll ask them to consider transferring the invitation to you. (To be honest, it'd be a very good deal for them.)

20-08-08 *Progress* (to E. G. Cavalcanti)

Cavalcanti-ism 1: *By the way, the argument I gave in Vienna, now that I remembered, was along these lines:*

Suppose you believe that (i) A quantum state is just an encapsulation of your subjective degrees of belief; (ii) The lesson of Kochen-Specker and Bell's theorems are that there are no objective facts to further specify your degrees of belief even in principle, at least in the cases where you assign a pure state to a situation; and (iii) You are no more central to the universe than any other agent.

Then considering a Wigner's friend scenario, assumptions (i) and (ii) imply that before you look into the box where your friend is measuring a quantum system, you cannot assign a reality to the measurement outcome of your friend. However, assumption (iii) makes you believe that your friend <u>*can*</u> *assign a definite outcome, whichever it is. That implies that as far as you are concerned, before you open the box that event of your friend measuring the system never happened. But you believe that relative to your friend it did happen. You conclude that the very existence of 'events' is relative to the observer. The world is not made of a collection of events aspersed in space-time, with well-defined causal relations between each pair of events, and between each event and each observer.*

To reinforce that view, you can devise two situations: in one you open the box and ask your friend "Did you see any outcome before I opened the box?". Of course she'll say your opening the box had no influence on that, and by assumption (iii) you'll believe her. So you do the same experiment, but now you perform a transformation that takes all the contents of the box, including your friend, back to its initial state, a little time after you presume the measurement has been done. The fact that the evolution was fully reversible is evidence of the fact that your friend was still in a coherent entangled state with the measurement apparatus, and therefore by (ii) you believe there's no objective fact of the matter relative to you as to which outcome she observed, and since you coherently reversed the interaction, there never will be. Yet, you still believe that relative to her own previous position as an observer inside the box, she must have observed something before you reversed the whole thing. But that observation is forever outside any causal relation with you or even your current time-reversed friend. In other words, that event is in a sense not in the same space-time as you are now, if by definition every event in a space-time have <u>*some*</u> *causal relation with every other event.*

The only way out of this conclusion, it seems to me, is to accept a hidden-variable interpretation in which there <u>*are*</u> *objective matters of fact about events, we just don't know what they are, or extreme solipsism. But I would agree with you if you say that the alternative above sounds much more exciting.*

It took me a long time, but at least I've made a little progress. I finally read the nice set of notes you sent me. I will work on a response during my flight(s!) to Sweden. But I think you'll be there so we can discuss things anyway.

My main qualm is with what you call solipsism at the end. I instead would call it extreme pluralism. It'll be fun talking again.

22-08-08 *Possible Collaboration?, 2* **(to T. Duncan)**

The role you foresee for me is something I can decently fulfill. As you say, we can bat around ideas—maybe I can become more involved if there are some concrete ideas worth pursuing. I love the way you quote William James's "Sentiment of Rationality". Maybe the right call is his essay on man's strenuous nature. I tell my daughter every day that she can change the world—one can suspect that the world is malleable, as the classical pragmatists did. But that is only reinforced by quantum mechanics—that idea is what drives me. So there is plenty of room to talk.

22-08-08 *The Two Freedoms Found, 2* (to H. C. von Baeyer)

Thanks for that. I read the interview a couple or three days ago, and both enjoyed it and grit my teeth too (at the choice of some phrasings). Getting caught up on email.

... Well I started that much of a note to you earlier (about five days ago) and never finished. But now I'm off to Sweden for one of Andrei's crazy conferences. But it unites Schack, Appleby, and me together for a while and there are some other interesting people there too (Bengtsson, Cabello, Larsson).

My key grit was with the word "property". I think it is dangerous business to say that it is a particle's properties that are teleported. See *My Struggles* starting at the section "Qubit and Teleportation Are Words".

But there was other good stuff in the interview that was really good stuff too. I should come back to an exposition of the positive things.

Let's talk soon again about the Pauli-Fierz project. Have you given it any further thought? Any progress? Or has it been forgotten for the meantime. I'll try to write some words in Sweden (particularly if I see a reply from you).

22-08-08 *Invite!* (to H. Mabuchi)

Thanks for letting me off the hook.

Mabuchism 5: *I'm thinking we should have another go at trying to pitch some kind of Perimeter-Stanford quantum foundations thing ...*

Actually I would like that very much. My grant writing skills are much better than they used to be. And I have a feeling Rob would be keen too. Do you have a time frame on which we should think about working something up?

You tell me you can do an experiment that can map out the weirdities of my extended Bloch sphere, and I'll be out to Stanford soon after Christmas. Actually "Bloch sphere" is in a way a misnomer: We now understand it's much more like a spider web sitting on a sphere. A whole lot of spherical caps are missing, and the states really reside at the intersections (the webbing).

Onward to Sweden; I am just about to land at JFK along the way.

25-08-08 *Quotes* (to A. Plotnitsky)

Please don't forget to send me the quotes from your talk today.

Arkady's Reply

> On one hand, the definition of the state of a physical system, as ordinarily understood [i.e. in classical physics], claims the elimination of all external disturbances. But in that case, according to the quantum postulate [of Planck], any observation will be impossible, and, above all, the concepts of space and time lose their immediate sense. On the other hand, if in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there could be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard *the space-time co-ordination and the claim*

of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively.

— Niels Bohr (1927), *PWNB* 1, pp. 54–55 (emphasis added)

02-09-08 *Real Lasers* (to J. W. Nicholson)

Learning that one of my high-school acquaintances is now involved in this:

> `http://www.thequantumalliance.com/`
> [[at the time the site had some kind of "laser product" for bio-feedback]]

I couldn't help but forward it on to you. Now that's real cutting-edge laser work, isn't it?

03-09-08 *Two (Important!) Things* (to C. Rovelli)

I have been meaning to write you ever since departing Paris, but the usual malaise in writing came about as soon as I left—too much sociality takes a toll on my correspondence. The reason I wanted to write you is because after meeting you, listening to your talk, and listening to the lunchtime conversation you had with Si Kochen, I felt a rather deep connection between several aspects of our individual (i.e., your and my) takes on quantum mechanics. Even the common imagery we both seem to use struck me as a surprise. What particularly intrigues me is the way we've come at this from different directions, but both end up on a radical thought: That even facts (particularly quantum outcomes) are relative. The two directions—and they certainly seem to be distinct—are 1) for you, relationalism more generally, and 2) for me, an understanding of quantum states in terms of Bayesian probabilities. I would not have initially thought of those roads as traveling to the same point, but there is something to think on here. Whereas you speak of "relative facts", we speak of "facts for the agent", but at least in some aspects they are surely the same thing.

I would be flattered if you would read some of my poetry on the idea before we meet and discuss these things again. Particularly, I want to advertise to you the new compilation I'm putting together as a point of departure. You can pick it up here: [See this samizdat, but with updated page numbers.] I hope that you'll find parts of it entertaining and thought provoking. Of interest for the present purposes, you might read a bit around where I discuss this phrase "fact for the agent" (and see how it compares or contrasts with what you are striving for). Here are some points of entry:

1) Page 687, starting at "Replies to Referee 4"

2) Page 650, starting at "Our Professor"

3) Page 646, starting at "My Two Cents"

4) Page 714, starting at "Facts-in-Themselves"

5) Page 716, starting at "Anti-Algebra, the Reprise"

Also, maybe my correspondence with Bas van Fraassen about what I was perceiving as the difference and similarities between my view and yours: See pages 550–557. Also points to Rüdiger Schack on the same (and Wigner's friend), pages 559–564.

That's one important thing. Now, to the next question. When *shall* we discuss these things again? When we parted in Paris, you said pretty firmly (and I don't believe I am making this up), "I will see you in September." Yet, our conference coordinator has not heard from you yet. Please do write and say you're coming. And particularly, because I would like to ask a further thing of you: Would you give the general PI Colloquium that week in fact? The talk you gave in Paris, from my point of view, would be perfect for it—mostly because it would open up to the wider PI community this difficult idea that we're trying to get at, but also because it was a simply beautiful talk.

Will you be around tomorrow or Friday? I would very much like to call and talk about this (Lee gave me your number): A colloquium from you would be great for PI.

03-09-08 *Er Er* (to D. M. Appleby)

Applebyism 21: *A conference on multiverses I could cheerfully miss. It is strange: I am not a Bohmian, but the Bohm theory never annoys me. Quite the contrary. I don't believe it, but I think it is an interesting hypothesis. I also enjoy talking to Bohmians—Owen is just the latest in a long list. But when it comes to multiverses I feel strong aversion. It strikes me as, not so much a theory, more an intellectual disease. Perhaps even a moral and spiritual disease: for if you think that everything happens it seems to me that you cannot help ending up with the belief that nothing really matters. Everett et al. trivialize the world, and perhaps for that reason I don't think I can even enjoy talking to an Everettian. At least, I can't think of an occasion when I ever have.*

Regarding your first paragraph. There was an interesting passage in the conversation where Hilary Greaves had David Albert draw a tree structure on the board, and then David Albert started to explain the idea of an "indexical fact" to Paul Davies. Davies, said something like, "So some branches are lit up because they contain rational agents; but some branches aren't lit at all." Albert protested about the idea of some branches being lit up by the presence of an agent—i.e., that that's not what he was getting at. But the funny thing was that Hilary, who was sitting beside me, went further. I overhead her say to herself, "Yeah, that *would* be crazy." I think that fares well with your assessment below.

03-09-08 *Your Old Thesis* (to G. Valente)

Could you send me a copy of the thesis you wrote after studying in Oxford. I don't have a copy of the final thesis, and recently I bought the Musil book—I'd like to review what you said about him (or which quote you used of his) in connection with QM.

Giovanni's Reply

> Glad to hear from you! Funny that you write to me because I'm in Paris at the moment… and I just got an unexpected e-mail from that Croatian guy who wrote his thesis in Oxford on your work five years ago…
>
> Unfortunately, I don't have a file with my master thesis anymore. It got lost when my old PC collapsed. I may have a disk where I saved the file at home in Italy and I certainly have a paper copy of the work there, but there's nothing I can send you right now. In any case, I recall that I made a reference to Musil only in the introduction, starting out by quoting the passage below which is taken from the section "If there is a sense of reality, there must also be a sense of possibility" (it's one of the first in the

book). According to my original quantum reading, it conveys the idea that the world is under construction, as a pragmatist would put it; that the realists (e.g. Ghirardi, Albert, Maudlin, etc.), who interpret the quantum state as a state of reality, would regard the possibilists (e.g. Chris Fuchs), who interpret the quantum state as a state of belief, as dreamers or troublemakers...; that all we're given is a bunch of possibilities, namely a set of probabilities, but the latter are of course determined by reality, namely by the outcomes of measurements. I think at the end I even joked that, in connection with the Bayesian interpretation of QM, in quantum theory we're really dealing with "systems without qualities", as we can't say anything about them before a measurement.

I'm sure you'll enjoy the book. Perhaps it'll awaken the deep Mitteleuropean spirit that harbours in your soul. I myself was thinking of reading it again soon.

From *The Man without Qualities* by Robert Musil:

> To pass freely through open doors, it is necessary to respect the fact that they have solid frames. This principle, by which the old professor had lived, is simply a requisite of the sense of reality. But if there is a sense of reality, and no one will doubt that it has its justifications for existing, then there must also be something we can call a sense of possibility.
>
> Whoever has it does not say, for instance: Here this or that has happened, will happen, must happen; but he invents: Here this or that might, could, or ought to happen. If he is told that something is the way it is, he will think: Well, it could probably just as well be otherwise. So the sense of possibility could be defined outright as the ability to conceive of everything there might be just as well, and to attach no more importance to what is than to what is not. The consequences of so creative a disposition can be remarkable, and may, regrettably, often make what people admire seem wrong, and what is taboo permissible, or, also, make both a matter of indifference. Such possibilists are said to inhabit a more delicate medium, a hazy medium of mist, fantasy, daydreams, and the subjunctive mood. Children who show this tendency are dealt with firmly and warned that such persons are cranks, dreamers, weaklings, know-it-alls, or troublemakers.
>
> Such fools are also called idealists by those who wish to praise them. But all this clearly applies only to their weak subspecies, those who cannot comprehend reality or who, in their melancholic condition, avoid it. These are people in whom the lack of a sense of reality is a real deficiency. But the possible includes not only the fantasies of people with weak nerves but also the as yet unawakened intentions of God. A possible experience or truth is not the same as an actual experience or truth minus its "reality value" but has—according to its partisans, at least—something quite divine about it, a fire, a soaring, a readiness to build and a conscious utopianism that does not shrink from reality but sees it as a project, something yet to be invented. After all, the earth is not that old, and was apparently never so ready as now to give birth to its full potential.
>
> To try to readily distinguish the realists from the possibilists, just think of a specific sum of money. Whatever possibilities inhere in, say, a thousand dollars are surely there independently of their belonging or not belonging to someone; that the money belongs to a Mr. Me or a Mr. Thee adds no more to it than it would to a rose or a woman. But a fool will tuck the money away

in his sack, say the realists, while a capable man will make it work for him. Even the beauty of a woman is undeniably enhanced or diminished by the man who possesses her. It is reality that awakens possibilities, and nothing would be more perverse than to deny it. Even so, it will always be the same possibilities, in sum or on the average, that go on repeating themselves until a man comes along who does not value the actuality above idea. It is he who first gives the new possibilities their meaning, their direction, and he awakens them.

But such a man is far from being a simple proposition. Since his ideas, to the extent that they are not idle fantasies, are nothing but realities as yet unborn, he, too, naturally has a sense of reality; but it is a sense of possible reality, and arrives at its goal much more slowly than most people's sense of their real possibilities. He wants the forest, as it were, and the others the trees, and forest is hard to define, while trees represent so many cords of wood of a definable quality. Putting it another and perhaps better way, the man with an ordinary sense of reality is like a fish that nibbles at the hook but is unaware of the line, while the man with that sense of realty which can also be called a sense of possibility trawls a line through the water and has no idea whether there's any bait on it. His extraordinary indifference to the life snapping at the bait is matched by the risk he runs of doing utterly eccentric things. An impractical man—which he not only seems to be but really is—will always be unreliable and unpredictable in his dealings with others. He will engage in actions that mean something else to him that to others, but he is at peace with himself about everything as long as he can make it all come together in a fine idea. Today he is still far from being consistent. He is quite capable of regarding a crime that brings harm to another person merely as a lapse to be blamed not on the criminal but on the society that produced the criminal. But it remains doubtful whether he would accept a slap in the face with the same detachment, or take it impersonally as one takes the bite of a dog. The chances are that he would first hit back and then on reflection decide that he shouldn't have. Moreover, if someone were to take away his beloved, it is most unlikely that he would today be quite ready to discount the reality of his loss and find compensation in some surprising new reaction. At present this development still has some way to go and affects the individual person as a weakness as much as a strength.

And since the possession of qualities assumes a certain pleasure in their reality, we can see how a man who cannot summon up a sense of reality even in relation to himself may suddenly, one day, come to see himself as a man without qualities.

04-09-08 *My Own Update* (to D. M. Appleby)

I'll read your longer note in detail tomorrow. One quick point, I used the full phrase "rational agent" in my stories because that was the phrase bandied about in the Greaves/Albert discussion. But I myself don't know what rational is or should mean. The key part of it for me, the part that played any role in my mind at all, is "agent"—by definition, "that which activates."

04-09-08 *Need Students* (to H. C. von Baeyer)

I have just learned that I got a US grant with enough funding to support 2 graduate students fully at U Waterloo through 4 years each. It's of no great use however for good Canadian students, as they will already have their own NSERC funding. Thus I figure it's most wisely spent on really good non-Canadians who want to be up here in the Mecca of quantum info/foundations. So, I'm writing to see if you have any good students that might be interested? The work I'm hoping to emphasize is, of course, "How come the quantum?", tackled from a quantum-information/Bayesian perspective.

Any leads you can give me would be most appreciated.

08-09-08 *Progress, 2* (to E. G. Cavalcanti)

That was helpful. But, OK, I give up on trying to reply to you: Too many things that I unfortunately *have to* write are getting in my way, and in any case, I will see you soon enough.

See you at *The Clock and the Quantum*.

Eric's Preply

> I guess I didn't express myself very well in those end remarks. I should have said that the argument *seems to lead* to the conclusion that either there are hidden variables or to an extreme solipsism. But when I say I agree with you that the "alternative above" sounds more exciting, I mean exactly the alternative which your research program is trying to find, the alternative which we hope will arise out of realising the full significance of the remark from Pauli, that the observer cannot be separated from the things it observes.
>
> Extreme pluralism is probably a better name for that worldview. However, I also have the strong hunch that this pluralism is of a form which is so beautifully symmetric that one cannot avoid the feeling of an unity behind the different perspectives or worldviews. The reason why we seem to be able to glimpse an order, a pattern behind these different perspectives is probably because behind the apparent pluralism stands an unifying simplicity. I think the search for the basis of this unification is the next step after the recognition of the pluralism.

09-09-08 *Rationalizing Agent* (to D. M. Appleby)

I've now read both of your notes on the subject. The short answer is, yes I do agree with you. I like the way you put this:

Applebyism 22 : *If the word "rational" is understood in that way then I don't believe a rational agent is capable of lighting up anything. Not even itself.*

The point is, the light—the will—comes first (or at least has a significant priority I think). Not only do we see something like this in alchemistic strains of thought, but it is a good piece of James and Dewey's psychology.

All this talk of rationality and where it stands in relation to the light, reminds me of something very different. G. H. Mead's discussion of causality in light of true indeterminism. I'll paste in a passage below about that. [See A. E. Murphy quote in the 23-09-03 note to Steve Savitt, titled "The Trivial Nontrivial."] Maybe you'll see why my mind loosely jumps between the subjects.

Anyway, reflecting on the two issues simultaneously led to the title of this note. "Rational agent" doesn't seem right indeed. But put "rationalizing agent" in its place, and it seems to ring of a little truth.

09-09-08 *Panpsychism* (to D. M. Appleby)

Applebyism 23: *Which brings me back to the term "rational agent". The word to which I take particular exception here is, not "rational", but "agent" (I do object to "rational", for the reasons I gave before, but I object even more to "agent"). To my mind this is an example of the "impersonal expressed in the passive voice". It should be "rational person" (or "rationalizing person"). Or "rational human", in all his or her laughing, crying, breathing, shitting, infinitely layered, ineffable totality.*

Reasoning is something that people *do. Not boxes, with an operating system.*

I continue to like "agent" over "person" or "human" because I think that is the relevant piece of the equation. However, in my case, I don't think it's because there is a Cartesian deep inside me that's trying to re-emerge, but rather a Copernican. That by learning about myself—my position—I learn something about everything else's position. Here's the way I put it to Sipe once. The relevant part is the last item. It is partially, though perhaps not completely, relevant to my discussion with you.

1. When I posit a physical system about which I will speak (by assigning it a Hilbert space, etc.), I am doing that in an almost naïve realistic way. I.e., the $\mathcal{H}$ I write down, represents a piece of the world that is out there independently of me.

2. When I assign a dimension d to that Hilbert space, I am hypothesizing an inherent *property* of that system. I might be right, or I might be wrong, but d is something I hypothesize of it, even if only provisionally. Realism again.

3. When I draw a quantum state out of the space of operators defined by $\mathcal{H}$, however, I am expressing a bundle of my expectations. These are not properties inherent in the system. They are subjective expectations that I bring into the picture (presumably because they have served me well in the past, or at least done me no harm). If I were to conceptually delete myself from the picture, these expectations would disappear with me. To that extent, the view might sound a little like—or at least be confused with—idealism (but that's only if one—as Howard Wiseman often does—forgets elements 1 and 2 above and one of the further elements below.)

4. The subject matter of those expectations, i.e., what they are about, refers both to me and the system I posit. They are MY expectations for the consequences (for ME) of MY interactions with the system. That, you might think is a kind of operationalism: For if I were to conceptually delete myself from the picture, those interactions would disappear too.

5. On the other hand, the reason we use the formal structure of quantum mechanics to bundle our expectations, to manipulate and update them, to do all that we do with them, is to better cope with the world. It is a means to help our species to survive and propagate. That is a kind of instrumentalism. That part of quantum mechanics is a tool like a hammer; it can be used to fix a lot of things, or simply as an aid to help defend ourselves.

6. Still one can never forget the ultimately uncontrollable nature of each quantum measurement outcome—and through Kochen-Specker, at least the way I view it, the non-pre-existence of those "outcomes". That smacks of realism in the oldest, most time-honored way. The world surprises us and is not a creation of our whims and fancies. Back to realism ... but the twist is quantum mechanics, as used by each individual user, only refers to the outcomes HE helps generate. (That smacks of alchemy ... dangerous to say so, but I call it like it is.)

7. Nevertheless, having learned a little from Copernicus, it seems we should ultimately try to abstract away from these personal encounters with the world (having learned what we could from the formalism concerned with gambling on them). If a tiny little system and I create something new in the world when we get together—i.e., we give rise to a birth or new fact—so must it be likely, it seems to me, that any two things give rise to a birth when they get together. That is "F-theory" or the "sexual interpretation of quantum mechanics" ... but you'll have to wait for the movie if you question me any further on that ...

09-09-08 *Panagentism* (to D. M. Appleby)

Just playing with titles. This might have been better for the last note.

10-09-08 *Easier Answers* (to D. B. L. Baker)

Bakerism 4: *Are you planning on staying long? What's your job like?*

I plan to stay forever if I can. This is the most fantastic job ever for the likes of someone like me. I make good money, and the intellectual freedom is wonderful. It gives me hope that I really will solve the problems of quantum mechanics before my days are up. And the work environment is like nothing I've ever seen: good coffee out for us continuously, muffins in the morning, cakes in the afternoon, wine and cheese every Friday afternoon. You can take a virtual tour of the place here `http://www.perimeterinstitute.ca/en/About/Facilities/Facilities_Overview/` and you can see our wine list here `http://www.perimeterinstitute.ca/en/Outreach/Black_Hole_Bistro/Black_Hole_Bistro_Overview/`. So, it's just an amazing place to be at every day.

10-09-08 *Your Old Thesis, 2* (to G. Valente)

Thanks a million for the quote. Yes, everything is going OK here, and Appleby, Schack, and I are on the tail of something big we think: A nice derivation of finite dimensional quantum state-space structure from a single inequality. Or, at least that's what we hope.

Good luck with all these academic things you're doing! Geesh, a degree in physics to complement your degrees in philosophy. Very impressive.

One technical point on what you said:

Valente-ism 5: *I think at the end I even joked that, in connection with the Bayesian interpretation of QM, in quantum theory we're really dealing with "systems without qualities", as we can't say anything about them before a measurement.*

I would say that you can't say anything particular about them *after* a measurement either.

10-09-08 *Really Weird and Not-So Weird Stuff* (to M. D. Sanders)

Sandersism 2: *OK, don't laugh at me, but I am curious if you think the CERN particle accelerator is BS or it might actually reveal something,* http://public.web.cern.ch/Public/Welcome.html*. I read a few of your latest papers (*http://www.pitp.ca/personal/cfuchs/nSamizdat-2.pdf*) indicating that you think the next development is just a matter of viewing things differently, ("Likewise, I'll bet the next big step in physics will only require that we see something right here in front of us.") so I wonder if you think that this CERN BS will actually change anything.*

I didn't go too deep cuz it is like 800 pages, but I tried. I also wonder about the skeptics that think the world will end when it fires up. (Sounds like Star Trek followers, but I don't know.) I don't believe it, but it makes me wonder how crazy people are.

*Anyway, if you have time I would like your opinion. I understand if you think this is stupid based upon where you are . . . you don't have to go too deep, just let me know if you think it will reveal anything or if you think it is a huge waste of money. Also, I'm curious if you are at the pajama party ☺ (*http://ohmyafly.wordpress.com/2008/09/08/largest-hadron-collider-pajama-party/*)?*

Sorry, but you are the only one I know close to this stuff . . .

The really weird stuff is that you were digging around through my notes. Now, that's an old friend! I hope you don't think less of me now! But did you see my TV debut: http://pirsa.org/07120048/. Losing a little hair on top, aren't I? My first comment is lame, but if you have the patience to wait, I get better. (Yeah, right!?)

Now to the not-so weird stuff:

1) Safety of LHC. Here's something to read, and it strikes me as decently authoritative:

   http://en.wikipedia.org/wiki/Safety_of_the_Large_Hadron_Collider.

   I think there's nothing to worry about whatsoever. For me personally, the most conclusive piece of evidence is this. At peak the LHC will be producing 14 TeV collisions (7 TeV each beam), but events like that happen in the upper atmosphere all the time, from cosmic rays slamming into us. The only difference with the LHC is that we'll be able to make the collisions on demand and in the presence of a good detector. Furthermore, the energies of the LHC are really nothing on that scale: Cosmic rays have been recorded not with a measly 14 TeV, but with 10^8 TeV. See:

   http://en.wikipedia.org/wiki/Cosmic_ray
   or
   http://en.wikipedia.org/wiki/Ultra-high-energy_cosmic_ray.

   So, even if these wackos like Rössler were by accident right about black-hole production, etc., (and they're not), then we'd already have black holes flowing through the earth all the time.

2) Cost of the LHC. Estimates run up to $10 billion. I'm pretty sure most of that money is European, though I would like to know what the US contribution is. In my own opinion, it's worth it. For one thing, a key point of the "standard model" of particle physics, the Higgs particle, keeps not being seen. Why does the rest of the model work if this particular major prediction of it keeps hiding? That's more than a 25 year mystery (I first heard of it in 1983), and if the LHC doesn't find it, it'll finally put the nail in the coffin. Secondly, there's the infrastructure for physics as a whole that CERN brings with it. Throughout the world, it helps keep physics education going, and as you can imagine, I'm all for that. To put the costs

in perspective, note that the Iraq war has been costing us $8 to $12 billion A MONTH since 2003. See for instance:

`http://en.wikipedia.org/wiki/Financial_cost_of_the_Iraq_War`.

You can bet your bottom dollar I would trade two months of the war to solve a 25 year old mystery in physics.

3) Efficacy of LHC. It remains true that I believe every word of what I said before: "I'll bet the next big step in physics will only require that we see something right here in front of us. It'll be something no big multi-billion dollar particle accelerator will be needed for. We just have to figure out how to take note of it." But by big step, I mean a REALLY BIG step—the stuff that has historically happened only every two to three hundred years, and can't be planned for beforehand. I think here particularly on Galileo's noticing that any two masses (regardless of their sizes and weights) accelerate toward the ground at the same rate. It is the key insight that ultimately underpinned Einstein's theory of gravitation (containing the essence of his "principle of equivalence"), which in turn led us to predict a load of things, from black holes to the big bang itself. Well, that fundamental phenomenon was right there for anyone to see and take note of in any of the previous 2,000 years of civilization. But no one did. No one had simply said, "Wow, two different rocks, two different metals, two anythings, dropped at the same time always hit the ground simultaneously. I wonder what the significance of that is?" It's observations like that that make the true revolutions in physics. And the LHC is not in that category—that's what I was alluding to. However, I don't belittle little steps in physics either.

   There is clearly something really big brewing on the horizon of physics. Where is all the "missing mass" and "dark energy" in the universe? See

   `http://en.wikipedia.org/wiki/Missing_mass`.

   Is it really just unseen "stuff", or do we have something more basic wrong in our calculations? Why is the universe's expansion accelerating rather than decelerating? These are bigger questions (potentially revolutionary), and the LHC's answers to smaller questions may give us guidance there. At the very least, they may say, "Give up with your usual ways of thinking. Look instead for something that's probably right in front of you."

4) No, I wasn't at the pajama party! I'm too old for that. The last time I was at CERN, I don't even think rap music had been invented yet.

I hope that was of some help.

15-09-08 *PanSICism* (to D. M. Appleby)

What is the new idea for SIC existence?

15-09-08 *Israel December* (to A. Wilce)

Wilce-ism 6: *PS – how did Victor Colussi's week at PI turn out?*

We loved him, had a lot of fun with him. He got a chance to see two old geezers in experimental math mode. While he was here, we were finding an obscene number of new properties for quantum state space when written in SIC language, all by Marcus doing numerical testing as the questions arose. Of course, we didn't have an idea how to prove anything at the time (we have now), but we really opened up a good vein in the mine while he was here. I hope that helped excite him about science.

16-09-08 *What Titles Could Not Be* (to J. Ismael)

I don't think we have your title and abstract yet for The Clock and the Quantum. Could I pressure you to send that in very, very soon?

From a 3 May 1998 note titled "Quantum Probabilities" to Jenann Ismael

> I have just read your nice paper "What Chances Could Not Be" (Brit. J. Phil. Sci. 1996). In particular, I am quite enthusiastic about your closing discussion on how quantum mechanical probabilities may be construed as solely epistemic in character. Have you developed this line of thought further in print? If so, I would very much like a reference to your follow-up articles.
>
> Carlton Caves and I have discussed what I think is a very similar point of view in: C. M. Caves and C. A. Fuchs, "Quantum information: How much information in a state vector?" in *The Dilemma of Einstein, Podolsky and Rosen – 60 Years Later (An International Symposium in Honour of Nathan Rosen – Haifa, March 1995)*, edited by A. Mann and M. Revzen, Annals of The Israel Physical Society, vol. 12, pp. 226–257, 1996. Also available at the Los Alamos World Wide Web E-print Archive `http://xxx.lanl.gov/abs/quant-ph/9601025`. (See, in particular, the discussion of Section 5.)
>
> Presently, we're in the process of writing a paper solely devoted to the idea, and I would like our bibliography to be as complete as possible. (This is how I ran across your paper in the first place.)
>
> Thank you for your help.

17-09-08 *PanSICism* (to P. W. Shor)

I hope you'll be able to come to the SIC workshop that Steve Flammia and I are putting together. I'd really like to see this problem knocked out, and think that an intense workshop with friends (a little talking time, a lot more time at the chalkboard instead) might help shake something loose.

I'm keeping my fingers crossed …

17-09-08 *Seeking Spekkens?* (to R. W. Spekkens)

Spekkensism 44: *How was the contextuality session in Växjö?*

The contextuality session seemed to be enjoyed by everyone participating, but all of the rest of the audience (the ones usually riveted about Bell inequality discussions) slept through most of it. I was a little disappointed that there weren't more conceptual discussions. Mostly there was focus on technicalities, etc.

22-09-08 *Threat of Double Secret Probation* (to A. Kent, A. Steinberg, and W. G. Unruh)

You three are our last hold-outs for supplying titles for your talks at the Clock and Quantum conference next week. It really would be nice to get them posted on the webpage for anyone making a last minute decision about coming. Here's the present schedule:

`http://www.perimeterinstitute.ca/conferences/clock-and-quantum`.

Think of me as Dean Wormer. I really need those titles (and abstracts would be nice too).

24-09-08 *Friends of the Tensor Product* (to S. Abramsky, H. Barnum, B. Coecke, K. Martin, A. Wilce and others)

At the beginning of the summer, my next-door neighbor Keith Rowe, an emeritus professor at University of Waterloo, died after a battle with cancer. Keith was a category theorist, with a love for his children and Canadian football. At the reception for his funeral, I believe I spied a paper titled, "Tensor Products of Categories," but I have not been able to find a trace of it since—maybe I am mistaken. On the other hand, at the end of the summer, Keith's wife Rosemary, came back from her summer stay in their cottage and showed me the papers listed below. I thought I might share the titles with you as a small tribute to him. If any of them look to be potentially useful in your own research (particularly the one that may not have been published), I can get you a copy the next time I see you.

- K. A. Rowe, "Images in Topoi," Canad. Math. Bull. **20** (1977), 471–478.
- K. A. Rowe, "Nuclearity," Canad. Math. Bull. **31** (1988), 227–235.
- D. A. Higgs and K. A. Rowe, "Nuclearity in the Category of Complete Semilattices," J. Pure App. Alg. **57** (1989), 67–78.
- K. A. Rowe, "All Tensor Products are Coequalizers," preprint, don't know if it was published. My summary: "Let E be a category equipped with a bifunctor. In most examples, the bifunctor will be an actual tensor product with special properties such as symmetry or associativity, but we make no such assumptions. We demonstrate that under very mild conditions, every tensor product of algebras with respect to a certain type of monad on E is a coequalizer in the category of algebras."

Samson's Reply

I wrote a paper a few years back, on 'nuclear and trace ideals in tensor *-categories', with Prakash Panangaden and Rick Blute, for which the Higgs-Rowe paper was a very important reference.

Nuclear and trace ideals in tensored *-categories
S. Abramsky, R. Blute and P. Panangaden
In *J. Pure and Applied Algebra*, Vol. 143, pages 3–47, 1999.

24-09-08 *Two Examples* (to R. Healey)

Attached are two examples making the logically pristine point that frequency data *alone* (nor any empirical data, for that matter) *never* determine future probability assignments. I.e., one must feed in a (prior) probability to obtain a (posterior) probability. Of course, this is just the subjective Bayesian point. But it is always nice to be reminded just how simple and uncomplicated that ultimate point is.

In the file `t.pdf` look pages 12-17 (of the total 55). In the file `s.pdf`, look at pages 25-28 (of the total 47). The latter has the example I just showed you in the stair well.

A really good discussion of this point—I think—can be found in Appleby's pedagogical paper: `http://arxiv.org/abs/quant-ph/0402015`.

As I've tried to convey to you before though, I *do not* believe that: Though probabilities are always subjective, the quantum world leaves no empirical mark. Rather it leaves its mark elsewhere than in telling us how to *set* probabilities. I believe it leaves its mark in giving us additional rules to live by beyond Dutch book coherence (when gambling upon the results of our interactions with the external world). In particular, there is a way to look at the Born rule that shows it not as a rule for *setting* probabilities, but rather as a rule for normatively *relating* probabilities. The subjective component of the prior never disappears; instead the world makes it empirical mark in encouraging us to tie our assignments together in stricter ways than de Finetti and Ramsey had envisaged.

We'll get down to business tomorrow.

26-09-08 *Keynes and Zabell Quotes* (to R. Healey)

Two quotes I had wanted to send you earlier in the week, but forgot to.

Here is a the Keynes quote on Ramsey:

> The application of these ideas [regarding formal logic] to the logic of probability is very fruitful. Ramsey argues, as against the view which I had put forward, that probability is concerned not with objective relations between propositions but (in some sense) with degrees of belief, and he succeeds in showing that the calculus of probabilities simply amounts to a set of rules for ensuring that the system of degrees of belief which we hold shall be a *consistent* system. Thus the calculus of probabilities belongs to formal logic. But the basis of our degrees of belief—or the *a priori*, as they used to be called—is part of our human outfit, perhaps given us merely by natural selection, analogous to our perceptions and our memories rather than to formal logic.

And a long quote by Sandy Zabell, from "Ramsey, Truth, and Probability":

> The key point is that previous attempts to explain induction had attempted to model the process by a unique description of prior beliefs [[references]], or by a very narrow range of possibilities [[references]]. De Finetti realized that because probability is a logic of consistency, one can never—*at a given instance of time*—uniquely dictate the partial beliefs of an individual; at most one can demand consistency. The essence of inductive behavior, in contrast, lies not in the specific beliefs that an individual entertains at any given point in time, but the manner in which those beliefs evolve over time. [[In this way it is exactly like classical logic: One is not judged as irrational for starting with the incorrect truth value for some proposition in one's considerations; one is judged irrational only if one makes an incorrect inference in the proof process.—CAF]] Let us pause briefly over this point.

I change my mind slowly; you do so with rapidity; you think I am pigheaded, I think you are rash. But neither of us is of necessity irrational. Disagreement is possible even if we share the same information; we may simply be viewing it in a different light. This is what happens every time the members of a jury disagree on a verdict. Of course it can be argued that the members of the jury do not share the same body of facts: each brings to the trial the sum total of his life experiences, and on juror tries to persuade another in part by drawing upon those experiences and thus enlarging the background information of their fellow jurors. It is the credibilist view of probability that if you knew what I knew, and I knew what you knew, then you and I would—or at least should—agree.

Such a metaphysical stance may well be, as I. J. Good says, "mentally healthy". But it is an article of faith of no real practical importance. None of us can fully grasp the totality of our own past history, experience, and information, let alone anyone else's. The goal is impossible: our information cannot be so encapsulated.

27-09-08 *The Old Jenann* (to R. Healey)

From, J. Ismael, "What Chances Could Not Be," Brit. J. Phil. Sci. **47**, 79–91 (1996):

> There is one possibility which I have refrained from mentioning because it amounts to the denial that there is anything properly called chance, insofar as it is distinct from any species of subjective probability. What I have in mind is the view that chance is just epistemic probability of a particular sort: consider two systems A and B, which differ with respect to the probability pertaining to A of some future event type (say a collapse into a state ψ at t). A either will or will not collapse into ψ at t, B either will or will not collapse into ψ at t, and there is nothing we can now ascertain about either system (e.g. no measurement we can perform) which will decide the case for certain. The occurrence of e is, however, differently correlated with facts about A and B which we *can* determine and with respect to which they differ, e.g. e-type events occur to $l/2$ of systems relevantly like A and only $l/4$ of those like B. This means that we can place A and B in ensembles of systems—as alike to them as we can presently ascertain—in which the frequency of e-type events is $l/2$ and $l/4$, respectively, and this is what the probabilities refer to. The difference between systems with different biographies of probabilities (so long as these assign non-maximal probabilities [i.e. $0 < pr < l$] to the same set of events), is not a difference in the properties of those systems but a difference in what we know about them.
>
> Something about so interpreting probabilities in quantum mechanics has struck physicists as objectionably subjective. Wrongly so, in my opinion. The epistemic probability of a proposition p can be (and often is) represented as a relation between p and a given agent, but can just as well be represented as a relation between p and a set of propositions, those representing the body of information possessed by the agent in question. The t-chance of e on A can be defined as the probability of e relative to the propositions which describe A's state at t or its pre-t history, i.e. as the personal probability of an agent who has no foreknowledge but is perfectly apprised of A's history and the laws which govern its evolution. There is nothing scientifically disreputable [about] chances so construed, indeed it is not unlikely that many physicists have been operating with such a conception without distinguishing it carefully from the view that the t-chances pertaining to A describe objective properties of A such as mass, spin, and

the like. The difference between the two views is just the difference there is in general between the view that some 'parameter' P is a redescription of those in another set $\{P_1, ..., P_n\}$, and the view that P is ontologically distinct from [them] but happens to covary with $\{P_1, ..., P_n\}$. In either case, fixing the values of $P_1, ..., P_n$ fixes the value of P in all physically possible worlds, but the value of P varies independently of those of $P_1, ..., P_n$ in the set of *metaphysically* possible worlds only in the latter case. The value of P fails to supervene on those of $P_1, ..., P_n$ *iff* P is ontologically distinct from $\{P_1, ..., P_n\}$.

Interpreted objectively, quantum probabilities describe properties ontologically independent of the system's other properties and partially characterize its intrinsic state, and it must be (metaphysically) possible for two systems—otherwise alike—to differ with respect to their chances. Interpreted epistemically, the intrinsic state of a system at t is imperfectly correlated with aspects of its post-t state (the events to which it assigns non-maximal probabilities). In this case, the probabilities reflect only uncertainty about the future of the system based on knowledge of its present state (excluding, of course, the chances), and it is impossible for two systems—otherwise intrinsically alike at t—to differ with respect to the t-chances pertaining to each. This view seems to me completely adequate to the role of probabilities in physics (though it would take a much longer paper to show this), and—of all those described—the most natural.

29-09-08 *Tumulka's Reply To You* (to W. G. Unruh)

I got a kick out of your exchange with Tumulka yesterday—particularly, the part where you proposed your universal 7-second-in-the-barrier theory. Roderich's reply that his theory has equations, whereas yours has none, makes all the difference—that that is what makes his a *scientific* theory—took me back a few years to a report I once wrote. I'll place it below in case you might enjoy it. Also a little further commentary below it too. [See 17-11-05 note "Cash Value" to H. Halvorson.]

01-10-08 *Taking Up for Spekkens (this time)* (to W. C. Myrvold)

From the abstract:

> The diversity and quality of these analogies is taken as evidence for the view that quantum states are states of incomplete knowledge rather than states of reality. A consideration of the phenomena that the toy theory fails to reproduce, notably, violations of Bell inequalities and the existence of a Kochen-Specker theorem, provides clues for how to proceed with this research program.

From a section in near the end:

> Contextuality and nonlocality. The Kochen-Specker theorem [28, 30] and Bell's theorem [31] state that any hidden variable theory that is local or noncontextual cannot reproduce all the predictions of quantum theory. The toy theory is, by construction, a local and noncontextual hidden variable theory. Thus, it cannot possibly capture all of quantum theory. In the face of these no-go theorems, a proponent of the epistemic view is forced to accept alternative possibilities for the nature of the ontic states to which our knowledge pertains in quantum theory. It is here that the novel conceptual

> ingredients are required. Note that since nonlocality is an instance of contextuality [57], the latter can be considered as the more fundamental of the two phenomena. Indeed, if quantum theory can be derived from a principle asserting that maximal information is incomplete and some other conceptual ingredient, then contextuality may be our best clue as to what this other conceptual ingredient must be.

From the concluding section:

> A principle stating that maximal knowledge is incomplete knowledge is likely to serve as a foundational principle in a simple axiomatization of quantum theory. This is the claim that we argue is made plausible by the strength of the analogy between the toy theory and quantum theory. Nonetheless, this principle is insufficient for deriving quantum theory. It is intriguing to speculate that we are lacking just one additional conceptual ingredient, just one extra principle about reality, from which all the phenomena of quantum theory, including contextuality and nonlocality, might be derived. To find a plausible candidate for a second such principle, it may be useful to adopt a similar strategy to the one used here to argue for the first principle: do not attempt to derive all of quantum theory, but rather focus on the more modest goal of reproducing a variety of quantum phenomena, even if only qualitatively and in the context of some incomplete and unphysical theory. In particular, attempt to reproduce those phenomena that the toy theory fails to reproduce. Armed with a conceptual innovation that captures the essence of the missing quantum phenomena, a path to quantum theory might suggest itself.

05-10-08 *Titles* (to R. Schack)

Just in from mowing the lawn. I want to record these titles before I forget them. More in a few days.

- Quantum Bayesian Quantum Certainty Is Not a Moore Sentence
- How a Quantum Bayesian Sees Decoherence and Einselection: Van Fraassen Reflection Turned Backwards

06-10-08 *Kent Peacock* (to A. Kent)

Speaking of double-slit toy models, see: `http://arxiv.org/abs/0209082`, the attachment, and the proposal below. Any interest in having this guy out?

On the double-slit, I also checked out Jammer today to see if I could see anything on historical toy models. Not too enlightening, but it did lead me to a tidbit that sounds a little bit like something I might have said. So, all in all, this exercise has been good for me, even if not you!

06-10-08 *SICkening Quantum Mechanics* (to P. W. Shor)

I wonder if I can push you for an answer on the SIC meeting we're having here Oct 26-30. We sure hope you can come, but will understand if you cannot. So far, the confirmed participants are (besides Flammia and myself):

D. Marcus Appleby
Ingemar Bengtsson
Howard Barnum
Markus Grassl
David Gross
Andrew Scott
Lane Hughston
Martin Roetteler

I'm also trying hard to John Conway and Simon Kochen here. Roger Penrose has also recently taken an interest in the problem. I think there's progress to be had if we can just put our minds together for a little while.

06-10-08 *Penrose* (to D. M. Appleby)

Applebyism 24: *If I now believe that the wave-function is non-physical that is in large part* because *I spent so long thinking about Bohm. (The philosophy teacher at my place of work has a quote pinned to the wall of his classroom: "the mark of an educated mind is the ability to entertain propositions without believing them". In other words, a readiness to explore the consequences of an hypothesis without being first convinced of its truth.) Taken in that spirit I think the Bohm theory is extremely valuable (and let me also say that my impression is that Bohm himself took his theory in precisely that spirit).*

You and Wayne Myrvold both made similar points to me. And I accept your arguments.

06-10-08 *Rethinking* (to M. Hemmo)

I want to open up and tell you about a struggle I'm having. As all the travels I've committed to for this year have unfolded (and ones climbed into the middle unexpectedly), I'm afraid I've become more and more broken. It's come to the point where I can hardly stomach the idea of another trip abroad. Basically, one trip abroad each month is killing me—professionally, family-wise, everything.

Yet, this meeting of yours "The Probable and the Improbable: The Meaning and Role of Probability in the Foundations of Contemporary Physics" *subject-wise* is one of the most important ones of the year for me and this Bayesian research program Caves, Schack, Appleby and I have been trying to put together. The idea of not giving our research some representation in Jerusalem is, thus, killing me almost as badly as the thought of traveling again.

So, I'd like to explore the following with you. Would you consider trading me out with Rüdiger Schack as a representative of our research program? I have asked him if he would give a broad talk, particularly addressing the criticisms we've received over the years (from yourself and others), and emphasizing the kind of ontology that goes with our quantum Bayesian stance (something not usually emphasized elsewhere). He has said that he would do that, and that he would be very pleased to go to Jerusalem if it is possible. I think he would be great for the meeting, and Itamar, and all the participants. And I keep my fingers crossed that you will consider this proposition.

I await what you have to say.

06-10-08 *SIC States* (to S. Abramsky and Y. J. Ng)

Thanks for giving me an opportunity to describe my beloved "SICs" at lunch the other day. Here are two things you can read that'll give you a little further introduction to the subject (particularly the aspects of it we discussed at lunch).

1. The proof that SICs are as close as one can get to an orthonormal basis on the cone of positive operators: `http://arxiv.org/abs/0707.2071`.

2. The proof that SICs are maximally sensitive to eavesdropping: `http://arxiv.org/abs/quant-ph/0404122`. To put things more into the context of your axiomatization, Samson, of orthogonal bases, it would be better to have a result proving that SICs form a "least clonable" set of states, but at the moment I don't have that. Though I suspect it is true—for I would guess that any spherical two-design is a least clonable set. Still, maybe this will be enough to get you thinking on the idea.

Finally, let me attach some of the unpublished stuff I was telling you about—namely, how SICs give a particularly clean way to think of the Born rule as simple modification to the law of total probability. The attached document comes from a grant proposal I wrote recently, but I hope to get those equations into an actual paper soon. Anyway, the words there will, I hope, also convey some feeling for the big picture of my foundations program.

Samson, I guess I will see you in New Orleans next week. Thank you both again for coming to *The Clock and the Quantum*. I hope you enjoyed it roundly.

06-10-08 *SIC Invitees* (to S. T. Flammia)

I've updated the Google document. And also nudged one last time Shor, Conway, and Kochen. I think if they can't come, we keep it a small intimate meeting and be done with it.

I also got Roger Penrose fairly interested in the problem last week. He worked out dimension 2, 3, 4 solutions in his head during Harvey Brown's talk. (Same method, as far as I can tell, as Lane Hughston's … though he made his work in $d = 4$, and the last I heard Lane hadn't.) He said the problem related to something he never quite solved in his PhD thesis. Funny. I encouraged him to come too, but he said he had too many other obligations.

06-10-08 *SIC Invitees, 2* (to S. T. Flammia)

Flammia-ism 3: *Wow, Penrose worked out the $d = 4$ solution in his head?!? I'm impressed. I'd be interested to know what other problems it relates to.*

OK, I updated it again, after hearing from Shor.

Unfortunately, I didn't understand anything he said either about his thesis or $d = 4$. One question, though, did arise that I can get my head around. Do we know that we can't construct a SIC on two qubits purely of tensor product states? Of course one can't tensor two SICs and get a new SIC, but that's a different question. The only demand here is that each SIC element simply be a product state of any variety.

Also, there appears to be a dangerous bug in this Google document thing. I can look at the documents of another Chris Fuchs out there, one at NDSU (wherever that is).

06-10-08 *A December Visit* (to K. A. Peacock)

Yesterday, I read your paper "Aristotle's Sea Battle and the Kochen-Specker Theorem," and enjoyed it very much. By my reckoning, what you write is still at the stage of an "idea for an idea", but it is a good idea for an idea! It'd be great to see how to give it some firm substance. I myself tend to believe as well that the ultimate import of KS is that it indicates that the universe isn't a block. I'll attach a document that says as much. [See "Delirium Quantum" `arXiv:0906.1968v1`.] Surely you won't like aspects of it, but perhaps the first and the last sections will tickle you nonetheless. You might also get a kick out of perusing a document I'm presently putting together titled "My Struggles with the Block Universe". Do a search on the terms "block" and "Kochen", and it should take you to the right places.

07-10-08 *Rethinking, 2* (to M. Hemmo)

Thank you so much. And I hope that Itamar will not be too hurt. I feel absolutely bad, but everything I told you yesterday is true, and I don't feel that I will be able to pull myself across the ocean again in December. In fact, my wife just reminded me that my second daughter's birthday is Dec 17, and I had forgotten.

So again thank you for going easy on me. And thank you very much for allowing Rüdiger the opportunity to represent our work and be an ear for any difficulties with it that might be sounded at the meeting.

07-10-08 *Kauffman's Email* (to L. Smolin)

Don't forget to send me Kaufman's email address. I'll send him an invitation to give a foundations seminar.

I should tell you a funny story. A couple months ago, while I was in Austria, I got a note from Rob Spekkens in Cambridge saying that he had run across a nice set of all six volumes of the collected papers of C. S. Peirce, and he offered to buy them for me. When he told me the price, I was floored: The whole set (hardcover) was only 45 pounds! In contrast, I looked in the index I've made of my home library and found that I had paid 20 quid for Vol 8 *alone* of the collected papers of Bertrand Russell (when I myself was in Cambridge last). When I wrote back to Rob accepting the offer, I noted that the bargain he found reveals just how much the Brits value American pragmatism!

I think the Kauffman will be a lot of fun, particularly if he gets good and radical.

07-10-08 *SICkening Quantum Mechanics* (to S. T. Flammia)

Well, we came close. See Kochen's note below: "I couldn't come, but John thought he might be able to make it. Today he told me that he has to stay in Princeton for family reasons."

If you give no objection, I think I'll close the ranks now. We'll have a meeting of:

D. Marcus Appleby
Ingemar Bengtsson
Howard Barnum
Steve Flammia
Chris Fuchs

Markus Grassl
David Gross
Andrew Scott
Lane Hughston
Martin Roetteler

and any locals who might want to show up.

08-10-08 *Another Title* (to R. Schack)

- Reviving Feynman's 'Only Mystery' of Quantum Mechanics in Bayesian Terms

09-10-08 *REU Pestering* (to L. O. Clark)

I liked Mark and Meg's work[163], but it is true that in the end I was not able to use much of it. To be honest, the most important thing I got from it was learning about Seth Sullivant's work (Ref 2 in the report):

> "Statistical Models are Algebraic Varieties,"
> `http://www.math.harvard.edu/~seths/lecture1.pdf`.

That work puts the SIC representation of quantum states into a nice context—the set of quantum states corresponds to a statistical manifold associated with an intriguing quadratic variety. It was invaluable for me to learn that. If you think you can turn it into a publication for other purposes, go for it! It'd be great for them.

10-10-08 *Block U Alternative* (to T. Duncan)

Duncanism 4: *What about "contingent universe" as a name for the alternative to the block universe?*

I think I like that better than "open universe," which is the only competitive term that I think I've noticed before. ... Well actually, John Wheeler used the phrase "participatory universe" and surely he was contrasting it to (the ideas underlying) the block universe.

> [T]he universe is wild—game flavored as a hawk's wing.
>
> — Benjamin Paul Blood

Todd's Reply

Yes, I also like contingent better than open, especially since open has a geometrical/GR meaning. Malleable universe is also not bad, in the spirit of your Pauli Project notes. The definition of contingent that made me latch onto it was "possible but not certain to occur," from `http://wordnetweb.princeton.edu/perl/webwn?s=contingent`.

[163]Maegen Demko and Mark Layer.

10-10-08 *Block U Alternative, 2* (to T. Duncan)

This one goes too far, but inspired by looking in wiki definitions: "alchemical universe". Thinking in particular of definition 3: "Of or pertaining to the creation of something special out of a common material."

14-10-08 *New Orleans Books* (to H. Barnum)

By the way, at least there's excellent book shopping here in the French Quarter. So, maybe not all of the trip is a waste. I just picked up these (recording mostly for my own record):

1. Jonathan Arac and Barbara Johnson, *Consequences of Theory*, PB, $5.00.
2. Justus Buchler, *Metaphysics of Natural Complexes*, PB, $12.50.
3. James A. Connor, *Kepler's Witch*, HC, $9.00.
4. Christopher Norris, *Derrida*, PB, 8.00.
5. Harald Ofstad, *An Inquiry into the Freedom of Decision*, HC, $18.00.
6. Joachim Schulte, *Wittgenstein*, PB, 6.00.

I also found a two volume set of Lotze's *Microscosmus*, 1885 edition. Thinking hard about that one, but it's $100 and they wouldn't bargain down.

15-10-08 *Guilty, but Committed* (to S. Capelin)

I must plead guilty. I got an upgrade on my flights yesterday and found that I essentially slept the whole way here. Then upon landing, I couldn't resist the idea of reading Wittgenstein in a New Orleans oyster house, while eating fried catfish and hushpuppies. Finally, I fell completely weak and went shopping at the used bookshops in hopes of finding a new gem to tingle my philosophy (record below). [See 14-10-08 note "New Orleans Books" to H. Barnum.]

But, I will do my dead level best to hammer you something out tonight. So please don't give up on me ... but don't get too angry with me either! All of these things are important for the grand scheme I have in mind.

16-10-08 *Aaronson's Book Proposal* (to S. Capelin)

Every other Christmas or so for the last few years, I have inevitably included one of chef Jamie Oliver's cookbooks in my wife's melange of presents. I suppose I do this because we enjoy his shows on television, and when gift time comes, it's an easy idea. I suspect that's the case for a lot of other husbands out there as well, but probably more importantly, it may also hold for thousands of women who reward themselves from time to time with the purchase of another of Oliver's books. Reading over Scott Aaronson's manuscript reminds me of the happy times I have had reading his blog, and leads me to wonder whether this, at least in small part, might be the business model destined for the present book. A blog is not the same as a syndicated television show, of course, but it is a wide and ever-renewing vehicle of attention nonetheless. Certainly, for those of us in the field of quantum information, Scott is as close as we have to a "Naked Chef."

I like this book. I like it a lot. Reading parts of it have made me feel young again. And parts of it reminded me of speech patterns I used to speak in—"a polynomial shitload," "a problem called DUH," "The-Mojo-All-Along Theorem"—but have gotten out of the habit of, presumably because of the culture around me that scientific writing should be above that. I love David Mermin's and Nielsen and Chuang's books on quantum computing, but neither have made me spontaneously laugh out loud like this one. To the extent that pleasures like this open the thought receptors, I count its method a success. And there are so many tidbits of wisdom in it—from the mathematical to the philosophical to the social[164]—even I was surprised by what I found myself thinking about as I absorbed Scott's thought style.

Which leads me to what I view as the greatest intellectual strength of this book—the ebb and flow of topics, and the unique way Scott presents them flowing them into one another. What one learns from this book that one will not learn from any other book yet in print is that quantum computing is a very *big*, very *deep* subject. The thoughts surrounding it are not narrow and specialized, but rather have very much to contribute theoretical physics as a whole. Scott gives a sense in this book that one really might tie this vast landscape together—that one really might one day put all these subjects (from cosmology to free will to quantum foundations to the foundations of mathematics) in service of each other. Having this in print would be a valuable service to the physics community.

But the presentation of the book clearly needs work, and this is where I have the hardest time pinpointing what I feel. When I first started reviewing this manuscript, I thought "Oh no!" One chapter, two chapters, and the beginning of the third were consumed, and despite my predisposition for not wanting to think it, I thought things were a bit silly. How would one get into a book like this? Slowly and imperceptibly initially, however, I did indeed get into it. Once I started to take the discussions at Scott's pace and Scott's way, and to think about the questions for the non-lazy, I found that my thinking the exposition silly and flippant simply disappeared—the stuff I was reading pretty much flowed into me without resistance. That very much impressed me. But I also know that I gave it probably more of a college try than a new reader would—I have seen Scott lecture, and I have read his blog, and felt that there must be something of substance here. So I plodded on until the tractor beam took hold, but maybe not everyone else would.

How to smooth this rough and risky (risky for sales) edge off, I don't know: I'm not the author; I'm not the editor. But I do very much think this book should be out there.

23-10-08 *SIC Conference – Titles and Abstracts* (to L. P. Hughston)

We'll miss you!

I talked to Roger Penrose about the SIC-existence problem when he was here a couple weeks back for a quantum foundations conference. The first exchange on it was cute. I briefly described the problem to him before the start of a talk. At the end of the talk, he tapped me on the shoulder and said, "Well, I can see it works in $n = 2$, 3 and 4." As far as I could tell, his $n = 3$ solution sounded pretty much the same as your own. When it got to $n = 4$, I didn't understand him as well. But the main cute thing was, it seems he did it all in his head in those 45 minutes. To the extent that he got a chance to think about it later, I think most of his thought was devoted to seeing if he could recover his $n = 3$ solution from his $n = 4$ solution by an appropriate projection. I think things there were less conclusive then.

[164]Here's an example of the social that tickled my fancy: "In my view, probability theory is yet another example where mathematicians immediately go to infinite-dimensional spaces, in order to solve the problem of having a nontrivial problem to solve!"

24-10-08 *P.S.* (to A. M. Steinberg)

Steinbergism 1: *By the way, I'm also still curious to hear why my suggestion about a Bayes-like rule for noisy projectors fails! (As for the more general discussion of what probability "means" and what valid gambling strategies are, maybe that will await a pub one day.)*

I will. In fact, I plan to put it in my talk on Sunday. I'll send you a link to that after it is posted on `pirsa.org`.

About the second issue, it is not nearly so philosophical as to require a pub. I'll send you the appropriate place in our writings to read about it.

Thanks for the opportunity of interaction with your group today. At Atticus, I got one amazing bargain (all five volumes of *Early Defenders of Pragmatism* for $65.00 total) and two other good books (on pragmatism of course) but not such great bargains.

25-10-08 *Bio and Blurb for TGQI* (to C. M. Caves)

Bio:

Christopher A. Fuchs is currently a Long-Term Visitor at the Perimeter Institute for Theoretical Physics in Waterloo, Canada and an Adjunct Professor at the University of Waterloo. For seven years previous to this, he was a research staff member at Bell Labs, Lucent Technologies in Murray Hill, New Jersey, both in the computer science and the physical science divisions. Fuchs received a PhD in 1996 from the University of New Mexico, under the supervision of Carlton M. Caves. He then held postdoctoral positions at the University of Montréal, Caltech (where he was a Lee A. DuBridge Prize Postdoctoral Fellow), and Los Alamos National Laboratory. His awards include the Albert A. Michelson Prize Lectureship at Case Western Reserve University and an E.T.S. Walton Award from Science Foundation Ireland. He was an associate editor for the journal Quantum Information and Computation from 2000–2008, and the organizer of a dozen international meetings in quantum information and quantum foundations. He is an author of more than 50 scientific papers and two outlandish books of email correspondence with many founders of the field. More information can be found by googling the terms "Fuchs" and "quantum" simultaneously—at the moment, he comes to the top of the heap.

Blurb:

Ten years ago, the field of quantum information was hardly a recognized branch of physics, even by the most forward-looking physics departments. Just ask anyone who was in the job market at the time. But things have changed, and the very existence of the APS Topical Group on Quantum Information is a harbinger of the idea that quantum information not only has a home within physics, but is a school of technique that will have an impact on physics exploration as a whole. By our ways, we can help change the world of physics. In fact, with over 900 members presently, it does not look far-fetched that with an enthusiastic recruitment effort, the GQI could attain APS division status. This would give our field a clout and visibility that should be of great benefit to our fresh graduates and help further infrastructure and funding stability the field is sorely in need of. We should also establish premier awards—paying careful attention to make them significantly competitive and desired—for the best PhD work and research in general. This would bring the best in our field to full view, with all the benefit that entails for everyone. Furthermore we should work hard to lower the gender barrier of physics, and as it seems more women are already attracted to quantum information than many other branches of physics, we have the opportunity to build on an existing base to make a real impact. Finally the GQI can do more to disseminate awareness of the potential of quantum informational techniques for physics problems everywhere, and through the organization of strong specialized conferences and their associated advertisement in departments across the States, we can help make our field a resource for physics that's long here to stay.

05-11-08 *As You Wake Up* (to R. Schack)

Thank god America has finally had some sense! The news of our new president is very good indeed—maybe there is a chance now that our world can move forward, rather than back.

05-11-08 *More Atticus* (to me)

> Moore, Addison W.
>
> *The Collected Writings of Addison W. Moore.* 3 Volumes
>
> Bristol: Thoemmes Pr 2003, 1st Edition, No Jacket, Hardcover, As New Book
>
> Description: After John Dewey, Addison W. Moore was recognized as the chief spokesman for the instrumentalist version of pragmatism. Never before available, this complete collection of Moore's work contains dozens of philosophical articles, essays, book reviews, writings by other philosophers, and reviews of his work, together with his book, *Pragmatism and its Critics* (1910). Vol 1: *Writings, 1896–1910.* Vol. 2: *Pragmatism and Its Critics.* Vol. 3: *Writings*, 1911–1927.
>
> Price: USD 45.00 other currencies, order no. H2409

05-11-08 *Slusher Lunch* (to L. Hardy and R. W. Spekkens)

Would either of you be available for an early lunch tomorrow? Dick Slusher, my old manager from Bell Labs (and now a director of a quantum information lab at Georgia Tech), is visiting IQC and giving their colloquium tomorrow. I'm in charge of him from 12:00 to 1:30, and I planned to lunch him at PI and give him a tour. Dick has always had a kind of inchoate interest in quantum foundations, and I thought it'd be fun for him if we could razzle, dazzle him during lunch with some foundationsy things. Dick always wants to know, "how is it relevant for my physics?" I'd really appreciate it if we could give him something to think on, as this is exactly what we should be about. Would a 12:30 lunch work for either of you? I hope you can come. Please let me know if you can, and I'll dig you up at lunch time.

BTW, seeing Wilczek's vision of "the grid" as the ultimate reality was quite revealing to me. Particularly, his use of "quantum fluctuations" made me think he could use a good dose of the quantum foundations mysteries—it might serve him well in these visions of ultimate visions.

06-11-08 *The New Things* (to R. E. Slusher)

I feel like I didn't answer you very adequately last night when you asked if there's been any progress in SIC world. What I wanted to express is that though not much new is known about the existence of these things, we have learned plenty about what quantum mechanics looks like when expressed in terms of them. On the one hand the state space seems to have a really intricate structure, but on the other hand we have hints that all the intricacy may arise from the simple equation I showed you last night.

Unfortunately, I don't have any of this in a completed paper yet. But, I attach three pieces of evidence to show you that I haven't been sitting on my duff. 1) The scientific piece of a grant proposal I wrote (and obtained). It is actually a decent overview of the new simple equations I showed you last night (the expression for unitary evolution and the modification of the law of total probability). 2) The beginnings of a paper on the actual shape of quantum state space. 3) A piece on rederiving that state space, taking the modified law of total probability as an axiom.

I hope you'll find some of this a little enjoyable.

06-11-08 *The Man Who Knew Certainty* (to R. Schack & C. M. Caves)

http://pages.stern.nyu.edu/∼abranden/

06-11-08 *Wilczek World* (to L. Hardy)

Hardyism 3: *I didn't go to Wilczek's talk. I probably should have done "for the cause" but needed a break.*

Yeah, it was funny: I was thinking of the things I had heard about how he was somewhat against PI having involvement in foundations, and at the same time seeing him talk about things that were in bad need of some foundational analysis. The most fundamental stuff he calls, "the grid." As far as I could tell, he means by this that spacetime is filled with a quantum field, continually undergoing "quantum fluctuations". It was the usual particle-physicist speak. But what does this phrase quantum fluctuation mean? The only sense I've ever been able to make of it is that it signifies a quantum state in superposition. The can of worms that's opened up here is precisely the meaning of the wavefunction and the attendant issue of quantum measurement. Maybe it'd be worth my while to read his latest book first, and then try to start up a dialog with him.

07-11-08 *Thoughts on Consciousness* (to T. Duncan)

Duncanism 5: *I've been drafting a paper to organize some thoughts on how to incorporate subjective awareness into our understanding of the natural world. I've attached the latest version in case you have time to take a look . . . critical feedback is most welcome.*

Thanks, I enjoyed that. I particularly liked this image of trying to recover color from a black and white drawing.

Your considerations reminded me of some of the ones in Schrödinger's little book, *Nature and the Greeks*. As I recall, he too held the opinion that any physical model must exclude the idea of subjective experience by its very nature, by its construction. Thus it is no surprise that subjective experience can never be found back in it. But then Copenhagen quantum mechanics perplexed him greatly, for he found subjective experience to be an integral part of the Copenhagen model. How could that be—he thought it must be a confusion in the Copenhageners. I think the book is worthwhile reading and deeply connected to your paper.

Also, I have to admit, I finally printed out "An Ordinary World." I hope to take it to Australia with me next week.

10-11-08 *Interview Questions – The Better Me* (to D. T. Pope)

Pope-ism 1: *What do you think of the many worlds interpretation? (For the purposes of this question, you can take the many worlds interpretation to be the idea that the universe splits into multiple copies whenever a measurement happens.)*

I think that despite all the attention that has been lavished on it by the popular science magazines (and science fiction magazines), the many worlds interpretation has little to no content. You say, "the universe splits into multiple copies whenever a measurement happens," but the whole motivation for the interpretation is to get away from ever making mention of "measurement" at all—the many-worlds interpretation is one species of reaction to the fear that the word "measurement" is too loose and fuzzy and ill-defined to be a significant piece of physical theory.

How does it tackle the problem that the quantum world seems to be making an on-the-fly decision whenever it gives us the outcome of a measurement? It says instead, the quantum world is making these decisions all the time, everywhere and everywhen. But since the quantum formalism gives no mechanism for this, and also hints of no bias for which way these decisions will go, the many worlders postulate instead that the world as a whole branches. It doesn't decide; it just branches. So that for those events in which there are observers around, in one branch the observer sees this outcome, and in the other branch he sees that one.

The trouble I see is that it is just a story. A story that could have been made up, and in fact was made up, before quantum mechanics. There is nothing particular to the quantum formalism about it. Everything that can happen, does happen—you might find that in the longing poetry of a parent who has recently lost a child. Why is my world the way it is? *Because* I am on this particular branch. Which branch? *My* branch. But there's always a better branch where everything is happy again.

Pope-ism 2: *What do you think of the pilot-wave interpretation?*

I think it has had 51 years of a chance but has never made a serious impact on physics (since its formulation in 1957). The de Broglie – Bohm idea was to supplement the equations of standard quantum mechanics for the wave function alone, with equations for where the particles should actually be at all times ... all said *on the condition* that one knows where the particles were initially. But one never knows such. So the pilot-wave theory adds a layer of equations that are never in the end used, outside of giving a warm fuzzy feeling to those inclined to take it. It would be different to me if the addition of those equations gave rise to simplifications in the calculations I need to do as a physicist. Then I would respect them more. But as it is, they just seem to be dangling appendages that have yet to offer any help in our getting to the next stage of physics.

Pope-ism 3: *What do you think of the Copenhagen interpretation? (For the purposes of this question, you can take the Copenhagen interpretation to be the idea that in quantum physics we can only know about the results of measurements. I.e. that it's meaningless talk about what electrons are doing when we're not measuring them.)*

The key idea behind modern variants of the Copenhagen interpretation is that quantum mechanical wave functions represent statistical information, full stop. That is, the wave function is not a property or value or quality *intrinsic* to a quantum system. Instead, when a physicist writes down a wave function, he is writing down his supposed information and nothing more. Information about what? His information about the *results* or *outcomes* of measurement interactions. It is not information about unknown hidden variables or pre-existent properties. Rather, it is his best statistical information on what *will come about* if he interacts with a quantum system this way or that way.

Opponents of the Copenhagen interpretation often present it in a negative light. They see it as an arbitrary injunction to ask no questions about what a quantum system is *really* doing. But I would say it is simply silent on that, recognizing instead an aspect of reality that is much deeper and wholly unexpected before quantum mechanics came onto the scene. It recognizes that in interaction, new things actually do come under the sun. That the big bang is not an isolated far off event, but that little *metaphorical* versions of it are happening all the time. And we have direct knowledge of this; for we see it any time a quantum experiment is done.

There is no story of what electrons are *really* doing simply because there is no completed story of what the universe as a whole is doing anyway. Parts of it are being created on the fly all the time. Quantum wave functions being purely information in character is but a stark recognition of this wonderful property of existence.

Now, how does this contrast with my insults to the many-worlds interpretation? Why would I say these ideas have content when the MWI does not? The recent wealth of discoveries in quantum information has taught us particularly the value of looking at quantum mechanics through information theoretic eyes. Particularly, to do quantum information, we've all discovered the necessity of first going out and purchasing all the textbooks on information theory (written by authors who knew nothing of quantum mechanics). To our great surprise: When one looked at the properties of information written abstractly, one found the properties of quantum wave functions. If it looks like a duck, walks like a duck, honks like a duck … it's probably a duck.

Pope-ism 4: *In the double-slit experiment with electrons, when you put detectors next to each of the slits, the interference pattern disappears. Why is this? (This is for a section on measurement disturbance. If possible, it would be fantastic for you to talk about the idea that measurement at the quantum level is a somewhat disruptive and invasive process. E.g. kind of like kicking something.)*

It is a different experiment. Why would anyone have ever expected its results to be the same as the original one? Aha, only if they had expected the experiment itself—the interaction itself—to be an inconsequential component in the phenomenon under study. But apparently it is not, and that is the lesson of quantum mechanics.

Things like this are encountered all the time in the sphere of psychological study. My wife asks me as we drive down the road, "What do you want for dinner?" I first say, "I have no idea, I'm not hungry." But soon I find myself revising the opinion, feeling the stomach acids start to move in my stomach. If she had not asked me, I might have remained without appetite for a few more hours of driving. But that's psychology. What is interesting is that we now find this phenomenon in our most fundamental physical theory. But that is no reason to think that the phenomenon must then be secondary and that we have been fooled by thinking we had a fundamental theory after all. Instead it is a call to recognize, "It is a world sensitive to our touch."

Pope-ism 5: *How big is the economic impact of quantum physics due to technologies such as computers, iPODs and other familiar electronic devices?*

Quantum computers will be very big. Ten years ago we already knew they would be exciting for what they can do in certain number theoretic problems, like factoring large numbers, and the promise they held for efficiently simulating more exotic physical systems. But make some quantum computing hardware available to hackers and lord knows what we will find can be done with them. I think we have no clue and cannot foresee. All we can do is fulfill this urge to prod matter to do new, exciting things for us.

Pope-ism 6: *In the double-slit experiment with electrons, what happens in between the source and the detector? What are the electrons doing in between the source and the detector and how do they pass through the slits?*

These questions are not the subject matter of quantum mechanics. See above. To think that they should have an answer is to think that the universe is like a book already written.

Pope-ism 7: *How important is quantum physics to physics overall and our understanding of the universe?*

I think we (physicists, mathematicians, computer scientists, philosophers) have taken only the tiniest baby steps toward incorporating the lessons of quantum mechanics into our worldview. I think on this passage of G. K. Chesterton,

> There are some people—and I am one of them—who think that the most practical and important thing about a man is still his view of the universe. We think that for a landlady considering a lodger it is important to know his income, but still more important to know his philosophy. We think that for a general about to fight an enemy it is important to know the enemy's numbers, but still more important to know the enemy's philosophy. We think the question is not whether the theory of the cosmos affects matters, but whether in the long run anything else affects them.

From this point of view, it is imperative to get the story of quantum mechanics straight. What will matter in the end is only how it affects our view of the cosmos.

10-11-08 *Dry Run* (to A. Kent)

Here was a dry run I took for myself before the interview with Damian that you egged me on about. [See 10-11-08 note "Interview Questions – The Better Me" to D. T. Pope.] Of course, I didn't repeat anything written below (my mind doesn't seem to work that way)—so the dry run was really a personal run. I suspect you'll agree with very little of what I write here, but maybe that's why I send it to you. I hate being in the limelight like this; I find it physically painful really. So, I send part of the pain on to you.

11-11-08 *Deleuzean Difference* (to S. Diamond)

It was good meeting you the other day, when we were all talking with Stu Kauffman.

I wanted to follow up on what I had asked about Deleuze and "difference". Reading the Wikipedia article on Deleuze this morning, I came across this description of his metaphysics:

> Deleuze's main philosophical project in his early works (i.e., those prior to his collaborations with Guattari) can be baldly summarized as a systematic inversion of the traditional metaphysical relationship between identity and difference. Traditionally, difference is seen as derivative from identity: e.g., to say that "X is different from Y" assumes some X and Y with at least relatively stable identities. To the contrary, Deleuze claims that all identities are effects of difference. Identities are not logically or metaphysically prior to difference, Deleuze argues, "given that there exist differences of nature between things of the same genus." That is, not only are no two things ever the

> same, the categories we use to identify individuals in the first place derive from differences. Apparent identities such as "X" are composed of endless series of differences, where "X" = "the difference between x and x'", and "x" = "the difference between…", and so forth. Difference goes all the way down. To confront reality honestly, Deleuze claims, we must grasp beings exactly as they are, and concepts of identity (forms, categories, resemblances, unities of apperception, predicates, etc.) fail to attain difference in itself. "If philosophy has a positive and direct relation to things, it is only insofar as philosophy claims to grasp the thing itself, according to what it is, in its difference from everything it is not, in other words, in its internal difference."

I suppose reading something like this once before was the source (in the dark recesses of my mind) that led of my query. The Deleuzean idea must have caught my eye because of my own thoughts on how best to interpret what is happening in the quantum mechanical measurement process. Attached is a little artsy piece of mine that lays that out a little more. [See `http://arxiv.org/abs/0906.1968`.] Sections 1 and 3.2 to 3.4 give some sense of what I'm talking about.

In any case, meeting you has brought Deleuze back onto my radar screen. I'm packing up his book *Bergsonism*, which has been unread on my shelf, to take with me on a short trip to Australia in a couple of days. Thereafter, I hope to get hold of his book *Difference and Repetition*.

12-11-08 *Quantum Systems Are Cranberries?* (to J. A. Smolin and others)

If I had known the answer would turn out to be this simple, I would have lost interest in the hunt years ago!

`http://www.nytimes.com/2008/11/12/dining/12cran.html?8dpc`

"The Zing Starts Here"

12-11-08 *Slightly Zingier Explanation* (to A. Kent)

See Footnote 10, at the bottom of page 9, of

`http://arxiv.org/abs/quant-ph/0205039`.

12-11-08 *Feynman Comment?* (to N. D. Mermin)

Did you somewhere in print comment on this quote of Feynman:

> We choose to examine a phenomenon which is impossible, *absolutely* impossible, to explain in any classical way, and which has in it the heart of quantum mechanics. In reality, it contains the *only* mystery. We cannot make the mystery go away by "explaining" how it works. We will just *tell* you how it works. In telling you how it works we will have told you about the basic peculiarities of all quantum mechanics.

It is from the first chapter in volume 3 of the Feynman Lectures, and concerns the double slit experiment with individual electrons. It seems like you had a discussion somewhere of how these double slit considerations cannot be nearly as decisive as Bell tests. I'd like to track that down and use some of the material in a paper I'm putting together.

Hope you're well and still thinking about coming to PI for a visit.

12-11-08 *Feynman Comment?, 2* (to N. D. Mermin)

I've looked all through *Boojums* (of which I again have a copy; got it for $8.95 in Madison, NJ as I recall), but haven't found anything.

Merminition 170: *What's the paper about?*

It's about this: [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] particularly the diagrams on pages 45 and 50 and the magical equation on page 46. It then turns the table and asks whether the magical equation (and its relatives on page 51) might be the fundamental idea behind quantum mechanics.

I'm glad I'm not going to that conference; glad I wasn't even invited. After my eighth (round) trip across an ocean in a year's span (and numerous other side trips in the States), I broke down. I've canceled trips left and right. I'm committed to staying home, home, home, only to break my stride when I take Emma to Paris in April for her 10th birthday.

Keep thinking; I really like to use a relevant quote to bolster this passage of mine:

> Richard Feynman wrote these words for the opening chapter on quantum mechanics in his monumental *Feynman Lectures on Physics*. It was his lead-in for a discussion of the double slit experiment with individual electrons or photons. Imagine if you will, however, someone well-versed in the quantum foundations discussions of the last 25 years—since the Aspect experiment, say—yet surprisingly unaware of when Feynman wrote actually this. What might he conclude Feynman was speaking of? Would it be the double slit experiment? Probably not. To the modern sensibility, a good guess would be that he was speaking of something to do with quantum entanglement or Bell inequality violations.

12-11-08 *Autumn at PI* (to N. D. Mermin)

Merminition 171: *I attach a* `.doc` *file giving the program of the Tempe workshop. No website that I'm aware of.*

Liked that title by Derek Abbott (whoever he is): "What happens when the laws of physics change?"

Give him these quotes by Poincaré. [See note to H. Halvorson, dated 10-03-06, titled "Singularities and Evolutionary Laws."]

12-11-08 *Bergsonism* (to N. D. Mermin)

Merminition 172: *Great stuff. I hadn't seen it before.*

The final paragraph is pertinent to my own remarks:

> No doubt many readers will be dismayed to note that I seem constantly to substitute for the world a system of simple symbols. This is not due simply to a professional habit of a mathematician; the nature of my subject made this approach absolutely necessary. The Bergsonian world has no laws; what can have laws is simply the more or less distorted image which the scientists make of it.

Yeah, it is great stuff. I love how he foresaw the meaning of the big bang.

Merminition 173: *What does he mean by "the Bergsonian world"?*

Something like in my credo to Joy Christian and Lucien Hardy. [See note to them dated 12-02-07, titled "Lord Zanzibar."]

12-11-08 *Boutrouxisme* (to N. D. Mermin)

Funny, you didn't ask about who Boutroux was. I remember the great joy I had one evening when I did a google search on the simultaneous names James, Renouvier, and Boutroux. To my amazement one of the references mentioning all three of these characters was a book by Teddy Roosevelt!

Here's a couple of quotes from the old man himself (Boutroux that is):

- E. Boutroux, *Natural Law in Science and Philosophy*, (David Nutt, London, 1914); translated by F. Rothwell, from E. Boutroux, *De l'Idée de Loi Naturelle dans la Science et la Philosophie Contemporaine*, (Société Français D'Imprimerie et De Librairie, Paris, 1895). This is an early work concerning the idea that nature's laws themselves might be evolutionary and not immutable.

  > The theory upheld in the present work is that no absolute coincidence exists between the laws of nature as science assumes them to be, and the laws of nature as they really are. The former may be compared to laws proclaimed by a legislator and imposed *a priori* upon reality. The latter are harmonies towards which we ascertain that the actions of different beings really tend.

  and

  > That which we call the laws of nature is the sum total of the methods we have discovered for adapting things to the mind, and subjecting them to be moulded by the will.

- E. Boutroux, *The Contingency of the Laws of Nature*, (Open Court, Chicago, 1920); translated by F. Rothwell, from E. Boutroux, *De la Contingence des Lois de la Nature*, (Ballière, Paris, 1874).

12-11-08 *PIAF, Peres School, Etc.* (to D. R. Terno)

Terno-ism 1: *Another matter: are you happy with the tentative titles that I gave to your lectures?*

If you could change the one titled "Non-locality in quantum mechanics" to "The Significance of Entanglement", I'd appreciate that. It'd be more honest to my attitude: I don't like to use the word nonlocality anywhere in relation to quantum mechanics. (For instance, the conclusion I draw from Bell inequality violations is NOT that locality goes by the wayside, but rather that "unperformed measurements have no outcomes".)

13-11-08 *Hanneke's Thesis* (to R. Blume-Kohout and others)

Spurred by Hanneke's tragic death, I printed out her 166 page Master's Thesis to have a copy in my office. (It can be found online at `http://philsci-archive.pitt.edu/archive/00004224/`.) However, I will be traveling to Australia for a week starting tomorrow. Thus if anyone would like to borrow the paper copy for a while, just let me know—there's no sense in cutting down more trees.

I think it would be nice if someone would report on the thesis at one of our Thursday foundations group meetings. And given the subject matter, I think it would be particularly nice if Robin would weigh in for at least part of that, telling us what he thinks of the argument.

13-11-08 *Can't Dubrovnik* (to J. R. Brown)

I must apologize, but it is now clear to me that I will not be able to come to the Dubrovnik meeting. The note below, which I paste in, explains what is happening. On April 18, the last day of the Dubrovnik meeting, I will be flying from Toronto to Paris.

However you have built such an expectation of this meeting in my mind that I have come to think it is pretty important to have some representation of our quantum-Bayesian/quantum-pragmatist thoughts there … lest we be forgotten when the history books are written. Thus I wonder if I might convince you to invite two of my strongest and most interesting colleagues in my stead? The two colleagues are Rüdiger Schack and Marcus Appleby (both in the UK). Schack is one of the masterminds of all that we have done in this quantum Bayesian turn, and Appleby is an amazing polymath who has contributed much in the last few years. Here are a couple of representative papers:

> Schack, `http://arxiv.org/abs/quant-ph/0404156`
> and
> Appleby, `http://arxiv.org/abs/quant-ph/0402015`.

Both physicists are very philosophically minded, steeped in knowledge of Wittgensteinian ideas. And I know that Schack has also put significant effort into the classical pragmatists, whereas Appleby has done the same with Hume.

The point is neither would disappoint you. Finally, let me mention this: I gather from a side remark in a note from Chris Timpson that he plans to be at the Dubrovnik meeting. Timpson has recently posted an excellent review and critique of our quantum Bayesian effort: `http://arxiv.org/abs/0804.2047`. It would be very nice if the simultaneous presence of these three would add some real sparks to your conference, and at the same time expose directions of progress for us.

I will keep my fingers crossed …

13-11-08 *Can't Dubrovnik, but Maybe You Can* (to R. Schack & D. M. Appleby)

In my continuing effort to return to an honest living, I have just canceled two trips in April that I had previously promised to undertake. One of them is a meeting in Dubrovnik, Croatia that is historically famous in the philosophical circles. As I understand, the place is truly strikingly beautiful and the food wonderful. My invitation to that meeting (from Jim Brown) and a poster for it are attached.

The reason I'm telling you all this is that when I canceled, I suggested to Jim that he invite you two. The reason is I would like to get some representation there of our quantum Bayesian, pragmatist, Pauli'an, and Wittgensteinian thoughts. Jim had already made me feel that he wanted to develop discussion along these lines, and that this would be a good forum for people to start thinking on these things. Thus I hope he will take the bait and invite you, AND also that you will go and talk on this subject if given the opportunity. I happen to know that Timpson will also be there, so I suggested to Jim that he might want to take the opportunity to generate some intellectual sparks via a session showcasing all three of you.

14-11-08 *Ein Begriffe Again* (to H. C. von Baeyer)

Do you remember the discussion we had on the phrase "ein begriffe", which I thought had turned up in the letter from Pauli to Bohr? What note was that, can you send it to me again? I have finally unearthed the original letters. It looks like Folse probably did transcribe that marginal note incorrectly.

If I don't hear back from you in the next hour, I'll get back to you in a couple days to continue the discussion. I'm on my way to Australia this afternoon, so that'll cause a little delay.

16-11-08 *SICs Exist!* (to D. M. Appleby and S. T. Flammia)

I now know it's unquestionably true! I met one on my way between Sydney Airport and Darling Harbour this afternoon. My taxi driver was a Punjabi SI… wait a minute, he must have meant Sikh not SIC. Aw crap. Back to reality.

26-11-08 *A Draft* (to A. Stairs)

Stairsism 2: *I've attached a draft of what I want to talk about in December. It starts with things I had been thinking about in the talk I gave at Western, but I think you may find that it's actually quite congenial to a good deal of what you want to say.*

That's funny, I already felt pretty damned with this sentence of yours:

> One might protest that this ignores the difference between probability one and truth, but there's not much to be gained by following that thought …

26-11-08 *Appreciation* (to A. Plotnitsky)

I fear I haven't yet expressed adequately how much I appreciated your visit. So, let me take this opportunity before signing off for Thanksgiving. I got loads out of our discussions, and today's discussion particularly. I hope you will believe me; I don't always show it in my face. Conversations with you inevitably teach me how to say things and think things in ways quite different than I am accustomed to, and that is a currency in itself. Your insights today, I felt were particularly keen, and I'm working to incorporate your ways of thinking into my mathematical developments. I want to get back to this concept of efficacity and develop it much further, as I have always felt that dimension d to some extent quantifies it. You helped me a see a clearer relation with d and the SICs today, and for that I'm grateful.

All my best for a safe trip home, and please give my best regards to Paula.

07-03-08 *Why Things Fall* (to N. C. Menicucci)

I was misremembering the Wootters result. What he tried to explain was why things fall, if we take gravitational time dilation as given. See:

`http://www.springerlink.com/content/g7863165m0w47665/fulltext.pdf`

29-11-08 *A Colleague* (to R. W. Spekkens)

Well reasoned. Particularly, I had not thought of this angle on it:

Spekkensism 45: *Nonetheless, he would undoubtedly benefit from spending some time here. Given that he seems keen to pursue foundations, and will ultimately be a representative of our field wherever he goes, it is probably a good idea for us to invite him.*

Strange thing, I go up and I go down on him. The more I analyze it, though, the more it seems to be a Necker cube phenomenon, rather than something objective about him. When I think of the science, I'm very down on him. When I think of the human behind these vague (almost surely non-productive) ideas, a human full of feelings with a wife and child, a different kind of instinct kicks in with me. Probably the only difference between him and any of a dozen other questionable thinkers who write me is that I've met him in person a few times. That associates a human struggle with the quack idea, and I find it hard to ignore.

01-12-08 *Prolegomenon* (to H. C. von Baeyer)

Thanks for sending your draft on Pauli's journey inward. I enjoyed reading it very much, and—you're going to curse me for this—found myself hoping that it is really just a prolegomenon to a more extensive project. It was very good indeed, and I think the writing will serve the purpose of pulling public interest into the man. Why did you struggle with it all year? Can you pinpoint what the difficulty was?

I caught a very small number of typos. Here they are: [. . .]

Now a question: In note 28, I didn't understand why you brought up the 137 story. Did it connect to something in the text that I missed?

I enjoyed the Fierz quote on page 16 very much.

Another question. What does "Gewissen" in note 3 refer to?

By the way, thanks for forcing me to look up "retorts". My reading of "retort" in the Heisenberg quote,

> The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names.

had always been as "response" or "answer back"—clearly not satisfactory, but that's what I was thinking. I never thought of chemical vessel!! (Shows what I know of chemistry.)

Really, let's talk more about your struggle with writing this. I'd like to be able to peer into your mind a little more.

BTW, Australia is long since past. I got back almost before I went. Still, it took its toll as expected, and I have canceled all other travels until my April Parisian outing. I am going to write and write in the next three months.

01-12-08 *The Slowest Reader on Earth* (to W. G. Demopoulos)

By the way, in preparation of your visit, I finally read the draft you sent me October 28th. Sorry to have taken so long. Not many comments at the moment: Where we agreed before, I think we continue to agree. And where we disagreed before, I think we continue to disagree. Particularly, in your treating measurement devices as NONquantum in the sense of being "unproblematically propositional." I.e., "effects are traces left on systems whose associated propositions are unproblematically determinately true or false."

One point of wonderment to me, however, was this. You say,

> To my knowledge it has never been maintained that the source of the interpretive problem that the Kochen-Specker theorem poses is the propositional framework, the framework that takes propositions belonging to particles as the subject of probability assignments.

I would say this is exactly what my colleagues Peres, Caves, Schack, Appleby, and I have been on about for a quite some years. But maybe I really am missing a subtle (or not so subtle) distinction between your system and ours. We should get this straight, whether I'm missing something.

It'll be great to see you again tomorrow. I only fear that the discussion may be a little too multi-pronged with so many people wanting to see Allen!

02-12-08 *My Sloppy Reading* (to H. C. von Baeyer)

von Baeyerism 27: *Just off the top of my head, did you notice that the page number in the note was 137?*

No, I had not noticed that. Of course, now that you say it, it is glaringly obvious. You might have pity on the other readers similarly blind as me, modifying what you write to something like: "Note the page number. Pauli would have been very pleased by it turning out to be 137. He considered this to be the most important number in physics, and its mystical connotations did not pass him by . . .

von Baeyerism 28: *The idea of entangling the observer with the observation is, of course, nonsense.*

That is right, and somehow I had not noticed that. But also, now that you bring it up, perhaps the definition could use some tuning. You write, "The term referred to the scientific view of the world as seen 'from the outside', as it were, by an observer who neither influences nor is influenced by the world." I wouldn't go so far as to say that the observer is not influenced by the world. That would preclude any (classical) Bayesian from updating his expectations upon the acquisition of data. A significant trouble I always perceived Pauli had with the "ideal E" was its one-sidedness.

Here's one of the formulations I've given the point from time to time (this from 2003):

> Everybody has their favorite speculation about what powers quantum information and computing. Some say it is the superposition principle, some say it is the parallel computation of many worlds, some say it is the mysteries of quantum entanglement, some say it is the exponential growth of computational space due to the tensor product. For my own part though, my favorite speculation is that it is Newton's Third Law: For every action, there is an equal and opposite reaction. Indeed I sometimes wonder if

the very essence of quantum mechanics isn't just this principle, only carried through far more consistently than Newton could have envisioned. That is to say, absolutely NOTHING is exempt from it.

What do I mean by this? What might have been exempt from the principle in the first place? To give an answer, let me note an equivalent formulation of old Newton. For every REACTION, there is an equal and opposite ACTION. Strange sounding, but there's nothing wrong with it, and more importantly, this formulation allows for the possibility of an immediate connection to information theory. In particular, we should not forget how information gathering is represented in the Shannon theory. An agent has gathered information—by the very definition of the process—when something in his environment has caused him to REACT by way of revising a prior expectation $p(h)$ (for some phenomenon) to a posterior expectation $p(h|d)$ (for the same phenomenon).

When information is gathered, it is because we are reacting to the stimulation of something external to us. The great lesson of quantum mechanics may just be that information gathering is physical. Even something so seemingly unimportant to the rest of the universe as the reactions that cause the revisions of our expectations are not exempt from Newton's Third Law. When we react to the world's stimulations upon us, it too must react to our stimulations upon it.

The question is, how might we envision a world with this property—i.e., with such a serious accounting of Newton's law—but in a way that does not make a priori use of the information gathering agent himself? If the question can be answered at all, the task of finding an answer will be some tall order. For never before in science have we encountered a situation where the theorizing scientist is so inextricably bound up with what he is trying to theorize about in the first place.

It's almost a paradoxical situation. On the one hand we'd like to step outside the world and get a clear view of what it looks like without the scientist necessarily in the picture. But on the other hand, to even pose the question we have to imagine an information gathering agent set in the middle of it all. You see, neither Shannon nor any of modern information theory has given us a way to talk about the concept of information gain without first introducing the agent-centered concept of an expectation $p(h)$.

So, how to make progress? What we do know is that we actually are in the middle of the world thinking about it. Maybe our strategy ought to be to use that very vantage point to get as close as we can to the goal. That is, though we may not know what the world looks like without the information gathering agent in it, we certainly do know something about what it looks like with him in: We know, for instance, that he ought to use the formal structure of quantum mechanics when thinking about physical systems. Beyond that, we know of an imaginary world where Newton's Third Law was never taken so seriously: It is the standard world of classical physics and Bayesian probability.

Thus, maybe the thing to do first is to look inward, before looking outward. About ourselves, at the very least, we can ask how has the formal structure of our *behavior* changed since moving from what we thought to be a classical Bayesian world to what we now believe to be a quantum world? In that DIFFERENTIAL—the speculation is—we may just find the cleanest statement yet of what the quantum world is all about. For it is in that differential, that the world without us surely rears its head.

To do this, we must first express quantum mechanics in a way that it can be directly compared to classical Bayesian theory, where the information-gathering agent was detached from the world. That is what this lecture is about ...

Rereading this, I enjoyed how it made a connection to the title of your draft ["Wolfgang Pauli's Journey Inward"].

02-12-08 *Prolegomenon (I'd better not use this word in the subject line, lest I accept it)* (to H. C. von Baeyer)

Well, I hope that is the first step toward accepting it! Really, you could do so much good, and of all the people I've met nominally interested in these matters, you're the only one with the right combination of skills to be able to pull off something of value if you set your mind to it.

I am relieved to find out the reasons for your blockage. Both yin and yang look reparable to me.

The point for me is not that all he was thinking (or hoping) will turn out to be right, but that SOME of what he was thinking is inspirational for a solid technical turn we can make in physics. I have gotten immense technical inspiration from the little bits I know about the Pauli (and Pauli-Fierz) correspondence. I crave more, and I think knowing more will get us to that technical point oh so much faster than if we have to slog our way there with our own less-than-Pauli'an minds.

I really do applaud this article. I thought I had completely lost you. Emma came home with nearly straight A's yesterday (except in "dance" which I didn't care about, and French, which I do care about, but in this case can blame on the teacher and the broken Ontario system for teaching French). I was so proud of her. So I was already on a bit of a high, but your article reinforced the same feeling. I am proud that Pauli'an things are still on your mind.

Hans's Preply

> I have now fixed the typos. Thanks! [Gewissen] is short for a book entitled *Wolfgang Pauli – Das Gewissen der Physik*. I don't have the bibliographic reference, which somehow got erased, here with me.
>
> Your question about my block is very good, and worth talking about.
>
> I had no trouble writing about Pauli's career in physics, up to page 14. I did that early in the summer, and have been polishing it since then.
>
> But then I stopped for two (possibly related) reasons, one literary, and the other psychological.
>
> The literary reason was that there is just too much material: The detached observer, the significance of symbols, archetypes, Fierz, the psycho-physical "neutral language", Kepler .... I couldn't figure out how to organize it all. Just taking one topic after another made it a laundry list, and no matter what I tried after p. 14, it all sounded tedious.
>
> Then I came across the letter in which Pauli sends the dreams, and I had an inspiration. I removed Fierz from the first 14 pages, popped in some ***s and told the story about the trip to Princeton. That provided a turning point, and then I decided to concentrate on just the letters from that period. If the topics I mentioned above came up, fine, if not, not. The neutral language, for example, made it only into one sentence as "common language". The ocean voyage fit perfectly with the metaphor of voyage that I had decided on long before. Now I had a structure!
>
> The other reason for my block is more serious. Crudely put, the question is: Do I believe all this stuff? Res Jost, a famous theorist and colleague of Pauli, did not.

Aldous Huxley did not. My brother Carl, a smart psychologist in Saskatoon, does not. They are all "rationalists". What am I?

The reason I'm taking it seriously is because Pauli is so damn smart. And so were Kepler, Newton, and Goethe, who were also mystics. Well, uncle Fierz comes to the rescue when he says "you know, I can really agree with you *provided* you express the reconciliation of science and mysticism as a goal for the future" (hundreds of years from now, Appleby told me). And lo! when you read Pauli, he says over and over again that he doesn't know how the two will be reconciled. So I'm off the hook! I don't have to believe all this mystical stuff, only that some day it may come back into vogue.

So you see, I had an inner and an outer conflict with this story. Yin and yang.

02-12-08 *Volumes* (to H. C. von Baeyer)

Wild idea. If I were to purchase one or more volumes of Pauli correspondence to have by my side, which would be the most important ones with respect to what I want to learn? I already have a couple of German-English dictionaries. Who knows what a little will power might accomplish.

03-12-08 *Seeking SICs Story* (to E. Goheen)

I hope you can use the whole thing. I spent quite some time in the middle of the night getting all the words just right, so that I might use this as an opportunity to educate the noninitiated in the PI community about the problem. Partly educational, partly entertaining. Story below.

I have to take a guest to breakfast. I'll be back online as of 11:00 if you have any questions.

Seeking SICs: An Intense Workshop on Quantum Frames and Designs

At the heart of quantum mechanics is Hilbert space. But what is at the heart of Hilbert space? Surprisingly, there are still some very basic—indeed, very fundamental—questions about finite dimensional complex vector spaces desperately seeking an answer. The theme of the workshop "Seeking SICs" was one of these questions.

For a Hilbert space of finite dimension d, can one always find a set of d^2 equiangular lines—that is, a set of lines for which the angle between any two is always the same? Such structures are called SICs (pronounced "seeks"), because in another language they can be thought of as "symmetric informationally complete" sets of quantum states. The phrase informationally complete captures the idea that the projection operators onto such a set of lines form a complete basis for the vector space of Hermitian operators, and hence of quantum states. A SIC, however, wouldn't be just any old basis for the space; it would be a nearly magical one. For, fixing a SIC in the background gives a means to express quantum states in a way that maps the whole structure of quantum mechanics into as mild a variation of (classical) probability theory as one can imagine: quantum states themselves become simple probability distributions; unitary evolution becomes, a up to a single constant, classic stochastic evolution; the Born probability rule becomes a simple variation on the classic law of total probability. The list of similarities grows. Thus SICs, if they exist, would be a highly valued commodity in the quantum foundations game. They would give a new, powerful way to rewrite (or dismiss!) many an old conceptual problem.

But do they exist? That's no easy question! The winner of the best intuition for it of all recent PI visitors is Sir Roger Penrose, who could "see" that they exist in $d = 2$, 3, and 4. But for the rest of us it's been a rough slog. After 9 years of toil since the problem first arose, nearly 20 papers, and some involvement from the best minds in quantum information theory (like Peter Shor, William Wootters, and Marcus Appleby), the most that is known either through hand proof, machine proof, or numerical investigation is that they (appear to) exist for all d from 2 to 47. Beyond that, nothing definitive is known ... but most people believe! And that's the fuel that can organize a conference.

The purpose of "Seeking SICs" was to amass as many of the best in the field as we could to have an intense go at it in the companionship and moral support of each other. The venue consisted of 11 organized talks across five days, with the remaining time devoted to informal idea sessions and chalkboard discussions. Several from the local quantum information community joined the meeting from time to time, bringing the total number of participants up to about 20 for some of the days. The meeting, as the name promised, was an intense event. By all accounts it was a success. Several of the participants particularly commented on how surprised they were to learn that so much UNpublished progress had been made since the last major works were posted 3–4 years ago. There was a great flood of information sharing, and perhaps one of the lasting achievements of the conferences was the establishment of a SIC wiki for building a strong online community to work on this problem.

What do we know now that we didn't know at the beginning of the meeting? Well, we now know by machine proof that SICs do indeed exist in $d = 14$ (it was only numerical before), and we know a method that leads to their hand construction in $d = 6$. More than that, we know the community has been reinvigorated and that maybe in the coming months we'll get a proof for the remaining (discrete) infinity of dimensions!

03-12-08 *I Twitter, Therefore I Am* (to A. Stairs)

Funny to learn about twittering from you yesterday nearly at the same time as a discussion about Jungian synchronicity ... and then to find this article in the *Washington Post* this morning: `http://www.washingtonpost.com/wp-dyn/content/article/2008/12/02/AR2008120202935.html?hpid=opinionsbox1`. Then, to read the content of this and find that it has something mildly to do with Zeilinger's view of quantum mechanics. "In the Information Age: Knowing equals Being," so said.

Surely there's a meaningful coincidence in there somewhere!

From a strange hour of the night ...

09-12-08 *Quantum Random Numbers* (to R. Schack & C. M. Caves)

I'm writing to make you both aware of a talk Ben Schumacher gave here at PI last week. It can be found at this site: `http://pirsa.org/08120020/`. The talk is not at all on quantum random number generators, but I took it that the deep idea in it is the same as the one that you two make use of. **"quantum correlations are monogamous"** = **"no inside information"** = **"unperformed measurements have no outcomes"**. Anyway, I think the talk was extremely exciting and helps put your work in a more general context.

Check it out.

10-12-08 *All the Qualifiers* (to R. W. Spekkens)

(Metaphorically) load your paper tray, print out

`http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`

and thumb through the document until you get to the pages listed below.

Note all the qualifiers. By my own admission I don't have a completely consistent story yet, but I don't think you can ignore the overwhelming theme. See particularly the last three quotes.

Obsession is my trademark, I suppose.

Funny: One interesting self-psychology thing I learned from this exercise is that I didn't mention "law of thought" even once in the last 130 pages of the document. I wonder what exactly that signifies.

- p. 14, "more a ... than" in:

> But that is precisely why I would call quantum mechanics more a "law of thought" than a "law of nature." Just as Boole did of probability theory. And just as Jaynes did with (classical) statistical mechanics. (In fact, either of you two—i.e., you or Jaynes—might just as well have written your quote above.)
>
> You might say, and I suspect you will say, the distinction I draw between "nature" and "thought" is only a semantic one. But I don't believe that: I think it is precisely in making that distinction clear (and operational) that we have a chance of closing the quantum foundations debate and moving on.
>
> Physical theory is about two things: what is and what we know (or what we believe). It's the process of putting the two things together that gives a prediction in any practical setting. Quantum theory, interestingly, seems to be a nontrivial jumble of those two things. I think that is a rather deep statement about the world, and one that we have not yet come to grips with.

- p. 16, "with a ... side-input":

> Since the overarching belief here is that "quantum mechanics is a law of thought" (with an almost trivial side-input of honest-to-god physics), it strikes me as being in a category much more like relativity. In the words of J. Bub, it is a "framework theory."

- p. 38, "PLUS" (in letter to J. Bub):

> I know I suggested I would write a longer letter soon, but I'm going to wimp out of it again for now. It would concern the main point of distinction I see between us (and also between myself and Pitowsky). Namely, A) that I view a large part of quantum mechanics as merely classical probability theory (which on my view may be an a priori "law of thought") PLUS an extra assumption narrowing down the characteristics of the phenomena to which we happen to be applying it to at the moment, while B) you are more tempted to view quantum mechanics as a *generalization* of classical probability theory (and with it information theory). I know that my view is not fully consistent yet, especially as I have always distrusted mathematical Platonism—which you pointed out to me I am getting oh so close to—but it still feels more right (to me, of course).

- p. 137, "substantial part of":

I think our greatest hint of that comes from quantum mechanics. I would say that what we're learning in a precise way from it is that there is something about the stuff of the world that makes it uncaptureable with a purely physical picture. We find that we cannot even draw a picture of the world without including our beliefs and belief changes as a crucial background in the sketch. (How could we if the world's not completed yet?)

Does that make me an extreme subjectivist? I don't know. Whatever it is though that I should be called, I think this willingness to accept a substantial part of quantum mechanics as simply "law of thought" will keep me from going down a misguided path. I.e., the path of trying to ascribe all the easiest terms in the theory a kind of physical reality independent of our presence as active agents.

- p. 205, "something that is ... not law of thought":

  Here's where I really think you sell yourself short by advertising your system as an extension or generalization of classical probability theory (with classical probability theory as a special case that's gotten by deleting one of the axioms). For I would say that your framework of "states" as vectors and "measurements" as applications of Bayes' rule is *classical probability theory*, full stop. Or, I should just say "*probability theory*, full stop"—for, the word "classical" seems to imply that it is a subject somehow within empirical science (rather than "law of thought" that antecedes science). In showing me that even quantum "measurements" can be viewed legitimately as nothing more than applications of Bayes' rule, you have done me a great service. For you demonstrate to me more clearly than ever that the concept of POVM ought to be put onto the subjective side of the shelf when I tear quantum mechanics into its two components. But your other intriguing axioms—like the simplicity and composite-system axioms—which you think give the possibility of generalizing upon classical probability, I would say are nothing of the sort. Instead, I would say they express just the opposite. These axioms seem to me to say something about what we are positing of nature. They express something that is not subjective and is not "law of thought."

- p. 272, "to the extent that":

  Now, to quantum mechanics. You find something contradictory about my liking both quantum mechanics and Rorty. Here is the way I would put it. Presently at least, I am not inclined to accept quantum mechanics "to be real, an 'intrinsic nature of reality'," except insofar as, or to the extent that, it is a "law of thought," much like simple (Bayesian) probability theory. Instead, I view quantum mechanics to be the first *rigorous* hint we have that there might actually be something to James's vision.

- p. 343, "there is a part that remains":

  Lee Smolin asks me how I could possibly imagine that the linear structure of quantum mechanics will remain when one moves into such a nonlinear regime as that given by the laws of gravity? I say I'm not fazed at all: Most of what he means when he speaks of quantum mechanics as an expression of physics, is for me but a law of thought. A wave function and its evolution are not properties intrinsic to the system for which they are about. Rather, if they are properties of anything at all, they are properties of their user's head—for they capture all his judgments about what might occur if he were to interact with the system of interest.

  The quantum foundational task as I see it is to baldly accept that A LARGE PART of the theory is simply not about a world without observers: It is only about our interface with the world. But there is a part that remains, and that part must be given a firm identification.

For only once we know how to do that will we know how to move forward when it comes to gravity. Only then will we see that almost all of the ways that have hitherto been considered for combining quantum mechanics with general relativity were far too unconstrained: I am willing to bet that they all essentially boil down to sheer speculation.

- p. 445, "branch of decision theory that is contingent upon properties of the world":

  Be warned that by the phrase I don't mean something like a Kantian a priori category, i.e., a position like von Weizsäcker's in his book *The Unity of Nature*. I don't mean something like, "an understanding using the terms of quantum mechanics is the precondition for possible experience." Rather I have started to toy rather strongly with a Darwinian kind of idea: Using the rules of quantum mechanics for manipulating and updating our expectations (i.e., as a "law of thought") is the presently best known means for survival, given that we are immersed in the particular world we are. That is, I want to view quantum theory as a branch of decision theory that is contingent upon properties of the world we live in ... and it is something we locked into only in our most recent turn in evolutionary development.

- p. 488, "retains a trace of":

  When I say that QM is a theory about a very small part of the world, you should literally think of a map of the United States in relation to the rest of the globe. The map of the US is certainly incomplete in the sense that it is obviously not a map of the whole globe. But on the other hand it is as complete as it can be (by definition) as a representation of the US. There are no hidden variables that one can add to the US map that will magically turn into a map of the whole globe after all. The US map is what it is and need be nothing more.

  Does that help any?

  I think a good bit of the problem comes from something that was beat into most of us at an early age. It is this idea: Whatever else it is, quantum theory should be construed as a theory of the world. The formalism and the terms within the formalism somehow reflect what is out there in the world. Thus, if there is more to the world than quantum theory holds out for, the theory must be incomplete. And we should seek to find what will complete it.

  But my tack has been to say that that is a false image or a false expectation. Quantum theory from my view is not so much a law of nature (as the usual view takes), but rather a law of thought. In a slogan: Quantum mechanics is a law of thought. It is a way of plagiarizing George Boole who called probability theory a law of thought. (Look at the first couple of entries in the Rüdiger Schack chapter of *Notes on a Paulian Idea*.) Try to think of it in these terms, and let's see if this helps.

  ... [Long story about probability theory, then continues with:] ... So I say with quantum mechanics. The story is almost one-to-one the same: You just replace probability distributions with quantum states. ... But then you reply, "But there's a difference; quantum theory is a theory of physics, it is not simply a calculus of thought." And I say, "That's where you err." Quantum theory retains a trace of something about the real, physical world but predominantly it is a law of thought that agents should use when navigating in the (real, physical) world. In particular, just like with probability theory, we should not think of quantum theory as incomplete in the usual sense. If it is incomplete in any way, it is only incomplete in the way that the US map is incomplete with respect to the globe: There's a lot more land and ocean out there.

Teasing out (your words) the trace of the physical world in the formalism—i.e., the part of the theory that compels the rest of it as a useful law of thought—is the only way I see to get a solid handle on what quantum mechanics is trying to tell us about nature itself.

With this let me now go back to the US map for one final analogy. I said that there is a sense in which the US map is as complete as it can be. However there is also a sense in which it tells us something about the wider world: If we tabulate the distances between cities, we can't help but notice that the map is probably best drawn on the surface of a globe. I.e., the US already reveals a good guess on the curvature of the world as a whole—it hints that the world is not flat. And that's a great addition to our knowledge! For it tells a would-be Columbus that he can safely go out and explore new territories. Exploring those new territories won't make the US map any more complete, but it still means that there is a great adventure in front of him.

- p. 532, "THIS ... rather than THAT":

  Concerning, "But I did think it was a Law of Thought for you." ... you never cease to shock me .... And you never cease to cause me to strive to try to convey the very simple little idea more effectively! I wonder when I'm gonna finally hit the sweet spot? Quantum THEORY, a law of thought: Yes. Resoundingly yes. But the quantum WORLD—i.e., that situation, that world, that reality, which conditions us to choose THIS law of thought rather than THAT law of thought (in other words some alternative or imaginary law of thought)—is something else entirely. It's the stuff that's here whether there are any law-of-thoughters around or not. That's what I really want to get at; that's what I've always really wanted to get at.

- p. 533, "mostly":

  This talk tried to set the tone of the meeting by demonstrating that much of the content of finite-dimensional quantum mechanics reduces to two simple modifications of Bayesian ideology—1) the setting of a theory of prior probabilities with regard to the outcomes of a single special quantum measurement, and 2) a modification of the standard Bayesian conditionalization rule for updating probabilities in the light of new information. From this perspective, the formal structure of quantum mechanics becomes *mostly* a "law of thought" (in the same sense that George Boole called probability theory a "law of thought") rather than a "law of nature." Where nature still rears its head—i.e., makes its contingently given empirical content known—is through the higher-level set of reasons for why decision-making agents in this world should use *this* law of thought (i.e., quantum mechanics) rather than *that* law of thought (i.e., some foil theory other than quantum mechanics).

- p. 642, "partially tongue in cheek"

  I am in agreement with your last sentence. Attempts like the former effectively erase the empirical content (or contingency) of quantum mechanics, and I just don't see that. When I myself invoke the slogan "Quantum Mechanics is a Law of Thought," I only do it partially tongue in cheek, quickly correcting myself. For if quantum mechanics were *only* law of thought, it would be like one of Kant's a priori categories of the understanding. On the other hand, I come dangerously close to viewing "probability theory" (i.e., the theory of coherent gambling) in such a Kantian kind of way. And, indeed, it is partially because of this that I would not want to view quantum mechanics as a *generalized* probability theory.

10-12-08 *Myrvold's "Chance"* (to R. Schack)

BTW, I had wanted to give you a heads-up about Wayne Myrvold's talk in Jerusalem. He proposes a kind of epistemically based notion of "chance" that to some extent tracks the sort of thing I say below (from a letter to Mermin two years ago):

> With regard to your phase-diagram remark in particular [I.e. "Don't be so quick to dismiss Leggett as an ideologue, just because he takes quantum states to be objective. It's hard to spend decades worrying about the phase diagram of helium-3 without believing that quantum states are objective."], I think it's got to be instructive to try to imagine what a turn-of-the-century physical chemist (trained in the worldview of classical physics) should have thought of the tables of heat capacities and whatnot that he had spent much of his life compiling. What can those numbers represent in the classical worldview? Why would two distinct chemists compile the same tables of numbers if those numbers didn't signify something objective and intrinsic to the chemicals themselves? Well, a Maxwell demon wouldn't compile the same tables. And there is surely a continuum of positions between us and him. What those tables represent is something about the relation between us, the coarseness of our senses, our inabilities to manipulate bulk materials, all kinds of things like that, and the materials themselves. Those numbers are just as much about us ("anyman") as about "it."
>
> Anyway, forgetting about Leggett, how would I convince the classical physical chemist of such a point of view? It would be damned hard (as history has already shown). Instead of skirting so close to such an anti-Copernican idea—that knowledge gained in science is as much about "us" as about "it"—he'd probably spend his life trying to poke holes in the conceptually simple Maxwell demon idea and trying to return his beloved thermodynamical quantities to their pristine objectivity (a place where they'll last forever).

I was shocked! I had never imagined I would agree with Myrvold about nearly anything. Anyway, I thought it was a good talk. I want to see if you have the same reaction, or whether I missed something devastatingly inconsistent between his idea and our program.

When do you leave for Jerusalem? I presume you'll have email contact there for chit-chat about the draft I'm working on. You will take your laptop, won't you?!?

11-12-08 *A Sorry Comrade* (to R. W. Spekkens)

I'm feeling very bad about this, because I've hardly had any interaction with the candidates and feel responsible to do so, but I'm just in too much pain to come in today. [. . .]

At the moment I've got my foot in our lobster pot, surrounded by a bath of warm Epsom salt water, and my laptop on my lap, writing on my "coherence" paper. Digging up things on Feynman for the paper, I came across the following quote from David Mermin in his article "Spooky Actions at a Distance: Mysteries of the Quantum Theory". I figure it might be of use when we finally put together our "roots" article.

> Most physicists, I think it is fair to say, are not bothered. A minority would maintain that this is because the majority simply refuse to think about the problem, but in view of the persistent failure of any new physics to emerge from the puzzle in the half century since Einstein, Podolsky, and Rosen invented it, it is hard to fault their strategy.

12-12-08 *Myrvold's "Chance," 2* (to R. Schack)

Schackcosm 108: *Isn't this more or less the point of Jaynes' approach to thermodynamics and all the semi-objective ways of getting entropy to increase: coarse-graining, mixing interaction with an environment, ignoring all details that are irrelevant for pressure, volume etc? The idea would be to define a standard of ignorance, that any honest and nondeluded person would subscribe to.*

Roughly, but with a slight twist. In place of "ignoring all details" something more like "biologically incapable of." "Ignoring" implies that you could get at these things if you wanted them. With hindsight, surely it is a kernel of thought motivated by your hypersensitivity program.

More in a minute.

12-12-08 *Lunch Break* (to R. Schack)

I'm sitting in my office eating a curried chicken sandwich.

My only worry in what you say below is the phrase "classical deterministic theory" which is usually taken to mean something about time dependence of variables. The crucial distinction is only in that classical variables are assumed to be "determinate" in the sense of possessing a truth value independent of measurement.

12-12-08 *F-Theory* (to R. W. Spekkens)

Spekkensism 46: *Did you realize that the string theorists are studying F-theory?*

I'm sure there are a lot of people named (and a lot of expletives starting with) F.

What a loon they would think of me, if they came across my credo below: [See 12-02-07 note "Lord Zanzibar" to J. Christian and L. Hardy.]

14-12-08 *Our Dismissal* (to C. M. Caves & R. Schack)

Here's a cute dismissal of us. See Footnote 7 in

`http://philsci-archive.pitt.edu/archive/00002839/01/epist_rev.pdf`.

14-12-08 *FPP5* (to G. Adenier)

Attached is my first contribution for the volume; it is a joint paper with Rüdiger Schack titled "Priors in Quantum Bayesian Inference."

I still owe you my paragraphs for the preface. I should definitely be able to get those to you tomorrow, but it will be in the afternoon (my time).

I would also like to have a second contribution to the volume; it is another joint paper with Schack, but I think this paper will be of lasting significance, somewhat like my earlier paper "Quantum Mechanics as Quantum Information (and only a little more)" was in one of your volumes several years ago. But because of that, it is a much more complex affair to write, and I have been working very hard at it. Is there any chance you could hold off until Wednesday for it? I would very much like to have it in the volume—as it is another important manifesto for me—but I understand I must be pushing you to the limit.

15-12-08 *Are You Out There?* (to J.-Å Larsson)

How late will you be out there today reading email and such? I'm about an hour or two away from getting our preface written (OK, I have to admit I just started writing), and I'd like to pass it by you for comment. Guillaume was hoping I'd submit it today.

Just checking. Now, I'm writing away. It shouldn't be hard.

P.S. Great news in SIC world today. Andrew Scott has finally pushed the numerics further: SICs are now known to exist (at least to the 38 significant figures) in dimensions up to $d = 60$! (I placed a space before the exclamation mark so that you wouldn't think I meant 60 factorial.)

15-12-08 *Draft* (to J.-Å Larsson)

Attached is the draft. I feel that a sentence or two could be added about your and Cabello's papers. Could you volunteer that? Also, I surely missed some important papers in the biblio; please add anything you'd like.

I hope the writing style doesn't offend you.

Foreword: Unperformed Experiments Have No Results

> This year in Växjö we thought we would try an experiment—it felt high time for a new result. Much of the foundations discussion of previous years has focussed on EPR-style arguments and the meaning and experimental validity of various Bell inequality violations. Yet, there is another pillar of the quantum foundations puzzle that has hardly received any attention in our great series of meetings: It is the phenomenon first demonstrated by Kochen and Specker, quantum contextuality. Recently there has been a rapid growth of activity aimed toward better understanding this aspect of quantum mechanics, which Asher Peres sloganized by the phrase, "unperformed experiments have no results." Below is a sampling of some important papers on the topic for the reader not yet familiar with the subject.
>
> What is the source of this phenomenon? Does it depend only on high level features of quantum mechanics, or is it deep in the conceptual framework on which the theory rests? Might it, for instance, arise from the way quantum mechanics amends the classic laws of probability? What are the mathematically simplest ways contextuality can be demonstrated? How might the known results be made amenable to experimental tests? These were the sorts of discussions we hoped the session would foster.
>
> There were eight speakers in our special session—D. M. Appleby (Queen Mary U London, United Kingdom), G. Bacciagaluppi (U Sydney, Australia), I. Bengtsson (Stockholm U, Sweden), A. Cabello (U Sevilla, Spain), J. Emerson (U Waterloo, Canada), C. A. Fuchs (Perimeter Institute, Canada), J.-Å. Larsson (Linköpings U, Sweden), and R. Schack (Royal Holloway U London, United Kingdom)—and the talks were of uniformly high quality and all very enlightening. In this volume, seven of the talks are represented with written contributions.[165] Three of the papers are devoted very directly to Kochen-Specker constructions, inequalities derivable from them, and questions of experimental feasibility: I. Bengtsson, "A Kochen-Specker Inequality"; A. Cabello, "Kochen-Specker Meets Experiments"; J.-Å. Larsson, "The Kochen-Specker

[165] The content of J. Emerson's presentation can be found in the paper: C. Ferrie and J. Emerson, "Frame Representations of Quantum Mechanics and the Necessity of Negativity in Quasi-Probability Representations," J. Phys. A **41** 352001 (2008). In relevance to this session, it is connected to the Spekkens reference on negativity below.

Paradox and Great-Circle Descents". G. Bacciagaluppi's paper "Leggett's Theorem without Inequalities" defines a new no-go theorem that quantum mechanics once again violates: the conjunction of parameter independence and a new condition called "conditional parameter independence." The paper by C. A. Fuchs and R. Schack, "Priors in Quantum Bayesian Inference," is meant to be a follow-up on C. M. Caves, C. A. Fuchs, and R. Schack, "Subjective Probability and Quantum Certainty," (Stud. Hist. Phil. Mod. Phys. **38**, 2007, p. 255), where A. Stairs' modification of the Kochen-Specker noncolorability result was developed into an argument that even probability-1 assignments do not signify the pre-existence of measurement values. The paper by D. M. Appleby, "SIC-POVMS and MUBs: Geometrical Relationships in Prime Dimension," though not directly about issues of contextuality, is part of a larger effort to rewrite quantum mechanics in a language that will be much more serviceable to these kinds of questions. Finally, the paper by C. A. Fuchs and R. Schack, "From Quantum Interference to Bayesian Coherence and Back Round Again," is an example of that larger effort: It argues that the essence of Peres's slogan boils down to the fact that the law of total probability is replaced by a modification when considering counterfactual SIC measurements alongside actualized quantum measurements.

We were very happy to have an opportunity to organize this special session in Växjö, where there is a long tradition of delightful, intense, and thought-provoking foundational discussions. As the reader will find, this experiment was one of those well worth performing.

C. A. Fuchs

J.-Å. Larsson

1. A. M. Gleason, "Measures on the Closed Subspaces of a Hilbert Space," J. Math. Mech. **6**, 885 (1957).
2. S. Kochen and E. P. Specker, "The problem of hidden variables in quantum mechanics," J. Math. Mech. **17**, 59 (1967).
3. A. Peres, "Unperformed Experiments Have No Results," Am. J. Phys. **46**, 745 (1978).
4. N. D. Mermin, "Hidden Variables and the Two Theorems of John Bell," Rev. Mod. Phys. **65**, 803 (1993).
5. A. Cabello, J. M. Estebaranz and G. Garcia-Alcaine, "Bell-Kochen-Specker Theorem: A Proof with 18 Vectors," Phys. Lett. A **212**, 183 (1996).
6. J.-Å. Larsson, "A Kochen-Specker Inequality," Europhys. Lett. **58**, 799 (2002).
7. D. M. Appleby, "The Bell-Kochen-Specker Theorem," Stud. Hist. Philos. Mod. Phys. **36**, 1 (2005).
8. R. W. Spekkens, "Contextuality for Preparations, Transformations, and Unsharp Measurements," Phys. Rev. A **71**, 052108 (2005).
9. J. Conway and S. Kochen, "The Free Will Theorem," Found. Phys. **36**, 1441 (2006).
10. R. W. Spekkens, "Negativity and Contextuality are Equivalent Notions of Nonclassicality," Phys. Rev. Lett. **101**, 020401 (2008).
11. A. Cabello, "Experimentally Testable State-Independent Quantum Contextuality," Phys. Rev. Lett. **101**, 210401 (2008).

15-12-08 *Bad Foot* (to K. A. Peacock)

Another interesting person for you to talk to is Rafael Sorkin (on the same floor as you). He too believes that part of the lesson of quantum mechanics is that the universe is continually "giving birth."

18-12-08 *Israel* (to R. Schack)

Schackcosm 109: *I think my talk went well. There was a long discussion.*

David Albert interrupted me after 5 seconds with the remark that the mottos "unperformed experiments have no outcomes" and "the concept of probability is unaltered in qm" are hardly controversial. His main objection in the discussion was that the Einstein criterion of reality is like "bachelors are unmarried", i.e. analytic, and can not be disproved by empirical arguments. Hence the Stairs argument shows that locality has to be abandoned.

It may be for an objective notion of probability (though I doubt it even there), but it certainly isn't for a subjective notion. It's amazing that these guys don't even realize the boxes they think in. And they're the ones who are supposed to be the philosophers.

20-12-08 *Friday* (to R. Schack)

Schackcosm 110: *Before you count Wayne as a new ally, be warned that he strongly believes in objective quantum states.*

Oh, don't worry; I know Wayne all too well. I just thought he was clear on this point, and I had wondered whether I was missing something. (My fear of having misheard something comes from the knowing-Wayne-all-too-well-point just mentioned.)

21-12-08 *OK, You Should Be Back Home* (to N. D. Mermin)

OK, you should be back home from Tempe by now. You told me to write you after your return. PI visit, when?

Lucien told me about the meeting. Makes me double glad I wasn't there.

But I wish Carl had been there, so that you two might have had a chance to talk. I sense you haven't thought as much about (the reasons for) quantum mechanics as you ought to have lately. Carl sent me a link to the talk he would have given. (See, it's all about the counterfactuals.) Link below. I'll forward it to you in case you want to peruse it.

Did you see Louisa Gilder's book? (She must have sent you a copy if she sent me a copy.) I don't know how well she did with everyone else's dialog yet, but she certainly got me on the mark. I was very pleased with that. (And pleasantly surprised. Since meeting her briefly three years ago, I had forgotten about her.)

22-12-08 *Nonlocal Boxes and Ehrenfest* (to N. D. Mermin)

Just back in from shoveling snow.

Merminition 174: *Have you ever run across this particular way of presenting history — in terms of highly imaginary conversations based on texts, memoirs, reminiscences?*

The only thing approaching it that I can think of is Susan Haack's article "We Pragmatists." She managed to put together a good conversation between herself, Peirce and Rorty—with P and R's contributions being solely from published stuff.

I haven't read more than a few beginning pages of the book yet. I do know that already at page 7 I didn't like the sentence, "But no one following Bohr, Heisenberg, Pauli, Dirac, or Born dared grasp, measure, or even name the deepest of all puzzles, entanglement. Then along came John Bell." I feel that Pauli, for instance, already had a decent grasp of entanglement in 1927. It's just simply that the lesson of it was taken to be something else than the Einstein-Schrödinger-Bell camp thought it should be, and so it was marginalized in their thought. The lesson wasn't nonlocality for Pauli, but nondetachedness of the observer. And "degree of detachedness" is something he did try to quantify.

Pet peeve of mine recently, with this incessant talk of "nonlocal boxes" everywhere I go. So, I put these lines in my "coherence" paper (the one I'm writing at the moment). Particularly the footnote:

> So, what is it that Bell inequality violations teach us that the double-slit experiment does not? A common, if quick and dirty, answer is that "local realism fails." Unpacking this phrase, one means more precisely the conjunction of two statements: 1) that actions or experiments in one region of spacetime cannot instantaneously affect matters of fact at far away regions of spacetime, and 2) that measured values *pre-exist* the act of measurement, which merely "reads off" the values, rather than enacting or creating them by the process itself. The failure of local realism means the failure of one or the other or both of these statements. This, many would say, is the essential "mystery" of quantum mechanics.
>
> But the mystery, as already emphasized, has two sides. Of the options, it seems the majority of physicists who care about these matters think it is locality (Option 1 above) that goes down the drain with a Bell inequality violation—i.e., that there really are "spooky actions at a distance."[fn] But there is a small minority that thinks the abandonment of Option 2 is the much more sensible conclusion and among these are the *quantum Bayesians*. In a slogan near to that of Asher Peres's, "unperformed measurements have no outcomes."
>
> [fn] Indeed, it flavors almost everything they think of quantum mechanics, including the *interpretation* of the *imaginary* games they use to better understand quantum mechanics itself. Take the recent flurry of work on Popescu-Rohrlich boxes. These are imaginary devices that give rise to greater-than-quantum violations of various Bell inequalities. Importantly, another common name for these devices is the term "nonlocal boxes." Their exact definition comes via the magnitude of a Bell inequality violation—which entails violation of locality or pre-existence of values or both—but the commonly used name opts only to recognize nonlocality. They're not called anti-realism boxes, for instance. The nomenclature is psychologically telling.

By the way, I really could use that reference to your remarks on double-slits. Please continue to search your mind.

Merminition 175: *Did you know that (as she claims) the famous Bohr-Einstein clock-in-the-box exchange was (from Einstein's point of view) not an attack on the energy-time uncertainty relation but an early version of EPR, the point of which Bohr simply missed. I've never seen it presented that way. Haven't had a chance to check out the evidence for that. It surprised me. Is that in Arthur Fine's book? Or Don Howard?*

Well, I do know it morphed into that sometime after the Solvay meeting. I know that from this passage in Max Jammer's article "The EPR Problem In Its Historical Development," in *Symposium on the Foundations of Modern Physics: 50 Years of the Einstein-Podolsky-Rosen Gedankenexperiment*, edited by P. Lahti and P. Mittelstaedt (World Scientific, Singapore, 1985), pp. 129–149, which I have quoted in the original samizdat:

> [W]hat Einstein had in mind is confirmed by a letter which Ehrenfest wrote to Bohr on July 9, 1931. As Ehrenfest reports, Einstein uses the photon-box no longer to disprove the uncertainty relation but "for a totally different purpose." For the machine, which Einstein constructs, emits a projectile; well after this projectile has left, a questioner can ask the machinist, by free choice, to predict by examining the machine alone *either* what value a quantity A *or* what value an even conjugate quantity B would have if measured on the projectile. "The interesting point," continued Ehrenfest, "is that the projectile, while flying around isolated on its own, must be able of satisfying totally different non-commutative predictions without knowing as yet which of these predictions will be made. . . ."

It would be intriguing if this is what Einstein was trying to get at in the first place.[166] On what page of Gilder's book are you referring to? I'd like to read it.

22-12-08 *Nonlocal Boxes and Ehrenfest, 2* (to N. D. Mermin)

I think I found the part in Gilder's book you were talking about. Pages 128–133. The history is going to be fun to read for me, but the commentary I can already feel is going to be quite painful. "They have no states of their own, and, as far as quantum theory is concerned, a measurement performed on one instantly affects its twin." More humbug!

22-12-08 *Reading* (to N. D. Mermin)

Now that I read two more paragraphs down, I see this pisses me off too:

> Bohr's books and papers—full of careful prohibitions about what cannot be contemplated . . . —have become holy writ . . . From the point of view of the history of entanglement, they are not worth one clear sentence from Einstein, Schrödinger, de Broglie, or John Bell, . . .

Harrumph!!

05-01-09 *After-afterthought* (to H. C. von Baeyer)

Applebyism 25: *Another example. You might write down a list of all the things you know about a person. It would never occur to you that the description was complete. Still less would you be inclined to identify the person with your description of them: to suppose that the person, like the description, actually consisted of a string of words. But the classical physicists were inclined to make an assumption rather similar to that.*

[166] [And apparently it was, as I later learned. See Don Howard's presentation on the subject at `http://www.nd.edu/~dhoward1/Early%20History%20of%20Entanglement/sld026.html`.]

With regard to Marcus's quote below and Hans's remark of liking it, surely in some other context I've sent you this Jamesian beauty before. But it is so apt here again, surely it is worthwhile sending to you again!

> Let me give the name of 'vicious abstractionism' to a way of using concepts which may be thus described: We conceive a concrete situation by singling out some salient or important feature in it, and classing it under that; then, instead of adding to its previous characters all the positive consequences which the new way of conceiving it may bring, we proceed to use our concept privatively; reducing the originally rich phenomenon to the naked suggestions of that name abstractly taken, treating it as a case of 'nothing but' that concept, and acting as if all the other characters from out of which the concept is abstracted were expunged. Abstraction, functioning in this way, becomes a means of arrest far more than a means of advance in thought. It mutilates things; it creates difficulties and finds impossibilities; and more than half the trouble that metaphysicians and logicians give themselves over the paradoxes and dialectic puzzles of the universe may, I am convinced, be traced to this relatively simple source. *The viciously privative employment of abstract characters and class names* is, I am persuaded, one of the great original sins of the rationalistic mind.

05-01-09 *What I Really Want Out of a Pauli/Fierz-Correspondence Study* (to H. C. von Baeyer & D. M. Appleby)

My two dear friends!

Of no one else but Schack do I feel such an intellectual kinship—you three are my triumvirate. Thank you both for the great New Year's reading gift of the last couple of days. It's funny how these things work out; it arrived at just the right time for me. For, I was able to put it in juxtaposition with my reading of the latest issue of *William James Studies*. The two together caused me to think very much about how I want to frame the coming year. And it struck me as worthwhile to try to record as clearly as I can at the moment what I am *personally* seeking to get out of a better knowledge of the byways explored by Pauli and Fierz (and potentially Jung). There is no implication in this that this is what you *should* be seeking as well—it is only an effort to tie the strands of my life together a little better.

Here is what it is really all about for me—it is the root of the root and the bud of the bud, as e e cummings put it. A good vehicle for setting up what I want to say is one of Hans's passages:

> Pauli summarily dismissed two extreme attitudes – total separation of science from religion, and complete surrender to mystical experience. The former approach was advocated in our times by the late paleontologist Stephen Jay Gould, who coined the phrase "nonoverlapping magisteria" for the respectful noninterference of the realms of nature and of morality, of what is and what should be. Discussing a book by a German physicist who also believed in the separateness of science and religion, Pauli had once written to Fierz that he was appalled, and continuing:
>
> > "[The book] is a reversion to the 19th century when religion and science lived in separate sections of the human soul – politely exchanging greetings at a distance, while continually reassuring each other that they had nothing to do with each other – and when the soul seemed to reside outside the boundaries of science."

> For Pauli it was obvious that science should be able to deal with the soul, and that the soul in turn can inform science.

in conjunction with one of Marcus's comments thereafter:

> It is certainly true that I attach more weight to the opus than I do to quantum mechanics. That is I am interested in quantum mechanics because I am interested in the opus, rather than the other way round. For Chris it is, perhaps, a little different. Though I don't think it is all that different, actually. It is true that Chris doesn't use the language of souls (at least I can't recall him doing so in my hearing). But he is deeply concerned with, for example, the question of human freedom. He will have to speak for himself, but I believe that for him too his interest in quantum mechanics is secondary to other considerations.

For it is true that I rarely speak of "souls," "religion," or "redemption." These terms are mostly dead terms for me—they don't stir my soul, so to speak—or maybe I simply don't understand them well enough yet to see their ultimate usefulness for what I *do* want to get at. (Much like I have never understood what the search for "elegance" can possibly mean when it comes to forming physical theories, say, as a criterion for string theory: it is a term that is dead to me.) It is maybe in this way, or more carefully, *in this detail*, that I part company from your tentative feelings on the opus.

Nonetheless, there is no doubt that I believe there is a place—a very important place—for humanistic concerns within physics proper. It seems to me it goes to the core of what quantum mechanics is trying to tell us. You'll find the point made over and over in my "Activating Observer" resource-material document, which both of you have versions of. But I thought the point was made very nicely in the *setting of pragmatism more generally* in this article that I was reading at the time your emails arrived—"'The Many and the One' and the Problem of Two Minds Perceiving the Same Thing" by Mark Moller in Vol. 3 of *William James Studies*, posted at `http://williamjamesstudies.org/3.1/moller.html`:

> Each of these claims about reality is crucial to James's attempt to offer an alternative to the metaphysical theories of the absolute idealists. The importance of the claim that reality is continuous and in flux is that it offers an alternative to the absolute idealists' view that the universe is "known by one [infinite] knower in one act, with every feature preserved, and every relation apprehended." This means that for the absolute idealists, the universe is forever fixed so as to make real change impossible. We make no difference in such a universe. We neither improve upon it through our efforts nor make it worse. James rejects this view completely. His aim is to argue for a conception of the universe that allows real change to occur in it and where our efforts have a role in causing it. He goes on in the passage from *The Many and the One* manuscripts quoted above to make his point:
>
>> This picture of the irremediably pluralistic evolution of things, achieving unity by experimental methods, and getting it in different shapes and degrees and in general only as a last result, is what has made me give to my volume the title of *The Many and the One*.
>
> According to James, we, as conscious agents in the universe, have an active role in introducing new content and unity into it. Such a view thus aligns his radical empiricism with his meliorism. In earlier essays, eventually published together as *The Will to Believe* (1897), and in lectures that he gave to teachers, eventually published as *Talks*

> *to Teachers on Psychology* (1899), James took the position against the absolute idealists that the ultimate fate of the universe has yet to be decided. He insisted that it is an open question as to whether evil will triumph over good or the other way around. This, in turn, led him to claim that our choices and actions do make a difference in the universe, and, in fact, a crucial one. They help to decide how "the everlasting battle of the powers of the light with those of darkness" will turn out. This melioristic attitude only makes sense if the universe is malleable to human action, and, thus, it is one of James's aims in developing his metaphysics to explain how this malleability is possible.

The very phrases "our choices and actions do make a difference in the universe" and "this melioristic attitude only makes sense if the universe is malleable to human action" mean outright that there must be room for a humanistic element of some sort within physics itself.

Here's the way I put it to Lucien and Joy Christian when I was in a poetic mood last year:

> My view of the universe is that it is many—that it ultimately cannot be unified, for it is alive and changing and creative in a very deep sense. Moreover, that reality is, to some not-yet well-understood extent, plastic: It can be molded by our actions. Thus, though humanity is quite well a Darwinian accident, now that it is here, it is a significant component of the universe that must be reckoned with.

Finally, it seems worthwhile for me to let James say it himself (from his article "Pragmatism and Humanism"). It captures with his beautiful sweep the romantic thought that really keeps me going from day to day:

> In many familiar objects every one will recognize the human element. We conceive a given reality in this way or in that, to suit our purpose, and the reality passively submits to the conception. You can take the number 27 as the cube of 3, or as the product of 3 and 9, or as 26 *plus* 1, or 100 *minus* 73, or in countless other ways, of which one will be just as true as another. You can take a chess-board as black squares on a white ground, or as white squares on a black ground, and neither conception is a false one.
>
> You can treat the adjoined figure as a star, as two big triangles crossing each other, as a hexagon with legs set up on its angles, as six equal triangles hanging together by their tips, etc. All these treatments are true treatments—the sensible *that* upon the paper resists no one of them. You can say of a line that it runs east, or you can say that it runs west, and the line *per se* accepts both descriptions without rebelling at the inconsistency.
>
> We carve out groups of stars in the heavens, and call them constellations, and the stars patiently suffer us to do so,—*though* if they knew what we were doing, some of them might feel much surprised at the partners we had given them. We name the same constellation diversely, as Charles's Wain, the Great Bear, or the Dipper. None of the names will be false, and one will be as true as another, for all are applicable.
>
> In all these cases we humanly make an *addition* to some sensible reality, and that reality tolerates the addition. All the additions 'agree' with the reality; they fit it, while they build it out. No one of them is false. Which may be treated as the *more* true, depends altogether on the human use of it. If the 27 is a number of dollars which I find in a drawer where I had left 28, it is 28 minus 1. If it is the number of inches in a board which I wish to insert as a shelf into a cupboard 26 inches wide, it is 26 plus 1. If I wish to ennoble the heavens by the constellations I see there, 'Charles's

Wain' would be more true than 'Dipper.' My friend Frederick Myers was humorously indignant that that prodigious star-group should remind us Americans of nothing but a culinary utensil.

What shall we call a *thing* anyhow? It seems quite arbitrary, for we carve out everything, just as we carve out constellations, to suit our human purposes. For me, this whole 'audience' is one thing, which grows now restless, now attentive. I have no use at present for its individual units, so I don't consider them. So of an 'army,' of a 'nation.' But in your own eyes, ladies and gentlemen, to call you 'audience' is an accidental way of taking you. The permanently real things for you are your individual persons. To an anatomist, again, those persons are but organisms, and the real things are the organs. Not the organs, so much as their constituent cells, say the histologists; not the cells, but their molecules, say in turn the chemists.

We break the flux of sensible reality into things, then, at our will. We create the subjects of our true as well as of our false propositions.

We create the predicates also. Many of the predicates of things express only the relations of the things to us and to our feelings. Such predicates of course are human additions. Caesar crossed the Rubicon, and was a menace to Rome's freedom. He is also an American schoolroom pest, made into one by the reaction of our schoolboys on his writings. The added predicate is as true of him as the earlier ones.

You see how naturally one comes to the humanistic principle: you can't weed out the human contribution. Our nouns and adjectives are all humanized heirlooms, and in the theories we build them into, the inner order and arrangement is wholly dictated by human considerations, intellectual consistency being one of them. Mathematics and logic themselves are fermenting with human rearrangements; physics, astronomy and biology follow massive cues of preference. We plunge forward into the field of fresh experience with the beliefs our ancestors and we have made already; these determine what we notice; what we notice determines what we do; what we do again determines what we experience; so from one thing to another, altho the stubborn fact remains that there is a sensible flux, what is *true of it* seems from first to last to be largely a matter of our own creation.

We build the flux out inevitably. The great question is: does it, with our additions, *rise or fall in value*? Are the additions *worthy* or *unworthy*? Suppose a universe composed of seven stars, and nothing else but three human witnesses and their critic. One witness names the stars 'Great Bear'; one calls them 'Charles's Wain'; one calls them the 'Dipper.' Which human addition has made the best universe of the given stellar material? If Frederick Myers were the critic, he would have no hesitation in 'turning down' the American witness.

Lotze has in several places made a deep suggestion. We naïvely assume, he says, a relation between reality and our minds which may be just the opposite of the true one. Reality, we naturally think, stands ready-made and complete, and our intellects supervene with the one simple duty of describing it as it is already. But may not our descriptions, Lotze asks, be themselves important additions to reality? And may not previous reality itself be there, far less for the purpose of reappearing unaltered in our knowledge, than for the very purpose of stimulating our minds to such additions as shall enhance the universe's total value. *'Die Erhöhung des vorgefundenen Daseins'* is a phrase used by Professor Eucken somewhere, which reminds one of this suggestion by the great Lotze.

It is identically our pragmatistic conception. In our cognitive as well as in our active

> life we are creative. We *add*, both to the subject and to the predicate part of reality. The world stands really malleable, waiting to receive its final touches at our hands. Like the kingdom of heaven, it suffers human violence willingly. Man *engenders* truths upon it.
>
> No one can deny that such a role would add both to our dignity and to our responsibility as thinkers. To some of us it proves a most inspiring notion. Signore Papini, the leader of Italian pragmatism, grows fairly dithyrambic over the view that it opens of man's divinely-creative functions.
>
> The import of the difference between pragmatism and rationalism is now in sight throughout its whole extent. The essential contrast is that *for rationalism reality is ready-made and complete from all eternity, while for pragmatism it is still in the making, and awaits part of its complexion from the future.* On the one side the universe is absolutely secure, on the other it is still pursuing its adventures.
>
> We have got into rather deep water with this humanistic view, and it is no wonder that misunderstanding gathers round it. It is accused of being a doctrine of caprice. Mr. Bradley, for example, says that a humanist, if he understood his own doctrine, would have to 'hold any end, however perverted, to be rational, if I insist on it personally, and any idea, however mad, to be the truth if only some one is resolved that he will have it so.' The humanist view of 'reality,' as something resisting, yet malleable, which controls our thinking as an energy that must be taken 'account' of incessantly (tho not necessarily merely *copied*) is evidently a difficult one to introduce to novices. The situation reminds me of one that I have personally gone through. I once wrote an essay on our right to believe, which I unluckily called the *Will* to Believe. All the critics, neglecting the essay, pounced upon the title. Psychologically it was impossible, morally it was iniquitous. The 'will to deceive,' the 'will to make-believe,' were wittily proposed as substitutes for it.
>
> *The alternative between pragmatism and rationalism, in the shape in which we now have it before us, is no longer a question in the theory of knowledge, it concerns the structure of the universe itself.*
>
> On the pragmatist side we have only one edition of the universe, unfinished, growing in all sorts of places, especially in the places where thinking beings are at work.
>
> On the rationalist side we have a universe in many editions, one real one, the infinite folio, or *édition de luxe*, eternally complete; and then the various finite editions, full of false readings, distorted and mutilated each in its own way.

My anti-Platonist tendencies certainly come out in my feeling of agreement with this quote. There is no sympathy in it to the idea that one might be *attuned* with a divine mind privy to "THE way" the universe is constructed—as Marcus, I think, rightly points out, seems to have been Einstein's modus operandi when theorizing. At most, one might be attuned to the content of this other quote of James:

> If you follow the pragmatic method, you cannot look on any [theory] as closing your quest. You must bring out of each [theory] its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

Physical theories, by this view, are conceptual means and tools for making change in the world.[167] Can one be attuned to such a thing in the Platonic sense that Marcus explores? Perhaps. But if so, it's not the medieval Platonism Marcus was talking about—it is instead being attuned to how best be an agent of change. (Perhaps the exemplar of this modified Platonism would be Barack Obama instead of Einstein ... so that a pragmatically modified Platonist would look on him and say, "Now, that man is someone who feels the Old One!")

In any case, what's important here is that I genuinely do see a humanistic role in the very maintenance of the universe. And all of this is potentially independent, and certainly broader, than considerations to do with quantum mechanics. The universe is partially powered by the inhuman, to be sure. But it is also partially powered by *belief*—that I do believe full well. And to the extent that religion or religious feeling are sources of belief, they do indeed play a role in the very construction of the universe. However, from this point of view religion is a special case of something much bigger—namely, belief generally.

I'll quote James one last time, so that you get a precise sense of what I mean here, but then I'll get back to the quantum and Pauli/Fierz and tell you way I'm saying all this. From "The Sentiment of Rationality":

> Now, I wish to show what to my knowledge has never been clearly pointed out, that belief (as measured by action) not only does and must continually outstrip scientific evidence, but that there is a certain class of truths of whose reality belief is a factor as well as a confessor; and that as regards this class of truths faith is not only licit and pertinent, but essential and indispensable. The truths cannot become true till our faith has made them so.
>
> Suppose, for example, that I am climbing in the Alps, and have had the ill-luck to work myself into a position from which the only escape is by a terrible leap. Being without similar experience, I have no evidence of my ability to perform it successfully; but hope and confidence in myself make me sure I shall not miss my aim, and nerve my feet to execute what without those subjective emotions would perhaps have been impossible. But suppose that, on the contrary, the emotions of fear and mistrust preponderate; or suppose that, having just read [W. K. Clifford's] *Ethics of Belief*, I feel it would be sinful to act upon an assumption unverified by previous experience—why, then I shall hesitate so long that at last, exhausted and trembling, and launching myself in a moment of despair, I miss my foothold and roll into the abyss. In this case (and it is one of an immense class) the part of wisdom clearly is to believe what one desires; for the belief is one of the indispensable preliminary conditions of the realization of its object. *There are then cases where faith creates its own verification.* Believe, and you shall be right, for you shall save yourself; doubt, and you shall again be right, for you shall perish. The only difference is that to believe is greatly to your advantage.
>
> The future movements of the stars or the facts of past history are determined now once for all, whether I like them or not. They are given irrespective of my wishes,

[167] This is not the dry instrumentalism one incessantly hears of in philosophy of science circles—that an instrumentalist is one who believes a scientific theory is *solely* an instrument for *prediction*. That is the term of insult they usually throw at Bohr, our quantum Bayesianism, and similar efforts: "it is merely instrumentalism," they say. (See Asher Peres's funny story at the beginning of `arXiv:quant-ph/0310010v1`.) But prediction only takes a secondary role here, at best. What's being talked about instead is a far richer concept than those guys—Jeff Bub, Harvey Brown, David Albert, and the like—are even aware of. For this kind of "instrumentalism" (if it should even be called that) the image to associate with a theory is not so much Babbage's analytical engine, but a hammer and a socket set. [[In a first draft of this footnote I had written "Babbage's prediction engine"; unfortunately that was not historically correct (as I just learned), and I couldn't bring myself to change the term purely for literary license.]]

> and in all that concerns truths like these subjective preference should have no part; it can only obscure the judgment. But in every fact into which there enters an element of personal contribution on my part, as soon as this personal contribution demands a certain degree of subjective energy which, in its turn, calls for a certain amount of faith in the result—so that, after all, the future fact is conditioned by my present faith in it—how trebly asinine would it be for me to deny myself the use of the subjective method, the method of belief based on desire!
>
> In every proposition whose bearing is universal (and such are all the propositions of philosophy), the acts of the subject and their consequences throughout eternity should be included in the formula. If M represent the entire world minus the reaction of the thinker upon it, and if $M + x$ represent the absolutely total matter of philosophic propositions (x standing for the thinker's reaction and its results)—what would be a universal truth if the term x were of one complexion, might become egregious error if x altered its character. Let it not be said that x is too infinitesimal a component to change the character of the immense whole in which it lies imbedded. Everything depends on the point of view of the philosophic proposition in question. If we have to define the universe from the point of view of sensibility, the critical material for our judgment lies in the animal kingdom, insignificant as that is, quantitatively considered. The moral definition of the world may depend on phenomena more restricted still in range. In short, many a long phrase may have its sense reversed by the addition of three letters, *n-o-t*; many a monstrous mass have its unstable equilibrium discharged one way or the other by a feather weight that falls.

These, however, are very big things and very big thoughts James is speaking of. And even though they stir my heart, they remain too vague to transform science as a whole (and indeed the world) in the way that I hope we'll one day transform it. I love the sound of what I'm hearing, but at the end of the day, I'm still not completely sure what I *am* hearing. How do I know I'm not simply fooling myself with pretty words? The main point I want to make at this juncture is that these ideas which drive me forward—these still-too-vague ideas—are *supra-* quantum mechanics. It is 19$^{\text{th}}$ century philosophy, not physics.

What now is the role of quantum mechanics within this system of thought I'm laying out? It is that it is a miniature version of these general points. BUT, though it is a miniature version, it is an extremely *precise* version! And that's its ace in the hole, as Hans said of Pauli's discussion with Huxley. Here's where I agree with Marcus,

> Quantum mechanics is important because it is as close to a refutation of the classical world picture as one could hope to get. It is very rare that something gets refuted in science as completely and finally as the classical hypothesis, of the world machine, has been refuted.

Thus, quantum mechanics is a precision laboratory for defining and testing these things *we think we see* on the horizon. When we've got it in quantum mechanics, we know we have it.

This finally is where I can express the value for me of learning more about the Pauli-Fierz and the Pauli-Jung correspondence. There is an amazing amount of development of the metaphysics of a malleable world in the writings of William James and F. C. S. Schiller[168].[169] And lo and

[168] F. C. S. should not be confused with the famous Schiller, namely Friedrich. F. C. S. stands for Ferdinand Canning Scott—a mostly forgotten pragmatist, who was one of the deepest of the lot if you ask me.

[169] And no disrespect intended, but Pauli and Fierz have got nothing on the sheer volume of even James's and Schiller's correspondence on these subjects, much less their published works. Pauli was an amateur in comparison ... as was necessarily the case, being a professional physicist most hours of the day.

behold, in which direction does that development lead? In significant parts at least, it leads in quite the same direction as Pauli's metaphysic. I don't know that I've ever emphasized this to you two. When James, for instance, asks the question, "What are the materials of the universe's composition?," the answer he tries to develop is that they are things *neutral* to the material/mental or physical/psychical distinction. In fact, he saw the material and mental as *complementary*, but exclusive, aspects of the basic stuff. So, you see the similarity.[170]

But James and Schiller knew no quantum mechanics of course. Particularly, they were not privy to this precision development laboratory for their metaphysic that Pauli and Fierz knew so well. Therefore, I need, I crave, to know more of the precise things Pauli and Fierz were talking about for just this reason. The suspicion is that it will inspire us and help us connect the dots between the quantum formalism and the bigger, much richer, and far-from-really-developed idea of a malleable/alchemical world.

Marcus says,

> Quantum mechanics, as it stands now, is little more than a set of calculational procedures. The calculations show that the vision of the world-machine is completely without empirical foundation. However, the mechanical vision of things has not been replaced with any other.

of which I have a minor qualm, but I think he is right in further saying, "The classical vision of things … was deeply wrong about the relation of mind to matter." For I think this neutral stuff that Pauli and James speak of—James called it "pure experience" but I am not completely happy with that term—will indeed be the "shocking" ingredient that will replace the mechanical vision of things. So, it is not that we have not started the process of replacement; it is only that it has been slow going.

Hans, I suspect, has wondered why I place so much emphasis on wanting to know the most, within all of Pauli's non-x-ing writings and correspondence, about the part to do with the idea of detached and nondetached observers (activating observers). It is because, as I see it, a thorough study of what it means for an observer or agent to be nondetached from the phenomena he helps bring about is the very starting point for a development of James and Pauli's neutral-stuff ontology. If we cannot get the activating-observer idea right and understand all its facets, then I feel we have no chance whatsoever of developing a full-blown "neutral monism"[171]. It is the very place to

[170] At this point, I'd definitely encourage you to read the whole of the Moller paper mentioned above. It gives quite a decent summary of the Jamesian metaphysic. Moreover, in it not only will you see a resemblance between James's ideas and Pauli's "neutral language" considerations, but also, in the objections to James's pluralism, you will find some difficulties that we quantum Bayesians (who take an "alchemical view" on quantum measurement) must eventually address.

[171] Neutral monism is what it is sometimes called in the literature, but a much better name for the idea in the Jamesian context would be "neutral pluralism." Until ten seconds after writing this, I thought I was the first person to make up this term. But apparently Ruth Anna Putnam beat me to it (damned the power of this internet; it is the second time it has foiled me today):

> [Putnam writes:] Another key element of James's radical empiricism is his rejection of mind/matter dualism as well as its reduction to either materialism or idealism. In its place, he offers—it is the title of one of his essays—a world of pure experience. In that world consciousness *as an entity* does not exist. But neither is consciousness a function of matter, for matter *as an entity* also does not exist. Ultimately there are only pure experiences (and, perhaps, experienceables—that is a difficult interpretative question), experiences which only in retrospect are *taken* either as part of a stream of thought or as physical objects. Although one is tempted to call this view a neutral monism, it is, in my opinion, more properly thought of as a neutral pluralism—neutral in not favoring either thought or matter, plural because "there is no *general* stuff of which experience at large is made. There are as many stuffs as there are 'natures' in the things experienced … and save for time and space (and, if you

start, for it forces the issue of this new ontology in its very statement. What is an observer if not psychical? What is an activator if not physical? How can we really combine these two aspects of the phenomenon via the help of a Paulian neutral language?

Unfortunately in the technical development of this first step, Marcus and I have hit a great snag by not yet understanding the structure (a.k.a. knowing how to prove the existence of) these damned SICs. The reason I say this is because I see no way forward to a more precise definition of the activating-observer concept than by a kind of "generalized Newton's third law" of the style I wrote you about last month. And I see no way to make progress in that intermediate program without the ability to completely rewrite quantum mechanics (and particularly the Born rule) in terms of SICs. For completeness, let me reproduce that little essay:

> Everybody has their favorite speculation about what powers quantum information and computing. Some say it is the superposition principle, some say it is the parallel computation of many worlds, some say it is the mysteries of quantum entanglement, some say it is the exponential growth of computational space due to the tensor product. For my own part though, my favorite speculation is that it is Newton's Third Law: For every action, there is an equal and opposite reaction. Indeed I sometimes wonder if the very essence of quantum mechanics isn't just this principle, only carried through far more consistently than Newton could have envisioned. That is to say, absolutely *nothing* is exempt from it.
>
> What do I mean by this? What might have been exempt from the principle in the first place? To give an answer, let me note an equivalent formulation of old Newton. For every *reaction*, there is an equal and opposite *action*. Strange sounding, but there's nothing wrong with it, and more importantly, this formulation allows for the possibility of an immediate connection to information theory. In particular, we should not forget how information gathering is represented in the Shannon theory. An agent has gathered information—by the very definition of the process—when something in his environment has caused him to *react* by way of revising a prior expectation $p(h)$ (for some phenomenon) to a posterior expectation $p(h|d)$ (for the same phenomenon).
>
> When information is gathered, it is because we are reacting to the stimulation of something external to us. The great lesson of quantum mechanics may just be that information gathering is physical. Even something so seemingly unimportant to the rest of the universe as the reactions that cause the revisions of our expectations are not exempt from Newton's Third Law. When we react to the world's stimulations upon us, it too must react to our stimulations upon it.
>
> The question is, how might we envision a world with this property—i.e., with such a serious accounting of Newton's law—but in a way that does not make a priori use of the information gathering agent himself? If the question can be answered at all, the task of finding an answer will be some tall order. For never before in science have we encountered a situation where the theorizing scientist is so inextricably bound up with what he is trying to theorize about in the first place.
>
> It's almost a paradoxical situation. On the one hand we'd like to step outside the world and get a clear view of what it looks like without the scientist necessarily in the picture. But on the other hand, to even pose the question we have to imagine an information gathering agent set in the middle of it all. You see, neither Shannon nor any of modern information theory has given us a way to talk about the concept of

like, for 'being') there appears no universal element of which all things are made."

> information gain without first introducing the agent-centered concept of an expectation $p(h)$.
>
> So, how to make progress? What we do know is that we actually are in the middle of the world thinking about it. Maybe our strategy ought to be to use that very vantage point to get as close as we can to the goal. That is, though we may not know what the world looks like without the information gathering agent in it, we certainly do know something about what it looks like with him in: We know, for instance, that he ought to use the formal structure of quantum mechanics when thinking about physical systems. Beyond that, we know of an imaginary world where Newton's Third Law was never taken so seriously: It is the standard world of classical physics and Bayesian probability.
>
> Thus, maybe the thing to do first is to look inward, before looking outward. About ourselves, at the very least, we can ask how has the formal structure of our *behavior* changed since moving from what we thought to be a classical Bayesian world to what we now believe to be a quantum world? In that *differential*—the speculation is—we may just find the cleanest statement yet of what the quantum world is all about. For it is in that differential, that the world without us surely rears its head.
>
> To do this, we must first express quantum mechanics in a way that it can be directly compared to classical Bayesian theory, where the information-gathering agent was detached from the world.

So, you see, there's work to be done all the way down to the SICs, and all the way up to a full-blown ontology of neutral pluralism and a malleable/alchemical reality. In the case of the SICs, I'm actually more confident: That problem is going to be solved if I have to recruit all of Hannibal's army. But in the case of the ontology it's just verbal hints we have presently—there was only so much James, Schiller, Dewey, and others could discern from general premises, without detailed empirical input.[172] Clearly we will have to see our way through in that big task. But Pauli and Fierz are the only two quantum physicists I know of who have already waded these waters to any depth. If I could get hold of what little they may have already strained from the rocks below, it would be invaluable for this effort. And that is what the title of this New Year's note is all about: What I Really Want Out of a Pauli/Fierz-Correspondence Study.

Thank you both for not losing faith that there is something really important here.

Happy New Year!

05-01-09 *Quick Comment* (to D. M. Appleby)

Applebyism 26 :

> [Chris said:] An agent has gathered information—by the very definition of the process—when something in his environment has caused him to REACT by way of revising a prior expectation $p(h)$ (for some phenomenon) to a posterior expectation $p(h|d)$ (for the same phenomenon).

Which is true. But it needs also to be stressed that what the agent acquires is, not something like this "je ejszwjm r cly gwjn c yn vqrg eugl srb xqmdbs" but something like this "a meaningful statement the agent understands".

[172] And only so much patience I have for straining through a set of thoughts framed so far away from direct quantum mechanical considerations.

The Shannon theory completely disregards meanings. This is reasonable enough if you look at it from the narrow perspective of a communications engineer (it is not the engineer's business to read the messages he transmits). But I don't think it is reasonable at all if you look at it from our broader perspective.

That is true. However, the long tail of meaning (like the long tail of a rat trying to hide under the cupboard) is not lost altogether. It is in the setting of the sample space upon which the probabilities are defined. The sample space is not given by nature alone. It is the agent himself who sets it. And that setting reflects the meaning he draws from the various outcomes, and the level of graining with which he decides to take note of nature.

Nonetheless your point on meaning back-reaction is well taken. If I can dig it up, I have a faint memory of Rorty commenting on Derrida that, for Derrida it is almost that what something is named actually modifies what it is.

Just read your note titled "Synchronicity???". Surely there is no need to rush a reply back to me, or even read what I've sent. All shakes out in due time!

05-01-09 *F-Theory, 2* (to R. W. Spekkens)

I sent the attached essay off to Marcus Appleby and Hans Christian a little while ago, and thought you might enjoy it too. Especially as the last two pages of it refer to something you once told me you liked in my melange of ideas. (The rest of it you probably won't like!) Still, it gives a few more hints of the weird ontology that swirls in the back of my head trying to find substance. [See 05-01-09 note titled "What I Really Want Out of a Pauli/Fierz-Correspondence Study."]

Rob's Reply

> You're right – the last two pages of that note did resonate with me. In particular, I like this comment:
>
>> That is, though we may not know what the world looks like without the information gathering agent in it, we certainly do know something about what it looks like with him in: We know, for instance, that he ought to use the formal structure of quantum mechanics when thinking about physical systems. Beyond that, we know of an imaginary world where Newton's Third Law was never taken so seriously: It is the standard world of classical physics and Bayesian probability.
>
> One of the things that I've been exploring in my toy theories is: what happens if we do take Newton's Third law seriously in the imaginary world of classical physics and we model observers as classical systems that are subject to the law? The answer is that, assuming no reliable supply of systems in dispersion-free ensembles (to use von Neumann's terminology), observers necessarily cause a disturbance to one variable whenever they gain information about a complementary variable.
>
> Please see the discussion beginning at time index 40:53 and ending at around 45:00 in my talk at `http://pirsa.org/08020051/`.
>
> Incidentally, there was a piece on religion in today's Guardian which discussed James' views at length. Given the content of the first half of your note, you may want to have a look: `http://www.guardian.co.uk/commentisfree/andrewbrown/2008/nov/07/religion-psychology-william-james`.

06-01-09 *De Raedt about Bell's Theorem* (to D. Kobak)

Kobakism 1: *I've recently discovered the papers of Hans De Raedt from Netherlands (there are plenty of them on* `arXiv`*), whose views on QM seem to be very peculiar. In a series of papers he and his coauthors claim to have constructed local realist "down-to-earth" models, mimicking quantum behaviour in different cases. They call it "event-based computer simulation models" – of more or less everything from double-slit single-photon interference to EPR correlations. As far as I understand these models are not supposed to be hypotheses about the real world, but more something like counterexamples to the common misconceptions.*

This is certainly most interesting by itself, but what I find completely puzzling is that I failed to find any reply to those papers from other people in quantum foundations community, or any discussions of these ideas whatsoever. Either I'm not looking well enough, or these works are constantly left completely unnoticed. This is maybe my first question: have you heard about these papers, and if yes – do you have any opinion about that?

I remember you well from the summer school, and I am flattered that you would think me worthy to try to lead you through these conundrums. I wish we could have kept you in quantum foundations and that you might have come here to study at PI. However with your question, I have had enough experience to know that these things are always the same—they are analogous to the experience a patent clerk feels when he is presented with yet another proposed perpetuum mobile. The poor clerk has the unedifying task of finding where the mistake is hidden.

So, in advance, I'll say I am sorry. I do not have the stomach for this kind of work. However, I will do the dirty trick of cc'ing this note to two friends of mine who have found some pleasure in finding the flaws in these kinds of arguments before—Profs. Larsson and Gill. For instance, they both have published experience tackling the Hess-Philipp objections to Bell and also Accardi's very similar sounding claim to De Raedt's of being able to "simulate" Bell violations. Thus maybe they know this work of De Raedt's already and can give you a quick answer. And if they do not have a quick answer, perhaps they may find it nonetheless a worthy enough windmill to slay.

Good luck to you all and may God save your souls!

06-01-09 *Pauli for the Library?* (to P. Goyal)

Are you still the foundations representative for the library committee?

My colleague Hans Christian von Baeyer is considering coming from time to time as he is working on a new book on the Pauli-Fierz correspondence on quantum foundations. Crucial to that program, however, is that he have access to all 9 volumes of the Pauli correspondence (there are 8 volumes presently in existence, and a final one to be published this year). He has all the volumes at his home department at William & Mary, of course, but for visits to PI he would have to pack them up each and every time. And they're not small books.

So the question is, could we purchase them for the library? They'd certainly be a long-term resource for the institute, as they include Pauli's correspondence with Einstein, Bohr, Schrödinger, Heisenberg, Born, David Bohm, and many, many others.

I'll keep my fingers crossed …

06-01-09 *Pauli for the Library?, 2* (to P. Goyal)

You might enjoy Hans Christian's book, *Information: The New Language of Science*. I thought it was a great (popular, but serious) account of what people are thinking. He was a professor of physics (just retired) at William & Mary, but has also been a writer for many years and has several books (the most recent being on Parisian parks as seen through the eyes of a practicing physicist). At the moment, aside from working on this new book, he's the book review editor at *American Journal of Physics*.

He's perfect for the job of this Pauli-Fierz (and Pauli-Jung) thing: a) he's fluent in English and German, b) he's a seasoned writer, c) he's a serious scholar beside, d) he's had a long interest in Jung's thoughts, e) his father was actually Fierz's best friend in college and a life-long friend thereafter, f) Fierz's twin brother, the psychologist, was Hans Christian's godfather, and g) Hans Christian's own brother is a psychologist. Perfect confluence of events!

Let me attach a little thing for both you and Luca that I wrote yesterday. [See 05-01-09 note titled "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer and D. M. Appleby.] It's a letter to Hans Christian and Marcus Appleby and lays out my metaphysical side fairly decently (with respect to Hans Christian's project). You might enjoy it for some light early-year reading.

06-01-09 *Pauli Volumes* (to H. C. von Baeyer)

I looked up links between Jung and James yesterday, and was interested to learn that they had actually met in 1909. Jung was much impressed with James. They mostly had conversation on parapsychology, and learned that Jung had apparently read *Varieties of Religious Experience* and the book *Pragmatism* (along with *Principles of Psychology* of course). James in his correspondence with Flournoy only acknowledged meeting "Yung" with "made a very pleasant impression" and didn't say anything further. He did however have something snide to say of his meeting with Freud (Freud and Jung were together for the visit); he said he seemed to be a man "obsessed by fixed ideas".

06-01-09 *One German Sentence* (to H. C. von Baeyer)

By the way, could I ask you to translate the thing Prof. Eucken said in the longer James quote I sent in yesterday's note?

Hans's Reply

> The correct (and this is not negotiable) capitalization is: "Die Erhöhung des vorgefundenen Daseins."
>
> A quick translation is "the enhancement of what is found to exist." I have no idea what it means, beyond what precedes it in the passage.
>
> Erhöhung is a word one might use to describe the enhancement of a sensation by means of a chemical stimulant. It also means intensification. Vorgefunden means "as found" or "pre-existing". Dasein is a typically German fuzzy word meaning "being" or "existence". (It can also mean daily life, but not here.)

06-01-09 *One German Sentence, 2* (to H. C. von Baeyer)

Well, if he could misspell Jung as Yung, I have no doubt he could get German capitalization wrong as well!

Thanks for the translation!

06-01-09 *Causaloid, Meet Psychoid* (to L. Hardy)

Yesterday, I put together a little note to try to spur on Hans Christian von Baeyer in the lonely task of distilling what Pauli and Fierz were on about in their correspondence. (The note is in the attached file, which was actually to both H. C. and Appleby.) [See 05-01-09 note titled "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer and D. M. Appleby.] Hans has already put together an introductory article on that side of Pauli, and I've been encouraging him to tackle a full-blown book project.

Anyway, in my research for the note, I came across this word, which I had never seen before. So: Causaloid meet psychoid; psychoid, this is causaloid.

> **psychoid** adj. In analytical psychology, soul-like, a term that Carl Gustav Jung (1875–1961) applied to the collective unconscious, which 'cannot be directly perceived or "represented", in contrast to the perceptible psychic phenomena, and on account of its "unrepresentable" nature I have called it "psychoid"' (Collected Works, 8, paragraph 840). [From Greek psyche the soul + -oid indicating likeness or resemblance, from eidos shape or form]

In any case, I thought you might enjoy the note for some light reading in these lazy days of the New Year.

06-01-09 *The Metaphysics of SICs* (to K. Martin)

BTW, for your amusement, attached is a letter I sent to Marcus Appleby and Hans Christian von Baeyer yesterday. [See 05-01-09 note titled "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer and D. M. Appleby.] Hans Christian has just written an article on the more spiritual side of Wolfgang Pauli, and I've been encouraging him to turn his efforts toward writing a full-blown book. (No ulterior motives here!) This letter is one of the many mortars I've sent to try to soften him up on the idea; and it reveals a good bit on the weird ontology that I think will ultimately be extracted from quantum mechanics. By the last three pages, the letter ties SICs back into the picture. It should be fun reading for you (even if it convinces you I'm insane). You see, we are working on noble things in this project!

06-01-09 *Remember to get Article on Lotze* (to me)

- Otto F. Kraushaar, "Lotze's Influence on the Pragmatism and Practical Philosophy of William James," J. Hist. Ideas **1**, 439–458 (1940).

07-01-09 *De Raedt about Bell's Theorem, 2* (to D. Kobak)

Kobakism 2: *De Raedt's works can well be completely false, but I wouldn't say that they are just crackpot (regarding what Chris said in the beginning about "perpetuum mobile" proposals). The reason I say so is how I learned about those papers: I was told to read them by a prominent solid state physicist, who is interested in quantum foundations as well. He is definitely a very good and very well known theoretical physicist and he was estimating De Raedt very high, as they had chance to work together (on some decoherence issues, not these "quantum simulators" of De Raedt).*

That's why I became interested in those works, and that's why I was surprised to find almost complete lack of responses from the QM foundations community. I have of course seen the comment *by Michael Seevinck and Jan-Åke Larsson on* arXiv*, but then there's a reply to that comment from De Raedt, and a very detailed one, I would say (*http://arxiv.org/abs/0706.2957*). As a result, there's an impression that those papers are just left unanswered, and since they don't seem to be "evidently crackpot", this situation is somewhat puzzling. I'm honestly sorry if all those topics are completely clear and non-interesting for you, but I can't help wishing that there were a detailed and bottom-line answer to De Raedt's writings, if they are really so false.*

What the lack of response reveals is this: That almost all the people who run across a paper like this have the same feeling I did when I saw it—i.e., the analogue of, "yet another perpetuum mobile." Then, they don't respond publicly, or even take the time to work through the paper privately, because they don't see that it is worth their while to do so, or that they do not have a lot of understanding to gain from the exercise.

The reason is new "refutations" of Bell (and all the other related inequalities and no-go theorems, CHSH, GHZ, Hardy's, etc., etc.) crop up almost literally *every* year. And this runs for many years back. De Raedt is by far not an isolated incident. In the mean time, many very good people (Gill, Larsson, Appleby, Mermin, Shimony, Hardy, van Enk, Kent, Seevinck, more) do in fact, from time to time, make the effort to find the flaws in the new argumentation ... AND they *always* find the flaws. I used to be among that industry when I was younger (20 years ago), though I never published. I suspect many other people are like me.

So, the issue is mostly one of age and previous experience. You are still young enough that you have not seen these things come and go for 20 years. Therefore the attitude you exhibit below is completely healthy and to be commended. It is not at all that the refutation of each new argument is obvious (at least not for me). On the contrary, it is often very difficult to ferret out the flaws, and it may take a lot of hard work. But that doesn't make the exercise less like a proposed perpetuum mobile analysis, it makes it very much of the same flavor.

An education in quantum foundations should consist in exactly what you are doing: Questioning well received opinion. But I feel very confident *in this particular case* that if you

a) study very carefully the logic in Bell's argument from some good references (I will list some of my personal favorites below), and

b) very carefully work out for yourself where the De Raedt proposal goes astray (perhaps initially being guided by the remarks Larsson and Gill have already made),

you will come out strengthened from the process, and see more clearly that Bell's reasoning was not arbitrary, but very beautiful instead.

References below.

1) N. D. Mermin, "Hidden Variables and the Two Theorems of John Bell" http://lin25.thphys.uni-heidelberg.de/~wetzel/qm2005/protected/hidden_var_mermin.pdf

2) N. D. Mermin, *Boojums All the Way Through* (Cambridge U. Press, 1990) http://books.google.com/books?hl=en&lr=&id=qT7J3Zl_OesC&oi=fnd&pg=PR11&dq=Mermin+Boojums&ots=R6bDgt_MOy&sig=CS5E947mMQr7yOPt4IydEElus7c

3) S. L. Braunstein and C. M. Caves, "Wringing Out Better Bell Inequalities" http://www-users.cs.york.ac.uk/schmuel/papers/90/bcB90.pdf

07-01-09 *Some History of the Block Universe* (to D. M. Appleby & H. C. von Baeyer)

I am sorry, this is one paragraph I had not responded to:

Applebyism 27: *I was thinking particularly of what in the notes I call the "classical world-machine". I didn't use the phrase "block-universe" because I had in mind the whole development of classical physics. I don't believe that Newton was thinking in terms of the block-universe: that particular version of the nightmare only comes with relativity. I guess you could say that the block-universe represents the final stage in the evolution of the world-machine. The senescence of the world-machine? The corpse of the world-machine?*

I have no problem with your new term; I quite like it. But I just wanted to point out that historically the "block universe" phrase/imagery did in fact precede relativity by quite a substantial time. I believe the first public appearance of the concept was in William James's 1884 lecture "The Dilemma of Determinism," where he writes:

> What does determinism profess? ... It professes that those parts of the universe already laid down absolutely appoint and decree what the other parts shall be. The future has no ambiguous possibilities hidden in its womb; the part we call the present is compatible with only one totality. Any other future complement than the one fixed from eternity is impossible. The whole is in each and every part, and welds it with the rest into an absolute unity, an iron block, in which there can be no equivocation or shadow of turning.

Later in the essay, he writes:

> The more one thinks of the matter, the more one wonders that so empty and gratuitous a hubbub as this outcry against chance should have found so great an echo in the hearts of men. It is a word which tells us absolutely nothing about what chances, or about the modus operandi of the chancing; and the use of it as a war-cry shows only a temper of intellectual absolutism, a demand that the world shall be a solid block, subject to one control, which temper, which demand, the world may not be bound to gratify at all.

And finally again:

> A mind to whom all time is simultaneously present must see all things under the form of actuality, or under some form to us unknown. If he thinks certain moments as ambiguous in their content while future, he must simultaneously know how the ambiguity will have been decided when they are past. So that none of his mental judgments can possibly be called hypothetical, and his world is one from which chance is excluded. Is not, however, the timeless mind rather a gratuitous fiction? And is not the notion of eternity being given at a stroke to omniscience only just another way of whacking upon us the block-universe, and of denying that possibilities exist? — just the point to be proved. To say that time is an illusory appearance is only a roundabout manner of saying there is no real plurality, and that the frame of things is an absolute unit. Admit plurality, and time may be its form.

In my further records in my computer, I also have this use of the term from his posthumous book *Some Problems of Philosophy*. Here, he asks rhetorically, "If the time-content of the world be not one monistic block of being, ...” Now, the manuscript for that book was certainly written after the discovery of special relativity (though certainly before 1910 when James died), but I am pretty sure he had no knowledge of relativity. Let me quote the passage in detail a) because I can (it’s already in my computer), and b) because it makes some connection to our discussion of religion, and that may be useful in the future. What I found in re-reading this passage is that though talk of the soul doesn’t seem to stir my soul (as I wrote to Hans and you in the previous note), apparently I am to some extent in tune with a moralistic take on things. [Good God, my American upbringing!]

> But pluralism, accepting a universe unfinished, with doors and windows open to possibilities uncontrollable in advance, gives us less religious certainty than monism, with its absolutely closed-in world. It is true that monism’s religious certainty is not rationally based, but is only a faith that ‘sees the All-Good in the All-Real.’ In point of fact, however, monism is usually willing to exert this optimistic faith: its world is certain to be saved, yes, is saved already, unconditionally and from eternity, in spite of all the phenomenal appearances of risk.
>
> A world working out an uncertain destiny, as the phenomenal world appears to be doing, is an intolerable idea to the rationalistic mind.
>
> Pluralism, on the other hand, is neither optimistic nor pessimistic, but melioristic, rather. The world, it thinks, may be saved, on condition that its parts shall do their best. But shipwreck in detail, or even on the whole, is among the open possibilities.
>
> There is thus a practical lack of balance about pluralism, which contrasts with monism’s peace of mind. The one is a more moral, the other a more religious view; and different men usually let this sort of consideration determine their belief.
>
> So far I have sought only to show the respective implications of the rival doctrines without dogmatically deciding which is the more true. It is obvious that pluralism has three great advantages:—
>
> 1. It is more ‘scientific,’ in that it insists that when oneness is predicated, it shall mean definitely ascertainable conjunctive forms. With these the disjunctions ascertainable among things are exactly on a par. The two are co-ordinate aspects of reality. To make the conjunctions more vital and primordial than the separations, monism has to abandon verifiable experience and proclaim a unity that is indescribable.
>
> 2. It agrees more with the moral and dramatic expressiveness of life.
>
> 3. It is not obliged to stand for any particular amount of plurality, for it triumphs over monism if the smallest morsel of disconnectedness is once found undeniably to exist. ‘Ever not quite’ is all it says to monism; while monism is obliged to prove that what pluralism asserts can in no amount whatever possibly be true—an infinitely harder task.
>
> The advantages of monism, in turn, are its natural affinity with a certain kind of religious faith, and the peculiar emotional value of the conception that the world is a unitary fact.
>
> So far has our use of the pragmatic rule brought us towards understanding this dilemma. The reader will by this time feel for himself the essential practical difference which it involves. The word ‘absence’ seems to indicate it. The monistic principle implies that nothing that is can in any way whatever be absent from anything else that is. The pluralistic principle, on the other hand, is quite compatible with some things being absent from operations in which other things find themselves singly or collectively engaged. Which things are absent from which other things, and when,—these of course

> are questions which a pluralistic philosophy can settle only by an exact study of details. The past, the present, and the future in perception, for example, are absent from one another, while in imagination they are present or absent as the case may be. If the time-content of the world be not one monistic block of being, if some part, at least, of the future, is added to the past without being virtually one therewith, or implicitly contained therein, then it is absent really as well as phenomenally and may be called an absolute novelty in the world's history in so far forth.
>
> Towards this issue, of the reality or unreality of the novelty that appears, the pragmatic difference between monism and pluralism seems to converge. That we ourselves may be authors of genuine novelty is the thesis of the doctrine of free-will. That genuine novelties can occur means that from the point of view of what is already given, what comes may have to be treated as a matter of *chance*. We are led thus to ask the question: In what manner does new being come? Is it through and through the consequence of older being or is it matter of chance so far as older being goes?—which is the same thing as asking: Is it original, in the strict sense of the word?

BUT WAIT!!! BULLETIN!!! I love the power of the internet! I just discovered that James used the block universe idea as early as 1882. This is a lower bound that I had not known of before! Moreover this passage is funny to boot; it comes from an article titled, "On Some Hegelisms." Here it is:

> Why may not the world be a sort of republican banquet of this sort, where all the qualities of being respect one another's personal sacredness, yet sit at the common table of space and time?
>
> To me this view seems deeply probable. Things cohere, but the act of cohesion itself implies but few conditions, and leaves the rest of their qualifications indeterminate. . . .
>
> [I]f we stipulate only a partial community of partially independent powers, we see perfectly why no one part controls the whole view, but each detail must come and be actually given, before, in any special sense, it can be said to be determined at all. This is the moral view, the view that gives to other powers the same freedom it would have itself,—not the ridiculous 'freedom to do right,' which in my mouth can only mean the freedom to do as *I* think right, but the freedom to do as *they* think right, or wrong either. After all, what accounts do the nethermost bounds of the universe owe to me? By what insatiate conceit and lust of intellectual despotism do I arrogate the right to know their secrets, and from my philosophic throne to play the only airs they shall march to, as if I were the Lord's anointed? Is not my knowing them at all a gift and not a right? And shall it be given before they are given? *Data! gifts!* something to be thankful for! It is a gift that we can approach things at all, and, by means of the time and space of which our minds and they partake, alter our actions so as to meet them.
>
> There are 'bounds of ord'nance' set for all things, where they must pause or rue it. 'Facts' are the bounds of human knowledge, set for it, not by it.
>
> Now, to a mind like Hegel's such pusillanimous twaddle sounds simply loathsome. Bounds that we can't overpass! Data! facts that say, "Hands off, till we are given"! possibilities we can't control! a banquet of which we merely share! Heavens, this is intolerable; such a world is no world for a philosopher to have to do with. He must have all or nothing. If the world cannot be rational in my sense, in the sense of unconditional surrender, I refuse to grant that it is rational at all. It is pure incoherence, a chaos, a nulliverse, to whose haphazard sway I will not truckle. But, no! this is not the world.

The world is philosophy's own,—a single block, of which, if she once get her teeth on any part, the whole shall inevitably become her prey and feed her all-devouring theoretic maw. Naught shall be but the necessities she creates and impossibilities; freedom shall mean freedom to obey her will; ideal and actual shall be one: she, and I as her champion, will be satisfied on no lower terms.

Near the end of the article, there's still one more use of the term:

> In the universe of Hegel the absolute block whose parts have no loose play, the pure plethora of necessary being with the oxygen of possibility all suffocated out of its lungs there can be neither good nor bad, but one dead level of mere fate.

Now I've got the fire in me to try to really figure out the first time the phrase was used either in correspondence or print. I never imagined I would actually use any of the early volumes of *The Correspondence of William James*, so I only accumulated Vols. 7–12 for my library. But that only delves back to 1890. It's time to go book shopping again!

08-01-09 *Fuchs-Graaf Inequality* (to A. Uhlmann)

Uhlmannism 1: *Mike Hellmund and I just sent a short paper to the archive, No.* `08120906`*, extending an inequality of you and Graaf to arbitrary dimensions. You have used the inequality for an entanglement of assistance estimate. But this we could not do in higher dimensions yet.*

It is so good to hear from you, though I must apologize for taking so long to respond. The holidays were unusually extensive this year.

That is a nice result of yours. I will try to tuck it away in the bookshelf of my mind. Particularly lately I have been wondering about the "significance" of low order trace powers. For instance, suppose A is a Hermitian matrix. Then one can characterize when A is a rank-1 projection in the following way. A is a rank-1 projection if and only if $\mathrm{tr}A^2 = \mathrm{tr}A^3 = 1$. One need not consider higher trace powers (and consequently higher order elementary symmetric functions). The statement comes about trivially mathematically, but I keep wondering about whether there is any deep information theoretic significance to it (for the purpose of quantum foundations, that is).

Here is a more technical question on my mind. I wonder if you have any insight. Let us have a d-dimensional Hilbert space, and let us *suppose* a SIC exists. That is, suppose there is a set of d^2 rank-1 projections P_i such that $\mathrm{tr}(P_iP_j) = 1/(d+1)$. If you are not familiar with the concept, see the attached paper. Now here's the question:

> Let A be a Hermitian matrix such that
>
> 1) $\mathrm{tr}A = 1$,
> 2) $\mathrm{tr}A^2 = 1$, and
> 3) $0 \le \mathrm{tr}(AP_i) \le 1$, for all $i = 1, ..., d^2$.
>
> Is there an interesting characterization of the further conditions required to ensure that $\mathrm{tr}A^3 = 1$ (and hence we have a rank-1 projection)?

I hope you are doing well. How is your health? What is your age now? It has been many years since we have seen each other.

08-01-09 *My Book Selections* (to Cambridge University Press)

Simon Capelin instructed me to select a few "Cambridge books to the value of about US$200" and contact you in remuneration for a report I wrote for him. See note below.

After a few hours at your website (it was a lot of fun!), I came to the following four selections:

- *William James and the Metaphysics of Experience*
  David C. Lamberth
  (Hardback) ISBN: 052158163X
  99.00 USD

- *The Divided Self of William James*
  Richard M. Gale
  (Paperback) ISBN: 0521037786
  39.99 USD

- *Wittgenstein and William James*
  Russell B. Goodman
  (Paperback) ISBN: 0521038871
  37.99 USD

- *The Dappled World*
  Nancy Cartwright
  (Paperback) ISBN: 0521644119
  34.99 USD

The total for these four comes to 211.97 USD. I hope that this is an appropriate amount.

09-01-09 *That Pragmatism/Science Conference* (to C. Smeenk)

Thanks for bringing that to my attention. I just wrote the note below to one of the organizers. I don't know about the organization behind it (the "Center for Inquiry Transnational"), but their "VP of Research" is John Shook. He has like a gazillion volumes on classical pragmatism that he's edited (great collections of old papers pro and con). I have literally 20 volumes on my bookshelf, and he has at least one proper book *Dewey's Empirical Theory of Knowledge and Reality*. (I know that because I have it on my shelf as well; apparently I picked it up in brand new condition for $6.98, while the list price is $49.95.)

Is there any chance that you and/or Wayne might go to the conference?

09-01-09 *Wild Chance – James Correspondence* (to C. Misak)

I'm wracking my brain over who I know that might be remotely interested in obtaining a good price on some volumes of *The Correspondence of William James*. I think the chances are slim that you'll be interested, but I'll ask anyway.

My dilemma is this: I presently own volumes 7–12, but have been thinking I really should back-fill volumes 1–6 (particularly as I've realized I'm missing some crucial years when James was starting to rebel from the block universe idea; I've lower bounded the phrase "block universe" to 1880–1882, but want to know if it goes further back). So, I want to get vols 1–6. But these volumes generally cost in the \$70–90 US range per volume + shipping, etc. On the other hand, Amazon.com has a special package for all 12 volumes that works out to \$33US/vol.

On wild chance would you have any interest in buying the top half of the volumes while I buy the bottom half?

Just an idea! But if you have no interest, do have an idea of anyone else who might? Or any other ideas in general?

09-01-09 *Wild Chance – James Correspondence, 2* (to C. Smeenk, W. C. Myrvold, D. Fraser, and S. Dea)

I just wrote the letter below to Cheryl Misak, but as I figured she wasn't a taker (already has them all nearby). So I'm widening the net now. Please see note below. \$33/volume new really is a fantastic price (except for volumes 1 and 12, for some reason, which crop up here and there for lower); see `http://www.abebooks.com/servlet/SearchResults?an=Skrupskelis&sts=t&tn=Correspondence+of+William+James&x=86&y=13`.

Is there any chance any of you would be interested in the top half of James's correspondence, i.e., 1890–1910? There's a significant number of letters to and from C. S. Peirce in there. Supposing you're not interested, do you know anyone who might be?

09-01-09 *Pragmatism Library* (to S. Dea)

By the way, if you ever need anything for short loan, I'm building up quite a pragmatism library at home. Just let me know. Attached is my present book list.

To that, I've also got a few things on the way. CUP owed me for some reviewing I did for them, so I've just gotten them to send:

- *William James and the Metaphysics of Experience*
  David C. Lamberth
  (Hardback) ISBN: 052158163X; 99.00 USD

- *The Divided Self of William James*
  Richard M. Gale
  (Paperback) ISBN: 0521037786; 39.99 USD

- *Wittgenstein and William James*
  Russell B. Goodman
  (Paperback) ISBN: 0521038871; 37.99 USD

- *The Dappled World*
  Nancy Cartwright
  (Paperback) ISBN: 0521644119; 34.99 USD

And then I went on a purchasing binge with Atticus and have these on the way:

- Herman J. Saatkamp, Jr. ed.
  Rorty & Pragmatism: The Philosopher Responds to His Critics
  Norman, Oklahoma, U.S.A.: Vanderbilt Univ Pr 1995, 1st Edition Fine Hardcover Fine.
  Price: USD 14.00

- Hodgson, Shadworth H.
  The Metaphysic of Experience, 4 Volumes
  Bristol: Thoemmes 2000, 1st Edition No Jacket Hardcover.
  Price: USD 90.00

- Wright, Chauncey (1830–1875)
  The Evolutionary Philosophy of Chauncey Wright. (3 Volumes)
  Chicago, Illinois, U.S.A.: Thoemmes Pr 2000, 1st Edition No Jacket Hardcover.
  Price: USD 90.00

Finally, I've got John Shook's annotated bibliography of pragmatism, 1898–1940, coming too. (It's got over 4,000 references in it.)

09-01-09 *New Propositions* (to S. Capelin)

It's a long time since I've gotten in contact with you, particularly with regard to your nice note below. But the new year is here, and it is time to come up for some air. I have several things I'd like to discuss with you. I'll number them individually.

1) I finally took up your kind offer of "about US$200" in CUP books in payment of the Aaronson review. I sent the attached note to Laura Clark but have not heard back from her yet. [See 08-01-09 note "My Book Selections" to Cambridge University Press.] Might I ask you to confirm that she got the note?

2) With regard to "My Struggles with the Block Universe" you will see from the running compilation on my web page that it has not developed too much since you last saw it. When I was most committed to developing the document last year and working at it fanatically, my stride was broken by an emergency grant proposal I had to write, and there is something about getting a fanatical stride broken: I never quite returned to the project. It's a bit like a horse tripping in a race, as opposed to tripping in a quiet pasture.

Still, I will return to it. I have every intention of posting a big—overabundantly complete—version of it on `arXiv.org` before the summer is here. And after that I would very much like to pursue publishing an abridged version of it with someone, maybe CUP if you're still interested. In the meantime, however, I'd like to field two more immediate proposals with you ... that are in fact more likely to be sounder business propositions for CUP anyway.

3) The first has to do with the fact that I never ended up following through with Springer on their proposal to publish my *Notes on a Paulian Idea*. A bit before I first met you, they wrote me (23 May 2008):

> Should we not receive any response from you in the coming 6 weeks, we will consider the project stopped and the contract Null & Void.

I never replied, and I think I am happier for it. Particularly, I am free of them, and the more I think about it and have gotten to know the things you have published, the more I am intrigued by the idea that CUP is the right home for the document anyway. Only last week did I learn that you published Mermin's *Boojums* as well as his more recent books.

So, well before thinking about *My Struggles*, what would you think of publishing *Notes on a Paulian Idea*? You have seen Mermin's foreword to it, and my previous self-compilation of praise on it (oh those mirrors!)—I sent you that last June—but let me show off two more recent comments, one from a young author, and one from an old one. I guess what they suggested to me is that the book really might be of broader readership than I had contemplated when Springer first said "yes" so long ago. [I.e., I think I really am ready to publish it now and want it done right.]

The first is praise from Louisa Gilder who wrote this in the acknowledgments of her new book *The Age of Entanglement*:

> Thanks also to Steve Weinstein, who gave me *The Shaky Game*; and especially to Andrew Whitaker, for writing the beautiful *Einstein, Bohr, and the Quantum Dilemma*, and to Chris Fuchs for his wonderful samizdat, *Notes on a Paulian Idea*—these two books sat on my desk rather than on the shelf.

And she accompanied the book she sent me with a personal letter saying,

> Three years ago you gave me your samizdat and it has been my constant desk-side companion ever since. My copy is now dog-eared, bookmarked, further indexed, and underlined everywhere and yet I'm still finding wonderful unexplored sections (just recently I read your letters to Henry Folse for the 1st time, culminating in the great story about Ben Schumacher and Star Wars!) ...
>
> P.S. Have you ever read the tiny book *84 Charing Cross Road*? It has exactly nothing to do with quantum information theory (it is a series of letters between a book lover in N.Y.C. and a bookseller in London) but somehow it feels to me like a companion volume to *Notes on a Paulian Idea*—that atmosphere of erudite but unsolemn enthusiasm, whether for books or P.O.V.M.s, is very satisfying and so vital.

The second praise came from Hans Christian von Baeyer. You must have seen his many books popularizing physics, or perhaps know him. One New Year's Eve, after probably too much wine, he wrote me the very sweet letter in the second attachment. [See H. C. von Baeyer's preply to my 03-01-07 note "New Year's Alchemy".]

So there: Between these quotes and the ones I had sent you previously, I think I can do no more salesmanship. If you want the book, you can have it, and I would say on very short order: I think little more needs to be done in further preparation of it than my adding a good subject index. The presently best indexed version can be found at my website in case you would like to re-peruse it.

4) This leads me to another project that I would like to pursue in short order—something I dreamed up only recently—and I wonder whether CUP might have an interest in it? Asher Peres was a very dear friend of mine, and recently I gave some lectures in Sydney in his honor at the first Asher Peres International (summer) School of Physics. My lectures were titled "Quantum Foundations, Asher Peres Style," and the associated transparencies can be found here

`http://www.perimeterinstitute.ca/personal/cfuchs/Peres%20School%201-110.pdf`

in case you want to peruse them too (though they won't be of much use without lecture recordings).

Anyway, in preparation for those lectures, I reread several of his foundational papers, and I was transported back to my youth in physics. I had simply forgotten how influential those papers were on me. And, indeed, how wonderfully written and concise and forcefully argued they all were. They lay the foundation for a vision of quantum mechanics that is diametrically opposite to the vision of it in Oxford (the vision of David Deutsch, Simon Saunders, Harvey Brown, Jeremy Butterfield, David Wallace, and Artur Ekert), and it struck me: What better place to have those papers republished than Cambridge! I think one could easily make a nice volume of his papers, and the result would get beautiful reviews from the likes of David Mermin, Abner Shimony, Ben Schumacher, and others. (Do you recall Mermin's fantastic review of Peres' book?) I think I counted that Asher had some 70 foundational papers, and we could pick and choose the very best and most influential on the community. The running theme in most every one of them is the idea that "Quantum States Do Not Exist" (a phrase of mine, but you'll find his precise quote on page 5 of my lecture; unbeknownst to me, his was the precursor to my own)—and he explores every aspect of that idea in these papers. I made that the theme of my lectures, and I propose to make it the title of the book. I would write an appropriate introduction explaining the far-influences of Asher's thought (and how it is still developing in pockets of the world) and put Asher, the man, in context with respect to the development of quantum information and the quantum information community. Attached is the piece on him I wrote for his memorial volumes of *Foundations of Physics*; it may give a sense of my writing style with respect to an introduction for the book. I would very much like to tribute Asher in this new way—giving him a new chance to influence the world of quantum foundations the way he influenced me.

That's my four topics. I hope I didn't write you too much to read in one go.

And I hope all is well for you and your family in this new year.

11-01-09 *More on Pauli* (to D. M. Appleby & H. C. von Baeyer)

Boy you sure make it hard for me to feel literary! I thought I was simply being clever with my choice of words—i.e., that talk of the soul doesn't stir my soul! It wasn't an accident. Nonetheless, you have many good points; I liked the soul vs. mind analysis.

We should start thinking about when the two of you might converge on PI again. When do you plan to come next, Marcus? Will you come for your school's Spring break?

It is fantastic what precipitated out of this New Year's discussion of ours. I've been reading voraciously again. I think I'm actually going through a second pragmatic awakening, of the force of the one Matthew Donald's prod set alight nearly 10 years ago. A whole new set of pieces have fallen into place. This time it's not about the theory of truth, but about pure experience. A quantum measurement is pure experience! (In the technical sense.) It is an example of the neutral stuff Pauli was seeking to describe. I hope to give you both an extended report in the coming week.

Marcus's Preply, 11-01-09

> I'll start (and, I fear, possibly finish) with some thoughts provoked by these passages from Hans's notes
>
>> Gieser's section ends: "Science as a discipline must in turn realize that science created by man always includes statements about man. The object of science will therefore always be man himself and his totality; in him is the ethical conflict between good and evil, in him is spirit and matter."
>
> and
>
>> That's the thing that eternally amazes me. How can the cynical modern materialists get around the fact that since time immemorial most of humanity has been religious?
>
> and this from Chris's
>
>> For it is true that I rarely speak of "souls," "religion," or "redemption." These terms are mostly dead terms for me—they don't stir my soul, so to speak—or maybe I simply don't understand them well enough yet to see their ultimate usefulness for what I *do* want to get at. (Much like I have never understood what the search for "elegance" can possibly mean when it

> comes to forming physical theories, say, as a criterion for string theory: it is a term that is dead to me.) It is maybe in this way, or more carefully, *in this detail*, that I part company from your tentative feelings on the opus.
>
> *Nonetheless*, there is no doubt that I believe there is a place—a very important place—for humanistic concerns within physics proper. It seems to me it goes to the core of what quantum mechanics is trying to tell us.

(I could have quoted much more from Chris in this connection, but I won't because cutting and pasting from Acrobat is a pain).

Indeed. As I hope I made clear in my last notes I don't think there is any *essential* difference between Chris and myself. It is just that he tends to focus strongly on the question of freedom while I tend to cast my net a bit wider. For instance, I talk of souls whereas he doesn't.

Why do I talk of "souls"? I used to have a conscience about using this word because I felt I didn't know what it means. And I still don't know what it means: if someone were to ask me what exactly I hope to convey by the word "soul" which I could not equally well convey by the word "mind" I would struggle. Struggle badly, in fact. Nevertheless, I now do use the word, because, although I don't have a very clear idea of what I mean by it, I am very sure that I do mean *something*. Something important.

The word "soul" is a word you simply don't use in respectable scientific discourse. Which is curious because there was a time when the words "soul" and "mind" were regarded as synonymous (if I remember correctly Descartes treats them as synonyms, for instance). But in the contemporary scientific culture it seems that that is no longer the case. The word "mind" is frequently used in the scientific literature; the word "soul" almost never. I suppose one might argue that it isn't used because it is regarded as archaic (in the way that the word "verily" is archaic, for instance). However, I don't think that can be the explanation because the word is in common use outside the world of science. Not only would every scientist understand what Chris means when, in the passage I just quoted, he says "they don't stir my soul". I think that on occasion they would use that phrase themselves. However, they would not do so in a scientific paper. The word seems to be regarded as being somehow "unscientific". The curious thing is that if you asked most scientists what exactly is wrong with the word "soul"—what offensive feature it has got which the word "mind" hasn't—I think they would probably struggle. Indeed, I think they would find it as hard to explain why they *don't* use the word as I find it to explain why I *do*. Nevertheless, they do seem to have a strong feeling that the word is inappropriate when used in a scientific context. This is an observable fact about the psychology of the average scientist. I take it to be evidence that the word has as much meaning for the average scientist as it does for me, even though neither of us can explain quite what the meaning is.

I think the answer to the question "why do I use the word?" may be that I am using it reactively. In avoiding the word "soul" I think the average scientist means to deny something. And whatever it is that he/she wants to deny, I want to affirm.

But, although I can't give an exact definition, perhaps I can give a rough indication. Consider once again the passage from Chris's notes which I just quoted. He begins by saying that the terms "soul", "religion" and "redemption" are dead for him, and then for additional emphasis he says that they "don't stir my soul, so to speak". I can't resist pointing out that there seems to be a contradiction here: for doesn't the fact that Chris uses the phrase "stir my soul" in this way show that the word "soul" is very far from being dead for him? Be that as it may, however. Whatever Chris meant or didn't mean by it, that phrase "they don't stir my soul" gets right to the heart of the matter. I think the word "soul" is an important word because it enables one to say things like this:

It stirs my soul

I felt it in my soul

He put his soul into it

etc. etc. Whatever it is that is conveyed by such phrases cannot be conveyed in mind-language. Just consider:

It stirs my mind

I felt it in my mind

He put his mind into it

So what exactly is the difference between these two groups of phrases? —Part of it is, I think, that the word "mind" is generally used to refer to the cold-blooded, ratiocinative aspects of psychic functioning. There has always been a tendency in modern science to attach overwhelming importance to reason. But in the course of the 400 years that have elapsed since the genesis of modern science in the 17th century the concept of reason has itself become narrower and narrower. The end result is that nowadays the word "mind" tends to convey little more than the idea of an extremely complicated calculating machine. The word "soul", by contrast, conveys a concept which is admittedly more primitive but which is also (it seems to me) richer, and more complete.

For instance when Chris wanted to expand on the idea of a term being "dead" to him he was forced to use the word "soul" rather than the word "mind" because for a calculating machine every term is dead.

I make a point of using the word "soul" because I just flat don't believe that a human being is nothing more than a calculating machine. I should say that I do so with misgivings. For it is, of course, true that the concept of the soul is primitive. It has a distinctly medieval feel to it. So there is a danger of my giving the impression that I am a Luddite, who wants to undo the work of the last five centuries and return to the Middle Ages. Of course, that isn't really the idea at all. My admiration of the 17th century creators of modern science is heartfelt. It is just that I share Pauli's belief that, along with their great achievements, the 17th century thinkers made some serious mistakes. My aim in all of this is only to do what I can to correct those mistakes. However, I do worry that my use of medieval language will obscure that fact.

12-01-09 *Happy Birthday William James* (to H. C. von Baeyer)

Thank you for that fine birthday present for William James. And thank you for teaching me that James's birthday precedes my daughter Emma's [today] by just one day!

I did find the point about thin language interesting. When you and Marcus arrive in Waterloo, perhaps you can help thicken my sauce a bit!

Hans's Preply, "Happy Birthday William James," 11-01-09

Here's a thought I wrote this morning before reading your two contributions.

On Language

In connection with the role of theology and the opus in our study of quantum mechanics I thought it would be useful to hear what Fierz had to say. Contrary to Pauli, who was an atheist by conventional Judeo-Christian standards, Fierz was a Christian, who felt free to interpret scripture in strictly metaphorical ways of his own choosing. Here's what he wrote to Pauli on 11 February 1956:

> ... The old story of paradise — which of course is much older than the creation story — culminates in humans eating from the tree of knowledge and then getting thrown out of the garden of Eden. In my opinion this story does not mean anything sexual, but the sexual just happens to be part of the problem. The story primarily means simply that we have now internalized the apple of *Erkenntnis* (knowledge, discovery, recognition) and that there is no way back.
>
> From this I conclude — for the story is believable and good — that the way to salvation is mediated by *Erkenntnis*. But this *Erkenntnis* is not a revelation. It is work — by the sweat of our brow — thistles and thorns — an *opus*. Besides thistles the field yields fruit, too, and we may enjoy those. But the enjoyment of the fruits of our *Erkenntnis* is not yet an enjoyment of power.
>
> Erkenntnis also implies that opposites are recognized — Adam "recognized" Eve; namely as woman who was the opposite of him, the man.[173]
>
> The discovered opposites will probably have to remain separated, at least for now. That's all right, because, or inasmuch as, they have been truly discovered. In the discovery they are unified insofar as there is someone who can see them both and who is prepared to carry the burden that is thereby put on him — i.e. the cross.
>
> All this sounds a bit theological; but I don't know a better language. The philosophical-conceptual language is too thin for me, too lacking in images, and then I have no idea what I'm talking about.
>
> From *Erkenntnis* I expect salvation (wholeness, well-being). Yes, I mean even from scientific *Erkenntnis* or research (for it has a methodology). This is not a rationalizing point of view. For the object of *Erkenntnis* is precisely the irrational. Internal images and fantasies are just as much objects as the external world. In order to recognize them we need a methodology with which we can operate on them (experiment!). In this methodology I include associations, amplification of images; your [Pauli's] comparison with mythological and other materials, etc.

I thought the bit about language would interest Chris, who's on the other side of the fence.

[173] [Fierz's footnote.] Contra Plotin and other observers: *Erkenntnis* is the the work of *Erkenntnis*, which requires a methodology. Contraries are recognized, i.e. one recognizes that they exist, not that they actually don't exist!

13-01-09 *The Remarkable Theorem* (to A. Uhlmann)

Uhlmannism 2: *Yes, this is an unexpected characterization by Jones and Linden (*`quant-ph/0407117`*) and, if I am right, independently by Patrick Hayden.*

[See 08-01-09 note "Fuchs-Graaf Inequality" to A. Uhlmann for a statement of the characterization.] I am pretty sure that I reported it to Patrick, though my memory may not serve me well. The only independent discoverer (beside Jones and Linden) I know of is Steve Flammia, at the time a student of Carlton Caves. When he first showed Carl the result, Carl didn't believe him. Later I was told about it, and I was shocked: I thought, surely it must be a folk theorem that Caves and I had somehow missed. So, I got in the habit of asking everyone I knew about whether they had ever seen the little theorem. And the interesting thing was that no one had! Here is a list of people I asked about it: Holevo, Leib, Ruskai, Calderbank, Lindblad, Gottesman, John Conway, and many more than that. No one had. So, it wasn't a folk theorem after all, even though the result is almost trivial. Thus, in my presentations, I got in the habit of naming it "the remarkable theorem" and giving profuse credit to Flammia and Jones and Linden for unearthing this beautiful little mushroom.

Uhlmannism 3: *Of course, the set of faithful density operators can be described by a finite set of strict inequalities, you mentioned the elementary symmetric functions.*

That's right; I am aware of this.

Uhlmannism 4: *I do not know. But the manifold of rank one projections has at every of its points an individual tangential hyperplane giving a necessary linear inequality. Therefore, I think, the additional conditions you are looking for cannot be linear if there are finitely many. I am also pessimistic about substituting a degree three polynominal equation by some degree two ones — but this may be a wrong expectation if there are in addition some distinguished linear equations or inequalities.*

Yes, I guess I was *hoping* (no intuition, just hoping) that the inequalities might bring the remaining conditions down to some degree two conditions. I suppose I'm pessimistic too. May I ask you, however, to expand on your remark about a "necessary linear inequality". (I.e., your second sentence in this paragraph.) I don't know what you are saying here.

Uhlmannism 5: *If P_1, P_2, … is a SIC, one perhaps needs some insight of what phases occur in the traces of their products.*

Appleby and I will soon post a very detailed study of the "triple products": I mean $\mathrm{tr}(P_iP_jP_k)$. They have a very remarkable structure.

Uhlmannism 6: *Next month I become 79. Up to now I can be satisfied. Of course, there are always things to be repaired!*

Very good! It is always good to become prime once again!

14-01-09 *The Remarkable Theorem, 2* (to A. Uhlmann)

Uhlmannism 7: *There is a curious convex set, given by the intersection of* $\operatorname{tr}\omega^2 \le 1$ *and* $\operatorname{tr}\omega^3 \le 1$. *This convex set is definitely larger than the state space if dim* > 2. *Do you know whether somebody has looked at it?*

I had contemplated looking at it at the Cambridge workshop on "foils to quantum theory" two summers ago. But then nothing ever materialized of the hope—other things got in the way. I also wanted to look at this convex subset of the probability simplex over d^2 points: The one where $\sum p(i)^2 \le 1$ and $\sum p(i)^3 \le 1$. I don't know that anyone has looked at these things in detail. If you get any results, I would love to hear about them!

15-01-09 *Waterloo Morning* (to D. M. Appleby and Several Others)

Dear old collaborators (in the warm parts of the world),

Don't you wish you were up here in beautiful Waterloo with me this morning? It's −29C outside (−20F for the Americans)! Think of the grand discussions we could have on the quantum on our walk to PI!

15-01-09 *Exchange of the Delusional* (to K. Martin)

Thanks for sending that. I had a great time reading it to my wife last night and trying to get the voices right. Beautiful writing—it felt it must have really captured the circumstances and atmosphere. But don't think for a moment there's no quantum there! I like the way William James put it:

> Taken as it does appear, our universe is to a large extent chaotic. No one single type of connection runs through all the experiences that compose it. If we take space-relations, they fail to connect minds into any regular system. Causes and purposes obtain only among special series of facts. The self-relation seems extremely limited and does not link two different selves together. Prima facie, if you should liken the universe of absolute idealism to an aquarium, a crystal globe in which goldfish are swimming, you would have to compare the empiricist universe to something more like one of those dried human heads with which the Dyaks of Borneo deck their lodges. The skull forms a solid nucleus; but innumerable feathers, leaves, strings, beads, and loose appendices of every description float and dangle from it, and, save that they terminate in it, seem to have nothing to do with one another. Even so my experiences and yours float and dangle, terminating, it is true, in a nucleus of common perception, but for the most part out of sight and irrelevant and unimaginable to one another. This imperfect intimacy, this bare relation of withness between some parts of the sum total of experience and other parts, is the fact that ordinary empiricism over-emphasizes against rationalism, the latter always tending to ignore it unduly. Radical empiricism, on the contrary, is fair to both the unity and the disconnection. It finds no reason for treating either as illusory.

In the vision of the world I hope to construct, there'll always be room for the likes of you, me, and even the phone lady. My experience is a string, yours a bead, and hers a feather. They are no lesser parts of the world than the results of the LHC. And it is quantum mechanics that gives us the most compelling case for that indication!

Attached is a little document I put together for Marcus Appleby and Hans Christian von Baeyer at New Year's. [See 05-01-09 note "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer and D. M. Appleby.] In other contexts I've been calling it "The Metaphysics of SICs."[174] You think I'm spending all this time just trying to find better descriptions of quantum channels? No, I've got much bigger things in mind!

I hope you enjoy this exchange of the delusional!

17-01-09 *It's Always Einstein* (to P. G. L. Mana)

Manalogue 14: *As for correspondences, I suppose you already have the book 'Letters on Wave Mechanics: Schrödinger, Planck, Einstein, Lorentz'?*

I read that book many years ago. I remember a most amazing letter from Einstein to Schrödinger, soon after S had found his time-independent equation. Einstein misread the draft of the paper and got the equation wrong. So he wrote to Schrödinger a letter saying what he didn't like about the (incorrectly recalled) equation. Then he said, "You will notice that the following equation" (in which case he wrote down the proper Schrödinger equation as if he were the inventor of it) "has all the appropriate properties." Finally he ended the letter by saying, "however I can find no interpretation of this function ψ which is its solution".

Beautiful! Tell me if I'm right with my memory.

19-01-09 *Phishing for Filosophy* (to R. Blume-Kohout)

I never have quick responses to anything … because I never have quick thoughts on anything. The best I can ever muster is to attach an old, partially relevant email, as I will do here. But it was good to juxtapose Fish's article today (which I had not read before your note) with this other one in today's NY Times that I had: `http://www.nytimes.com/2009/01/19/books/19read.html?hp`. The attachment concerns my way of saying the humanities are important to the quantum world, as the quantum world is important to the humanities. (It was a New Year's letter written to Appleby and Hans Christian von Baeyer.) [See 05-01-09 note titled, "What I Really Want Out of a Pauli/Fierz-Correspondence Study."] I'll leave it to you to tell me whether it means I think physics is a liberal art.

20-01-09 *From Pragmatism to Pure Experience* (to M. Bächtold)

Thank you for sending your new email address. I hope you are enjoying your new position and finding it a productive place to think. I can see that you are somewhere in France, but could not figure any more detail than that from your email address. Where are you now?

Thank you for enquiring on the paper. I am in the middle of its construction and hope to have it for you in about a month. It has been very tough going for me—first with the collapse of Bell Labs, then the flood of our home in New Jersey, and then the move to Canada. But all is clear intellectually now, and this paper has become more important than ever for me. In the course of

[174] It is a strange thing that those "other contexts" would be my note to Keye just nine days earlier, in which I had sent him the very same attachment. However, I have a faint memory that I did this on purpose for one reason or another.

developing it, I have greatly clarified my thinking. Particularly, I ended up in a place I had not expected to. What I try to argue now is that in a very serious sense, quantum measurement is an instance of Jamesian "pure experience"—and that quantum mechanics is our laboratory for finally making that notion precise. This is quite a turn for me. My initial attraction to pragmatism had been solely for "theory of truth" reasons: It was a means of making sense of the idea of quantum measurements being generative (i.e., as not simply revealing pre-existent facts), yet, at the same time, allowing that one could be "certain" of a measurement outcome via an initial pure-state assignment. The certainty must be interpreted as subjective Bayesian certainty (i.e., it lives in the realm of ideas), and in any measurement interaction, it can be *made* true or false. So, the "truth" arising from a quantum state assignment is something that comes after the fact—at the conclusion of measurement—and is not something pre-existent with the state itself. (For after all, "quantum states do not exist" as the Bayesian slogan goes.) But that was my initial attraction to the pragmatist line of thought. Where I go now is to understand quantum measurement as an instance of a more general phenomenon—something that is neutral between the physical and the mental (agential would be a better second term). I try to view these "pure experiences" as the active monads of the world, similar to James and similar to John Wheeler with his "elementary acts of observer-participancy" being the building blocks of the world. What I am trying to do in the paper is to show that this is the logical endpoint of my earlier less-radically-empirical pragmatism. And I hope to set up a framework for making these issues much more precise than hitherto possible.

I hope you will like the final result, and that it will have been worth your wait.

21-01-09 *Old Poetry* (to G. L. Comer)

Comerism 21: *Congratulations on your new post! Will you get to meet the head of NSA?*

I'm aiming for the Obama cabinet. I suspect he needs some advice on quantum foundations. ... Though he already shows promise, indeed: He seems to already have good pragmatist credentials. Did you notice the line about our "uncertain destiny" in yesterday's speech?

21-01-09 *More Obagmatism* (to G. L. Comer)

This one from Paul Begala's report of the speech on CNN. (Paul was at the University of Texas just before me, or maybe there was some overlap. He lost against Hank the Hallucination, a cartoon character, when he ran for student-body president.)

> The president closed by quoting the words from Tom Paine that Gen. Washington ordered read aloud at Valley Forge. "Let it be told to the future world," Washington said, "that in the depth of winter, when nothing but hope and virtue could survive ... that the city and the country, alarmed at one common danger, came forth to meet [it]."
>
> The quote shows Obama's belief in unflinching courage, unblinking realism and an unrelenting faith that by coming together we Americans can bend history to our will.

What other philosophy than pragmatism would admit that we "can bend history to our will"?

21-01-09 *Pauli Retally* (to R. W. Spekkens and L. Hardy)

The title's got to give you a giggle.

After Dawn's high number yesterday for these volumes, I decided to look into it myself. The result is attached. A more accurate number is around \$1600 CAD total (or \$1270 US), excluding shipping.

I think Rob is right. Except for Appleby's and my quest for clues toward constructing a "psycho-physically neutral" language (as Pauli called it) for quantum phenomena—which I do very much consider modern research in quantum foundations—this purchase really should be considered more in the line of a resource for the history of physics. I.e., in most anyone's hands but ours, it will be historical reading. And maybe that is not what PI should be in the business of—at least not at such a price premium.

Thus, I want to make the following offer and see how you'll consider it. If PI will pony up half the expenses, I'll cover the other half with my ONR grant. And PI gets a great set of books (however you classify the subject) that it can house forever. Or at least until the inevitable fire that comes to all book collections.

Does that sound reasonable? If so, then I'd like to get to action so that some volumes might be here before Appleby's and von Baeyer's visit Feb 15.

21-01-09 *Pauli Retally, 2* (to H. C. von Baeyer and D. M. Appleby)

Well, I tried (see below), but it seems neither of my senior colleagues in foundations are willing to stand up for a Pauli-correspondence purchase for the PI library. The view of Rob, at least, was that though it may be a resource for the history of physics that is not our business. And when he discovered that the volumes are not translated even, that really pushed him over the edge. Oh you lucky ones at the "liberal arts" institutions; your libraries are so much more tolerant.

I even tried to sweeten the pot by promising to launder half the expenses from Marcus and my (still itinerant) ONR grant. But it was a no go. And somehow I couldn't find myself laundering the full expenses when I honestly feel it should be housed at PI.

Thus before you arrive, I will try to get as many of the volumes as I can by Guelph-Waterloo library loan.

Maybe I should apply for a humanities grant to ease my conscience.

Marcus's Reply

> Compared with the size of the ONR grant \$1270 isn't that much. I wouldn't mind at all if you used the grant to cover the whole cost. Though maybe it ought to be in the possession of someone who can read German (definitely not me!).
>
> I think more hangs on the distinction between "physics" and "history of physics" than meets the eye. In other words the fact that it appears obvious to almost everyone that you can pursue research in "fundamental physics" whilst having, at best, only a very sketchy, journalistic understanding of the "history of physics" shows that almost everyone is looking at physics in a profoundly wrong manner. I think it has to do with objectivism.
>
> The mistake the objectivists make is to misunderstand the relation between (on the one hand) the world and (on the other hand) our thoughts about the world. In particular objectivists tend to identify the conceptual/linguistic/mathematical structures pertaining to our understanding of the object with the object itself (e.g. Everettians identify the state vector of the universe with the universe itself). It is a really weird

mistake when you come to think of it. As if I were to identify the name "Pauli" with the man of whom it was the name and, furthermore, to assume that anyone who challenged that identification was denying the reality of the man himself. But people don't see that it is weird. And that shows that the error is very deep-seated.

The idea that "history of physics" is irrelevant to physics itself is due, I think, to the fact that people are thinking that "history of physics" is to do with us (our ways of understanding), while physics itself they tend to identify with the object (what we are trying to understand). So that makes it look as though "history of physics" and "physics" are two entirely different things.

It is not so, of course. Physics is an intellectual structure, of our own creation. It is true that it has what the philosophers call "intentionality": it points at something different from us. But just as a pointing finger, though it points at something distinct from the body, is itself part of the body; so physics, though it is about things different from us, is still itself part of us.

The fact that physics is specifically *our* intellectual structure means that it is, like us, something organic. And it is fundamental to organic entities that they evolve: in other words, that they have a history. It would never occur to a neurophysiologist that you can try to understand the brain whilst completely ignoring the brain's evolutionary history. The same principle applies (or should apply) to physics which, after all, is a production of the brain. (I was going to say it is a production of the brain at the software level. But I am not sure that is right. One of the differences between brains and computers is that there is not the same clear-cut distinction between software and hardware in the case of the brain. A brain which knows quantum field theory is, probably, physically very different from one which only knows how to make stone axes.)

I am presently trying to understand why some *Mathematica* code is giving me the wrong answer. To do that I am going back over all my working, starting from the beginning. In other words, I am examining the history of how I got to this point. It is surely the natural way to proceed.

Similarly with quantum mechanics. The reason that there is a whole industry devoted to the foundations of quantum mechanics, but no such industry devoted to the foundations of relativity, is that quantum mechanics seems, as judged from the perspective usual among physicists, at least on the face of it, to make no sense (wave function collapse, etc etc). In tackling this problem most people start from the assumption that it only *seems* to make no sense when judged from our usual perspective. However, it is surely no less reasonable to think that there is something wrong with the usual perspective. To track down the error it is surely very reasonable to examine the evolution of that perspective. To look at the history of it, in other words. And it seems to me that such an examination obviously belongs to physics proper.

22-01-09 *Bose-Einstein-Bayes* (to N. C. Menicucci)

Menicuccism 2: *What would you say is the key difference between quantum and classical indistinguishability of particles (e.g., considering bosons/fermions versus classical particles), if any?*

I wish I had a real answer for you, but I don't. The only thing I know is that I still stick by the tenets below. [See 12-03-07 note "More Serendipity" to G. L. Comer.]

23-01-09 *Feynman Comment?, 3* (to N. D. Mermin)

Are you still out there?

Did you ever have any insight in the question below? If you cannot recall any of your own writings, can you recall anyone else's?

I've had an excruciatingly hard time writing this paper, but it is slowly coming together. The upshot is I want to return "interference of probabilities" to the place in quantum foundations where Feynman thought it was. (Though Bayesianified and quantum informationified, of course.) It'll help the drama if I have some quotes of opposition to fight it out with.

You never told me you didn't like the title *Notes on a Paulian Idea* before.

23-01-09 *First Reports on NPI* (to S. Capelin)

Well, Ref B is pretty clearly Scott Aaronson. There's only one lunatic out there who's ever read the whole thing!

24-01-09 *Pauli Retally, 3* (to D. M. Appleby and H. C. von Baeyer)

[Concerning Appleby's reply to "Pauli Retally, 2" . . .] That was indeed an eloquent defense! Would you mind if I forward it to Rob and Lucien? I've given up on the book issue, but I think it's something they should think about for the future.

The reason I've been reluctant to purchase the set solely for ourselves is in significant part the language issue. Putting it in the library and making it available for everybody was the way I had planned to ease my conscience. I mean, ideally I'd like to go through the volumes page for page and catalogue potentially interesting items, but that's most of the use I'd have for them. (Anticipating my only having the patience to scan for significant words, since I don't know the language.) If I felt I was doing more good for the community, I could see doing that. But without PI (as embodied by Rob and Lucien) recognizing some value in the gift, it kind of galls me to give it to them.

02-02-09 *Your PhD Thesis* (to P. G. L. Mana)

Manalogue 15: *During the last few days I've been thinking about our lunch discussion, about what Chiribella said at the panel discussion, and about how you enthusiastically commented on his words. There's much in the current language we use to describe quantum-mechanical phenomena that throws sands on our eyes and precludes us from thinking better, deeper, unbiassedly, and along new paths. Bell criticised the word 'measurement', but there are many, many other words that lurk around in our physical speech and cover our eyes. Yesterday at the group meeting we discussed, at some point, on whether we could do without the concept of particle or of wave in quantum mechanics and quantum field theory. Then, in presenting an experiment (Mach-Zehnder), sentences like 'there is one photon in this mode' came forth. In presenting experiments with such a language we are already committing to an idea, and there is therefore little hope for us to be capable to reinterpret the physics behind them in new ways.*

This is a point that Ed Jaynes made very beautifully somewhere as well, I recall.

Manalogue 16: *Personally, I can speak of any quantum experiment whatever without ever mentioning the word 'particle' even once, and translate what the others say in that particle-less language.*

Similarly for me: the concepts of both particle and wave hold no use in my own understanding of the world.

Manalogue 17 : *Take for example the sentence 'The probability of finding the system in such and such a state', which I hear almost daily.*

You *never* hear that language from me.

Manalogue 18 : *In classical analytical mechanics people speak all the time about the 'evolution of the probability distribution' on phase space. That's nonsense because a probability is timeless; there is not even any updating in the Bayesian sense when we speak about that 'evolution'. And even the term 'updating' is misleading indeed. Bayes' formula simply relates probabilities having different contexts.*

I believe de Finetti put the latter point like this: It is only the following out of our set opinion.

03-02-09 *Title Thoughts* (to S. Capelin)

I'm jumping the gun here, but I'm sober enough to know that if you guys do accept the book, the title at the least will have to be changed. I just wanted to record these thoughts while they're on my mind, and you seem like the appropriate repository for my finding the note again.

- Coming of Age with Quantum Information
- What's Under Quantum Information?
- Quantum Information, Quantum Reality, Quantum Personality

Just playing with words—I know it's got to make connection to a wider audience, but it also has to be true to what the book was intended to be.

Hope you don't mind my bothering you like this.

04-02-09 *Title Thoughts, 2* (to S. Capelin)

Capelinism 1 : *Actually, I like the suggestion that the book should include brief biographical sketches of the people your emails were sent to.*

I do as well. It is a good idea and, I think, it would make for a fun project (to try to say something fun about my old friends).

Capelinism 2 : *Would you be willing to tighten it up a little?*

Potentially. Since the beginning, I have been acutely aware of the repetition factor. I tried very hard to guard against it when I first put the thing together. But the trouble remained that I have variations and variations on the formulations of the ideas I was seeking. Some of them never converged; sometimes the different formulations continued to hold positive aspects that I did not want to throw away. But I will think about the idea of tightening—one thing I fear is that it would be a major undertaking.

Also, there is the comment of Referee A. One of the reasons *I believe* you can dip into the book almost anywhere and pick up the thread is precisely that so much of the book is repetitious, working out variations on variations. After all, it is just notes on an idea.

I didn't like my new titles either much. So I'm glad you're of two minds.

04-02-09 *That Savage* (to R. Schack)

Do you remember a place where L. J. Savage said something *like* "there is a continuous scale between pure coherence and objective probability and the issue is, where does one make the cut? Objectivists go to one extreme, radical personalists go to the other. One may well think there a further normative rules beyond coherence, but when does one stop the process of adding more structure before the abyss of objective probability?"

At least I recall a discussion like that somewhere. I'd like to quote a bit of it for our paper, but cannot find it. Any hints you have would be most useful.

05-02-09 *Savage Morning* (to R. Schack)

By the way, I came across this paper, which I found interesting: L. J. Savage, "Difficulties in the Theory of Personal Probability," *Philosophy of Science* **34**, 305–310 (1967).

The penultimate paragraph harkened back to some of our discussion from the early days of "you're going to tell me a beamsplitter isn't a beamsplitter!":

> The idea of facts known is implicit in the use of the preference theory. For one thing, the person must know what acts are available to him. If, for example, I ask what odds you will give that the fourth toss of this coin will result in heads if the first three do, it is normally implicit not only that you know I will keep my part of the bargain if we bet but also that you will know three heads if you see them. The statistician is forever talking about what reaction would be appropriate to this or that set of data, or givens. Yet, the data never are quite given, because there is always some doubt about what we have actually seen. Of course, in any application, the doubt can be pushed further along. We can replace the event of three heads by the less immediate one of three tallies-for-head recorded, and then take into our analysis the possibility that not every tally is correct. Nonetheless, not only universals but the most concrete and individual propositions are never really quite beyond doubt. Indeed, as you know better than I, such seeming statements of fact must actually be recognized as universals. Is there, then, some avoidable lack of clarity and rigor in our allusion to known facts? It has been argued that since indeed there is no absolute certainty, we should understand by "certainty" only strong relative certainty. This counsel is provocative but does seem more to point up, than to answer, the present question.

I also liked this line from the bottom of page 307 / top page 308:

> Yet, in the very notion of this choice of a framework there are impressive difficulties. Is it good, or even possible, to insist, as this preference theory does, on a usage in which acts are without influence on events and events without influence on well-being?

05-02-09 *The Paulian Idea* (to S. Capelin)

I'm OK with what you say about the title.

But here's another potential idea for the future with regard to the title: Use 'Notes on a Paulian Idea' as a subtitle. *Coming of Age with Quantum Information: Notes on a Paulian Idea*. That would preserve a bit of continuity with the previous edition, but possibly put a more public face on it.

The choice of title came about because reading Pauli particularly turned my conception of quantum mechanics upside down. Not Bohr, not Heisenberg, but Pauli. It was through him that I started to realize that one might have it all: A Copenhagen-style understanding of quantum measurement *without* giving up on reality itself (as so many are apt to accuse me of). [See note titled "The Reality of Wives" [in this volume] to get a sense of the constant defense I'm on.] The beginnings of my transition are documented in the first three pages of the Letters to Greg Comer chapter. Then really, almost all the correspondence throughout built up around the core of Pauli's idea … seen through the lens of quantum information and Bayesianism. The two quotes on page vii are what I consider the essence of the Paulian idea. Nondetached observers, yes, ones who participate in shaping reality, but nonetheless raw reality too—i.e., the world is not a dream or an expression of my fancy. That was the idea that hit me over the head, and it is why I paid homage to it with my title. (Note: Those early letters to Comer were included for completeness; otherwise all correspondence in the book comes from my postdoc period, starting in 1996.)

I would like to deliver the book to you *no later* than June 1. I'm trying my hardest to make this Spring a great house-cleaning exercise.

06-02-09 *Swedes, PDFs, and Experiments* (to R. Laflamme)

Speaking of the geometry of Hilbert space, do you think you could do an experiment like the one in Figure 1 of the attached paper?!?! [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`, but Figure 2 instead.] It would be another way to test the Born Rule, and I got the impression you have some interest in that. The first interesting system with regard to these considerations, however, would be a quTRIT. The paper is still under construction. In fact, it is far from complete (many of the main results are not in it yet, sections unfinished, tentative paragraphs, etc.), but the present draft does at least contain the definitions needed to make sense of what I'm talking about. Let me know if you have an interest in learning more. (This is the stuff I'm getting my ONR funding for.)

06-02-09 *4.1 and PI* (to R. Schack)

By the way, this may have been the Savage quote I was thinking of:

> All views of probability are rather intimately connected with one another. For example, any necessary view can be regarded as an extreme personalistic view in which so many criteria of consistency have been invoked that there is no role left for the person's individual judgment. Again, objectivistic views can be regarded as personalistic views according to which comparisons of probability can be made only for very special pairs of events, and then only according to such criteria that all (right-minded) people agree in their comparisons.

It is not nearly as detailed as I remembered, but then again my mind very often fills in a lot of things that were not there originally!

06-02-09 *Something Concrete* (to Å. Ericsson)

It dawned on me that it might be fun to give you something concrete to think about in your off hours as you wait to come to Canada. As you get a chance, have a look at this paper and this presentation:

“Statistical Models are Algebraic Varieties,” by Seth Sullivant

`http://www.math.harvard.edu/~seths/lecture1.pdf`

and

[Something now lost], by Seth Sullivant

`http://www2.warwick.ac.uk/fac/sci/statistics/staff/research_students/zwiernik/wag_june.pdf`

One thing I’d like to explore with you after you arrive and get settled is whether it is fruitful to think of quantum state space as a “statistical model”. And if so, what standard tools we might bring from that area of research to better understand quantum states.

To put this in context, let me attach a paper I’m constructing at the very moment. It is far from complete (many of the main results are not in it yet, sections unfinished, tentative paragraphs, etc.), but the present draft does at least contain the equations you’ll need to make sense of what I’m talking about. Look at equation (30) or alternatively equations (27) and (28). These allow us to think of the set of pure quantum states as an algebraic variety on the probability simplex. By the definition of the links I gave you, quantum state space becomes a “statistical model”. The first question that can be asked then is whether it is a meaningful statistical model? Is it fruitful to think of it that way?

I’m looking forward to your teaching me some geometry!!

07-02-09 *De Raedt Papers* (to C. Ferrie)

Ferrie-ism 1: *Certainly a few of the informal conclusions will drive away some of the more war-torn veterans of Bell’s theorem. But, I really do get the sense that this new paper has some merit in its formal mathematical results. The abstract does not do it justice. The paper, it seems to me, is really about data analysis and the assumptions which go into forming models of the data.*

Sorry, Chris, but it’s my firm belief that your mind is presently darting around some very nasty places. Basically, I think you are charmed by a complicated, but ultimately untenable patent application. I’m alluding to my perpetuum mobiles again. So, I reissue my challenge. With regard to the two attached papers [N. D. Mermin, “Quantum Mysteries for Anyone,” J. Phil. **78**(7), 397–408 (1981); N. D. Mermin, “Bring Home the Atomic World: Quantum Mysteries for Anybody,” Am. J. Phys. **49**(10), 940–943 (1981)]: Pinpoint *exactly* where Mermin makes an unjustified assumption in his analysis. You may use any insights you want from De Raedt *et al.* or Jaynes or anyone else, but stick to the issue. What is wrong with Mermin’s specific argument?

The point here is that (I well believe) there is some very clear thinking going on behind Bell. And papers like De Raedt’s are just so many obfuscations. Stick with a crystal clear argument like Mermin’s and tell me what’s wrong with it.

09-02-09 *Qu* (to O. J. E. Maroney & H. Westman)

I should comment on Owen’s note before walking into the next meeting this afternoon.

Maroneyism 2: *This sounds interesting, but is definitely going to put off the physics side as presented, which would be a shame as the last one got a good mix. [I.e., the last PIAF meeting.] — Is it possible to lump “Perspectivalism” together with “The role of agents” in some way? That might reduce the overall scariness to the physicists of the list.*

My own opinion is, "then too bad for the so-called 'physicists'." It may well just be that the scariness of terms like "agent" is what has kept us in the quantum muddle all these years. The fear of a single little idea: that there is a distinction to be drawn between the objective and subjective, and that quantum theory rather than being exclusively about the one or the other, is about both. So one has to get the distinction clear, or there'll be no progress at all. It's not quite a 12-step program like Alcoholics Anonymous, but recovery has to start somewhere.

There, I said my piece, now I'll go defend it in chamber.

10-02-09 *Quanta Vista* (to R. W. Spekkens, L. Hardy, and H. Westman)

Spekkensism 47: *Hey guys, here's the synopsis:*

> **Quanta Vista: Frame, gauge, context and agent in quantum theory**
>
> Topics:
>
> - Reference frames
> - Relationalism
> - Contextuality
> - Gauge and symmetry
> - Subjective elements in the quantum formalism
>
> *I've slightly modified the topic list. I've replaced "role of the agent" with "Subjective elements in the quantum formalism" to avoid the impression that we're talking about consciousness causing collapse.*

My first quick impressions.

1) I like "subjective elements in the quantum formalism" very much. It captures the issue just right from my perspective.

2) But your fear of agent didn't reach up to the title. In any case, I don't think the title is snappy enough now. Personally, I think you would do better to drop agent from it but *leave* context: "Quanta Vista: Frame, gauge, and context in quantum theory." Sounds physicsy, non-frightening, and captures the matter. With the bullet on subjective elements, I get my cake and can eat it too ... but in a less threatening way.

3) I disagree on throwing out contextuality—more precisely Kochen-Specker—as a subject matter. These are topics the community ought to have a chance to think of in proximity of each other if progress is going to be had. If anything sticks out like a sore thumb, it's consistent histories. I'd say drop that subject this round. I really don't see how it fits.

4) I fear that the philosophers are a bit under-represented this time. Last year we had 7/29. I think we ought to maybe think harder there.

5) I personally would like to bump up Timpson—he's the only philosopher in the standard philosophy-of-physics crowd who really gets the subjective/objective divide. I.e., he's not like most other philosophers of physics who basically just see information as a new kind of fluid. Also, given van Fraassen's paper or papers on Rovelli (one had a title "Rovelli's World") I think it's entirely appropriate to try to recruit him. Though, I think like Rovelli himself, he's unlikely to come.

6) Carlton is spelled Carlton.

I'll have more impressions later. This is becoming a worthwhile exercise.

10-02-09 *Quanta Vista, 2* (to R. W. Spekkens, L. Hardy, and H. Westman)

Spekkensism 48: *Speaking as someone who thinks about each of these things, the connection between frame-dependence, agent-dependence and context-dependence is pretty weak at present.*

I read over the note again and disagree with this even more. Each topic touches on the idea of something "numerically additional" to the quantum system (to use a William Jamesian phrase). Frame, agent, and context all capture this. As well as gauge. It is a unified topic if you look at it this way. They are all about things non-intrinsic.

Spekkensism 49: *I would be very surprised to see many connections drawn between these threads at the meeting.*

Like a good democracy, the only way to set it at work is to set it at work. Give the participants a chance to think about *all* these things in a compact span of time.

10-02-09 *Instance of James* (to R. W. Spekkens)

> Do you wish, like so many of my enemies, to force me to make the truth out of the reality itself? I cannot: the truth is something known, thought or said about the reality, and consequently numerically additional to it.
>
> — from Chap 15 of *The Meaning of Truth*

10-02-09 *Your Thesis* (to J. R. Gustafson)

Hans Christian von Baeyer has told me of your PhD thesis. If you have it in electronic form, may I ask you to email me a copy?

Hans Christian is taking on a project to potentially write a book (or at least some articles) on the Pauli-Fierz correspondence, and will be coming here to the Perimeter Institute to get started on the project next week. And I myself have had a long interest in Pauli (see for instance, my *Notes on a Paulian Idea*, posted at my webpage, which Cambridge U. Press will soon republish).

I think your thesis will be an invaluable resource for our discussions.

John's Reply

> By now I assume you have received the electronic copy of my Ph.D. thesis. I hope you find it helpful.
>
> Your request got me to pick up my thesis again and to think of unfinished business. Since you or Hans Christian von Baeyer are in a better position than I am to follow up on one of the unfinished activities, let me pass on to you that there is one source of Pauli materials likely remaining untapped … the 1300 or so Pauli dreams collected by Jung. Jung published only some of them (see for example Sir Herbert Read, Michael Fordham, and Gerhard Adler, ed., *The Collected Works of C. G. Jung, Vol. 12, Psychology and Alchemy* (London: Routledge & Kegan Paul Publihers, 1953). The rest may still remain in Jung's archives, perhaps in Switzerland. Since Jung excluded many of Pauli's dreams from publication because they might reveal the name of the author, and because Pauli often mentioned in his dreams "physics stuff," I think the unpublished dreams … if

they still exist ... might be excellent resources for deciphering a small part of the mind of Pauli. I thought I might pass this trivia on to you and ask you to forward it to H. C. von Baeyer.

11-02-09 *Invitation to Speak at University of Rochester* (to B. Weslake)

I remember you well. Congratulations on your position in Rochester.

And I'm flattered that you're discussing this Bayesian business. I want to encourage it!

I could give a talk on your March 25 slot if you'd like. The topic would be the attached paper. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] *Beware*, it is still under construction. In fact, it is far from complete (many of the main results are not in it yet, sections unfinished, tentative paragraphs, etc.), but it'll give you some sense of the topic. And if you guys are still in ongoing discussions on our Qubayesian point of view, it may help give some sense of the constructive side of the program (which isn't much revealed by my older papers).

I've marked my calendar, but be sure to remind me again anyway as the time draws nearer.

11-02-09 *The Still Way-Incomplete Paper* (to H. C. von Baeyer)

Well I didn't make much more progress on it since last writing you. It still has a long way to go—many of the main results are not in it yet, sections unfinished, tentative paragraphs, etc.—but I wanted to get it to you before your flight nonetheless, in case you might want something small to read.

Particularly with regard to our conversations this coming week, I plan to completely scrap the old ending of the paper and start over (that ending was from the preliminary version sent to a conference proceedings). The new, yet-to-be-written ending will make a transition from Peres to Pauli and have a discussion on the undetached observer.

Anyway, I just wanted to give you some sense of how this all fits together in my mind.

Hans's Reply, "Reaction," 13-02-09

> I'm in Toronto with the three most important volumes, so we'll have plenty of grist. Let the old mill grind!
>
> I read your paper on and between planes and like it very much. Here are a couple of reactions.
>
> Objectivity. This is a very heavily laden word and I wish we could get away from it. Intuitively it is defined mostly by its opposite — subjectivity. I notice that you have started using "personalized" as an alternative, and I'm not sure that's much better. A lot of resistance to the Bayesian view stems from the perception that it is solipsistic, and therefore not "objective."
>
> Pauli thought that q.m., which he believed to be all about information, is objective in the following sense: Once the observer has set up an experiment and has turned on the switch, it proceeds on its own without influence from his feelings or desires — unlike a poem or a work of art. This doesn't say much about the DESCRIPTION of an experiment, but he felt that he had to insist on the objectivity of physics.
>
> Examples. I would love to see some examples of simple phenomena, especially since you start with the double slit, and mock other text-book exercises. Those are

good rhetorical anchors, because all readers are familiar with them, but I would enjoy seeing you return to them and deal with them explicitly in your new language. (here is a reference to a recent paper of yours that might contain examples of descriptions starting with different priors.)

See you soon!

12-02-09 *On the Charge of "Bias"* (to N. D. Mermin)

Referee 1 wrote this:

> The paper is proselytizing a particular point of view, rather than drawing together different sources to provide an unbiased review (i.e. one which would discuss the pros and cons of this idea, and compare it seriously with other interpretations of QM.)

Referee 2 wrote this:

> The authors illustrate their main points with examples and a large number of (text) citations. They do this in such a strong manner that I felt they almost try to inculcate the reader with their viewpoint. In fact, the paper naturally touches on various philosophical aspects (such as the interpretation of quantum states), which are partly lacking a consensus in the scientific community. While the authors, of course, have a right to expose and justify their points of view (and, actually, do this in a very convincing way), I am not sure the style is appropriate for RMP.

With regard to 1, I thought, "Well yeah it's proselytizing, of course. Why should I bother to 'compare it seriously with other interpretations' when the whole point of all my papers is to show that a QBist-style interpretation—even without all the details hammered out—is simply natural, obvious, and should hit the reader over the head."

With regard to 2, I thought, "To justify one's points 'in a very convincing way' is inappropriate for RMP? What would you really want instead?"

Anyway, I expand on these initial thoughts, by quote my old friend Willy James from his lecture "The Types of Philosophic Thinking":

> What distinguishes a philosopher's truth is that it is reasoned. Argument, not supposition, must have put it in his possession. Common men find themselves inheriting their beliefs, they know not how. They jump into them with both feet, and stand there. Philosophers must do more; they must first get reason's license for them; and to the professional philosophic mind the operation of procuring the license is usually a thing of much more pith and moment than any particular beliefs to which the license may give the rights of access. Suppose, for example, that a philosopher believes in what is called free-will. That a common man alongside of him should also share that belief, possessing it by a sort of inborn intuition, does not endear the man to the philosopher at all-he may even be ashamed to be associated with such a man. What interests the philosopher is the particular premises on which the free-will he believes in is established, the sense in which it is taken, the objections it eludes, the difficulties it takes account of, in short the whole form and temper and manner and technical apparatus that goes with the belief in question. A philosopher across the way who should use the same technical apparatus, making the same distinctions, etc., but drawing opposite conclusions and denying free-will entirely, would fascinate the first philosopher far more than would the

> *naïf* co-believer. Their common technical interests would unite them more than their opposite conclusions separate them. Each would feel an essential consanguinity in the other, would think of him, write *at* him, care for his good opinion. The simple-minded believer in free-will would be disregarded by either. Neither as ally nor as opponent would his vote be counted.
>
> In a measure this is doubtless as it should be, but like all professionalism it can go to abusive extremes. The end is after all more than the way, in most things human, and forms and methods may easily frustrate their own purpose. The abuse of technicality is seen in the infrequency with which, in philosophical literature, metaphysical questions are discussed directly and on their own merits. Almost always they are handled as if through a heavy woolen curtain, the veil of previous philosophers' opinions. Alternatives are wrapped in proper names, as if it were indecent for a truth to go naked. The late Professor John Grote of Cambridge has some good remarks about this. 'Thought,' he says, 'is not a professional matter, not something for so-called philosophers only or for professed thinkers. The best philosopher is the man who can think most *simply*. ... I wish that people would consider that thought-and philosophy is no more than good and methodical thought-is a matter *intimate* to them, a portion of their real selves ... that they would *value* what they think, and be interested in it.... In my own opinion,' he goes on, 'there is something depressing in this weight of learning, with nothing that can come into one's mind but one is told, Oh, that is the opinion of such and such a person long ago. ... I can conceive of nothing more noxious for students than to get into the habit of saying to themselves about their ordinary philosophic thought, Oh, somebody must have thought it all before.' Yet this is the habit most encouraged at our seats of learning. You must tie your opinion to Aristotle's or Spinoza's; you must define it by its distance from Kant's; you must refute your rival's view by identifying it with Protagoras's. Thus does all spontaneity of thought, all freshness of conception, get destroyed. Everything you touch is shopworn. The over-technicality and consequent dreariness of the younger disciples at our American universities is appalling. It comes from too much following of German models and manners. Let me fervently express the hope that in this country you will hark back to the more humane English tradition. American students have to regain direct relations with our subject by painful individual effort in later life. Some of us have done so. Some of the younger ones, I fear, never will, so strong are the professional shop-habits already.
>
> In a subject like philosophy it is really fatal to lose connexion with the open air of human nature, and to think in terms of shop-tradition only. In Germany the forms are so professionalized that anybody who has gained a teaching chair and written a book, however distorted and eccentric, has the legal right to figure forever in the history of the subject like a fly in amber. All later comers have the duty of quoting him and measuring their opinions with his opinion. Such are the rules of the professorial game-they think and write from each other and for each other and at each other exclusively. With this exclusion of the open air all true perspective gets lost, extremes and oddities count as much as sanities, and command the same attention; and if by chance any one writes popularly and about results only, with his mind directly focussed on the subject, it is reckoned *oberflächliches zeug* and *ganz unwissenschaftlich*.

Rereading that passage, I think it gets to the point.

13-02-09 *It Wasn't Real Complex* (to R. W. Spekkens, L. Hardy, and R. Blume-Kohout)

Here's what I should have said yesterday. See attached. It's not hard to prove after all that there is no minimal informationally complete POVM for real Hilbert spaces that will give me my infamous diagram.

Suppose I want a map $\Phi : \mathcal{S}(\mathbb{R}^d) \longrightarrow \mathcal{S}(\mathbb{R}^d)$ of this form

$$\Phi(X) = \frac{1}{n}\sum_i \Pi_i X \Pi_i \;, \tag{89}$$

where $\mathcal{S}(\mathbb{R}^d)$ denotes the $\frac{1}{2}d(d+1)$-dimensional space of symmetric operators, the $\Pi_i = |\psi_i\rangle\langle\psi_i|$ are rank-1 projectors, and the set operators $E_i = \frac{1}{n}\Pi_i$ form a minimal informationally complete POVM. Then we must have

$$\frac{1}{n}\sum_{i=1}^{\frac{1}{2}d(d+1)} \Pi_i = I \;, \tag{90}$$

and consequently

$$n = \frac{2}{d+1} \;. \tag{91}$$

Furthermore suppose Φ has this characteristic

$$\Phi(\rho) = \alpha I + \beta\rho \tag{92}$$

for all density operators ρ. To be trace preserving,

$$\alpha d + \beta = 1 \;. \tag{93}$$

These are the properties needed to be able to draw the diagram I always draw and have the Born Rule interpretable as a simple modification of the law of total probability.

The question now is, are there any operators Π_i that can do this for me? Here's a way to see that there generally aren't. Let Φ act on one of the Π_k:

$$\frac{1}{n}\sum_i \Pi_i \Pi_k \Pi_i = \alpha I + \beta \Pi_k \;. \tag{94}$$

Since $\Pi_i\Pi_k\Pi_i = \mathrm{tr}(\Pi_i\Pi_k)\Pi_i$, it follows that we must have

$$(1-\alpha-n\beta)\Pi_k + \sum_{i\neq k}\Big(\mathrm{tr}(\Pi_i\Pi_k) - \alpha\Big)\Pi_i = 0 \;. \tag{95}$$

But the Π_i are linearly independent. Consequently, it is necessary that

$$\alpha + n\beta = 1 \tag{96}$$

and

$$\mathrm{tr}(\Pi_i\Pi_k) = \alpha \qquad \forall i \neq k \;. \tag{97}$$

That is, the E_i must form a SIC-POVM.

For real vector spaces, however, SIC-POVMs generally do not exist.

16-02-09 *The Historical Roots of Our SICs* (to D. M. Appleby and H. C. von Baeyer)

It's entirely appropriate that I came across this passage in the Atmanspacher and Primas article tonight:

> As a consequence of the non-conservation of parity, Pauli discovered a group of transformations relating left- and right-handed neutrinos and antineutrinos—the so-called Pauli group (Pauli 1957). In 1957, he started an initially enthusiastic collaboration with Heisenberg on his nonlinear spinor equation which should provide a unified description of all elementary particles. One reason for Pauli's excitement about this project was that this spinor equation was invariant under the Pauli group.

Pauli 1957: "On the conservation of the lepton charge," Nuovo Cimento **6**, 204–215.

17-02-09 *Historical Question* (to D. Gottesman)

I've got Marcus Appleby and Hans Christian von Baeyer here this week working on identifying the various notions at work in the phrase "detached observer" in the correspondence between Pauli, Fierz, and Bohr, and of course Marcus is always working on SICs. So I was delighted last night when I came across the funny little tidbit below, particularly upon learning that Pauli's interest in his failed "world theory" with Heisenberg centered on its invariance under the group. [See 16-02-09 note "The Historical Roots of Our SICs" to D. M. Appleby and H. C. von Baeyer.]

Anyway, completing the tale in my mind, leads in part to the following historical question. Who invented the phrase "generalized Pauli group"? Was it you? And its first use of the group in quantum information was you? Or Manny Knill? Or still someone else. Presumably it's also called Weyl-Heisenberg group because Weyl and Heisenberg used it. But I wonder if Heisenberg was post-Pauli in this case. (I think Weyl goes back to some discussion in his book on group theory and QM.)

For fun, it'd be nice to compile these facts.

17-02-09 *Dedication* (to A. Y. Khrennikov)

Thanks for all the patience with me in sending in the conference proceedings. In trying to make this one an important paper, I lost sight of my deadlines! So, I turned in what I could for the conference proceedings, and then decided to keep plugging away at the paper making a more complete version of it before posting on the archive. I'm still not finished!! But I did just write a little dedication to you on the front page on the still-incomplete draft. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] See footnote 1 in the attached:

> This is a much expanded version of a paper originally appearing as C. A. Fuchs and R. Schack, "From Quantum Interference to Bayesian Coherence and Back Round Again," in *Foundations of Probability and Physics – 5, Växjö, Sweden, 24–27 August 2008*, edited by L. Accardi, G. Adenier, C. A. Fuchs, G. Jaeger, A. Khrennikov, J. Å. Larsson, and S. Stenholm, AIP Conference Proceedings Vol. 1101, (American Institute of Physics, Melville, NY, 2009), pp. 260–279. Per always, we thank Andrei Khrennikov for organizing this wonderful series of meetings, which has been so instrumental in crystalizing our quantum-Bayesian thoughts.

It's heartfelt. These meetings have been very important for my thought. Hopefully I'll get the paper finalized and posted next week.

18-02-09 *Pop Culture* (to D. M. Appleby and H. C. von Baeyer)

"Reality is at the deepest level the response to the observer."

Just heard that on the CBC show "Being Erica" (a story about a woman whose psychologist keeps sending her back in time to revisit her past) in a small discussion about quantum mechanics.

18-02-09 *Saint of the Retort* (to H. C. von Baeyer)

Thanks for this!

Hans's Preply, "Augmented Index Entry," 18-02-09

I have found an important reference to the Detached Observer in Pauli's Kepler Study. Hence the new last item below:
Here is an entry for the (non-existent) subject index of *Wolfgang Pauli: Writings on Physics and Philosophy*, Enz and Meyenn, ed. (Springer 1994).

Detached Observer: pp. 23, 24, 33, 40, 43, 47, 122, 132, 134, 152, 260
Compiled by HvB

PS In 1953 Pauli wrote to Panofsky that he is trying to introduce the term "detached observer" (in English) to physics. Maybe we should give him a posthumous hand.

18-02-09 *Talk* (to A. Wilce)

Wilce-ism 7: *I suppose it's not too early to be thinking about what talk to give. Let me give you some choices, and ask you to tell me what's of most interest:*

(1) I could talk about my paper on formalism and interpretation – basically the same talk I gave in Jerusalem (though I think perhaps Jon has given a similar talk in the last year or so);

(2) I could talk about an axiomatization of QM by way of the characterization of homogeneous self-dual cones (the notes I sent you before Christmas);

(3) I could give a slightly updated version of the talk I gave in Obergurgl (adding some remarks making contact with Lucien's axioms).

Let me know what sounds good.

(1)!! (If it's the one to do with Everett potentially being contentless.)

Alex's Reply

Below are two different abstracts for more-or-less the same talk, emphasizing different aspects thereof. Let me know which one you like better, and I'll send it along to Karen.

Title:
Entanglement and measurement in general probabilistic theories.

Abstract:
Quantum mechanics is a non-classical probability theory, but hardly the most general one imaginable: any compact convex set can serve as the state space for an abstract probabilistic model (classical models corresponding to simplices). From this altitude, one sees that many phenomena commonly regarded as "characteristically quantum" are in fact generically "non-classical". In this talk, I'll show that almost any non-classical probabilistic theory shares with quantum mechanics a notion of entanglement and, with this, a version of the so-called measurement problem. I'll then discuss what's required for an abstract probabilistic theory to admit a somewhat simplified version of Everett's response to this problem – an exercise that turns out to be instructive in several ways.

Title:
Measurement dynamics in general probabilistic theories.

Abstract:
One can view quantum mechanics as a dynamical theory with a familiar mathematical apparatus, but a mysterious probabilistic interpretation, seemingly incompatible with the dynamics. Alternatively, one can view QM as a probabilistic theory with a more or less standard interpretation, but a mysterious formal apparatus, badly under-determined by that interpretation. In this talk, I'll try to take both of these complementary points of view at once, by asking what's required for a general (non-classical) probabilistic theory to support a minimally reasonable measurement dynamics – an exercise that turns out to be instructive in several ways.

19-02-09 *Transcript from "Being Erica"* (to D. M. Appleby and H. C. von Baeyer)

A more accurate transcript from last night's episode . . .

Erica: Here I am trying to relive the perfect day and I can't. Because every time something changes, even if it's a little bit, I freak out.

Dr. Tom: Yeah. (Picking up a leaf from the ground.) Quantum mechanics. You see, we can never know with any kind of certainty how an atom will behave naturally. (Pointing to the leaf.) Because the very instant that we look at an atom we alter it. The very act of looking is never a passive thing. It has an effect. In fact physicists tell us that reality at its deepest level is the response of the observer. You know, it's kind of like you reliving past events.

20-02-09 *My Own Index* (to H. C. von Baeyer)

Uses of the word "detached" in "The Activating Observer"

14, Atmanspacher and Primas
48, Bohr

49, Bohr
119, Enz
122, Enz
138, Feyerabend
149, Folse
153, Folse
159, Folse
163, Folse
165, Folse
167, Folse
169, Folse
170, Folse
190, Fuchs and Peres
309, von Meyenn
338, Pauli
340, Pauli
345, Pauli
346, Pauli
353, Pauli to Jung, 27 May 1953
408, Rosenfeld
455, von Weizsäcker

21-02-09 *PhD Acquired* (to G. Plunk)

Plunkism 7: *I just defended my thesis successfully at UCLA and wanted to announce this to the people who were especially influential along the way. I just wanted to let you know that the path of research that you launched me on in the summer of 2002 has concluded well! I have taken a post-doctoral job at the University of Maryland and expect that I will stay in theoretical physics for the long haul.*

That's excellent news! Congratulations. I hope you like College Park. I'll be there next Friday and Saturday for an APS meeting.

Funny coincidence you would write me now. Just the other day, I started working on a section of the paper (that I'm presently constructing) that makes use of one of your results from our summer together. See Section 5.3, page 23, of the present draft attached. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] Too bad the paper's not completed yet, so that I could have replied with an even more forceful "funny coincidence"!

Take care.

22-02-09 *James, Gustafson, and the Urungleichung* (to D. M. Appleby)

Applebyism 28: *I have been reading Gustafson, and the following passage struck me. I am not sure if it bears on this afternoon's discussion, but I feel it might do. Anyway, I thought it worth recording the point before I forgot it.*

On p. 58 Gustafson says this:

Walter Moore, in describing Schrödinger's renunciation of Mach's philosophy, could also have been describing Pauli's:

> There are a number of fairly obvious defects in presentational phenomenalism [positivism]. For instance, it fails to explain the close relationship between mathematical reasoning and theoretical physics; mathematical operations and symbols do not denote empirical sensations, and yet one cannot do science without them. Also, experiments are planned interactions of the scientist with the environment; how can they be explained as mere collections of sensations? Mach fails to explain the enormous predictive power of physical theories; how can it be that [Paul A. M.] Dirac predicts a positive electron and [Carl David] Anderson finds it in a cloud chamber?

Pauli would become fascinated with the deep meaning of mathematical symbols. His "mind's eye" was opened through his explorations of complex numbers, Maxwell's equations, and special-relativity transformations.

I think this is a fair description of the way Pauli felt—i.e. a fair description of the way in which Pauli was definitely not an empiricist (a Platonist even, I think I have seen it said). But be that as it may. The reason I am citing it is not that, but its bearing on what you said about James's belief that relations are part of experience. Could we say that the word "relations", as used by James, includes mathematical relations?

I think James would include mathematical relations as well, insofar or to the extent they are part of experience.

22-02-09 *The Shape of Hilbert Space* (to G. L. Comer)

Comerism 22: *One question: should I prepare for terabytes of musings on a Feynmanian Idea?*

Nope. Just Jamesian ones. I had a new epiphany at the beginning of the year, and I've been gearing up to write a lot again. Wheeler's "elementary quantum phenomenon" IS a modern, precisified version of James's "pure experience"—I get it now after all these years. So I REALLY MUST get *My Struggles with the Block Universe* on the archive this Spring and then start a new chapter. When the real conclusion of the present paper is written (I'm scrapping the old conclusion), it'll allude to some of that.

24-02-09 *Good Thinking* (to R. Schack)

Do you have a complete reference on I. J. Good's article on the number of kinds of Bayesian? It is driving me crazy that I have not been able to find it on the web.

24-02-09 *HCvB and CAF* (to H. C. von Baeyer)

I would be honored to write something with you. I agree with your assessment, only too bad we didn't have it ready to go by mid-December '08! If it remains OK with you, I would expect my participation to be somewhere neutrally (appropriately) between "by me with your help" and "jointly". I.e., you could take the lead, and I would try to be constructive from the side at first, but becoming more involved as more structure starts to arise. Furthermore, as a point of personal policy for me this time around, you would not find me insisting on idiosyncratic terms like "imprimatur," etc! I will be very happy to see something of good substance get out into the airwaves, and I can't really see how to do that without your being at the helm.

Perhaps the only sensitive issue I foresee at the moment is the desire to "tie cautiously into the science/religion debate." You can probably already sense I would want to be very cautious with that. But I also feel that something should be said about the humanistic side of physics—so there is definitely room for negotiation here.

I feel like I will learn a lot with this project. Certainly I learned very, very much from my effort with Asher on the *Physics Today* piece. Somehow in my mind, this will be a bit like that: A great opportunity to crystallize thought.

I hope your cold goes away soon. Marcus and I spent yesterday x'ing and testing things on the computer. We discovered that apparently the conditions arising from the urungleichung also imply $0 \le \mathrm{tr}\rho^3 \le 1$. (We haven't shown it yet; it's only numerical at this stage.) This inequality is still not full quantum state space, but it is a little closer. I.e., another piece of evidence that if we keep squeezing on the urungleichung, eventually the full condition on quantum state space will indeed pop out.

Hans's Preply, "The Detached Observer," 24-02-09

> I am working on the essay for home use that I began at PI. In addition, though, I want to reply to your remark that I should consider publishing something.
>
> An article about the DO at the level of AJP sounds very appropriate. It would be a fine tribute to Pauli 50 years after his death, it would be good PR for Bayesians, and it would tie cautiously into the science/religion debate. It would talk about the classical DO, about Pauli's quantum mechanical NDO, about your effort to define and quantify the NDO in the language of information, and finally about Pauli's unfulfilled dreams of pushing the NDO toward a more neutral language.
>
> Such an article could be written by you, by me with your help, or jointly. Since you are the senior partner in this effort (my grey locks notwithstanding) I would like to ask you to choose among these three options. I am totally happy with each one!
>
> Unfortunately I brought home from Waterloo not Marcus's cold, but the Canadian arctic cold!

Hans's Pre-Preply, "Detached Observer," 20-02-09

> Here is the losgelöste Beobachter.
>
> **Pauli's Detached Observer**
>
> **Bibliography**
>
> [A] Karl von Meyenn *et al.* (eds.) *Wolfgang Pauli: Wissenschaftlicher Briefwechsel mit Bohr, Einstein, Heisenberg u.a.* (Springer 1979–2005).
>
> [B] Wolfgang Pauli *Writings on Physics and Philosophy*. (Springer 1994).
>
> [C] Suzanne Gieser *The Innermost Kernel*. (Springer 2005).
>
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Nomenclature

According to von Meyenn, the phrase *detached observer* (DO) first appeared in February 1949, in a lecture on complementarity in Zurich.[175] The first mention in the letters seems to be in January 1951.[176] The German version is *losgelšter Beobachter*, which Pauli explicitly translates as *detached observer*.[177] (He also used *abgekapselter Beobachter*,[178] meaning an *encapsulated observer*, and once he wrote the English phrase *loose and untied observer*[179].) Bohr, in German, used *aussenstehender Beobachter*,[180] or *observer standing outside*. In French Pauli liked *observateur détaché*.[181] In the English translation of the essays there are 11 references to DO,[182] in the letters many more. In 1953 Pauli remarks that he has tried several times to introduce the DO into physics.[183] In 1956 he notes that following his dispute with Bohr about the use of the phrase DO neither he nor Bohr ever used it again in print.[184]

It would be useful to find a compelling antonym for DO. Pauli used *non-detached observer*[185] once, and *miteinbezogener Beobachter*[186] several times. The latter adjective can be translated as *involved* or *included (in the description of the phenomenon)*; the term *bezogen* means *related*. Fuchs uses *activating*. Other suggestions include *attached, active, bound, participating, linked, joined, entangled, enmeshed*. In this essay we stick to *non-detached observer* (NDO).

The only antonym I have found is *non-detached observer*[187] (NDO).

Commentaries

Folse[188] comments on the DO, but von Meyenn does not believe that Folse has fully understood Pauli.[189] Gieser devotes five pages to the DO.[190] Enz comments on the DO in his biographical sketch.[191] The editorial apparatus of the letters includes numerous footnotes about the DO.[192]

[175] [A] vol. IV part II, p. 149. The essay is translated into English in [B] where DO occurs on page 47.
[176] [A] vol. IV part I p. 247.
[177] [A] vol. IV part II p. 237.
[178] [A] vol. IV part III p. 665.
[179] [A] vol. IV part I p. 436
[180] [A] vol. IV part III p. 106.
[181] Cf. footnote 177.
[182] [B] pp. 23, 24, 33, 40, 43, 47, 122, 132, 134, 152, 260.
[183] Cf. footnote 177.
[184] [A] vol. IV part III p. 784.
[185] [A] vol. IV part III p. 147.
[186] [A] vol. IV pp. 697, 738, 824.
[187] [A] vol. IV part III p. 147.
[188] Henry J. Folse *The Philosophy of Niels Bohr* (Amsterdam 1985) p. 214.
[189] [A] vol. IV part III p. 107.
[190] [C] p. 131.
[191] [B] p. 22 ff.
[192] [A] vol. IV part I p. 343; vol. IV part II p. 149; vol. IV part III p. 107 etc.

The classical detached observer

Pauli defined the DO in virtually identical words in his 1949 lecture on Complementarity and his 1952 essay on Kepler,[193] which was published in a book co-authored by Jung: "… [T]here is a basic difference between the observers, or instruments of observation, which must be taken into consideration by modern microphysics, and the detached observer of classical physics. By the latter I mean one who is not necessarily without effect on the system observed but whose influence can always be eliminated by determinable corrections."

The DO serves to characterize classical physics. Einstein claimed: "There is such a thing a the real state of a physical system, which exists objectively, independently of any observation or measurement, and can in principle be described by the modes of expression used in physics."[194] Pauli calls this point of view the *ideal of the detached observer* and points out that it is only one special form of physics. Indeed, "this ideal is now revealed as a special case of more general possibilities of explaining nature."[195]

Heisenberg calls "the idea of an objectively real world, whose smallest parts exist objectively in the same way as stones and tree, independently of whether we observe them or not," the *ontology of materialism*. Pauli prefers to call the same idea the *ideal of the detached observer*, pointing out that the fundamental elements of the real world might be, for example, fields, which don't resemble stones and trees and are not "small."[196]

Fierz uses the DO in his definition of absolute space: "… I believe that absolute space is appropriate for classical physics. The *classical objective real world*, which I like to call *the absolute world* because it is detached from the observer, is located in absolute space. This space is absolute because it is independent of the physical reality that fills it."[197]

The non-detached observer in quantum mechanics and the dispute with Bohr

I will examine the NDO with particular reference to two sources — the essays in which it was first introduced,[198] and the final epistolatory exchange with Bohr.[199] In the case of the Kepler essay, Pauli's tireless exchanges with Panofsky concerning the translation into English bear witness to the pains he took to explain himself precisely. Later, in the Bohr letters, he writes explicitly: "I shall try to make my point logically clear, by defining my concepts, replacing hereby the disputed phrase by other words." Other references in the letters and essays serve as glosses on these two primary sources. An advantage for anglophones is that the essays have been translated, and the Bohr letters were written English.

In passing from the classical DO to the quantum mechanical NDO a complication arises. The context of the DO has two parts depicted as (*system* | *observer*). The context of the NDO, on the other hand, is tripartite: (*system* | *instruments* | *observer*).

[193] [B] p. 40 and p. 260. The latter version is more grammatical.
[194] [B] p. 47.
[195] [A] vol. IV part II p. 149.
[196] [A] vol. IV part III, p. 121.
[197] [A] vol. IV part I p. 383.
[198] Cf. footnote 193
[199] [A] letters [2015] and [2041] from Pauli, [2035] and [2047] from Bohr.

Later in this essay, the first of the two vertical lines will be referred to as the "Pauli cut" and the second one as the "Bohr cut."

The first definition of the NDO read: "In microphysics ... every observation is an interference of indeterminable extent, both with the instruments of observation and with the observed system, and interrupts the causal connection between phenomena preceding and subsequent to it ... In this sense we may say that irrationality presents itself to the modern physicist in the shape of selecting (auswählende) observation." A footnote refers to the technical elaboration of this process in the "reduction of wave packets."

The second definition adds, implicitly, the element of *information*: "In microphysics ... the natural laws are of such a kind that every bit of knowledge gained from a measurement must be paid for by the loss of other, complementary items of knowledge. Every observation, therefore, interferes on an indeterminable scale both with the instruments of observation and with system observed and interrupts the causal connection of the phenomena preceding it with those following it."

24-02-09 *First Consequences of Our Conversation* (to H. C. von Baeyer and D. M. Appleby)

Since our conversations last week, I have not been able to shake the nasty feeling of the complete sterility of what Huw Price, Harvey Brown, Rob Spekkens, Daniel Dennett, and others in that lot think of when they say the word "reality." Marcus's displays of disgust have been quite contagious to me! So I couldn't quite contain myself today as I was improving and polishing my Introduction for the "Coherence" paper. You'll see what I mean if you look at the last paragraph of Section 1, attached. Now I have to hope that the concluding section (not written yet) can live up to the expectations I plant there!

> Finally with Section 7 we close the paper by discussing how our work is still far from done: Hilbert space, from a quantum-Bayesian view, has not yet been derived, only indicated. Nonetheless the progress made here gives us hope that we are inching our way forward to a formal expression of the ontology underlying a quantum-Bayesian vision of quantum mechanics: It really does have to do with the Peres slogan, but tempered with a kind of 'realism' that Peres would probably not have accepted forthrightly. On the other hand, it is not a 'realism' we expect to be immediately accepted by most modern philosophers of science either. (See Footnote 2 and Ref. [50, Sec. 4.1] for a sampling of relevant instances of opposition, and Refs. [63, 64, 65] for the vision more generally.) Part of what must go is the ontology of the block universe [66, 67, 68], as well as the ontology of the 'detached observer' [69, 70]. The 'realism' of the standard vogue is too narrow a concept to be used for our purposes. Reality, the stuff of which the world is made, the stuff that was here before agents and observers, the stuff that prevents us from vanishing in a dream—it strikes us—is more interesting than that.

24-02-09 *Good Thinking* (to W. C. Myrvold)

Do you have a complete reference on I. J. Good's article on the number of kinds of Bayesianism? It is driving me crazy that I have not been able to find it on the web.

Wayne's Reply

Attached:

i) Original publication, as a letter to the editor of *The American Statistician*.

ii) Scan of reprint in *Good Thinking*, which (I think) I got from Branden Fitelson's web site, together with an article in which the categories are explained more fully.

25-02-09 *Undetachedly Denigrating Chance* (to H. C. von Baeyer)

I think the 13 October 1951 letter from Pauli to Fierz is one we'll want to get straight translation-wise. I was apprised of it from Laurikainen's *Beyond the Atom*, pp. 196–197.

Compare Pauli to the three paragraphs below Eq. (1) in the CFS paper `quant-ph/0608190`:

> Within the context of experimental situations with large sample sizes, where Bayesian updating leads to similar posteriors for exchangeable priors, the geometric analogy, combined with the Principal Principle to connect chance with probability, would appear to work quite well. This gives rise to the idea that the Principal Principle accounts for the concept of objective chance in physics. However, from a Bayesian perspective, the introduction of chance is completely unmotivated. More urgently, in those cases where the idea is not already fraught with obvious difficulties, it serves no role that Bayesian probability itself cannot handle.
>
> To illustrate one such difficulty, return to the coin-tossing example discussed above, and assume that there is an objective chance q that a coin-tossing event will produce Heads. As we have seen in the discussion above, the chance cannot be deduced from physical properties of the coin alone, because the probability of Heads also depends on initial conditions and perhaps other factors. An advocate of objective chance is forced to say that the chance is a property of the entire "chance situation," including the initial conditions and any other relevant factors. Yet a sufficiently precise specification of these factors would determine the outcome, leaving no chance at all. The circumstances of successive tosses must be different to give rise to chance, but if chance aspires to objectivity, the circumstances must also be the same. Different, but the same—there is no way out of this conundrum as long as objective and chance are forced to co-exist in a single phrase. Subjective probabilities easily dispense with this conundrum by maintaining the category distinction. The differences between successive trials are differences in the objective facts of the initial conditions; the sameness is an agent's judgment that he cannot discern the differences in initial conditions and thus assigns the same probability to every trial.
>
> But what of probabilities in quantum mechanics? Given the last paragraph, one might well think—and many have thought—there is something different going on in the quantum case. For, in repeating a preparation of a pure state $|\psi\rangle$, aren't all the conditions of preparation the same by definition? Any subsequent probabilities for measurement outcomes will then be determined by applying the Born rule to $|\psi\rangle$. They are not subjective probabilities that come about by an inability to take all circumstances into account. Thus quantum states (and hence quantum "chances") are objective after all, and the Principal Principle is just the kind of thing needed to connect these quantum chances to an agent's subjective probabilities—or so a very beguiling account might run.

(At that stage in his thinking at least) Pauli seems to get round our "different, but same" conundrum by saying the difference is the observer: He ain't detached. No two "identical" experiments are the same, because no two observers are the same. Another way to say it is, there is an extra index around, and that index is the observer himself.

Hans's Reply, "The Same and Not the Same," 26-02-09

I don't have Laurikainen's book. Here's the context. I will of course be happy to refine, expand, and revise as needed.

The same and not the same

[Square brackets enclose my comments.]

From the letter of Pauli to Fierz, dated 13 October 1951, number 1289 of Pauli's Scientific Correspondence:

In part 1 of the letter Pauli, in an irritated tone, corrects Fierz's alleged misunderstandings of general relativity.

In part 2 he refers back to the natural philosophy of the Italian Renaissance. (cf. Atmanspacher, Primas, & Wertenschlag 1995 p. 242 ff). Then:

"The *anima mundi* [world soul], which was also an *anima movens* [motion causing soul] belonged necessarily to the Neoplatonism of the Renaissance (cf. Ficino). Every planet had its individual soul, but how did these relate to each other: spiritually, via the *anima mundi*, of which they are a part. (N.B. I see Mr. Fludd immediately wrinkling his brow at the mention of "part' — so let's say, for his sake: via the *anima mundi*, with which the individual souls are identical, insofar as they belong to the light principle.)

But in the 17th century the *anima mundi* went out of fashion — the idea *paled*. (I would happily learn the attitude of your epigone Henry More and his circle toward this idea — what kind of a Neoplatonism is that, anyhow, *without* the anima mundi?) And exactly through the resulting gap, proportion, geometry, mathematics pushed into the ideas about motion, and pushed toward empiricism, toward measurement. One sees this process clearly not only in Kepler, but also in Galileo. The latter *rejected* not only the Aristotelian-Peripatetic tradition, but also *Neoplatonism*, including the anima mundi, and referred back the *Pythagoreans and to Plato himself.* ("The more ancient is always the new!")

But with this progress (analytic geometry, Newtonian mechanics), space moved up to the Olympus of the Absolute and the relationship between soul and matter became a special problem which vanished in the twilight of "parallelism", just as Venus vanishes in the morning twilight.

But now we seem to be beginning to suffer from the fact that one went too far in the 17th century (cf. my Kepler study) and from then come "revenants" that haunt me during the night, and occasionally also during the day — the way Venus returns as evening star. [Pauli's footnote: I believe that *every* addition to consciousness proceeds in such a way that something which was previously conscious disappears into the unconscious, and returns much later. That's what I want to express with the image of Venus, and also with the maxim: "The more ancient is always the new!"] When something becomes invisible, it still endures and remains effective. General relativity has brought back, in its space-time rippled by matter, the idea attributed to the Peripatetics, of the physical quality of space points (places) — in the transmuted form of the g_{ik} field (even if g.r. couldn't bring back the entire *horror vacui*!)"

Part 3 "Now comes the great crisis of the quantum of action: one has to sacrifice the unique case [or event] and its "meaning", in order to save an objective and rational description of the phenomena. When two observers do the same thing it is really even physically no longer the same thing; in general only the *statistical averages* remain the same. *The physically unique can no longer be detached from the observer* — and therefore slips through the net of physics. The unique case is *occasio* [opportunity, the fleeting moment] and not *causa* [cause, of which there are 32 varieties in philosophy]. I am inclined to see in "occasio" — which includes the observer and his choice of the experimental arrangement — a "revenant" (of course in "transformed form") of the *anima mundi* that was repressed in the 17th century. La donna é mobile [the lady is fickle] — including the *anima mundi* and the *occasio*.

Something has remained open here, which formerly seemed closed, and my hope is that through this gap *new concepts* will penetrate in place of "parallelism", concepts which should be uniformly physical and psychological simultaneously. May a "happier progeny" achieve this."

Part 3 then concludes by saying that the concept of *archetype* does not have the right attributes for this purpose, because it cannot be applied naturally to atomic physics. Maybe *automorphism* will work.

Part 4 praises Fierz's proposal to divide every quaternity into two pairs of opposites: one compensatory labeled + and −, and one complementary labeled p and q. This scheme yields four elements $+p$, $-p$, $+q$ and $-q$.

25-02-09 *Image of Occasio* (to H. C. von Baeyer)

See

`http://web.archive.org/web/20110704222210/http://www.iisg.nl/occasio/occasio-image.php`,

where it is written:

> The image of Occasio is taken from a work by famous Czech philosopher and pedagogue Johan Amos Comenius (Moravia 1592 – Amsterdam 1670), the *Orbis sensualium pictus* (1658). The book contains pictures with text in Latin and the vernacular, and was intended for teaching. Occasio is the Opportunity we must seize before it flies away and vanishes: "*Occasioni (quae, fronte capillata sed vertice calva, ad hoc alata facile elabitur) attendit captatque eam*", or, "She watches Opportunity (which, having a forelock but being bald at the back of its head and being winged, escapes easily) and seizes it." "She" is Prudentia, looking at the past with one of her two faces and at the future with the other. Occasio was chosen by Tjebbe van Tijen as an emblem appropriate for a project aiming to archive such volatile materials as Internet documents. An other image of Occasio can be found at Alciato Emblem 122 (Latin) from the 1621 edition of Andrea Alciato's *Book of Emblems*. See also Prudentia, from *Comenius Orbis Sensualium Pictus* at the Universidad Nacional de Educación a Distancia.

26-02-09 *Undetachedly Denigrating Chance, 2* (to H. C. von Baeyer)

Thanks. Here was Laurikainen's translation of the part of "The same and not the same":

> Now there comes the major crisis of the quantum of action: one has to sacrifice the unique individual and the "sense" of it in order to save an objective and rational description of the phenomena. If two observers do the same thing even physically it is, indeed, really no longer the same: only the *statistical averages* remain, in general, the same. *The physically unique individual is no longer separable from the observer*—and for this reason it goes through the meshes of the net of physics. The individual case is *occasio* and not *causa*. I am inclined to see in this *occasio* which includes within itself the observer and the selection of the experimental procedure which he has hit upon—a revenue of the *anima mundi* which was pushed aside in the seventeenth century (naturally "in an altered form"). La donna è mobile—so are the *anima mundi* and the *occasio*.

Your word revenant is certainly better than his revenue.

I've been Pauli'ing most of the day when not talking to drivers. I'm finding I really enjoy Gieser's book, though I'm only 43 pages into it.

Hans's Reply

> "Unique individual" sounds like a person, which is wrong. The original is "das Einmalige." This word has a sense of being unique in space and time — "something that occurs only once". In relativity it is called an event, and since Pauli has just mentioned GR this might be an appropriate word.

27-02-09 *Vivienne and the Universe* (to L. Hardy and V. Hardy)

How tickled I was this morning to awake in this lonely Washington hotel to find an announcement of Vivienne's birth. And what a name! One that means being alive itself. My thoughts of course wandered to my favorite vision for an ontology of this world we live in—that the big bang is here and now, everywhere, being the sum total of all acts of creation. I hope that one day Vivienne will come to understand how deep her role goes in the vast order of things.

Please let me offer her this little passage from William James to commemorate the occasion:

> Lotze has in several places made a deep suggestion. We naively assume, he says, a relation between reality and our minds which may be just the opposite of the true one. Reality, we naturally think, stands ready-made and complete, and our intellects supervene with the one simple duty of describing it as it is already. But may not our descriptions, Lotze asks, be themselves important additions to reality? And may not previous reality itself be there, far less for the purpose of reappearing unaltered in our knowledge, than for the very purpose of stimulating our minds to such additions as shall enhance the universe's total value. 'Die Erhoehung des vorgefundenen daseins' ['the enhancement of what is found to exist.'] is a phrase used by Professor Eucken somewhere, which reminds one of this suggestion by the great Lotze.

Congratulations to both of you, and congratulations especially to Vivienne!

02-03-09 *Getting Facts Straight* (to D. C. Cassidy)

It was good meeting you Saturday. I was grateful for the accident that brought me to your table! I meant what I said about your book: I loved it. I read it about the same time as Moore's biography of Schrödinger, and there was no comparison.

I'd like to make sure I get the facts straight about von Meyenn. Would you mind saying everything again, but this time in email. A) Is his biography of Pauli actually finished, or is it something he is writing on? More importantly, B) please repeat what you said about the delay in the Briefwechsel project. Is it that Springer lost patience with him? Or simply that they're not foreseeing a sufficient $$ return on the project to go another volume? It frightens me immensely that this project might slip into oblivion: I have a vested interest in this side of Pauli (for various lines of research it might inspire in quantum foundations), and I would like to get oriented on how I might help.

In case you're interested, you might peruse my book *Notes on a Paulian Idea* to see why I believe these more private matters of Pauli can have an impact on the technical side of physics. It's the third item down on my webpage. (It was previously published by a small university press; but I just learned this weekend that Cambridge U. Press will be reissuing it soon with the title *Coming of Age with Quantum Information: Notes on a Paulian Idea*.)

Thanks for your help.

03-03-09 *Dates* (to R. Schack)

Schackcosm 111 : *Very provisionally, would this work: after Växjö, you come to Egham, we drive to Hay-on-Wye, then we both fly to Perimeter, where I stay for two weeks.*

That arrangement sounds perfect from my perspective. We'd have a nice continuous time to really get some things thought out.

Schackcosm 112 : *If I want to find out how James overcame the Miller-Bode objection, i.e., how he solved the problem of two observers, where would I look?*

Here's what Lamberth says in his (excellent) book *William James and the Metaphysics of Experience*:

> When James unveiled his radical empiricism in the 1904-5 *Journal of Philosophy* "series," both the issue of the compounding of consciousness and the related problem of solipsism (or of direct realism and a shared world) were on his mind. "Does Consciousness Exist?" goes straight to the issue of the compounding of consciousness, as well as James's direct epistemological view, while "The Thing and its Relations" and "How Two Minds Can Know One Thing" pursue the philosophical issues that I am construing broadly as having to do with co-consciousness or a shared world. That James himself remained unsatisfied with the position in those articles, however, is clear from the notebooks he kept between 1905 and 1908 on Dickinson Miller's and B. H. Bode's objections to his articles on radical empiricism.

So, those three articles are places for you to start, and they can be found on the web. Unfortunately, I don't yet have the book *Manuscript Essays and Notes* which contains the spoken-of notebook. What I suspect is that this is an issue we're going to have to ultimately work out for ourselves—those old notebooks, when dug up, will only get us so far. Still I'll have them waiting here when you arrive.

05-03-09 *Graduate Openings* (to M. A. Graydon)

All commendable answers. Sorry to pry like that. I'm feeling pretty comfortable with my inclination of decision. I hope you'll join the effort here: I think we can make some great progress getting quantum mechanics sorted out. And I'm quite sympathetic to one of the lines in your statement of interest: What we want in this quantum foundations game is to get at a clear-cut description of the reality underlying quantum mechanics. The quantum-Bayesian approach is an oblique approach (because it takes as its starting point agents and decisions), but it is a firm and careful approach: The ultimate residue will be the reality itself, whatever it is.

I think I have two very good other students lined up as well, along with a postdoc. We could make a great team. I'm going to work to get everyone in the team some desk space at the Perimeter Institute, for much easier interaction.

Attached is a still further improved version of the paper. See Eq. (44) describing unitary time evolution: I hope it'll knock your socks off. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.]

07-03-09 *The Beautiful Shape* (to D. M. Appleby, cc H. C. von Baeyer)

Thanks for the alternate proof of Tommy. Åsa Ericsson and I discussed Tommy a lot yesterday—she's just moved to Waterloo—and so I forwarded her the note. Hope you don't mind. I'm trying to get her in the thick middle of this problem.

Let me tell you and Hans a story.

Yesterday as I was parking the car, Katie asked me, "What's your favorite color?" "Blue," I said, as if she hadn't already heard the same answer a thousand times before. But then she asked me something she's never asked, "What's your favorite shape?" I was just about to say "ball" when I caught myself. "What a wonderful question Katie! My favorite shape is Hilbert space; I've never told you that." She said, "What does it look like?" I said, "I can't tell you yet, because we're still trying to figure it out. That's what I work on every day at PI." "If you don't know what it looks like, how do you know you like it?" "Because it's a beautiful shape, the most beautiful shape ever. That much we can already tell!" She looked perplexed; I came away delighted.

If Pauli is right, that shape must already be in our archetypes. On this rainy grey Saturday, I think I'll sit down with a book I ran across in DC the other day. The title is *Robert Fludd* and its back cover says, "All Fludd's important plates are collected here for the first time, annotated and explained ..." Maybe that's another way to look for our lovely, but still hidden shape.

09-03-09 *The Beautiful Shape, 2* (to D. M. Appleby, cc H. C. von Baeyer)

Regarding:

Applebyism 29: *I am slightly puzzled, though, because the thoughts in your note (the beauty of the hidden shape, and the idea that it is already there in our archetypes) are, to my way of thinking, decidedly Platonic in character. ... I have had the impression that you didn't wish to go down that path*

It is a bit like the story of Niels Bohr's horseshoe. Upon seeing it hanging over a doorway someone said, "But Niels, I thought you didn't believe horseshoes could bring good luck." Bohr replied, "They say it works even if you don't believe."

Yes, you have read me correctly in the past. Saturday's note was mostly a reflection of my wispy mood of the moment. But you've caused me to reflect. Perhaps my flipping through the Fludd book was more akin to this. With the warmer weather, Kiki has started to dream of what she will do with the porch, whether she'll put rocking chairs on it, a table, what kind of table, where they'll be placed, things like that. But in her inability to act (the weather is not that warm yet), she's lately taken to looking through catalogs for quite long lengths of time. She's not doing it, however, to buy anything—just to get ideas, she says. I think she uses the catalogs mainly to stimulate the right parts of her brain. And now come to think of it, maybe the monkey in me was doing the same. Kiki had been looking at catalogs all afternoon, and at some level I fell in line with her: I reached for the closest thing I had to a catalog.

The book I have is this one: `http://www.amazon.com/Robert-Fludd-Hermetic-Philosopher-Surveyor/dp/0933999690/ref=pd_bbs_sr_2?ie=UTF8&s=books&qid=1236592436&sr=8-2`.

There's all sorts of wonderful pictures in there, even ones with decidedly SICish features: a seven pointed rose, a tetragrammaton, or, say, this one `http://data5.blog.de/media/739/3229739_d0eb33369c_m.jpeg` which evokes in me a whiff of the urungleichung. But as you've told me with the *I Ching*, one cannot force these things. And in my looking, I have the overwhelming feeling that I'm probably forcing these interpretations.

Marcus's Reply

The link to amazon didn't work for some reason. And when I did a search on Amazon for Robert Fludd it came up with a long list (rather to my surprise: I hadn't realized he was so well known). Is the book you are talking about the one by Joscelyn Godwin?

Anyway, Plato. I wanted to write at length, but I have had an enormous number of distractions today. I have done nothing since lunch-time, and I badly want to get back to the calculation I was doing. So I hope you will forgive me if I am a little brief.

But yes, the word "wispy" would describe my own state of mind when I use the word "Platonic". I don't really know, with any exactitude, what I mean by it. Just that there is something there which feels right, and which I feel needs development. Badly needs, in fact. Plato, the man, is not too important really. What is important is that I strongly feel that there is something there (I am not sure what exactly, but something) that matters, and which is being ignored in the current climate. In my mind this is all tied up with our discussions about Pauli. In fact I am pretty sure I have seen Pauli described as a Platonist (though I can't remember by whom).

When we were talking once you said something (again, I can't remember what exactly) which suggested that in your mind Platonism was all tied up with the block universe. I think you were thinking that the Platonic archetypes are something immutable. But immutability is certainly not the point for me. For me the word "Platonism" has no such connotation.

Rather "Platonism" (as I use the word) has to do with the connection between the inner and the outer. The idea of people like Popper is that physics progresses in two stages. First one guesses a hypothesis (Popper uses the word "conjecture"), and then one tests it. Philosophers of science focus on the second stage: testing hypotheses. But to my mind the really interesting, and by far the most important part is the process by which one comes up with the hypothesis in the first place. Testing hypotheses is a non-trivial activity. But it hardly takes genius. Whereas I think it does take genius to think of the hypothesis. At least it does if the hypothesis has any depth to it. If, for example, it is like Faraday's hypothesis of field lines, or Kepler's hypothesis of elliptical orbits.

I should explain that I am using the word "genius" in what I believe is the correct sense, which differs significantly from what has become the usual sense. Nowadays the word "genius" means an extremely intelligent person. But originally it meant something quite different: a guiding or tutelary spirit (according to Wikipedia). I think I have also seen it described as a "fertile spirit". At any rate a genius, in the original sense of the word, is not a person. Genius isn't something anyone can *be*. It is something one can *have* ("have" in the sense one can *have* a daughter, or a father, or a friend) (and just as one can't own a daughter, or a father, or a friend, so one cannot own genius) (if anyone—anyone at all, Einstein and Faraday not excluded—has the temerity to think they own their genius—has the arrogance to identify themselves with their genius—they will surely and deservedly lose it). I think this is a very important distinction. The point is not that Faraday or Einstein were especially intelligent people. Doubtless they were above averagely smart. But I would question whether they were so much more smart than lots of other people, whom no one now remembers. I don't think smartness is the point about someone like Faraday. Rather it is the fact that Faraday was somehow communicating with something in the depths of his soul. And, what is more, something maybe not all that personal to Faraday. Something, in short, for which the word "genius", in the old sense, seems appropriate.

And that is what Platonism means to me. It is the idea that the sources of science are as much internal as external. Science, as practiced by a man like Faraday, isn't just a matter of taking careful note of data coming in through the external senses. It is also, and perhaps even more importantly, a matter of listening to something located deep down in his own inner being.

That is also the point about archetypes, as I understand them. And I feel that you had in mind something similar in the note you sent me about the beautiful shape.

10-03-09 *Vienna Saturday Afternoon (9 years later)* (to Å. Ericsson)

I wanted to say thanks for yesterday, for your commentary on my paper: It will help me improve it. So, please don't stop if you've got a mind to keep going. Attached is the latest version. It still hasn't incorporated most of yesterday's comments, but I hope to address that soon. In any case, it already incorporates a "thanks" to you!

This morning I was looking through my files for stuff on an old summer student, and I ran across the note below from December 2000. Look how excited I was about an idea of mine. In the end, there was a fundamental flaw in the approach I was taking. But notice the similarity between my phrases below and the things I say in Section 3.1 of the present paper. Nearly nine years: I am nothing if not stubborn!

From a letter to Greg Comer dated 2 December 2000

Did I tell you I'm in Vienna? Tomorrow I go to Budapest and then I come back to Vienna until Dec 12. It's been a very busy last few days for me: I've finally done it! I have a fully Bayesian derivation of complete positivity for quantum time evolutions (that's the generalization of unitarity for density matrices, taking into account measurement-disturbance nastiness). This is the best work I've done in six years. As suspected, quantum time evolutions boil down o *nothing* but Bayes' rule in disguise. I've been working furiously to dot the i's and cross the t's and get something written up before I leave here.

So, I may not be writing you email until my return to the States. Keep thinking about those dictionaries: that is a very deep train of thought.

12-03-09 *Cover Art* (to S. Capelin)

Attached is one idea. Maybe a real artist could base something on it. It is a transparency I have used in my talks for years, and is meant literally to convey what I have been calling "The Paulian Idea." It captures the following points:

1. That the quantum state lives in the head of the observer, not in the system being measured. Quantum states are epistemic.
2. That measuring devices should not be considered independent of the measuring agent, but rather are more like prosthetic hands than external pieces of the world.
3. That the quantum system (which is an external piece of the world) is something like an alchemist's "philosopher stone"—its conceptual role is like a catalyst that transforms the alchemist.
4. That the result of a measurement is like a miniature act of creation. These are the sparks depicted as flying between the measuring device (measuring device) and the quantum system.

Anyway, that's one idea, if it is something that can be made tasteful.

Another idea stems from another rhetorical trick I play in my talks. See pages 2, 7, and 9 of `http://www.perimeterinstitute.ca/personal/cfuchs/Peres%20School%201-110.pdf`. I often put a picture of a block down and say, "This is a quantum system." But then I superpose a state vector $|\psi\rangle$ on top of it, and say, "This symbol represents *my* knowledge of it." "No me, in the room? Then no state vector in the room." And then I lift the vector off the block. Then, I say, "But the quantum system remains; it hasn't disappeared, only the state vector." That's another way of putting some of what I said above—it is an essential aspect of The Paulian Idea. That quantum systems exist in a substantive way, even if state vectors do not. So, another idea for cover art would be of a cubical block with a stylistic state vector rising off of it.

That's the two best ideas I have at the moment.

13-03-09 *Graduate Openings, 2* (to M. A. Graydon)

You should make sure you are making the right decision for yourself. Ross Diener was just out here, and we spent a good bit of time together. You two should get together and share notes, dreams, plans. Research is a big step: It's not at all like class work. You write your own questions, and you seek your own answers in byways that no one has ever traveled. In a way, you put your own personality *into* the world itself. Emotion, personality, and science become pretty inextricably entwined. So, take into account the whole package in your thinking.

13-03-09 *The Unfinished Universe* (to M. A. Graydon)

In rereading what I just wrote, I found myself thinking of a passage from William James that I've always liked, and I thought I'd send it too. File attached. [See passage in 05-01-09 note "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer & D. M. Appleby.] Read it as a metaphor for the group I'd like to assemble. Picking up on the last line in it—"the universe, unfinished, growing in all sorts of places"—I see our "additions" as helping the universe grow in all sorts of places.

23-03-09 *Bayesian Chance* (to W. L. Harper)

I want to cite your Bayesian Chance paper with Chow and Murray in the paper I'm writing at the moment. Has it been published anywhere, or posted anywhere?

I started reading it this morning. It gives me a great foil for some remarks I want to make at the end of my own paper, where I presently have the placeholder: "sketch how to build a fully Bayesian theory of objective indeterminism without invoking objective chance."

26-03-09 *The Varieties of Optimistic Experience* (to A. Ney & B. Weslake)

Thanks again for the conversation yesterday and last night. Particularly, last night, it helped refocus my thoughts on the issue of temperament in science and philosophy. Here's a quote from William James that I've always had some respect for:

> The history of philosophy is to a great extent that of a certain clash of human temperaments. Undignified as such a treatment may seem to some of my colleagues, I shall have to take account of this clash and explain a good many of the divergencies of philosophies by it. Of whatever temperament a professional philosopher is, he tries, when philosophizing, to sink the fact of his temperament. Temperament is no conventionally recognized reason, so he urges impersonal reasons only for his conclusions. Yet his temperament really gives him a stronger bias than any of his more strictly objective premises. It loads the evidence for him one way or the other, making a more sentimental or more hard-hearted view of the universe, just as this fact or that principle would. He *trusts* his temperament. Wanting a universe that suits it, he believes in any representation of the universe that does suit it. He feels men of opposite temper to be out of key with the world's character, and in his heart considers them incompetent and 'not in it,' in the philosophic business, even though they may far excel him in dialectical ability.
>
> Yet in the forum he can make no claim, on the bare ground of his temperament, to superior discernment or authority. There arises thus a certain insincerity in our philosophic discussions: the potentest of all our premises is never mentioned. I am sure it would contribute to clearness if in these lectures we should break this rule and mention it, and I accordingly feel free to do so.

I was acutely aware that there was no time in my talk to explain or to even sketch the things that drive me the most in this quantum-Bayesian quest—it is the pursuit of a malleable world. Let me just point to these papers as a momentary band-aid: `http://arxiv.org/abs/quant-ph/0404156` and `http://arxiv.org/abs/quant-ph/0608190`

Also, let me share the present attachment, which very much addresses the optimism/pessimism issue discussed last night. [See "Delirium Quantum" `arXiv:0906.1968v1`.] It is a conversation I had with Howard Wiseman on the subject: He too labeled quantum Bayesianism a kind of pessimism, and I did my best to lay out what I find "wildly optimistic" in it. Actually, relooking, the whole document is devoted to this issue. See, for instance, the paragraph on page 15 that starts "Doesn't that just make you tingle?" Anyway, it is the potentest of the premises that drive me personally in physics—for instance, the drive that leads me to reformulate quantum mechanics in this way or that—and it is indeed worth declaring forthrightly.

26-03-09 *Review of the Magic Equation* (to C. R. Stroud, Jr., J. H. Eberly, A. Ney, Y. Shapir & B. Weslake)

Thanks for the seeming interested in my talk yesterday; it's much appreciated. However, this morning as I awake, I find myself feeling disappointed that the key idea from near the end—that of taking the "magic" formula as a fundamental axiom of quantum mechanics—didn't have a chance to come across so well. Thus, let me ease my conscience by sharing a draft of a paper I'm presently writing with Schack on the subject. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] It is attached. The first five sections of it are stabilized now and should be completely readable. I hope it will give you a much better picture of where I think quantum mechanics comes from.

If you have any constructive or deconstructive comments on the draft, I would love to try to incorporate them into an improved paper.

It was great discussing with you all. Thank you for that and the fine wine and the fine hospitality.

Excerpt of Joe Eberly's Reply

> I agree that it's very attractive to have the possibility of an axiom that can encompass several desiderata at once, including the Born Rule, even if only to 38 decimal places.

26-03-09 *QB Coherence* (to W. G. Demopoulos)

... from a hotel in Rochester. I apologize for taking so long to reply to you. I kept hoping to finish the draft of my paper so that I could send it along in a reply. It hasn't happened; I continue to keep fiddling with the manuscript, and I presently judge myself only about 80% complete.

Still I think it is finally at a stage where it might benefit from your input. Particularly Section 2, of which I am not completely happy with at the moment. Sections 2 and 4 could probably use the most Demopoulizing actually.

Any input you might have would be invaluable.

To answer your other questions. I'd like to stay over when I visit London, if it fits with my other obligations; but I'll tell you definitively as the time draws nearer.

About Smeenk's talk, it was *very*, *very* good. I enjoyed it tremendously and indirectly it connected with several things that I worry about most in my forming view of quantum mechanics.

27-03-09 *New Slogan* (to R. Schack)

QM is only about the clicks you put yourself in a position to see (feel, experience).

27-03-09 *New Slogan, 2* (to R. Schack)

Schackcosm 113: *Ahhh, we need some time to discuss these things. The point is that qm is only about your active experience, right?*

Yep! Sad to think this formulation took so long. It came to me while driving back from Rochester yesterday. It looks like we have two strong supporters in Joe Eberly and Carlos Stroud (both involved in quantum optics, both students of Jaynes). Do you know them?

27-03-09 *Quantum Fiddling and Twiddling* (to N. D. Mermin)

A while back I mentioned a paper to you that I was just starting to construct; I asked whether you could remember where you had remarked on Feynman's "only mystery" of quantum mechanics. Well, I'm still twiddling on the draft. But it is now about 80% complete, and particularly the first 5 sections are pretty stabilized. Still, I'm not completely happy with the introductory section. If you've got the time/interest, might I ask you if you have any constructive feedback for the introduction. No promises that I'd incorporate anything of course, but you've shaken me from my dogmatic slumbers before, and you might again. And you're certainly the best test kitchen I know.

Hope things are going well for you.

I invented a new slogan yesterday: QM is only about the clicks you put yourself in a position to see (feel, experience). You're the second eyes to see it.

28-03-09 *Clicks Happen – (the bumper sticker)* (to N. D. Mermin)

Last night I reported your note to Kiki after we'd had a little wine, and I found myself speaking in quite a mocking voice ... "David's spennnndinggg threeee months in Cooopenhaggggen." That sort of thing. Hearing myself, I realized I was actually childishly jealous! You lucky boy!

Funny, we just had a discussion on the Bohr, Mottelson, Ulfbeck paper in our quantum foundations group meeting the week before last. The consensus was it's damned unclear. But I've said that before in the singular. I got much more from the more poetic Ulfbeck-Bohr paper (the one sans Mottelson). There is an idea in there that I like; that each quantum event is very literally unique.

Yesterday's slogan was meant to imply that stuff happens all the time, everywhere. But when I use quantum mechanics, I'm talking very strictly about the stuff that impinges on ME (and results partially because of ME).

If you do decide to delve into the paper a few days from now, let me know. I'll send you whatever is the latest draft at that point.

I'll think about how to make use of the Bohr archive. Interesting proposition.

30-03-09 *What If?* (to W. G. Demopoulos)

Demopoulosism 38: *Here's a simple idea apropos our conversation about conditionalization: What would be anti-subjectivist about the idea that there is convergence to the same algorithm for assigning subjective probabilities? Of course this has the effect of converging to the same assignment on discovering an outcome, but agreeing to a common algorithm is not the same as agreeing to base our probability assignments on an objective and physical property of things, as I assume an objective chance theorist would hold. The situation is similar to our agreement to rules of inference* *in logic. (Here I'm not talking about logics of events, etc., where there is something like a physical interpretation of what's going on, but of pure logic.)*

I don't think there is anything wrong with converging to the "same algorithm" (Lüders' Rule for instance). Nothing wrong with two agents starting with different state assignments initially, but having a common assignment after viewing a particular piece of data.

01-04-09 *Lunchtime Conversation* (to R. W. Spekkens)

Our lunchtime conversation yesterday has prodded me write a new essay. Hopefully I'll have it generated next week after Marcus is gone. The subject was really all about this slogan I'm trying to polish into something pithy: "QM is only about the clicks you put yourself in a position to see (feel, experience)." It is the very point of a theory from the inside of the world (i.e., if there is no God's eye view, that is what you have). I want to flesh that out and put it in the context of the Norsen argument (which I view as a recalcitrant attempt to view quantum states onticly[200], via the apparatus of the EPR criterion of reality). It will be a good exercise.

05-04-09 *E on Time's Illusion* (to D. M. Appleby)

Here's a quote close to the one you were thinking of. It's from a letter of Einstein's to the son and sister of Michele Besso, 21 March 1955:

> Now he [Besso], too, has just preceded me in his departure from this strange world. This means nothing. To us believing physicists the separation of past, present and future has only the significance of an illusion, albeit a stubborn one.

06-04-09 *Where's ONR?* (to K. Martin)

Hey, have you watched "The DiVincenzo Code" on YouTube? It redeems Oxford Physics in a way. (Nothing can redeem Oxford Philosophy.) Seriously. Get some beers and watch it slowly—I did that in Australia a few months ago—it's a lot of fun.

Isn't it like 4 AM there right now?

06-04-09 *A History-of-Knowledge Thing* (to D. P. DiVincenzo & C. H. Bennett)

A fitting story for John Wheeler. John himself would slide between two formulations, that our quantum measurements here and now determine what we can say of the past, and that our measurements here and now create the past. As far as I can tell he never completely decided between the two very different ideas.

I've always been intrigued by an argument of George Herbert Mead (who by the way was born in South Hadley, MA) that in a world with real indeterminism, the past must indeed be open or malleable, something like John's second formulation. Below is Arthur Murphy's summary of Mead's argument. [See Murphy's quote in 23-09-03 note "The Trivial Nontrivial" to S. Savitt.]

[200] Or ontically? How do you spell it?

Coming back down to earth though, I recall how disappointed I was when I learned that John, though he always gets (and took?) credit for it, did not invent the word "black hole". Someone in an audience called it out after one of his talks on gravitational collapse. This story of Xe-135 might be of a similar type.

David's Preply

> I read with interest some of the stories of Wheeler in the new *Physics Today*. One that really caught my eye was the story that, in a few minutes of work at Hanford, it was Wheeler who realized that Xe-135 was the principal poisoner of nuclear-pile reactions. This is one of the profound engineering insights of the 20th century — it was the lack of this knowledge that may well have blown up Chernobyl.
>
> But then, in browsing a book called *Nuclear Technology*, I see a detailed story attributing this insight to Fermi! (He is rushed by train to Hanford, does some serious thinking, etc.) So, I wonder, what is right? Is this one of these things that can be made to come out either way by those exoplanet residents 65 light years away that are just receiving the first Hanford light?

06-04-09 *Completing the Story of Incompleteness* (to L. Freidel)

Here's the lines from that old paper of mine that I was telling you about:

> There are two issues in this … that are worth disentangling. 1) Rejecting the rigid connection of all nature—that is to say, admitting that the very notion of *separate systems* has any meaning at all—one is led to the conclusion that a quantum state cannot be a complete specification of a system. It must be information, at least in part. This point should be placed in contrast to the other well-known facet of Einstein's thought: namely, 2) an unwillingness to accept such an "incompleteness" as a necessary trait of the physical world.
>
> It is quite important to recognize that the first issue does not entail the second. Einstein had that firmly in mind, but he wanted more. His reason for going the further step was, I think, well justified *at the time*:
>
>> There exists … a simple psychological reason for the fact that this most nearly obvious interpretation is being shunned. For if the statistical quantum theory does not pretend to describe the individual system (and its development in time) completely, it appears unavoidable to look elsewhere for a complete description of the individual system; in doing so it would be clear from the very beginning that the elements of such a description are not contained within the conceptual scheme of the statistical quantum theory. With this one would admit that, in principle, this scheme could not serve as the basis of theoretical physics.
>
> But the world has seen much in the mean time. The last seventeen years have given confirmation after confirmation that the Bell inequality (and several variations of it) are indeed violated by the physical world. The Kochen-Specker no-go theorems have been meticulously clarified to the point where simple textbook pictures can be drawn of them. Incompleteness, it seems, is here to stay: The theory prescribes that no matter how much we know about a quantum system—even when we have *maximal*

information about it—there will always be a statistical residue. There will always be questions that we can ask of a system for which we cannot predict the outcomes. In quantum theory, maximal information is simply not complete information. But neither can it be completed. As Wolfgang Pauli once wrote to Markus Fierz, "The well-known 'incompleteness' of quantum mechanics (Einstein) is certainly an existent fact somehow-somewhere, but certainly cannot be removed by reverting to classical field physics." Nor, I would add, will the mystery of that "existent fact" be removed by attempting to give the quantum state anything resembling an ontological status.

The complete disconnectedness of the quantum-state change rule from anything to do with spacetime considerations is telling us something deep: The quantum state is information. Subjective, incomplete information. Put in the right mindset, this is *not* so intolerable. It is a statement about our world. There is something about the world that keeps us from ever getting more information than can be captured through the formal structure of quantum mechanics. Einstein had wanted us to look further—to find out how the incomplete information could be completed—but perhaps the real question is, "Why can it *not* be completed?"

Indeed I think this is one of the deepest questions we can ask and still hope to answer.

The real question is why can it not be completed? I still like that formulation and still think it has a lot of fruit to bear.

07-04-09 *Quantum Mechanics and Pragmatism* (to M. S. Leifer)

There's nearly as many varieties of pragmatism as there are Bayesians:

- I. J. Good, "46656 Varieties of Bayesians," in I. J. Good, *Good Thinking: The Foundations of Probability and Its Applications*, (University of Minnesota Press, Minneapolis, 1983), pp. 20–21.

07-04-09 *Your Thoughts* (to N. Bao)

You wrote, "My largest concern is with whether or not I am absolutely dead-set sure that I want to do Quantum Information / Quantum Foundations in grad school." It is hard for me to see how that reconciles with what you wrote in your statement of purpose for U. Waterloo: "At this point, I truly believe that quantum information theory is what I want to do for the rest of my life." My guess is you were seduced somewhat by the old-school prestige of Stanford in comparison to U. Waterloo and the self-confidence of the students there. But those things make little impression on me: Great physics is had by great passion. Almost nothing else matters. On the other hand, there is something John Wheeler once said that did make a great impression on me. He was asked by someone in an audience, "What differences do you see between the students at Princeton and the students at University of Texas?" Wheeler replied, "Only that the students of Princeton *know* they're smart." The implication was that the local students just needed more nurturing, but therein stopped the difference. And so it was true: No student at Princeton invented quantum information theory—that came almost exclusively from the University of Texas, with Schumacher, Wootters, Deutsch, and Zurek there at the time. It was the place that mattered, not the prestige of the place. Nor was it the breadth of the place—it was really all about focus and the sense of exploring a true frontier: That brought out the best in everyone involved.

07-04-09 *Question* (to A. Plotnitsky)

Shamefully I'm still working on the SIC paper I told you about. Attached is the present draft; it's still only about 80% complete! [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] Feel free to cite it however; it will I hope soon be posted on the quant-ph archive. (OH, there was also a very preliminary version in Andrei's conference proceedings; a citation can be found in the footnote on the first page.)

Though it's only about 80% complete, the first five sections are relatively stabilized. Any comments are most welcome!

You can read about Glauber's coherent states here:

> `http://en.wikipedia.org/wiki/Coherent_state`

and find the original references therein. That the coherent states can be used as an informationally complete POVM to give a Bayesian representation of quantum states, can be read about here

> `http://en.wikipedia.org/wiki/Husimi_Q_representation`.

Beware though, that that article calls Q a "quasi-probability" when in fact it is an honest-to-god probability—nothing quasi about it.

08-04-09 *Media Interview for FQXi about Hugh Everett* (to G. Stemp-Morlock)

Thanks. I much appreciate that. Only two changes I'd make to the quote; I'll set them off with brackets:

> "It was a reaction to the Copenhagen interpretation that went in a particular direction, and it was a healthy move because ultimately one does want to get away from thinking about observers as an integral part of quantum mechanics and it does do that," said Fuchs. "But, what price do you pay [for] a view of the world that is not very particular to [that] world?"

Might I also suggest you modify this sentence slightly: "believes it is a contentless interpretation, that is, it doesn't tell us anything beyond the Copenhagen interpretation that could lead to new developments in quantum foundations." In the present formulation, if MWI is contentless, then the only conclusion is that Copenhagen is contentless as well! I'd never agree with that. So I would prefer for you to simply strike the words "beyond the Copenhagen interpretation". For instance, you could make it:

> Christopher Fuchs ... believes it is a contentless interpretation, that is, it doesn't tell us anything that could lead to new developments in quantum foundations.

The main point is that one could make up a many-worlds interpretation of *any* theory. That is, despite all the posturing of the believers, there is nothing particular to quantum mechanics that actually pushes it upon us. That is why I say it is contentless.

I wish I could find the quote of John Wheeler where he says something similar to the latter part of your sentence, i.e., "it doesn't tell us anything that could lead to new developments in quantum foundations." If you want me to look, I can do it when I get to the office.

Good luck with your article.

08-04-09 *Mermin the Epistemicist* (to R. W. Spekkens)

If I read you correctly yesterday, you seemed a little surprised when I said that Mermin ought to be in the epistemic team. I discovered he says it pretty succinctly here:

> `http://arxiv.org/abs/0808.1582`,

starting particularly at the last paragraph on page 2.

09-04-09 *Testing Quantum Certainty* (to R. Schack)

How does that sound for a title for yet another paper we must write this year?

Can you spot the flaw in `http://arxiv.org/abs/quant-ph/0601205` from our perspective? Apparently the only way I'll ever build (true and enduring) respect with my colleague Spekkens is to respond thoroughly to this paper.

By the way, it's an easy (and fun) exercise. I encourage you to try it.

09-04-09 *QBism, Certainty, and Norsen* (to R. Schack)

Between the two files attached is a draft for a draft of a paper responding to Norsen. Let me know what you think. Using him as a foil is, I think, a chance to do our certainty paper much better.

I'd like the new paper to begin with something like this:

> Travis Norsen has recently given a very clear exposition on the underpinnings of the idea that quantum mechanics all on its own implies a violation of Bell Locality, without any need for a hidden variable assumption. The paper has been a great service to the community. However, in making his claim, Norsen made significant and unabashed use of a variant of the EPR criterion of reality. In contrast, the present authors some years ago wrote a not-so-very-clear paper arguing in effect that the EPR criterion of reality is at odds with a personalist Bayesian account of probability, and that holding strictly to the Einstein notion of locality indeed forces its bankruptcy in the quantum context. In this paper, we plan to do the argument much better justice by using the clarity of Norsen's paper as a counterpoint with which we must contend. By sharpening ourselves with respect to Norsen blah blah blah . . .

09-04-09 *Request to Review Manuscript* (to The Physics Teacher)

I'm sorry, I read the first two sentences of this paper—"In 1964 John Bell proved a theorem allowing the experimental test of whether what Einstein derided as "spooky actions at a distance" actually exist. We will see that they do."—and I feel a revulsion. All I can do is reject it. This is such an old issue, such an old mistake, much like the continued proposal of a perpetuum mobile.

What the violation of a Bell inequality demonstrates is that "local realism fails." Unpacking the term local realism, one means more precisely the conjunction of two statements: 1) that actions or experiments in one region of spacetime cannot instantaneously affect matters of fact at far away regions of spacetime, and 2) that measured values pre-exist the act of measurement, which merely "reads off" the values, rather than enacting or creating them by the process itself. The failure of local realism means the failure of one or the other or both of these statements. It does not mean what the author says above. See, for instance:

- S. L. Braunstein and C. M. Caves, "Wringing Out Better Bell Inequalities," Ann. Phys. **202**, 22 (1990).
- N. D. Mermin, "Hidden Variables and Two Theorems of John Bell," Rev. Mod. Phys. **65**, 803 (1993).

for very thorough discussions of this point. Please save the average physics teacher by sending this article back home.

10-04-09 *Testing Quantum Certainty, 2* (to R. Schack)

Schackcosm 114: *The exercise is indeed easy. Unperformed experiments have no results. Even probability 1 predictions are subjective. There is nothing in the world which guarantees the outcome of a probability one measurement. You name it.*

This "Nature IS nonlocal" theme is everywhere at the moment. Have you seen Albert's paper in Scientific American *(February I think)?*

I haven't had the courage to look at it, though everyone wants me to.

Long discussion with Rob yesterday. It seems he's not unsympathetic to "even probability 1 predictions are subjective" most generally, but that the general point may become inoperative in physical theories like quantum mechanics. That is to say, it seems he wants the role of physical theory to be a bridge between belief and fact. Something of that flavor.

10-04-09 *SICing a Symbol* (to H. C. von Baeyer and D. M. Appleby)

Good Good Friday to both of you. I slowly continue my refreshed study of Pauli, and last night, just at midnight, got to Gieser's chapter on *The Reality of the Symbol*. Previous to this, I had never paid much attention to this concept of a "symbol." However, here are some of Gieser's words that quite struck me, and I haven't been able to get them off my mind all night:

> Jung sees a living symbol as the best possible expression of something divined but not yet fully known – something which cannot be represented in a more characteristic way than in the form taken by the symbol. If one says that the cross is a symbol of divine love, then according to Jung one gives a semiotic *explanation* of the cross, which is something quite different from seeing it as a symbol. If on the other hand one believes that the cross is beyond all conceivable explanation, but that it is still the most apt expression of an as yet unknown and incomprehensible fact, then one has a symbolic attitude to the cross. The symbol always consists of a known or rational part and an unknown or irrational part, which is not accessible to reason. The known part of the symbol is represented by its current form while the unknown part opens up to the non-visual aspect of the archetype. The state of tension between known and unknown gives the symbol a numinous character, which lends it a power and attraction. Our fascination with and manipulation of the symbol gradually leads to a discovery of the true characteristics of the object and the symbol increasingly produces real knowledge. In this way the unknown is made conscious and thus the symbol loses its power and attraction and 'dies'.

It's obvious what I thought upon reading this. By these lights, the SICs are surely a symbol for Marcus and me. There's almost a discussion in Gieser's description that Marcus and I had several times over in the last few years. For somehow we felt, long before we could articulate why—long before the urungleichung or anything like that—that the SICs were somehow the "most apt expression" of quantum-state space. There has been a "power and attraction" in these things that neither of us have understood and neither of us have been able to yield to (in our own ways).

I'm quite thankful I read this passage, as I suppose I should be, given that today commemorates the birth of the symbol Gieser uses as her example.

Marcus's Reply

> Yes. Absolutely.
>
> But maybe I would like to add a couple of qualifications. If a symbol is defined to be something that
>
> > consists of a known or rational part and un unknown or irrational part, which is not accessible to reason
>
> then I think that the concept of divine love *also* has a good deal of the symbol about it. I am sure I don't understand what is meant by it. So in that sense it is certainly unknown (at least to me). But at the same time it doesn't mean nothing (at least to me). Religious language like this leaves me perplexed. But it doesn't leave me feeling perplexed in the way I would feel perplexed if someone were to say (or rather write) "oub##4 b12xp".
>
> I had a conversation with Hulya the other night which may be relevant here. She has been reading *Zen and the Art of Motorcycle Maintenance*, and she criticized it on the grounds that it isn't deep (though I should perhaps add that she was at pains to stress that she hasn't got very far into it yet and may change her mind). This led to a discussion as to what counts as deep. We decided that it was best to begin with an example that is the complete opposite of deep. Suppose one were to take a USB stick and put onto it a list of the names and addresses of everyone living in Waterloo. That would strike Hulya and I as totally shallow. The reason is (we decided) that it contains exactly what one consciously chose to put there, no more, and no less. If someone lives in Waterloo then their name and address will be on it, but their telephone number won't be. There is no ambiguity to it at all. But if something is deep there is always a lot of ambiguity.
>
> Of course mere ambiguity is not sufficient. Otherwise one could make any book deep simply by leaving it out in the rain, so that the ink runs, and then getting a dog to chew the pages. It is important also that the ambiguity be suggestive. Of course that is not enough either: for it mustn't be suggestive of just anything but specifically of something that strikes us as — what exactly? Here I find myself at a loss. The best I can do is to say that it must be suggestive of something that strikes us as deep. Which, it may appear, does not get us very far.
>
> Still I think it does get us somewhere. If a book is deep that means (Hulya and I decided) that in some sense it reaches out beyond itself. It actually contains more than the author consciously chose to put in it. A deep book is one that the author themselves might read and re-read and each time discover something new. Just as if they were re-reading a book written by someone else. In short a book is deep if the

supposed author isn't exactly the author at all. At least not completely the author. As if the author had a hidden co-author.

Hardy, in *A Mathematician's Apology*, says something about this. He says that a chess puzzle is shallow because, even if it is a very difficult problem, it is open and shut. Once you have solved it then you have solved it. There is nothing more to do. By contrast Euclid's proof that there are infinitely many primes is deep (according to Hardy). Mathematicians have been coming back to it for thousands of years, and they keep on finding new connections.

I think what I so hate about classical physics (even General Relativity—pace Hans) is that it is built on the idea of what Gieser calls a semiotic explanation. The universe, classically conceived, consists of a great mass of dead facts. You can imagine putting these facts on a cosmic USB stick and what would result would be exactly like a list of the names and addresses of everyone in Waterloo. Ugh!

Quantum mechanics is wonderful because it dispels this nightmare. Classical physicists see the fact that quantum mechanics doesn't permit complete knowledge as a disaster. They couldn't be more wrong. Just because it forbids a complete description (the kind of description which would allow us to reliably predict the result of any experiment) it permits the possibility of depth. It is not a disaster but a liberation.

Which brings me back to Jung and symbols. I profoundly disagree (indeed profoundly dislike) his idea that

> Our fascination with and manipulation of the symbol gradually leads to a discovery of the true characteristics of the object and the symbol increasingly produces real knowledge. In this way the unknown is made conscious and thus the symbol loses its power and attraction and 'dies'.

One sees from this that Jung was classical at heart. As if one were to say that our inability to know position and momentum simultaneously is only temporary. Eventually we will discover Einstein's true theory, and then we can go back to thinking of the universe as an enormous telephone directory.

And, to go back to the point with which I began, I think his concept of symbol is too narrow. As he sees it the cross is symbolic, but divine love not. It seems to me that just about every word in the gospel stories is charged with meaning. Deep meaning. I don't understand them. I am certainly not saying that I am a Christian in any conventional sense (though I probably am in some extremely unconventional sense). But they are shot through with the quality which Jung attributes to a symbol:

> a numinous character, which lends it a power and attraction.

I would say everything in them is symbolic. If one is going to use that word.

But I agree completely about SICs.

Hans's Reply

Dear Marcus, thank you for that excellent and useful gloss on the word symbol. I agree with your critique of the last part of the passage, but I don't know whose words they are. Probably Gieser's, possibly Jung's, but certainly not Pauli's. I can't imagine Pauli talking about "true characteristics of" or "real knowledge about" an object. Or to give up a symbol as dead, after making it the centerpiece of his enterprise. In fact the sentence reminds me of the point that Pauli and you have made often: that most people, even good physicists, remain convinced, or at least hopeful, classical thinkers, and that it will therefore take a long time for the scientific community to switch to a radically new worldview.

Chris's Reply to the Replies

Oh, you guys make me feel guilty! I too had disliked the latter part of Gieser's description, but I decided to keep the whole paragraph for completeness sake. I should have made a comment! ... Please don't think less of me!

11-04-09 *QBism, Certainty, and Norsen, 2* (to R. Schack)

Schackcosm 115: *I have now studied your handwritten notes. I like the parts where you explain why Norsen's argument falls to pieces from a QBism perspective. I am not too sure about your explanation of what locality means from a QBism perspective, though. We had these discussions last year. It looks to me as if locality is built into the QBism formalism from the start, so that it becomes impossible within that formalism to ask, e.g., does nature allow superluminal signalling?*

Indeed, we don't have a very good story of signaling yet, superluminal or otherwise. It dawned on me for the first time this morning that until we can say something intelligible about that we're going to always be exposed to the charge of solipsism. We must understand how to describe "communication" within the QBism framework! I'm a slow learner.

13-04-09 *Easter Pictures and the Beginning of Depth Psychology* (to D. B. L. Baker)

Fantastic pictures, I enjoyed seeing them very much. Made me miss you again old man. Do you still go to church regularly?

Attached is a representative of my own Easter pictures. Sorry, not much better fare at the moment. The girls are on the balcony outside of Katie's room.

I'd like to write you a little essay on "depth" and "deep thoughts" harkening back to our days in high school. I've been thinking about what we found to be "deep" then, when we would discuss this or that, songs of Paul Simon, Rush, The Clash. This has come about because in reading a book about Carl Jung, I came across a definition of "symbol" that struck my fancy, making sense of what I've been doing the last four years. It's not so very different from our thinking about a song over and over. See note below.

The particular example being discussed—a thing I call a SIC (pronounced "seek")—is a certain mathematical structure we've been trying to prove the existence of, on and off since 1999. In the last four years or so, it's turned into a real obsession. You can read about it, if you're interested, in somewhat general terms (which I wrote up for the Navy) in the attached PDF.

I'll also place further down a part of Appleby's reply, which reminded me of our old high school discussions indeed.

14-04-09 *Your Thesis, 2* (to J. R. Gustafson)

I think you should post it, for instance on the quant-ph archive.

In case you're interested, I attach my "Resource Material for a Pauli'an / Wheeler'ish Conception of Nature" file. It's been a long time since I've updated it. For instance, it doesn't have your thesis listed yet, or Gieser's book, etc. But there's plenty there for you to read and think about, if you want to start thinking in these directions again. (And if you find any typos, please let me know!) One of these days, I'm going to finish putting it all together and post it as well.

15-04-09 *Math Question* (to P. Hayden)

Haydenism 1: *Thanks for coming to dinner on Wednesday (and for the grace under pressure at the lecture). We were unfortunately seated at more than a correlation length apart so didn't get a chance to talk. Did you still want to pose that math question?*

I'm just now replying to this note! Let me start out with an apology, not for the sloth here, but for forgetting to say goodbye to you after dinner the other evening! Wine does mysterious things. And let me apologize too for not doing a better job of storytelling. With just a tad of thought, I could have pulled off a relevant (and funny) punch line for the story I did tell. I've been kind of beating myself. I wish I could blame the wine for this one, but truth be told it was only the aging of my synapses.

With regard to the math problem, it was really just a way to lure you in to thinking about technical aspects of my QBism program. Attached is a draft (about 80% complete) of a paper I'm writing at the moment. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] Sections 1 to 5 are now relatively stable, so they should at least make some sense at this point. The basic mathematical question is how much of the structure of density operator space is implied simply from having a maximal set with respect to the inequality in (97)

$$\frac{1}{d(d+1)} \le \sum_i p(i)s(i) \le \frac{2}{d(d+1)}\ ? \tag{98}$$

It would have been better to tell about these things at the board, so you wouldn't have to wade through all this nonsense to get to the basic question … but time flies. If you do wade, though, and have any insight, I'm all ears. Else we can talk at the ONR meeting in May.

15-04-09 *The Egocentric Paradigm* (to R. W. Spekkens)

The right-hand column of Lindley's discussion (attached) reminds me of our conversation after the group meeting Thursday.

> The Bayesian, subjectivist, or coherent, paradigm is egocentric. It is a tale of one person contemplating the world and not wishing to be stupid (technically, incoherent). He realizes that to do this his statements of uncertainty must be probabilistic. This is important on its own for it rules out a large class of behavior patterns, like sampling theory statistics, but is it enough? Once I coined the aphorism "Coherence is all." Was I right? It is when we consider two coherent Bayesians that new features arise. Does their egocentric behavior allow them to talk to one another? In one respect it does. Suppose that you and I are both coherent and you tell me that your probability for A, were B to be true, is α, say. How does this knowledge affect my probability for A given B? It is easy for me to do the coherent calculations in terms of my assessments that were A and B both true, you would say α, and that were $\overline{A}$ and B both true, you would say α. For to me α is just data and the likelihood ratio updates my probability in the usual way. A weather forecaster who announces rain whenever it is subsequently dry,

and vice versa, is badly calibrated but very useful. So I can respect your egocentricity; and you, mine.

15-04-09 *My Own Credo on Hidden Variables* (to D. M. Appleby & R. W. Spekkens)

Just to make sure everyone understands my honest position on hidden variables, I paste in an old note (and an old referee report) below. [See 17-11-05 note "Cash Value" to H. Halvorson.] It has similarities to Rob's shape-like remark. I'm certainly not opposed to postulating entities beyond the clicks. But the entities had better have "cash value" to get my attention. And I haven't seen a hidden-variable (or ontic-variable theory if you will) yet that has gotten my attention in that way.

15-04-09 *The Well-Calibrated Bayesian* (to R. Schack)

Schackcosm 116 : *Internet access speed truly awful. Not sure that I'll be able to access Dawid's paper. I expect that I'll be a lot less negative now. I have a worry that this kind of reasoning might lead to a concept of "correct" probability assignments. Well calibrated probabilities are "better" than others. But that might not be so bad in the light of our discussions at IHOP. I would pay more for forecasts from a well-calibrated Bayesian.*

Please remind me of those discussions as well.

In the present case (i.e., with regard to the particular paper I sent you), I was more intrigued by the coherence issues to do with the concept. What he tries to establish in this paper is that any forecaster will expect himself with probability 1 to be well-calibrated. Then he asks, how can that be, since every forecaster surely knows that he is fallible. Second to that, though, I wonder if his calibration might just be arbitrary (and also, as some of the commentators expressed, operationally without content).

About Lindley, I very much like the sound of him, except for this Cromwell's Rule business. It is surely inoperable—same point de Finetti made.

15-04-09 *Marcus on the Symbol 3.5 Years Ago* (to H. C. von Baeyer)

I've been putting together a coffee table book called *The Unpublished Appleby*, and consequently rereading some old notes and paper drafts he's written over the years. Attached is one example. Have a look at the paragraph just preceding Section 5:

> The recognition, that quantum mechanics doesn't supply us with an exact mental replica of the world, may initially be experienced as a disappointment. In the same way, the little boy I mentioned above may feel disappointed when, in spite of repeated attempts, he finds that he is simply unable to get his foot physically into the picture of the shoe. And perhaps his first reaction will be throw the picture away in disgust, as completely useless. However, he will eventually come to realize, with time, and a great deal of mental effort, that the power of a symbol does not depend on its being an exact replica. More than that: he will come to realize that symbols are powerful precisely because they do not replicate their objects.
>
> I believe that progress depends on our relearning that nursery lesson.

So, I guess this symbol talk is nothing new between us! (You see, I'm a very slow learner.)

15-04-09 *Two Letters to Track Down* (to H. C. von Baeyer)

Here are two letters that it might be interesting to look at in more detail: Pauli to Jaffé, Aug. 1954 [1865], PLC IV/2 Pauli to von Franz, 12 Nov. 1953 [1672], PLC IV/2.

I was intrigued by there being in the first a variation of the thing Pauli told Fierz about a "black mass":

> Suddenly I had a remarkable *feeling* experience. The "observation" of microphysics appeared to me to be a kind of black mass and I felt remorse. Remorse with regard to matter, which appeared to me to be a maltreated *living thing*. (Biological implication.) — The practice of this black "mass of measuring" in the external world transforms only *its* condition, not that of the observer. [Translation by Gieser, presumably.]

Second letter apparently says more about the black mass concept.

Hans's Reply

> Chris, [1865] is a very short postcard to Jaffé. It mentions the black mass, as did the long important letter to Fierz [1864] written on the same day.
>
> [1672] to von Franz does not mention the black mass. (It is nevertheless a useful letter because it spells out like none other the significance of the yin/yang in Pauli's thinking.)
>
> So far I have not seen other mentions of the black mass, but I have not searched. This is why volume 9 — online — would come in very handy!

15-04-09 *Some Similarity to Your Mumbles on Probability* (to D. M. Appleby)

See the description of the book below. It seems to have some similarity to an idea I've seen you groping for.

Happy holiday with Hulya.

> *Projective Probability*
>
> James Logue
>
> Description: This book presents a novel theory of probability applicable to general reasoning, science, and the courts. Based on a strongly subjective starting-point, with probabilities viewed simply as the guarded beliefs one can reasonably hold, the theory shows how such beliefs are legitimately "projected" outwards as if they existed in the world independent of our judgements.
>
> Product Details: 192 pages; tables; ISBN13: 978-0-19-823959-8 ISBN10: 0-19-823959-9
>
> About the Author: James Logue, Fellow and Tutor in Philosophy, Somerville College, Oxford; and Lecturer in Philosophy, Oxford University

15-04-09 *QBism, Certainty, and Norsen, 2* (to R. Schack)

By the way, the reason I was re-looking at Dawid originally is because I was thinking of how to respond to Lucien's passages below:

Hardyism 4: *From a mathematical point of view, any interpretation of probability that allows us to help ourselves to the basic mathematical properties of probabilities will suffice. These properties are that probabilities are positive, sum to one over mutually exclusive outcomes, and the validity of Bayes' rule for converting between joint and conditional probabilities. One approach that does this might be called the* best prediction available interpretation. *In this we interpret the probability to be the best prediction available of a theory for the empirical relative frequency (the ratio of times a particular outcome is seen to the total number of times the experiment is repeated when the latter is very large). The best prediction available interpretation asserts that the purpose of a theory is not to actually make exact predictions (i.e. we do not need to say exactly what the empirical relative frequency is for a given denominator) but to rather give the best prediction it can. This approach remains neutral with respect to what might be regarded as the deeper interpretation of probability but is consistent with such deeper interpretations. It is consistent with the idea that probability is a degree of belief as promoted by the Bayesians. It is also consistent with the idea that there is some deeper structure in the world supporting some objective property that might be called a propensity.*

Hardyism 5: *What about a "best prediction available" interpretation for probability. Probability is the best prediction available of a theory for the empirical relative frequency. We do not expect our theory to give a better prediction than this and we are content that nature only agrees so far with the prediction of theories. This is a sort of probability squared approach. Once we let go of the idea that a theory has to predict everything — even probabilistically — we are free to take such an approach. This "best prediction available" works well for the next definition. Best prediction available could be regarded as being like the degrees of belief idea but more radical. Alternatively, we might say that there is structure in the world such that the best prediction available is the prob. This now starts to sound like a propensity approach but where the propensity "resides" in the deeper structure rather than just the object itself. Interesting that the best prediction available approach can be interpreted as both the degrees of belief and the propensity interpretations. It is a sort of withholding commitment approach.*

I wanted to ask him, how would one know if one made the best prediction available? It could only be settled with respect to a prior, and now how do you interpret that prior? Something along those lines. But it should be fleshed out better. How would you respond?

When all is said and done though, I still harbor those old fears of being indefensible to someone like Dawid who writes:

> I have been justly chastised by the discussants for spreading alarm about the health of the body Bayesian. Certainly it has held together more successfully than any other theory of statistical inference, and I am not predicting its imminent demise. But no human creation is completely perfect, and we should not avert our eyes from its deficiencies. In its solipsistic satisfaction with the psychological self-consistency of the schizophrenic statistician, it runs the risk of failing to say anything useful about the world outside.

It probably doesn't help that yet again (after writing up those hand-written notes for Rob), Rob called the position solipsistic.

16-04-09 *QBism, Certainty, and Norsen, 3* (to N. D. Mermin)

I'm not sure I understood either of your remarks concerning page 2 and page 3. Could I ask you to try again? Particularly, I'm lost on this one:

Mermintion 176: *Probably what you mean to say is that only a minority thinks that the (well-known) fact that measured values do not pre-exist the act of measurement, is enough to undermine the view that locality has to abandoned.*

??? Please say it again in a different way.

Not unrelated: Have you seen `quant-ph/0601205v2` by Travis Norsen? Attached are my lecture notes contra that paper. You will find in them an unequaled clarity with respect to my usual writings. Anyway, the page 2 and 3 sentences you commented on were molded by the attitude I take in those lecture notes.

How long are you staying in Copenhagen? And we can't even get you to Waterloo for a minuscule week of conversation!

16-04-09 *QBism, Certainty, and Norsen, 4* (to N. D. Mermin)

Mermintion 177: *We're about to leave for a long weekend in Berlin, so it may be a while before I can reply. I'm surprised that you didn't understand what bothered me. So I guess it's not that you were being careless (as I had assumed) but that there is a real difference in point of view here.*

I am never careless in papers. I am sometimes wrong, but I am never careless.

Indeed!

16-04-09 *QBism, Certainty, and Norsen, 5* (to N. D. Mermin)

Mermintion 178: *I would never have suggested that you were careless in a paper. But I thought you had said that this was a preliminary draft. Why are you up so early in the morning?*

Chronic insomnia. It has gotten particularly acute in Canada.

OK, it's a draft. Mostly I want to understand how you found those few sentences confusing, and I did not understand you at your previous attempt.

Is it that I should change:

> But there is a small minority that thinks the abandonment of condition 2, that measured values pre-exist the act of measurement, is the more warranted conclusion and among these are the quantum Bayesians.

To something more like:

> But there is a small minority that thinks the abandonment of condition 2, that measured values pre-exist the act of measurement, at the same time as the absolute holding fast of condition 1, is the more warranted conclusion and among these are the quantum Bayesians.

Is that the sort of thing you thought needed fixing?

16-04-09 *QBism, Certainty, and Norsen, 6* (to N. D. Mermin)

Mermintion 179: *On page 3 you say*

> But there is a small minority that thinks the abandonment of condition 2, that measured values pre-exist the act of measurement, is the more warranted conclusion [to reach from Bell inequality violations]

This implies that were it not for violations of Bell inequalities, that small minority (along with the majority) would have believed that measured values pre-exist the act of measurement. That's just not so. Practically nobody believed that measured values pre-exist the act of measurement. They didn't believe it before Bell. They didn't believe it before EPR. EP&R were condemned as heretics for suggesting that measured values might pre-exist the act of measurement. It was almost a truism, that values were brought into existence by the act of measurement — that measurement outcomes were a joint manifestation of the system in combination with the process of measurement.

So I would say that what you should have said is

> But there is a small minority that thinks that the non-existence of values that pre-exist the act of measurement is enough to account for Bell inequality violations, without requiring the abandonment of condition 1).

But of course that only makes sense if you do not assert earlier on page 2 that measured values pre-existing the act of measurement is one of the two pillars of local realism. But indeed it is not a pillar. In Bell's original paper (and in EPR) it is a deduction from the EPR reality criterion. In the post-Bell era there are many assumptions much weaker than the EPR reality criterion. For example that joint distributions exist for the outcomes of all the experiments — the one you actually did and the ones you might have done but didn't — without making any commitment to whether such experiments reveal pre-existing values.

Or you can assume the "conditionally independent" form for the joint distributions implicitly used by CHSH and explicitly invoked by Jon Jarrett which I'm told goes all the way back to Reichenbach, though I've never found it there. That's what I was verbally trying to describe in my note to you yesterday.

Hope that helps. I don't think I'm just being perverse. I think most people in the Bell-EPR business will react in the same way I have done.

Time to run. More from Berlin, if we have wireless in the hotel room and I also have insomnia.

You are right, and now I understand. I was leaving too much out by not mentioning the EPR considerations. I will fix this. Good thing it was only a draft!! (I now understand I can still wear the label 'careless' when it comes to a draft.) Have a safe trip to Berlin.

16-04-09 *Gieser's Book, Preliminary and Completed Notes* (to H. C. von Baeyer)

Some things that interested me with the book, along with corresponding page numbers. Don't know whether it'd be of any use to you, but one never knows. Sadly, I've yet to get back to finishing the book. I hope to get back to it soon after returning from Paris. Before Paris, too busy; during Paris, I only want to think about Pure Experience. [Fragment from 09-04-09 note.] ...

OK, I got more active than I thought I would before Paris. Below is my complete set of points of interest with regard to Gieser's book. Strangely, I enjoyed almost the whole book, even though looking back on it now, I don't think I learned anything at all particularly. It's probably all just about acquiring a taste, somewhat like I once did for beer many years ago.

I believe there is at least one typo below from my previous partial compilation, but I haven't been able to re-find it.

I'll answer your questions on von Meyenn later today (or tomorrow morning).

Suzanne Gieser, *The Innermost Kernel: Depth Psychology and Quantum Physics. Wolfgang Pauli's Dialogue with C. G. Jung*

"he aroused the malice of the object" – 20, 21
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Gieser off track (seemingly); James rejected the very existence of the 'thing in itself' – 119
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Archetypes & prior probability – 292, 293
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'I spoke and thus saved my soul' – 298
Pauli on astrology vs I Ching – 298
Pauli evolving away from static worldview – 299
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Mathematics as aspect of nature – 308, 309
Weyl on numbers – 309
Automorphism & Fuchsian function – 310
Numbers as psychoid – 313
Number 3 associated with birth – 313
Yin-Yang and neutral monism – 323
quantum measurement and black mass – 323, 324, 342, 343
Quarternary system and tetractys – 330
Primas article – 337
Newton and alchemy – 338
Inner experiences as autonomous existences – 338, 339
Archetypes structure matter? – 347

17-04-09 *Pearls of Bayesianism* (to R. W. Spekkens)

This paper looks quite interesting to me:

`http://ftp.cs.ucla.edu/pub/stat_ser/r284-reprint.pdf`

I'd like to (read it and) discuss it, if you're interested, after I get back from Paris on the 28th. If he is right that "probability theory deals with beliefs about an uncertain, yet static world, while causality deals with [BELIEFS ABOUT] changes that occur in the world itself" (my crucial addition), then I'm half-Bayesian as well.

17-04-09 *Two Quotes Before I Forget* (to R. W. Spekkens)

I copied down two quotes yesterday that bear a little on our discussion last week. Let me record them:

1) From "Subjective Probability and Its Measurement," by J. M. Hampton, P. G. Moore and H. Thomas (J. Roy. Stat. Soc. A **136**, 21–42 (1973)):

> Bruno de Finetti believes there is no need to assume that the probability of some event has a uniquely determinable value. His philosophical view of probability is that it expresses the feeling of an individual and cannot have meaning except in relation to him.

2) From Dennis V. Lindley's comment on A. P. Dawid's article (J. Am. Stat. Assoc. **77**, 611–612 (1982)):

> The Bayesian, subjectivist, or coherent, paradigm is egocentric. It is a tale of one person contemplating the world and not wishing to be stupid (technically, incoherent). He realizes that to do this his statements of uncertainty must be probabilistic. This is important on its own for it rules out a large class of behavior patterns, like sampling-theory statistics, but is it enough? Once I coined the aphorism "Coherence is all." Was I right? It is when we consider two coherent Bayesians that new features arise. Does their egocentric behavior allow them to talk to one another? . . .

Particularly what caught my attention was the descriptor "egocentric," which I decided I like very much. There is a world of difference between "egocentric" (which is an accurate account of my proposed take on QM) and "solipsistic" (which is not).

26-04-09 *Server Config* (to D. M. Appleby)

Applebyism 30: *I am glad you and Emma are enjoying your time in Paris. I know what you mean. Strolling round big cities, with a long history behind them, does indeed give one insights one can get no other way. Just the intangible feel of the place, that one somehow picks up without even setting foot in a museum or gallery or cathedral or whatever. And Paris must be one of the most important foci in the whole of human history. So many of the things around us (the very existence of the United States) depend on events which took place in those few square miles.*

Hope you get well soon! I really liked that phrase "foci of human history"! Maybe that indeed is how the spirit sinks in. (I'm turning on to the word "spirit", by the way, since our soon-after-new-year's discussions; for instance, I now think Hans should use the labels "spirit" and "matter" on his Möbius strip logo.)

28-04-09 *Spirit and Matter* (to D. M. Appleby and H. C. von Baeyer)

Here was the quote I had wanted to send you last night, but I got too lazy to look it up (I was only up briefly from the jetlag).

It comes from the very beginning of William James's article "Does Consciousness Exist" (which I started to read for the third time while in Paris). Previously, I had not noticed any of this, but I suppose our recent discussions set me up for it this time.

> 'Thoughts' and 'things' are names for two sorts of object, which common sense will always find contrasted and will always practically oppose to each other. Philosophy, reflecting on the contrast, has varied in the past in her explanations of it, and may be expected to vary in the future. At first, 'spirit and matter,' 'soul and body,' stood for a pair of equipollent substances quite on a par in weight and interest. But one day Kant undermined the soul and brought in the transcendental ego, and ever since then the bipolar relation has been very much off its balance. The transcendental ego seems nowadays in rationalist quarters to stand for everything, in empiricist quarters for almost nothing. In the hands of such writers as Schuppe, Rehmke, Natorp, Munsterberg – at any rate in his earlier writings, Schubert-Soldern and others, the spiritual principle attenuates itself to a thoroughly ghostly condition, being only a name for the fact that the 'content' of experience is known. It loses personal form and activity – these passing over to the content – and becomes a bare Bewusstheit or Bewusstsein überhaupt of which in its own right absolutely nothing can be said.

28-04-09 *Spirit and Matter, 2* (to D. M. Appleby and H. C. von Baeyer)

Applebyism 31: *How did James answer the question?*

Isn't it a very peculiar question? Suppose someone were to write an article entitled "Does sleep exist?" or "Does perception exist?". Or how about "Does smelling exist?".

They wouldn't do it of course. Or, if they did, no one would publish it. But why not? If one thinks the existence of consciousness is doubtful, then surely the existence of perception is doubtful too. Isn't perceiving something more or less the same as to be conscious of something?

Gassendi's response to the Cartesian "cogito ergo sum" was to argue "I walk therefore I am". How if someone were to write an article entitled "Does walking exist?"

It should be taken in the spirit of de Finetti's provocative remark, "PROBABILITY DOES NOT EXIST." Just a tad further down than my quote, James writes:

> To deny plumply that 'consciousness' exists seems so absurd on the face of it – for undeniably 'thoughts' do exist – that I fear some readers will follow me no farther. Let me then immediately explain that I mean only to deny that the word stands for an entity, but to insist most emphatically that it does stand for a function. There is, I mean, no aboriginal stuff or quality of being, contrasted with that of which material objects are made, out of which our thoughts of them are made; but there is a function in experience which thoughts perform, and for the performance of which this quality of being is invoked. That function is knowing. 'Consciousness' is supposed necessary to explain the fact that things not only are, but get reported, are known. Whoever blots out the notion of consciousness from his list of first principles must still provide in some way for that function's being carried on.

This is the article where James introduces the idea that reality is made of a neutral stuff that is neither thought nor thing. But that thoughts and things are aspects of the neutral stuff (when taken in relation to other patches of the stuff).

07-05-09 *Bell's Inequality* (to B. Dreiss)

Thanks for your thoughtful note. The key to understanding that statement from my old paper with Asher Peres is in focusing on the part: "any objective theory giving experimental predictions identical to those of quantum theory". That means any theory that gives quantum states an objective level of reality, rather than a Bayesian-style significance (as I strongly support), will have this spooky action-at-a-distance character. Bell was correct on that point. Asher and I were putting such extensions of quantum theory in contrast to quantum theory itself.

Attached are a couple of pieces that help clarify this. One is a paper that I am presently constructing (it's about 80% complete) representing the state of the art with respect to our quantum-Bayesian research program. The opening section very much addresses your query, and among other things you'll find a very extensive collection of references there to do with the QBism program more generally. The other two files are copies of some lecture notes I wrote a few weeks back to do with the QBism *rejection* of nonlocality as the lesson to be learned from Bell inequality violations. The lesson instead is that quantum "measurements" are not measurements at all in the historic sense, but *actions* one can take on external systems that elicit *consequences*. Bell inequality violations negate not one bit the reality of those external systems. The external world is there whether you act upon it or not. Bell inequality violations only negate the pre-existent reality of the consequences to those actions: No actions on the external world, no consequence. That is the subject matter of quantum theory, from our take.

Now to Jaynes. You're right, I should have a specific reply to him. Many years ago—about 15!—Carl Caves (certainly a disciple of Jaynes in his own right) explained to me what was wrong with Jaynes's argument, and I haven't looked at that aspect of his work since. Jaynes is certainly one of my heroes, but that doesn't absolve him from making mistakes. I will read his paper carefully again and form a statement on it (and send it to you). But that will probably take me a good few weeks: I've got the present papers to pump out first, and a meeting each week for the next four to travel to!

I was born in Cuero, TX; do you know the town? Where do you live in Australia? I've been flying to the continent about once to twice a year lately, and I'll be in Sydney in December.

07-05-09 *Curious George* (to H. Price)

Lund? Your auto-reply says you're on "research leave". Any interesting enclaves of pragmatism there?

By the way, I gave a joint physics/philosophy colloquium in Rochester last month for Brad Weslake. It was a good time. But I had a funny encounter with him and Alyssa Ney when I mentioned that the term "block universe" originates with William James' 1882 article "On Some Hegelisms" (as far as I can tell). A) They didn't know that, but then B) they assured me that James had the *wrong* definition of a block universe!

07-05-09 *Obsession with a Footnote* (to H. Price)

That's what David Albert would say of my whole philosophy, I'm sure ...

I just haven't been able to get that damned footnote exactly right. Here's the latest attempt:

```
\footnote{See Footnote \ref{Misunderstandings} and Ref.\
\cite[Sec.~4.1]{Timpson08} for a sampling of instances of
opposition to our inchoate ontology, and see Refs.\
```

```
\cite{Nagel89,Dennett04,Price97} for details on the ``view from
nowhere'' / ``view from nowhen'' {\it weltanschauung\/} more
generally.}}
```

I think I'm finally feeling better about this one ... and wanted to share the moment!

08-05-09 *Your Talk at UWO* (to W. C. Myrvold)

Let me just give you the title and abstract from the new paper.

Quantum-Bayesian Coherence

> In a quantum-Bayesian take on quantum mechanics, the Born Rule cannot be interpreted as a rule for setting measurement-outcome probabilities from an objective quantum state. But if not, what is the role of the rule? In this talk, I will argue that it should be seen as an empirical addition to Bayesian reasoning itself. Particularly, I show how to view the Born Rule as a normative rule in addition to usual Dutch-book coherence. It is a rule that takes into account how one should assign probabilities to the consequences of various intended measurements on a physical system, but explicitly in terms of prior probabilities for and conditional probabilities consequent upon the imagined outcomes of a special counterfactual reference measurement. This interpretation is seen particularly clearly by representing quantum states in terms of probabilities for the outcomes of a fixed, fiducial symmetric informationally complete (SIC) measurement. We further explore the extent to which the general form of the new normative rule implies the full state-space structure of quantum mechanics. It seems to get quite far.

Initially the words "objective" and "counterfactual" were italicized, but I figured that would get in your way, and so I stripped it out.

Does that sound OK?

09-05-09 *The Excruciating Hangover* (to O. J. E. Maroney)

Below is the passage from Marcus I was thinking of. It was in the middle of a very lengthy (and not all that coherent) letter he wrote to Rob.

Attached also is a paper I'm writing at the moment. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] It still has a good way to go to being completed, but all the advertisements are already in place. Please see the final paragraph on page 7 (ending on page 8) and the two associated footnotes. Like my care to put the Martin Gardiner quote on realism at the beginning of my recent talks, I'll be writing that section with you in mind ... ☺

Excerpt from D. M. Appleby's note to R. W. Spekkens, 14 April 2009:

> I would like to stress that I really meant what I said about finding hidden variables interesting. I have been arguing in this negative way because I felt you were trying to force me in a direction I do not choose to go, and I felt obliged to give some reasons for resisting. But now that I have done so let me be a little more positive.
>
> Perhaps you ought to know that Chris and I do not think entirely alike. To some people, indeed, it seems that we think quite differently. When I was at PI in the summer

I had several lengthy discussions with Owen. At a fairly early stage in those discussions Owen exclaimed, in tones of incredulity, "are you aware that Chris goes around telling people that you are a quantum Bayesian?" It took many many beers, and some truly excruciating hangovers, to convince him that Chris was not misrepresenting me. And perhaps he never was fully convinced. To Chris and I it seems that the differences between us are of very minor importance. But that is not always how it seems to other people.

Anyway one of these minor differences is that, although I do not believe in hidden variables, I do find them interesting. Another difference is our previous intellectual history. Though never a true believer, I was for a long time very close to the hidden variables school of thought, to such an extent that people often assumed that I was a Bohmian. (And perhaps there was a sense in which I really was a Bohmian—for Bohm himself was not a Bohmian in the sense this term has come to have. In particular he didn't believe in hidden variables.) I was also a convinced anti-Copenhagenist. When the paper that Chris wrote with Peres "Quantum Mechanics Needs no Interpretation" first came out my sympathies were all with the critics. But then I actually met Chris, and that brought about a revolution in my thinking.

I should explain what exactly this revolution consisted in. I didn't suddenly decide that the reasons I had previously had for rejecting the Copenhagen Interpretation were invalid. On the contrary all the bad things I used to say about the Copenhagen Interpretation, before I met Chris, I continue to say now. I thought in the past, and I still do think that Bohr is impossibly obscure, and Heisenberg and Pauli not a lot better. I used to think, and I still do think that Bell was absolutely bang on the nail when he described the Copenhagen interpretation as "unprofessionally vague and ambiguous" (I am quoting from memory, and may not have reproduced his words exactly). Chris didn't cause me to see clarity where I had formally seen obscurity. But what he did bring about was a fundamental change in my attitude to that obscurity. Specifically he led me to think that buried in all the obscurity there were the germs of something true. Formerly I had seen the Copenhagen interpretation as dirt, fit only for the bin. But now I see it as soil, in which something good may eventually be got to grow.

12-05-09 *Read Me* (to R. F. Wachter)

Wachterism 1: *I'm looking for a few bullets about research directions in the area of "quantum-classical" computing & communications. Sorry for the vagueness. I could use a few — just what comes to mind as you read this.*

- (In the application of quantum cryptography) Quantum information knows when it has been read. (Or leaves a trail that it has been read.)
- Why is quantum information a better resource for communication channels than classical information?
- What powers quantum information's sensitivity to eavesdropping?
- Quantum communication is what you get when you build the world of components that are inherently sensitive to the touch. If you push, they go further than might have been expected.

Since I have no idea what you need, I don't know if you want questions or answers. Just making things up.

- Need formalism that makes quantum and classical information directly comparable, so that inherent differences are revealed in a direct way.
- Quantum information is sexy.
- *Mechanica Quantica Lex Cogitationis Est* (this is the quantum Bayesian version of *Semper Fi*)
- Quantum information is the way it is because the world is the way it is, but there is still much confusion on exactly what way the quantum world is.
- Quantum information exploits the fantastic creative power of quantum matter.
- The field of quantum information is just applied quantum foundations.

Give me more guidance, and I'll try to make up something more relevant.

- quantum cryptography protocols that are even more sensitive to eavesdropping than present protocols
- better quantum error correction through better understanding of the basic structure of quantum channels
- understanding geometry of quantum-state space is key to understanding the power and potential of quantum information processing

Do you say uncle yet?

12-05-09 *Found It!* (to R. Schack)

I finally found that damned William James quote I'd been wanting to use! It was in his "unpublished" manuscripts (and they apparently are not scanned to the web as so many of his other things are).

> Of every would be describer of the universe one has a right to ask immediately two general questions. The first is: "What are the materials of your universe's composition?" And the second: "In what manner or manners do you represent them to be connected?"

12-05-09 *Figure* (to R. Schack)

Have you looked at the whole paper now? If so, then I guess sit tight for a while.

I find myself completely burned out tonight. The most energy I've been able to muster is to start reading about the Bode-Miller objections, and James's 5 year(!) struggle with them. When it's our turn, we've got to do better! We at least have a leg up on him when it comes to 'certainty'. Strangely, he only seemed to get halfway there in his conception:

> When an event that is not yet actual is nevertheless *certain to occur* in [the] future, it is more than a bare possibility. We speak of its enjoying even now a *virtual or potential existence*.

Hopefully I'll wake up in the middle of the night and do some work on the paper. I still want to add a couple of points in Section 5.2. Add a $d = 3$ discussion to Section 5.5. I want to add another paragraph to 6.1. And then there's the dreaded Section 8. I have a vision that the paper should be 54 pages long.

15-05-09 *Emailing: 459164a* (to G. L. Comer)

At first I thought I was going to hate this article, but then I found that I loved it. I want to print it out when I get back to the office. I particularly like the phrase "their expectations about the consequences of their actions"—because I say that is exactly what a quantum state is.

I read the article (and am writing to you) in the back of a van returning me to Waterloo from the Toronto airport.

Free will in the back of a van ... I haven't done that since high school.

16-05-09 *Measurement Problem and the Reality of Wives* (to R. D. Gill, J. Emerson, A. Plotnitsky, and T. M. Nieuwenhuizen)

Dear Richard, Joseph, Arkady, and Theo,

Thanks for the entertaining banter. I found seven emails in my box when I returned from today's outing to buy hostas and suddenly felt very popular ... that is, *until* I actually read the notes and learned that I'm merely imaginary. Oh well, I guess I have to live with that.

But it reminded me of a story I wrote down for John Preskill several years ago, titled "The Reality of Wives." I reproduce it below for anyone interested. [See 21-07-01 note "The Reality of Wives" to A. J. Landahl & J. Preskill.]

Imaginary wishes,

17-05-09 *Reply to Several Things* (to K. Martin)

Martinism 2: *James told me you had over 50,000 mp3's, is that right? If so, when combined with your email collection and obvious love of books, one can begin to formulate interesting theories about you.*

53,694 to be precise, as of today at least. And 930 books. Finally, here's a number I've never tabulated. (It took me about 20 minutes to do it this morning.) Since May 1997, I apparently have written 31,675 emails. As you suspected, I like to keep lists.

20-05-09 *Measurement Problem and Dates* (to R. Schack)

I'm in London, ON, where I presented our paper to the philosophy dept yesterday. Wayne Myrvold had one point that may call for some modification to our Section 4.2. He complained that we are giving a diachronic reading to the Principal Principle, but that its raw statement (as a requirement on conditional probabilities) is a synchronic one. I think he's at least partially right. He points out that one would only adjust one's updated probability to the conditional if one thinks that being apprised of the value of a chance is actual "learning." I.e., it doesn't so shake up one's world picture that one adjusts for this new knowledge by something other than strict conditioning. That is certainly a technical point that is right in other contexts. On the other hand, I think the reading we gave the PP is the usual one: I.e., that *implicit* in it is the idea that knowing the chance is the pinnacle of knowledge, i.e., implicit in the concept is the diachronic usage of it. I want to go back to Lewis's original statements to see more precisely how he's using it. I hope it doesn't cause me too many annoying acrobatics in recomposing the paragraphs to reflect Wayne's point.

Another thing that must be rewritten somewhat is Section 4.1. When I was in Washington, Patrick Hayden pointed me to a later paper than the one we cite, where nonminimal (but still finite) two-designs are systematically constructed in every finite dimension. I believe they have an exponential number of outcomes though—I've got to get the reference—though that's not all that important. Anyway, we just have to admit straight up that it's the minimal two-designs we like. So, I'm going to shift the argument to emphasize that with larger two-designs, though the urgleichung holds, the priors must then be restricted to a lower dimensional subspace of their defining simplex. That is surely an unpalatable feature.

28-05-09 *PIAF Postdoc at UQ* (to G. J. Milburn)

Thanks for this. I will distribute it appropriately.

By the way, I could stand to get some references from you. Attached is a paper I'm constructing at the moment. [See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.] The references I need are for the middle paragraph of page 26, the one starting with "The most telling reason …":

> But the quantum-Bayesian view cannot abide by this. For, the essential point for a quantum-Bayesian is that there is no such thing as *the* quantum state. There are potentially as many states for a given quantum system as there are agents. And that point is not diminished by accepting the addition to coherence described in this paper. Indeed, it is just as with standard (nonquantum) probabilities, where their subjectivity is not diminished by normatively satisfying standard Dutch-book coherence.
>
> The most telling reason for this arises directly from quantum statistical practice. The way one comes to a quantum-state assignment is ineliminably dependent on one's priors. Quantum states are not god-given, but have to be fought for via measurement, updating, calibration, computation, and any number of related pieces of work. The only place quantum states are "given" outright—that is to say, the model on which much of the notion of an objective quantum state arises from in the first place—is in a textbook homework problem. For instance, a textbook exercise might read, "Assume a hydrogen atom in its ground state. Calculate …." But outside the textbook it is not difficult to come up with examples where two agents looking at the same data, differing only in their prior beliefs, will asymptotically update to distinct (even orthogonal) pure quantum-state assignments for the same system. Thus the basis for one's particular quantum-state assignment is always *outside* the formal apparatus of quantum mechanics.

I remember your once expressing very eloquently this point with respect to quantum process tomography. (It may have been at the Konstanz meeting, but it might have been elsewhere as well.) It would be nice to cite some of the nitty-gritty of coming to a particular quantum state assignment (how one must do tomography and calibration and stuff like that). Any suggestions?

Are you here at PI this week? If so, I could also just print this out and give you a copy.

29-05-09 *The Elusive Nature of the Quantum State* (to G. J. Milburn)

John Wheeler used to say, if you really want to get a job done, give it to a busy man! It looks people are using that philosophy in spades with you.

Milburnism 2 : *Your paper looks fascinating. I am not sure if I will get time today to give a considered response to your request, but I should be able to get back to you early next week. Would that be soon enough?*

It surely would. I've still got about five more pages of material to write in it anyway.

BTW, aside from the particular section I asked you the question about, if you want also to get a quick summary of what the paper is about (i.e., without reading the whole thing), just go directly to the figure on page 20. The point of the paper is that the equation

$$q(j) = (d+1)\sum_{i=1}^{d^2} p(i)r(j|i) - \frac{1}{d}\sum_{i=1}^{d^2} r(j|i)$$

holds some chance of being taken as the *single* postulate of quantum mechanics.

02-06-09 *Unprofessionally Vague and Ambiguous* (to D. M. Appleby)

How have things been going the last week? When you can muster the energy and thought, we should discuss your post-Växjö plans.

On an immediate matter: I've heard you use the phrase in the title of this note several times. I know it is something John Bell said, but can you give me the precise coordinates of it. In which essay does it appear. I have his unspeakable book at home, so I should be able to find it if just given a hint.

Using him, will be part of the completion of these paragraphs of mine:

> Of every would be describer of the universe one has a right to ask immediately two general questions. The first is: "What are the materials of your universe's composition?" And the second: "In what manner or manners do you represent them to be connected?"
>
> — William James, 1903–1904

> This paper has focussed on adding a new girder to the developing structure of quantum-Bayesianism ('QBism' hereafter). As such, we have taken much of the previously developed program for the background of the present efforts. If a reader wants to know the core arguments for why we choose a more 'personalist Bayesianism' rather than a so-called 'objective Bayesianism', she should refer to Refs. [17,20,55]. If she wants to know why a subjective, personalist account of certainty is crucial for breaking the impasse set by the EPR criterion of reality, she should refer to Refs. [21,55]. This paper was meant to get a new phase of the program off the ground without dwelling too much on the past.
>
> Still, fearing James' injunction, we know it is our duty to discuss anew one term of this paper that has so far been waived about most uncritically: It is 'measurement.' How can one really understand the proclamation 'Unperformed measurements have no outcomes!' in a deep, soul-satisfying way? Answering this question, we feel, is the first step toward characterizing "the materials of our universe's composition."
>
> Believe it or not, we take our cue from John Bell: Despite our liberal use of the term so far, the word 'measurement' should be banished from fundamental discussions of quantum mechanics.

04-06-09 *Tomorrow. Tonight?* (to K. Martin)

I'm sorry to hear this. [...]

Look, Keye, take care. Write a poem about this experience, and look for whatever insight it gives you on quantum mechanics (remembering that every single experience should be used to give insight on quantum mechanics).

10-06-09 *Typos and Experience* (to N. D. Mermin)

Sadly, I'm still working on that paper I showed you last. I put it on the shelf for a while, and now I'm back to it, bound and determined to get it posted by the end of the week. (There'll be smaller, twin paper after my visit with Rüdiger, titled "Quantum-Bayesian Decoherence" where we show how Zurek's thinking is *exactly* backwards, when seen through Bayesian eyes.)

I was working on Footnote 29 (where I mention you), when I found your typo. I might as well attach the latest version. I reworked the intro after the last comments you sent me. It was indeed crucial that I make explicit the EPR-criterion-of-reality business prior to Bell's argument, and I've done that now. Still, I suspect you won't like the new version either—for one thing, I didn't give any respect to all the alternatives to EPR-criterion that you mentioned.

Have a look at Figure 3 and the long caption; you might enjoy it. I notice that you too have been leaning on the word "experience" lately. At least in the paper that I cite in Fn 29, but also I ran across your *Phys Today* piece a couple days ago. [I wonder too whether you've yet appreciated the beauty of the second equation in the caption to Figure 2. You've never mentioned it if you have.]

Comments are still welcome, but you're probably tired of me.

More on my new views of "experience" below. [It concerns another one of my papers in the works, that one for a collection on "pragmatism and quantum mechanics."]

11-06-09 *The Paulian Idea, Full Frontal* (to R. Schack)

Regarding,

Schackcosm 117: *"From within any part, the future is undetermined" is exactly what a many worlder would say. It suggests that the evolution of the whole is determined, which is not at all what we want to say. Of course you point that out in the footnote. But there you say that there is no whole. Why do we have to take a view on whether there is a whole beyond saying that q.m. isn't about it if it exists? Why do we have to go into this discussion at all? Why not simply point out that from any agent's perspective, there is uncertainty, full stop? I also don't like "treating a part as an agent". We are not treating parts of the universe as agents. Agents are primitives at the moment.*

I think it is better to just write a better footnote, incorporating your remarks into that. The thing is, I do want to "take a view" ... that is the whole point of that and the next paragraph, and the previous two James quotes.

Let me try to finesse it all. Give me another shot. Perhaps "treating a part as" is not the best choice of words, but this whole last part of the discussion was to build toward James's (and Pauli's) "pure experience" ... where agenthood is not primitive, but rather one of two complementary aspects of something neutral.

Did you read the version where I had the Wheeler quote at the end?

11-06-09 *The Paulian Idea, Full Frontal, 2* (to R. Schack)

Did you see my "apologetic" comment on my `quant-ph` posting of "Delirium Quantum":

> Stuffed away in a conference proceedings for years, I finally worked up the nerve to post this because I needed to cite it in a paper with some proper equations. Stay posted.

Funny thing is I just got a letter from the editor of the *NeuroQuantology Journal* saying that they would like to publish the paper!!

Different worlds, different worlds!

11-06-09 *The Paulian Idea, Full Frontal, 3* (to R. Schack)

Schackcosm 118: *I don't understand "But perhaps this republican-banquet vision of the world that so seems to fit with a QBist understanding..." It's not even clear to me from this sentence if the republican-banquet vision is ours or not.*

It is certainly my present vision. I thought I had already declared enough ownership in the sentence: "It is almost as if one can hear in the very formulation of the Born Rule one of William James's many lectures on chance and indeterminism." ... which itself refers to urgleichung stuff a couple of sentences above.

Think like William James: absolute clarity might be the enemy of poetic flow.

12-06-09 *In Defense of a Nomenclature that is Psychologically Rather Mature* (to J. Barrett)

Thanks Jon. Your note actually pushed me over the edge in some way: Before I had always said "anti-realism box" to myself tongue in cheek. But now, knowing that it'd be a pleonasm, I'm starting to take it quite seriously. Actually, it really does capture the idea I was shooting for in Section 8!

BTW, attached is a more extended diatribe on why I don't like to call qm "nonlocal". Rüdiger and I plan to make that into a proper paper later in the year.

Jon's Preply

> "Indeed, it flavors almost everything they think of quantum mechanics, including the interpretation of the imaginary games or toy models they use to better understand quantum mechanics itself. Take the recent flurry of work on Popescu-Rohrlich boxes [5]. These are imaginary devices that give rise to greater-than-quantum violations of various Bell inequalities. Importantly, another common name for these devices is the term 'nonlocal boxes' [6]. Their exact definition comes via the magnitude of a Bell-inequality violation-which entails the non-pre-existence of values or a violation of locality or both-but the commonly used name opts only to recognize nonlocality. They're not called anti-realism boxes, for instance. The nomenclature is psychologically telling."

The phrase "nonlocal box" is made up of two words: "nonlocal" and "box". The second word here is intended to call to mind a "black box" — that is a device or process that admits an input, and produces an output, but whose internal workings are either unfathomed or unfathomable. By this light, the terminology does already recognise the non-preexistence of values as a possible interpretation, and "anti-realism box" would be a pleonasm.

More seriously: the paper looks very interesting and I'm looking forward to a better look. Will either of you be at the Reconstructing Quantum Theory conference at PI in August?

13-06-09 *Reasons for Rewriting Quantum Mechanics* (to A. S. Holevo)

I finally wrote down a paper version of the thoughts that were going through my head the last time I saw you. Let me call your attention to it: `http://arxiv.org/abs/0906.2187`. I hope that, being in paper form now, it will help convince you that what I have in mind is the very antithesis of a hidden-variable program. (I remember well the pain of your remarks at the end of my talk!)

I hope things are going well with you and that maybe we will have a chance to see each other a little when you are in Toronto.

13-06-09 *At Last, At Last!* (to G. M. D'Ariano)

I finally fulfilled the promise I made to you long ago: I finally posted a paper on the foundational significance of SICs. You can find it here: `http://arxiv.org/abs/0906.2187`.

It'll be great seeing you again this week; I'm just on the drive between Waterloo and Toronto on my way to Sweden.

13-06-09 *Still Seeking SICs* (to J. Bub & I. Pitowsky)

I have finally written up the stuff I was telling you about one evening at Bill Demopoulos's party. Since you seemed interested, let me give you a pointer to it: `http://arxiv.org/abs/0906.2187`. I hope maybe it'll give you a little food for thought, and show a little more why I resist thinking of quantum theory as a *generalization* of Bayesian probability theory.

I hope you are both doing well. I'm on my way to Sweden and plan to have a thoughtful midsummer there.

13-06-09 *Discussion Points* (to W. K. Wootters)

I'm so glad you're coming to PI this Fall. Do you yet know when you will arrive? It's going to be a fantastic year here, I deem: Beside you Howard Barnum, Richard Healey, and Steve Bartlett will also come. For different periods of time each, I suppose, though I do know that Howard is coming for a full year. Furthermore, we have another added treat: Marcus Appleby will come for a month-long visit every other month. So, I'm going to be one happy boy ping-ponging between all you guys.

I finally put down on paper my reasons for thinking SICs are important for quantum foundations, and I wonder if you might have an interest. If so, you can find the paper here: `http://arxiv.org/`

abs/0906.2187. Now that I think about it, I should have cited your "probability tables" paper in there, and I can think of exactly where (page 6). I'm sorry about that. It was surely the root of the root of the tree that led me up to this point. I'll fix it in the revised version. One thing is for sure though, I hope my enthusiasm for SICs in that paper is contagious enough that we'll have many long discussions on related items when you visit.

13-06-09 *Update* (to D. M. Appleby)

Thanks. I'll print your notes out and read them on my flight later today. We can discuss in Sweden.

Likewise, would you mind having a read of Section 8 (only section 8, don't worry about the rest) in my: http://arxiv.org/abs/0906.2187. I'd like to discuss some of that with you as well.

The other day when I was digging up Wheeler quotes for the above said Section 8, I ran across a little exchange between Wheeler and Richard Rorty. Wheeler had just given a talk where he explores the idea of taking the "elementary quantum phenomenon" as the fundamental building block of the universe. Rorty's question came afterward. What struck me was how prescient the question was respect to the title of your talk in Växjö. I thought you might enjoy:

> RORTY: What puzzles me is the degree to which the surprisingness of quantum physics is an empirical, and the degree to which it is a philosophical matter. That is, what I don't know enough to grasp is whether the philosophical overlay on science, the nineteenth century philosophical account of the nature of science, was so inadequate in itself that practically anything would make one doubt it. As it happened, quantum phenomena did. But it may have been so faked up that it merely needed a suggestion to collapse in favor of something like a pragmatist understanding of science. But I take it that Professor Wheeler feels that it is quite specific quantum results that simply contradict fundamental empirical theses which had been presupposed by the older theories. So the relation is much more direct.

13-06-09 *From Pragmatism to Pure Experience, 2* (to M. Bächtold)

I apologize again for keeping you waiting so long.

Yes, I am still working on the paper—intellectualistically, even if the writing has come to a temporary standstill. I can quite understand if you have well lost patience with me, but I really want to get this paper written in one form or another. If you need me to drop out, I can do that, but I am trying very hard to pull my quantum Bayesianism into an ontology, and until I get this paper out of my system, I feel it's all going to be clouds to me. I need to write this to clarify things to myself: It is a bit like giving birth. So, I would still like to have a contribution to your volume.

The bad thing is I've gotten all hung up over the Miller-Bode objections. They are as relevant in this context (the quantum context), I am discovering, as they ever were. They turned the skeletal manuscript I told you about below into a kind of scarecrow. Rüdiger Schack and I are going to meet June 18 and spend until July 3 intensely trying to come to a conclusion on this, and I very much hope a baby will be born.

Could you possibly give me until the end of August for a submission? To demonstrate some piece of my good faith that this is topmost on my mind, let me show you another paper much closer to completion (five more pages to go!) that may give you some sense of the one I'm trying to produce for you. The one for you would be an infinitely more philosophical piece (with little to no equations), but it would build off the last paragraph on page 7 / first paragraph of page 8, and particularly all of Section 8 (only 3/4 complete itself). Please read through those parts, even though the punchline is not included yet. The title of Section 8 is a direct allusion to my paper for you. I plan to get the present paper submitted to the archive this week before flying to Sweden Saturday. I just thought the writing there might give you some flavor of what is to come.

What I am aiming for is a pluralistic ontology something like "pure experience." Quantum measurement should be an instance of it. But it is so very painful to get the ideas straight.

I will live with whatever your opinion is.

13-06-09 *Measurements Generate Reality* (to A. Zeilinger)

I finally posted something proper on the foundational import of the representation of quantum states I was speaking of the last time I saw you: It's all about "Unperformed experiments have no outcomes." I hope you will enjoy it, as I know this is our greatest common interest. The paper can be found here `http://arxiv.org/abs/0906.2187`.

I suspect you guys just had a great conference in Vienna.

13-06-09 *Reply* (to H. C. von Baeyer)

von Baeyerism 29: *I have loaded my Kindle with* Pragmatism *for free, and I'm reading it with great pleasure. "Theories thus become instruments, not answers to enigmas, in which we can rest." So no rest for you!*

Good for you! I've always liked the continuation of that line: "We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid." What a slap in the face to rationalism. A good theory is not a description of what is there, fixed and unchangeable, but a *tool*, not different in kind from a hammer, for actually reshaping nature.

I'm as far as Boston now. I'm looking forward to plenty of insights this week. I don't know how it happens, but I always find being in Sweden shakes loose at least one new QBist thought.

13-06-09 *Abstract to QTRF5* (to I. Bengtsson)

Bengtssonism 2: *If possible I would be delighted to have you in "my" session; I have some premonitions about the contents.*

Your premonition is probably correct. You can find the full story here now: `http://arxiv.org/abs/0906.2187`.

I've just started my long journey to Sweden. I hope we get a good chance to discuss things there. Åsa told me that you may now have more sympathy for a "quantum mechanics within probability theory" (as opposed to "quantum mechanics as a generalization of probability theory") than you once had. I'd like to hear what you're thinking.

13-06-09 *Single-User Theory* (to J. Emerson)

Here's that note I started to construct but never finished. Also included are the attachments to have gone with it. I think at this stage I won't go ahead and finish the note after all. That's because part of the answer was superseded by my recent posting `http://arxiv.org/abs/0906.2187` which includes some of the material from the note below anyway. So, why don't you have a look at Section 1 and Section 8 in the posting, and the notes attached here. Maybe that'll at least give you enough information so that you can say with good reason that you do indeed disagree with "what Fuchs thinks about reality".

— — — — — — — — —

I'll respond quietly (aka, privately) to a couple of points in one of your emails. Happy Victoria Day, by the way. After this note, I'm out to work in the garden again.

Emersonia 1: *By the way, I was a student of Ballentine's and I agree almost entirely with his point of view. I'm not quite sure what Fuchs thinks about reality and I probably would disagree with it.*

If you're not sure what I think, how do you know that you would *probably* disagree with it? ☺

I believe of reality what Martin Gardner believes of it. The first attachment is a quote from him:

> The hypothesis that there is an external world, not dependent on human minds, made of *something*, is so obviously useful and so strongly confirmed by experience down through the ages that we can say without exaggerating that it is better confirmed than any other empirical hypothesis.

As for quantum mechanics, I believe it is the best tool we (each of us) presently have for seeing our way through our *encounters* with that world. Another way to put this particular point: I see quantum mechanics as a single user theory. I can use it, you can use it, she can use it. Any of us. And when any of us uses it, we are using it to better prepare for our own (personal) encounters with the world. We use it to make sure our *beliefs* about the consequences of those encounters are *consistent* with each other. It is a calculus for "consistifying" our beliefs.

Saying this much—but only this much—is not inconsistent with a Ballentine-Emerson program of trying to (or hoping to) complete quantum mechanics. At least as far as I can tell, it is not inconsistent with that program. For it could be that quantum mechanics is a multi-user theory as well, and that I, Chris, am ignoring an extra, rich layer of the theory. For this option to be had, those things that I take to be ostensibly personal encounters with the world, would have to be more public things.

13-06-09 *Once a Solipsist, Always a …* (to H. M. Wiseman)

Our old conversations have been on my mind again lately. (That's a good thing.) Anyway, to honor it, I made an oblique reference to you in my most recent posting `http://arxiv.org/abs/0906.2187`. A little challenge: Let's see if you can find yourself.

Howard's Reply

> A little challenge indeed. How many friends do you have left in Australia? ☺
>
> I don't think I would have said "Rubbish! … It would mean quantum mechanics collapses into a kind of solipsism—a theory that there is only the self." but rather something like "Yes indeed, and this crypto-solipsism is, as I think Einstein saw, why quantum mechanics is not complete, why it does not describe the world in which the

many observers live and make predictions etc, and why we should try to find a theory which does."

However, exactly what I would have said I could only judge after a careful reading of your paper rather than the cursory one needed to meet your challenge. I'll try to provide that sometime soon.

Thanks for the honour.

14-06-09 *Once a Solipsist, Always a . . . , 2* (to H. M. Wiseman)

. . . from a Heathrow lounge . . .

Wisemanism 35 : *How many friends do you have left in Australia?* ☺

Good point!

One point of language: I was a little worried that an Australian would ever use the word "rubbish". Rob Spekkens told me that Terry Rudolph would most certainly say "bullshit". But I could swear I've heard you say "rubbish" before (though I don't actually remember to what).

Howard's Reply

Probably. I'm more polite than Terry.

14-06-09 *Seeking a Home for the SIC'ening Bayesians* (to J. I. Cirac)

Rüdiger Schack and I just posted a rather long paper covering two subjects rather dear to me: 1) what quantum mechanics looks like in terms of symmetric info complete measurements (i.e., what I talked about during my last visit to you), and 2) what this all means for the "quantum Bayesian" interpretation of quantum mechanics that Caves, Schack, Peres, Appleby, etc., and I have been putting together for some time.

Here's a link: `http://arxiv.org/abs/0906.2187`.

May I ask you to have a look at it and give me an opinion on whether you think Rev. Mod. Phys. is an appropriate home for it? It's quite extensively referenced, both on things to do with SICs and things to do with the quantum Bayesians (pro and con). It's definitely partially review article, and no doubt "in depth" as your webpage calls for.

If it is a style of article you think worthwhile, we'll submit it to RMP and see what the referees say. Otherwise, we'll seek another home for it, no problem.

I'm glad to hear you'll be spending a little time at PI. Do you know when you will likely come in the coming year?

16-06-09 *Feeding Quantum Mechanics* (to B. C. van Fraassen)

You once wrote me this (seven years ago actually!):

van Fraassenism 13 : *Now when it comes to theories that give us probabilities, whether absolute or conditional, I'll agree with scientific realists that literally read they say that there are objective probabilities in nature. But accepting such a theory does not involve believing that. Rather it involves appointing the theory as an 'expert' for guidance of our subjective probabilities concerning observable events. The metaphor of 'expert' is cashed out (as by Haim Gaifman) as follows. Suppose* *that I appoint Peter as my expert on snuffboxes. That means for my subjective probability P and Peter's subjective probability q the constraint:*

$$P\Big(A \,|\, q(A) = x\Big) = x$$

with generalizations of this to intervals, odds, conditional probabilities, for statements A that are about snuff boxes.

Thus the issue of whether there are objective probabilities in nature or whether to believe in them is finessed: there are only the theory's probabilistic pronouncements accepted as input and my own subjective probabilities.

That is clearly not how you are approaching it overall. But perhaps there are connections? I'd like to know how the QM probabilities are fed into your subjective probability as a whole – I wonder if it will not be similar. After all, even if a quantum state is read as a compendium of probabilities, and you say something like "this material is in quantum state such and such", your own subjective probability function has a domain much larger than facts pertaining to this material.

I now have a very direct answer for you. (Sorry, sometimes it takes me a while!) If you have a small amount of time, I'd be very flattered if you'd look at my new paper to see what I mean: `http://arxiv.org/abs/0906.2187`. (The key idea is in the figure on page 20.) I hope it gives you some food for thought.

(By the way, it will actually be followed in a month or so with a sibling paper titled "Quantum-Bayesian Decoherence." In that one, we make use of your Reflection Principle to argue that Zurek's decoherence program is just exactly opposite the way a good Bayesian would think about what's going on.)

Hope life in California is treating you well.

16-06-09 *Latter-Day Wheeler* (to W. H. Zurek)

Here's that passage I was thinking of. I was wrong though, it's not from his December 2000 op-ed in the *New York Times*, but rather his 1998 autobiography.

> Many students of chemistry and physics, entering upon their study of quantum mechanics, are told that quantum mechanics shows its essence in waves, or clouds, of probability. A system such as an atom is described by a wave function. This function satisfies the equation that Erwin Schrödinger published in 1926. The electron, in this description, is no longer a nugget of matter located at a point. It is pictured as a wave spread throughout the volume of the atom (or other region of space).
>
> This picture is all right as far as it goes. It properly emphasizes the central role of probability in quantum mechanics. The wave function tells where the electron might be, not where it is. But, to my mind, the Schrödinger wave fails to capture the true essence of quantum mechanics. That essence, as the delayed-choice experiment shows, is *measurement*. A suitable experiment can, in fact, locate an electron at a particular place within the atom. A different experiment can tell how fast the electron is moving. The wave function is not central to what we actually know about an electron or an atom. It only tells us the likelihood that a particular experiment will yield a particular result. It is the experiment that provides the actual information.
>
> Measurement, the act of turning potentiality into actuality, is an act of choice, choice among possible outcomes. After the measurement, there are roads not taken. Before

> the measurement, all roads are possible—one can even say that all roads are being taken at once.

In the *New York Times* pieces, he only says this and doesn't say anything explicit on the wave function:

> My mentor, the Danish physicist Niels Bohr, made his peace with the quantum. His "Copenhagen interpretation" promulgated in 1927 bridged the gap between the strangeness of the quantum world and the ordinariness of the world around us. It is the act of measurement, said Bohr, that transforms the indefiniteness of quantum events into the definiteness of everyday experience.

The last paragraph of the more relevant quote expresses a world of difference between John's view at the time and standard many worlds (of Deutsch, Wallace, and the rest of the Oxford crowd), and one I thought you did good justice expressing in your *Physics Today* piece: Before the measurement "one can say all roads are being taken at once"; "after the measurement there are roads not taken". Most many-worlders—I'm thinking of Tegmark in particular—would probably dismiss that as a literary flourish, one that's not crucial for how John visualized things. The typical many-worlder would say, "Before measurement, all roads at once; after measurement, all roads at once. And anyway, what is this thing called measurement? Let's just get rid of it. To the extent that it's there, it's a messy concept that should be derived and secondary." For Wheeler, as far as I can tell—and I have read everything he's ever written—it held the most central place.

Anyway, I was very proud of your piece with Misner and Thorne, and thought it was just wonderful.

19-06-09 *Your Talks* (to D. M. Appleby)

From Hay-on-Wye; what a fantastic town!! Rüdiger and I have been discussing QBism, and I feel it is just a glorious day for the project. We'll be in the UK for the next three days, then 2 weeks together in Waterloo. I have this feeling really good things are going to happen.

Thanks for sending your talk. I read it last night before bed and definitely enjoyed it.

21-06-09 *Rainy Day Train* (to C. Eriksson)

Thanks for the note. Emma is 10 now, and Katie is 7. Attached is a recent picture of the two of them just after they got out of bed one morning. They're good kids; I'm very proud of them—all the time I tell them they'll "change the world".[201]

Just flying home from London. Between Sweden and now, I went book shopping in a strange little town in rural Wales. They have 35 bookstores, with nearly 3/4 million books between them! (I bought 27 books in point of fact.) A great vacation!

21-06-09 *Renege* (to Y. J. Ng)

It is no problem. I am just flying back home from London. Right after the meeting in Sweden, Rüdiger Schack and I took a little vacation to the town Hay-on-Wye in rural Wales. It's an amazing place: There are 35 second-hand bookshops there! And so I'm coming home with a

[201] Because the world is malleable and "ever not quite".

suitcase stuffed with 27 specimens for my library. Speaking of Rüdiger, you might enjoy our new paper: `http://arxiv.org/abs/0906.2187`. It tells a more complete version of the story I told you during your last visit.

Paul Frampton was at the same meeting that I was in Sweden. It was good seeing him again after all these years.

22-06-09 *More Thoughts* (to D. M. Appleby, H. C. von Baeyer and R. Schack)

1) Reading your note below,

and

2) thinking of the story where Wheeler commands the equations "Now fly!"

and

3) thinking of this quote that I copied into my first samizdat: "I forbade any simulacrum in the temples because the divinity that breathes life into nature cannot be represented."

and finally,

4) thinking of this passage from "my friend" James:

> I am as confident as I am of anything that, in myself, the stream of thinking (which I recognize emphatically as a phenomenon) is only a careless name for what, when scrutinized, reveals itself to consist chiefly of the stream of my breathing. The 'I think' which Kant said must be able to accompany all my objects, is the 'I breathe' which actually does accompany them. There are other internal facts besides breathing (intracephalic muscular adjustments, etc., of which I have said a word in my larger *Psychology*), and these increase the assets of 'consciousness,' so far as the latter is subject to immediate perception; but breath, which was ever the original of 'spirit,' breath moving outwards, between the glottis and the nostrils, is, I am persuaded, the essence out of which philosophers have constructed the entity known to them as consciousness. That entity is fictitious, while thoughts in the concrete are fully real. But thoughts in the concrete are made of the same stuff as things are.

I found myself asking Rüdiger how to say "primal breath" in German. He proposed: urhauch. (Now Marcus, try to pronounce that one!) The neutral stuff fighting it out at the republican banquet, and by way of that making our universe, is urhauch.

Just trying the word on for size.

Marcus's Preply

> I have just been thinking about your friend James.
>
> [. . .]
>
> Anyway, back to James, and pure experience. I take your point about what he meant, and I think perhaps I semi-agree with him. However, I have some significant qualifications.
>
> In the first place I think he chose the wrong name. Instead of "pure experience" it might be better to call it, say, "ur-stuff". Or perhaps "primal-stuff". The trouble with "experience" is that the name isn't really neutral, it seems to me. It is strongly biased on the mind side. Also, "experience" suggests (to me) that what we are talking about is all out on the surface. But I think that what we are talking about is deep: behind or

below what we see is loads of stuff that we can hardly guess at. "Experience" suggests to me a sheet of water only millimeters thick. But I think what we talking about is an ocean, and it goes way way down.

Both aspects go way way down. Not only the external matter aspect (the fact that quantum systems are unpredictable). But also the internal mind aspect.

In this room where I am now writing the principal cause of uncertainty is me (so far as the things I can observe without ancillary apparatus are concerned). I am pretty confident that the book on the table in front of me will still be there in 5 minutes time, unless I decide to move it. I am likewise pretty certain what the clock will read in 5 minutes time, unless I decide to adjust it. But I have no idea what I myself will be thinking/feeling/writing/doing in 5 minutes time. In a very real and immediate sense I am a mystery to myself.

(I would guess I will still be writing this in 5 minutes time, assuming I don't get up to stretch my legs. But what will I be writing? —It is almost as much a mystery to me who is writing it as it is to you who is reading it. The ideas are forming themselves as I write, in a way which, though not independent of my will—is in fact an expression of my will—is nevertheless far from being fully known to me.)

I prefer "ur-stuff" or "primal-stuff" to "pure experience" because I think it conveys this sense of something mysterious, not fully known.

Another objection to "pure experience" is that it doesn't convey the notion of agency. At least it doesn't to me. One just sits there and has experiences, like it or not. Experiencing, as I understand the word, is passive.

It is of course true that if you go into the neurophysiology of it experiencing things turns out to be far from passive. It is highly active, in fact. But I don't think that is in the ordinary language meaning of the word.

Perhaps "ur-stuff" doesn't have the connotation of agency either. But it does at least have the advantage that it doesn't really have any connotations at all, beyond the connotation of something elemental. Or even pre-elemental. It is anyone's guess what an ur-thing might do or feel or be.

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However, though I prefer "ur-stuff" or "primal-stuff" to "pure experience" I am still not totally happy. The term suggests something structureless. Something, in other words, like a vector (c.f. the slides for my talk) which owes all of its properties to its relations with other vectors, and has no intrinsic properties of its own, considered in isolation. And I don't like that because I think that as soon as you start to think that way you are going to end up with something which is like classical field theory, in as much as it invites the misunderstanding that what is in question is a complete description.

Perhaps I wouldn't be happy with any name at all (c.f. the Tao Te Ching: "it was from the nameless that Heaven and Earth sprang"). But I guess that doesn't get us very far.

At all events I think it is essential that we avoid the error of thinking that any of our terms denote structureless simples.

In this connection I very much liked your response to Bell's criticism of the way the concept of measurement is used in the Copenhagen Interpretation in section 8 of your paper. Bell, of course, wants a complete description of the measurement process, framed in the language of beables (as does Rob and many others). He is wrong about that. Not only is it OK to use terms which aren't defined in the way that Bell would like: it is absolutely essential that we do so.

We need in fact to recognize that it is not only necessary, but actually essential that we use terms which are similar to the terms of ordinary language in as much as there is no attempt to resolve them into atomic or other elemental components. No one thinks it necessary to define the meaning of the word "chair" by giving a detailed description at the atomic level. The word "chair" is defined relative to its human uses, as something one can sit on. Similarly with the word "measurement".

You do that in your latest paper, and I think you are absolutely right.

(I do have a question however. My own inclination is to take a measurement to be any process by which I or someone else acquires information. If I look at the chair opposite me, and see that it is about 6 inches from the window, then that is a measurement I am inclined to say. Similarly, if an astronomer tells me that such-and-such a white dwarf star has such-and-such mass, then that too is a measurement. Moreover I am tempted to say that I have measured the star [as opposed to saying that I have measured the astronomer] [because it is the star that I have acquired information about]. My question is: do you go along with that? And, if not, can you make it a bit clearer what you do count as a measurement?)

****

A quick point about pragmatism. I was meaning to tell you in Växjö that I am not totally unsympathetic to pragmatism.

The reason I believe quantum mechanics—the reason that I have thought it worth spending years of my life trying to understand it better—is that quantum mechanics works. I have always thought (not just now, but back in the days when I was sympathetic to Bohm) that the fact quantum mechanics works means that it must be in some sense true.

So I will agree with James that the concept of "truth" is intimately related to the concept of "what works". I also share James's conviction that we need to get away from the idea that knowing the truth equates to the possession of some kind of map.

My only disagreement is that I feel it can't be right to actually identify truth and "what works". I feel the concepts are logically distinct, and that it is important to keep them that way.

****

One last remark. The thought in my last email (about Wheeler-Feynman) grew out of a conversation I had with David Craig after my talk. A little to my surprise he actually liked the talk. He especially liked the point about the counter-factual element in the classical field concept, and the point that this means a classical field is much more similar to the Bayesian view of the wave-function than is generally realized. I had the impression that I had got to him there. However, he then asked what I would say about the space-time manifold. I think that is a good question. For of course as soon as you start thinking in space-time terms you do find yourself being driven in a block-universe direction. At any rate, I find myself being driven in that direction. The point about Wheeler-Feynman grew out of my attempts to find some way of getting round that difficulty. Trying to find some more appropriate way of thinking about space and time.

23-06-09 *Fragments* (to D. M. Appleby and H. C. von Baeyer)

Let me reply to a couple of your points.
To

Applebyism 32: *I do have a question however. My own inclination is to take a measurement to be any process by which I or someone else acquires information. If I look at the chair opposite me, and see that it is about 6 inches from the window, then that is a measurement I am inclined to say. Similarly, if an astronomer tells me that such-and-such a white dwarf star has such-and-such mass, then that too is a measurement. Moreover I am tempted to say that I have measured the star [as opposed to saying that I have measured the astronomer] [because it is the star that I have acquired information about]. My question is: do you go along with that? And, if not, can you make it a bit clearer what you do count as a measurement?*

and

von Baeyerism 30: *I'm still re-reading Section 8 and appreciate Marcus's question about measurement.*

here is the way I respond.

With every movement and every breath, I take actions upon the world surrounding me: These are actions with potentially unpredictable consequences for my experience, and actions that the rest of the world could not have "foreseen" coming either. With every breath I contribute to the ongoing making of the world. "Quantum measurement" refers to the special cases of this broad idea where one feels comfortable conceptualizing a distinct number of consequences arising from one's actions, imagines one can write down actual numerical expectations (probabilities) for those consequences, and indeed bothers to conceptualize the actions in the first place to some mathematical precision. In other words, "quantum measurement," as an idea, arises only in very controlled and introspective settings. But wash away the numerics and the need for mathematical precision, and it is something happening all the time, everywhere around us.

I like the way Misner, Thorne and Zurek put it in their *Physics Today* piece on John Wheeler last month. It carries at least part of what I was trying to say above (though "relation" sounds so much more static than what I, and I think John himself, was trying to get at). Let me copy it:

> Quantum mechanics in 1976 was the backbone of atomic, nuclear, and condensed-matter physics as well as quantum chemistry. Yet the essence of "the quantum" was a mystery that gripped Wheeler's attention; quantum measurement, he thought, was the crux of the mystery. Why? Because quantum measurement is only a euphemism for the relation between observers—us—and the rest of the physical universe. So quantum measurement, he said, is where "the quantum gets personal."

Getting a more precise handle on this cluster of ideas, I feel, is the next stage of our research. We are long from getting there, but that is the direction to start turning as we make smaller bits of progress.

Applebyism 33: *The Wheeler story and the quote from your samizdat are just so totally appropriate. I love them. (Where did you get the quote in your samizdat from, by the way?—Just to save me the trouble of looking it up).*

I got the quote many years ago (1997) while reading some crazy book by Jean Baudrillard, but I think he himself got it from another source. Let me see if I can find it on the web. ... Well, I didn't. Maybe Baudrillard made it up, but he put quotes around it as if it came from somewhere else.

Applebyism 34: *I am not so sure about the quote from James. I do think that introspection, considered as a route to understanding consciousness, is totally useless. And perhaps that is all that James is trying to say. But I must say that whenever I look into myself, in an effort to find out what consciousness is, I don't notice any particular connection with breathing. I don't notice a connection with anything else, mind you.*

You miss the point of the quote. It is that "knowing," for us, is a bodily function, neither more nor less sacred than breathing itself. It is that Descartes well should have said (at least for the track Descartes was pursuing), "Spiro, ergo sum." But in James's exact passage, Kant was his target. His point was that it is not "subject" that always accompanies "object," but that it is "breath."

24-06-09 *Those Einstein Quotes* (to W. H. Zurek)

Sorry for the delay: Here are those Einstein quotes I said I would send you. It turned out that I only had two of them in my computer in an acceptable form; the third and most important, that from the Schilpp volume, had only been an abridged version I use in my talks from time to time—so I wanted to add all the missing words before sending it on to you. Anyway, everything is collected in the attached `.pdf`.

It was good sparring with you the other day. We should do it more often—it'd certainly lead to more mutual understanding, which would be most useful for me.

Einstein's Personal Argument for the Incompleteness of Quantum Mechanics (a selection)

Einstein, in a letter to Erwin Schrödinger dated 19 June 1935, referring to the EPR paper:

> For reasons of language this [paper] was written by Podolsky after much discussion. Still, it did not come out as well as I had originally wanted; rather the essential thing was, so to speak, smothered by the formalism.

Now, to let Einstein speak for himself. Particularly note in the quotes below Einstein never invokes what has become known as the EPR criterion of reality: "If, without in any way disturbing a system, we can predict with certainty (i.e., with probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity." [See Arthur Fine's discussion in the Afterword to the Second Edition of his book *The Shaky Game*, where he has taken to calling it snidely the Podolsky Criterion. Fine also makes the point somewhere that these quotes are not atypical; never once after the EPR paper did Einstein use the precise argument of the paper.]

Einstein, in his "Autobiographical Notes" section of the 1949 Schilpp volume (with some translation details modified by Rüdiger Schack):

> If one asks: does a ψ-function of the quantum theory represent a real factual situation in the same sense in which this is the case of a material system of points or of an electromagnetic field, one hesitates to reply with a simple "yes" or "no"; why? What the ψ-function (at a definite time) asserts, is this. What is the probability for finding a definite physical magnitude q (or p) in a definitely given interval, if I measure it

at time t? The probability is here to be viewed as an empirically determinable, and therefore certainly as a "real" quantity which I may determine if I create the same ψ-function very often and perform a q-measurement each time. But what about the single measured value of q? Did the respective individual system have this q-value even before the measurement? To this question there is no definite answer within the framework of the theory, since the measurement is a process which implies a finite disturbance of the system from the outside; it would therefore be thinkable that the system obtains a definite numerical values for q (resp. p) the measured numerical value, only through the measurement itself. For the further discussion I shall assume two physicists, A and B, who represent a different conception with reference to the real situation as described by the ψ-function.

A. The individual system (before the measurement) has a definite value of q (resp. p) for all variables of the system, and more specifically, *that* value which is determined by a measurement of this variable. Proceeding from this conception, he will state: The ψ-function is no exhaustive description of the real situation of the system but an incomplete description; it expresses only what we know on the basis of former measurements concerning the system.

B. The individual system (before the measurement) has no definite value of q (resp. p). The measurement value only arises through the act of measurement in cooperation with the probability which is given to it in view of the ψ-function. Proceeding from this conception, he will (or, at least, he may) explain: the ψ-function is an exhaustive description of the real state of the system.

We now present to these two physicists the following instance: There is to be a system which at time t of our observation consists of two partial systems S_1 and S_2, which at this time are spatially separated and (in the sense of classical physics) are without significant reciprocity. The total system is to be completely described through a known ψ-function ψ_{12} in the sense of quantum mechanics. All quantum theoreticians now agree upon the following: If I make a complete measurement of S_1, I get from the results of the measurement and from ψ_{12} an entirely definite ψ-function ψ_2 of the system S_2. The character of ψ_2 then depends upon *what kind* of measurement I undertake on S_1.

Now it appears to me that one may speak of the real factual situation at S_2. Of this real factual situation, we know to begin with, before the measurement of S_1, even less than we know of a system described by the ψ-function. But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former. According to the type of measurement which I make of S_1, I get, however, a very different ψ_2 for the second partial system $(\psi_2, \psi_2^1, \ldots)$. Now, however, the real situation of S_2 must be independent of what happens to S_1. For the same real situation of S_2 it is possible therefore to find, according to one's choice, different types of ψ-function. (One can escape from this conclusion only by either assuming that the measurement of S_1 (telepathically) changes the real situation of S_2 or by denying independent real situations as such to things which are spatially separated from each other. Both alternatives appear to me entirely unacceptable.)

If now the physicists, A and B, accept this consideration as valid, then B will have to give up his position that the ψ-function constitutes a complete description of a real factual situation. For in this case it would be impossible that two different types of

ψ-functions could be coordinated with the identical factual situation of S_2.

The statistical character of the present theory would then have to be a necessary consequence of the incompleteness of the description of the systems in quantum mechanics, and there would no longer exist any ground for the supposition that a future basis of physics must be based on statistics.

Einstein in a 1952 letter to Michele Besso:

What relation is there between the "state" ("quantum state") described by a function ψ and a real deterministic situation (that we call the "real state")? Does the quantum state characterize completely (1) or only incompletely (2) a real state?

One cannot respond unambiguously to this question, because each measurement represents a real uncontrollable intervention in the system (Heisenberg). The real state is not therefore something that is immediately accessible to experience, and its appreciation always rests hypothetical. (Comparable to the notion of force in classical mechanics, if one doesn't fix *a priori* the law of motion.) Therefore suppositions (1) and (2) are, in principle, both possible. A decision in favor of one of them can be taken only after an examination and confrontation of the admissibility of their consequences.

I reject (1) because it obliges us to admit that there is a rigid connection between parts of the system separated from each other in space in an arbitrary way (instantaneous action at a distance, which doesn't diminish when the distance increases). Here is the demonstration:

A system S_{12}, with a function ψ_{12}, which is known, is composed of two systems S_1, and S_2, which are very far from each other at the instant t. If one makes a "complete" measurement on S_1, which can be done in different ways (according to whether one measures, for example, the momenta or the coordinates), depending on the result of the measurement and the function ψ_{12}, one can determine by current quantum-theoretical methods, the function ψ_2 of the second system. *This function can assume different forms*, according to the *procedure* of measurement applied to S_1.

But this is in contradiction with (1) *if one excludes action at a distance*. Therefore the measurement on S_1 has no effect on the real state S_2, and therefore assuming (1) no effect on the quantum state of S_2 described by ψ_2.

I am thus forced to pass to the supposition (2) according to which the real state of a system is only described incompletely by the function ψ_{12}.

If one considers the method of the present quantum theory as being in principle definitive, that amounts to renouncing a complete description of real states. One could justify this renunciation if one assumes that there is no law for real states—i.e., that their description would be useless. Otherwise said, that would mean: laws don't apply to things, but only to what observation teaches us about them. (The laws that relate to the temporal succession of this partial knowledge are however entirely deterministic.)

Now, I can't accept that. I think that the statistical character of the present theory is simply conditioned by the choice of an incomplete description.

Einstein in a 1948 letter to Walter Heitler:

[T]hat one conceives of the psi-function only as an incomplete description of a real state of affairs, where the incompleteness of the description is forced by the fact that observation of the state is only able to grasp part of the real factual situation. Then one can at least escape the singular conception that observation (conceived as an act of consciousness) influences the real physical state of things; the change in the psi-function through observation then does not correspond essentially to the change in a real matter of fact but rather to the alteration in *our knowledge* of this matter of fact.

24-06-09 *The Section 8 Interpretation of Agency* (to W. H. Zurek)

And by the way, speaking of the sparring, I really would appreciate it if you'd read through Section 8 of the paper I gave you (Sec 8 only, don't bother with the rest): `http://arxiv.org/abs/0906.2187`. I feel it's the best I've done yet for answering your perennial question, "So what is the observer made of?" My answer has always been, "Well he's made of the same things as you. And that is not greatly distinct in physical kind from what dogs, or amoebas, or even tables and chairs are made of." What goes wrong in your question, from my view, is that it goes backwards. "Agent" refers to a position within quantum theory, not to the detailed architecture of some specific types of quantum systems. Anyway, read my Section 8: I do myself better there.

24-06-09 *(again?)* (to S. Savitt)

Savittism 8: *[T]he quote from Will Durant in your newest paper is terrific. I am stealing that for my metaphysics course. Of course, I also am inclined to think that freedom in an indeterministic universe is also illusory.*

The Jamesian idea (which really goes back to / comes from Renouvier) is that freedom is a more basic concept than either determinism or indeterminism. Freedom is not something that subsists in one or the other kind of universe, but rather an expression of an ultimate kind of (ontological) pluralism, that can allow to some extent for both.

25-06-09 *Review of the Magic Equation, 2* (to C. R. Stroud, Jr., J. H. Eberly, A. Ney, Y. Shapir & B. Weslake)

Getting a note from Andrew Jordan on some APS GQI business reminded me of you all. I finally posted the paper associated with the talk I gave you guys. If you're interested, it's here: `http://arxiv.org/abs/0906.2187`. (I'll give a prize to the first person who finds the instances of "rubbish," "salacious," and "bastardization" in it.)

25-06-09 *Section 2* (to A. Y. Khrennikov)

I meant to tell you before leaving Sweden: Given our discussions on "contextuality", you might want to read my discussion in Section 2 of the paper I gave you. In case you've lost it, you can regain it here: `http://arxiv.org/abs/0906.2187`.

25-06-09 *Down with Unitarity* (to T. Rudolph, cc J. Rau)

I was talking with Jochen Rau the other day, and he told me about a result of his that was quite similar, if not identical, to your old one that any unitary operation can be simulated by a sequence of closely spaced measurements. I figure I should put you two in touch with each other.

(You're both addressed on this email.) If that result still hasn't been pulled out of the drawer, it'd be nice for it to appear out in the external world so more people can discuss it. (I'm also thinking of John Wheeler's words, which I dug up for Wojciech the other day. Read below.)

P.S. Terry, by the way, if you haven't seen this one of mine `http://arxiv.org/abs/0906.2187`, I'll give you a challenge: If you find all the instances of "womb," "rubbish," "salacious," and "bastardization" in it, I'll reward you with the first annual *Chris Fuchs Creepy Word Prize* the next time I see you, to be redeemed at the pub of your choice, either in London or Waterloo. (No cheating; Adobe search tool invalidates challenge.)

Wheeler quote below. [See 16-06-09 note "Latter-Day Wheeler" to W. H. Zurek.]

26-06-09 *Up, Up, and Away!* (to N. D. Mermin)

Rüdiger is here with me for two weeks; we're hard on the trail of what can be meant by "others' experiences" from a quantum-Bayesian view. Our first task is to get the simple idea of "communication" straight. Harder than you might think ... or at least harder than we might think.

26-06-09 *This and That* (to M. Tait)

I had several things I wanted to tell you by email soon after my last visit to London, but now that too much time has passed, I have forgotten most of the things! Sorry about that: But at least I finally write.

Here's a partial list.

1) The philosopher I was trying to remember who interprets Bohr in a very Kantian way is John Honner. The book I read was titled, *The Description of Nature: Niels Bohr and the Philosophy of Quantum Physics*. The main thing I remember is how the guy meticulously discussed something like more than 20 drafts of the Como lecture.

2) I finally posted the paper which I was speaking on at your seminar. You can find it here: `http://arxiv.org/abs/0906.2187`. You might enjoy Section 8 of it, where I turn to more philosophical matters, and lay my heart on the line, so to speak.

3) Attached is a hefty piece of paper, that's far, far from complete, but since you have some interest in these matters, I thought it might be of some use to you as a point of entry into some literature. You might have a look at the Folse entries in particular; he is my personal favorite Bohr interpreter.

Maybe we should get you to PI for some more extensive discussions. Any interest in dropping in for maybe a week or two in the Fall or Spring?

26-06-09 *Laws Workshop* (to S. Weinstein)

You said, women. What about some of these people:
Jane E. Ruby, who wrote:

- J. E. Ruby, "The Origins of Scientific 'Law'," J. Hist. Ideas **47**, 341–359 (1986).

Or Lorraine Daston, who co-edited:

- *Natural Law and Laws of Nature in Early Modern Europe.*

I don't know the people at all, but I do remember enjoying Ruby's article.

Here's another one: I think Cheryl Misak (from U. Toronto) would be excellent. She could talk on C. S. Peirce's conception of evolving laws, for instance. And what about Nancy Cartwright?

Moving away from women, what about Ian Hacking? I think he's always great. What about Shimony?

- A. Shimony, "Can the Fundamental Laws of Nature Be the Results of Evolution?," in *From Physics to Philosophy*, edited by J. Butterfield and C. Pagonis (Cambridge U. Press, Cambridge, 2000), pp. 208–223.

- Y. Balashov, "On the Evolution of Natural Laws," Brit. J. Phil. Sci. **43**, 343–379 (1992).

You see in my mind the ideas of "information theoretic laws" and "law without law" are fundamentally linked. That's how these particular names came to mind.

Arthur Fine?

And maybe from the other side of the fence: How 'bout Daniel Dennett?

Hope that's been of some help.

27-06-09 *Aczél* (to P. G. L. Mana)

Thanks; these references are useful.

Also let me take a moment to mend my evil ways: I was pretty rude, cutting Rüdiger off yesterday. Here is the idea he was trying to express to you, and I got in the way of his expressing it adequately. As Appleby says "probability is single case, or nothing" (see Marcus's paper `http://arxiv.org/abs/quant-ph/0408058`). That is, everything that can said of the meaning of probability applies to the single-case alone—consideration of long sequences of trials adds nothing to our foundational understanding of the subject. Similarly for all meaningful concepts within probability theory, and "expectation" is one of those. It is perfectly meaningful in the single case, and Dutch-book considerations (like in Section 2 of our `http://arxiv.org/abs/0906.2187`) can show that.

Consider a single lottery ticket of the form:

If outcome is 1, pay x_1 dollars.
If outcome is 2, pay x_2 dollars.
If outcome is 3, pay x_3 dollars.
Etc.
If outcome is n, pay x_n dollars.

where the outcomes $i = 1, ...n$ are a mutually exclusive exhaustive set, and place that in contrast to a set of other lottery tickets, n of them, each of the form

If outcome is i, pay 1 dollar.

The amounts one will freely buy or sell the latter tickets for define one's "probabilities" p_i for the events. The question now is at what price should one freely buy or sell the first (conglomerate) lottery ticket above for? The Dutch-book answer is E dollars, where

$$E = \sum_i p_i x_i \ .$$

If an agent buys or sells the lottery ticket for anything other than that amount, then a Dutch-bookie can force him to a *sure* loss of money *in a single trial*. No repetition of trials is needed at all.

And that single case consideration gives meaning to the concept of "expectation."

27-06-09 *Abstracts for Reconstructing Quantum Theory* (to P. Goyal)

Title: Quantum-Bayesian Coherence (or, My Favorite Convex Set)

Abstract: In a quantum-Bayesian delineation of quantum mechanics, the Born Rule cannot be interpreted as a rule for setting measurement-outcome probabilities from an objective quantum state. (A quantum system has potentially as many quantum states as there are agents considering it.) But what then is the role of the rule? In this paper, we argue that it should be seen as an empirical addition to Bayesian reasoning itself. Particularly, we show how to view the Born Rule as a normative rule in addition to usual Dutch-book coherence. It is a rule that takes into account how one should assign probabilities to the outcomes of various intended measurements on a physical system, but explicitly in terms of prior probabilities for and conditional probabilities consequent upon the imagined outcomes of a special counterfactual reference measurement. This interpretation is seen particularly clearly by representing quantum states in terms of probabilities for the outcomes of a fixed, fiducial symmetric informationally complete (SIC) measurement. We further explore the extent to which the general form of the new normative rule implies the full state-space structure of quantum mechanics. It seems to go some way.

Here are three papers associated with the approach:

- `http://arxiv.org/abs/0906.2187`
- `http://arxiv.org/abs/quant-ph/0205039`
- `http://arxiv.org/abs/quant-ph/0404156`

29-06-09 *'Acquiring Information' Is Not the Primary Idea* (to D. M. Appleby)

A short answer to your long letter!

Applebyism 35: *However, there are some points on which I do agree with Bell. One is that I think he was right to insist on the maximum possible degree of clarity and precision, and right* *again when he accused Bohr et al. of falling short of that standard. Hence my question. If the term "quantum measurement" refers to every process by which we acquire information then it is sufficiently sharp for me. But if it only refers to a certain subset of such processes the question arises: which particular subset, defined precisely how?*

I am not expecting you to answer this question here and now. Like I said, I am groping myself, and I appreciate that you are in the same position. I am only saying that this is, as it seems to me, a question which eventually has to be answered.

I thought I had answered that (even to some precision) with my lines:

> With every movement and every breath, I take actions upon the world surrounding me: These are actions with potentially unpredictable consequences for my experience, and actions that the rest of the world could not have "foreseen" coming either. With every breath I contribute to the ongoing making of the world.

At this point let me send you to some pages of my collection: Particularly three notes to Bas van Fraassen: "'Action' instead of 'Measurement'," on pages 550–551, "Questions, Actions, Answers, & Consequences," on pages 553–555, and "Canned Answers," on pages 555–557.

I can do the story of mutual information (in the second note above) better now, thinking of both random variables in terms of the consequences of actions (i.e., experiences), but the present words should be a decent start.

In short, I am answering you with the "refers to every process by which we" you say above; I just protest at the phrase "acquire information."

29-06-09 *Quantum Bayesianism + Book* (to M. Schlosshauer)

In a recent email David Mermin mentioned knowing you, and it caused me to remember how kind you were in Växjö. If I haven't already expressed it, thanks for the interest in QBism. If you have any further, good pointed questions, feel free to send them. The exercise is definitely good for me, whatever this research program ultimately morphs into.

By the way, I recall your questions mostly centered on "certainty." Let me forward you a note I wrote to Appleby this morning, where I address some of that (particularly in the third note to van Fraassen). [See 29-06-09 note to D. M. Appleby titled "'Acquiring Information' Is Not the Primary Idea."]

On another note, Rüdiger and I will soon write a much smaller companion piece to the last paper—this one titled "Quantum-Bayesian Decoherence"—where we try to put decoherence in QBist terms. When the time comes, I'll forward you a draft if you don't mind.

29-06-09 *Off-the-Shelf Ontology vs. the Republican Banquet* (to C. G. Timpson)

I was just talking to Rob Spekkens and he told me he hadn't heard back from you concerning the PIAF meeting in late September. I sure hope you can come! I know that you're supposed to be coming to the Reconstructing Quantum Theory workshop a month earlier, but I feel your insights will be needed even more at the PIAF meeting. Particularly, I would love a proper philosopher to give David Albert the "what for" (old Southern language) if he happens to show up and throws his usual dismissal of anything approaching an epistemic view of quantum states. It would be nice to have a balanced discussion for once. So, please, please, please do come; I so hope you'll find a way to make it happen.

On another note, I finally posted a summary of the things my little team has been working on for the last year. You can find it here if you haven't already seen it: `http://arxiv.org/abs/0906.2187`. I hope you will enjoy it (and see some real progress in it). In my usual way, I've included some footnotes that are bound to cause me to be banned from Oxford. On the more philosophical side, you might jump to Section 8, which can be read independently of the rest of the paper.

Rüdiger and I have quite a palette of things to write up this year, and I'm hopeful that we'll pull it all together. The next paper will be titled "Quantum-Bayesian Decoherence" (you can guess what that will be about), but the one following that will be "Quantum Bayesian Quantum Certainty Is Not a Moore Sentence" ... and you can guess what we'll try to argue there as well!

Anyway, please let me encourage you again to come to PIAF and help set the stage. Help keep me on the run, and maybe we'll both grow from the exercise! (It's in the reciprocity of our names!)

29-06-09 *Unpacking a Little Rhetorical Traction* (to H. M. Wiseman & E. G. Cavalcanti)

Sorry to keep you waiting so long for a reply. It would have come much more quickly if Howard hadn't thrown us for a real loop with his final note: It has been impressively difficult for us to decide whether Howard exists or not! We just didn't know what to answer, and all expediency of reply was lost at that point.

But we've somewhat recovered now. Let me tackle your notes in little spurts.

First and foremost I apologize that our little rhetorical flourish at the beginning of the paper—i.e., our two-paragraph statement on "local realism" designed to get some traction on the intro—would cause so much trouble and consternation. As you surely know, it had very little to do with the paper as a whole (issues to do with locality were not mentioned again). The trouble is localized, we believe, in your not being able to "read off" our intentions when it came to the phrase "reads off" (in quotes in the text). We certainly didn't mean the naive realism that Howard rightly points out would have been the setting up of a straw man. Instead, we meant very much [double quotes being Howard's own words]:

> 'realism' (in the sense of the theorem) = "measurement results are determined by inputs ... plus any number of other 'hidden' variables specified in the past of all measurements" *without recourse to* "the experimenter (who is assumed for this purpose to be outside the description of the physical system)"

Howard is right when he says,

Wisemanism 36: *I don't think Chris and Rüdiger's paper claims that* local causality *can be saved, only that* locality *can be saved. Whether* locality *is a concept worth saving is then a point for discussion.*

We do indeed think locality can be saved. And that is because we reject:

Wisemanism 37: *any agent who is a genuine realist, and who believe[s] he can predict with certainty the result of a measurement, must believe that there is a pre-existing value "out there" in the world. (This is the Bayesian-friendly version.)*

This is the point of our previous paper with Carl, "Subjective Probability and Quantum Certainty". Of course, you are free to define a "genuine realist" any way you like, and by rejecting this sentence we then become nongenuine realists. But realists, we believe we remain.

Attached are a set of notes I wrote up for one of our group meetings here a couple months ago. They represent my response to Norsen's paper "Bell Locality and the Nonlocal Character of Nature" (I keep talking about Eqs. (11) and (12) in that paper). The notes are most certainly tentative and not carefully formulated enough yet, but they represent the skeleton of a paper Rüdiger and I are going to write on the subject before the summer is out. Feel free to look over the notes with this proviso. Maybe they possibly shed some light on what we were thinking when we wrote our little bit of rhetorical traction in the posted paper.

Finally, with regard to,

Wisemanism 38 : *So in summary it seems to me ... that perhaps you don't appreciate the import of Bell's theorem.*

Bell's theorem is absolutely significant to us: Rejecting the EPR criterion of reality, while proposing to hold on to Einstein locality, Bell's theorem causes us to reject the particular definition of 'realism' in the equation above. That's pretty earth-shaking to us: I would never want to think we "minimize Bell's work"; we just take home a different conclusion than the usual one.

Thus ends my first installment. More replies later.

29-06-09 *Disturbing the Solipsist* (to H. M. Wiseman & E. G. Cavalcanti)

Wisemanism 39 : *I disagree with your idea that this formulation shows that QM probabilities have* more structure *than classical probabilities. I think it's just a different structure. I mean if we allow you to use SIC-POVMs as a tool, then surely we should allow a classical probabilist the concept of non-disturbing measurements as a tool. Thus I would argue that in exactly the same way that you have derived constraints on QM probabilities, we can "derive" the "constraint" (which is usually taken for granted but which you disavow) that $p(B) = \sum_A p(B|A)p(A)$ even when A is only a hypothetical event. You seem to argue this is illogical because A may disturb B. But I could equally claim that your SIC-POVM measurement could disturb the quantum system* more than is necessary*. If you are allowed minimally-disturbing SIC-measurements, then in the classical case you have to accept no disturbance.*

We do not argue that the law of total probability, i.e., $p(B) = \sum_A p(B|A)p(A)$, is "illogical". Only that it is a judgment that one may or may not make, when comparing the counterfactual upper path (which necessarily generates the $p(B)$ you mention, via Dutch-book coherence) to the 'factualized' lower one (which generates the probability $q(B)$ we discuss). We certainly don't bar any Bayesian from making the judgment that $q(B) = p(B)$; we only point out that one need not have to make it. In that sense, you are right, quantum theory is just a "different structure" (or rather a different addition to raw Bayesianism) ... *but* I don't believe we claimed anything stronger than that in the paper. Try reading that section again and see if you still find it coming out so overly strong.

Thus ends my second installment. More replies later.

29-06-09 *Forgotten Reason for the Title* (to H. M. Wiseman & E. G. Cavalcanti)

In case you were wondering about the last note's title, it was referring to:

Wisemanism 40: *Thus in the end, the difference comes down to the fact that quantum measurements necessarily induce disturbance (of our probabilities), which is indeed an old lesson in QM that we don't need Bell's theorem for.*

but I had forgotten to mention that.

29-06-09 *Eric's Note* (to E. G. Cavalcanti & H. M. Wiseman)

Thanks for defending me as a "metaphysical realist in the sense that he believes in the existence of a world outside of oneself." Rarely do I have a friend strong enough to tread those waters!

Cavalcanti-ism 2: *Now I think really Chris should say that being a Bayesian does not a priori entail anything about ontology whatsoever, since that is just a statement about epistemology.*

This is certainly true, and I would not have wanted to convey otherwise. This is why I have the attached slide prepared for my talks: It is meant to express that one can equally validly talk of one's Bayesian degrees of belief about "states of pre-existent reality" as one can talk of one's Bayesian degrees of belief about the "consequences of one's actions." Nothing about the *raw* structure of probability theory changes in the move from one subject matter to the other. It just so happens—from our point of view—that quantum mechanics represents a use of the latter type.

But that is Bayesianism per se. It still leaves me room to disagree with this:

Cavalcanti-ism 3: *It is just saying that QM is a theory of epistemology, and that this leaves open the question of ontology.*

And that is because I don't view QM as an expression of raw probability theory. It is *dressed* so to speak with an extra normative assumption concerning how to gamble on factuals in terms of counterfactuals (the urgleichung being one such example), and consequently gives in part a theory of priors (i.e., the convex set of states on the simplex). I imagine the adoption of this dressing as a statement supervening on an ontology yet to be completely fleshed out. So, QM as a theory of decisions still has an essential empirical component.

Cavalcanti-ism 4: *I think Chris seems to want to say something about ontology, and I think he really wants to say something that sounds like a relational ontology. (Whatever that is). Maybe it would be easier if he were explicit about it?*

You don't much (if ever) hear me say things about "relational ontologies" because right now that doesn't seem like the right direction to develop to me. This is one of the reasons I was so intrigued by our first conversation together: You seemed to get that distinction in my thought. You asked something like, "Do two quantum Bayesians live in the same spacetime?," and I answered something (mystical) like, "Only when they interact with each other." More seriously, I am much taken with the image of a "republican banquet" kind of ontology, like that expressed in the two quotes by James and the one by Durant in Section 8.1 of the paper. Relational ontologies, to the extent that anyone has sketched any detail in them to me, always seem to rely on an image of a static, block-world lacking any good notion of struggle and creation. Things just are, though now the things that are are relations between nodes rather than facts residing on nodes. That's not a picture that attracts me much. The other night Rüdiger and I were semi-jokingly saying that the dimension d of a quantum object quantifies how much "will" it has. But, maybe some variant of that will not turn out to be so much of a joke after all.

Thus ends my third installment. Tomorrow comes the difficult one: Whether Howard Wiseman indeed exists.

I bid you good night.

29-06-09 *Relationalism vs. the Republican Banquet* (to C. Rovelli)

Rob Spekkens walked into my office a few minutes ago with some very disappointing news: You won't be coming to our PIAF meeting this year, just as you didn't come last year. I suspect you won't reconsider, but I want you to anyway: If you want the seeds of relationalism you have planted in quantum interpretation to thrive and grow, there is no more fertile ground than here at PI. We really, truly need to hear your voice on the subject. Discussion would proceed so much faster that way. Please do reconsider!

It's funny too that Rob walked in as I had just sent off the note below to a few colleagues who are reading a paper I posted a couple of weeks ago: `http://arxiv.org/abs/0906.2187`. [See 29-06-09 note "Eric's Note" to E. G. Cavalcanti & H. M. Wiseman.] I don't know that you would be interested in the whole thing (the paper), but you might find some amusing things in Sections 8 and 8.1 with regard to the perennial issues. The note below addresses whether the ontology I'm shooting for is a relational one. At the moment, I don't see that it would be, as Eric Cavalcanti had suggested. I'd be curious to get your reaction if you find anything you can pinpoint. And it'd be better if I could finally lure you here to hear it in person!

30-06-09 *A New Name for Some Old Ways of Thinking* (to M. Schlosshauer)

Schlosshauerism 1: *In your note to van Fraassen entitled "Canned Answers," you mention a file* `certainty.pdf`*. Might this be helpful to me too in bringing about further enlightenment?*

I'll do better than that: Let me point you to pages 330–335 of "My Struggles"—you'll find there a couple of notes titled "Utter Rubbish and Internal Consistency" (Parts I and II). The long de Finetti quote is in there, but also I bear my soul and pay penance for the greatest mistake of my Bayesian life. It's a bit embarrassing really ... but maybe there is something to be learned from it.

Schlosshauerism 2: *I believe you know everything you need to know about decoherence, but if there's anything I can help answer or comment on, let me know. Also, if you ever would like a (free) copy of my book on decoherence, I'm happy to have Springer send you one.*

Yes, that would be most helpful for the upcoming project. I would be very proud to have a copy! You can use the address below.

Schlosshauerism 3: *Finally, I'm quite interested in getting into some William James. If you had to choose and recommend just one of his books, which one would it be?*

That's an easy choice. It is the book titled *Pragmatism*. It conveys all the spirit, while leaving aside all the dry technical arguments (of Dewey and Schiller predominantly). Below is a collection my favorite quotes from the book.

Happy reading, and tell me what you think in the end.

W. James, "The Present Dilemma in Philosophy," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

> For a hundred and fifty years past the progress of science has seemed to mean the enlargement of the material universe and the diminution of man's importance. The result is what one may call the growth of naturalistic or positivistic feeling. Man is no lawgiver to nature, he is an absorber. She it is who stands firm; he it is who must accommodate himself. Let him record truth, inhuman though it be, and submit to it! The romantic spontaneity and courage are gone, the vision is materialistic and depressing. Ideals appear as inert by-products of physiology; what is higher is explained by what is lower and treated forever as a case of 'nothing but'—nothing but something else of a quite inferior sort. You get, in short, a materialistic universe, in which only the tough-minded find themselves congenially at home.

W. James, "What Pragmatism Means," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

> Metaphysics has usually followed a very primitive kind quest. You know how men have always hankered after unlawful magic, and you know what a great part in magic *words* have always played. If you have his name, the formula of incantation that binds him, you can control the spirit, genie, afrite, or whatever the power may be. Solomon knew the names of all the spirits, and having their names, he held them subject to his will. So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's principle, and to possess it is after a fashion to possess the universe itself. 'God,' 'Matter,' 'Reason,' 'the Absolute,' 'Energy,' are so many solving names. You can rest when you have them. You are at the end of your metaphysical quest.
>
> But if you follow the pragmatic method, you cannot look on any such word as closing your quest. You must bring out of each word its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid. Pragmatism unstiffens all our theories, limbers them up and sets each one at work.

W. James, "Some Metaphysical Problems Pragmatically Considered," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

Let me take up another well-worn controversy, *the free-will problem.* Most persons who believe in what is called their free-will do so after the rationalistic fashion. It is a principle, a positive faculty or virtue added to man, by which his dignity is enigmatically augmented. He ought to believe it for this reason. Determinists, who deny it, who say that individual men originate nothing, but merely transmit to the future the whole push of the past cosmos of which they are so small an expression, diminish man. He is less admirable, stripped of this creative principle. I imagine that more than half of you share our instinctive belief in free-will, and that admiration of it as a principle of dignity has much to do with your fidelity.

But free-will has also been discussed pragmatically, and, strangely enough, the same pragmatic interpretation has been put upon it by both disputants. You know how large a part questions of *accountability* have played in ethical controversy. To hear some persons, one would suppose that all that ethics aims at is a code of merits and demerits. Thus does the old legal and theological leaven, the interest in crime and sin and punishment abide with us. 'Who's to blame? whom can we punish? whom will God punish?—these preoccupations hang like a bad dream over man's religious history.

So both free-will and determinism have been inveighed against and called absurd, because each, in the eyes of its enemies, has seemed to prevent the 'imputability' of good or bad deeds to their authors. Queer antinomy this! Free-will means novelty, the grafting on to the past of something not involved therein. If our acts were predetermined, if we merely transmitted the push of the whole past, the free-willists say, how could we be praised or blamed for anything? We should be 'agents' only, not 'principals,' and where then would be our precious imputability and responsibility?

But where would it be if we *had* free-will? rejoin the determinists. If a 'free' act be a sheer novelty, that comes not *from* me, the previous me, but *ex nihilo*, and simply tacks itself on to me, how can *I*, the previous I, be responsible? How can I have any permanent *character* that will stand still long enough for praise or blame to be awarded? The chaplet of my days tumbles into a cast of disconnected beads as soon as the thread of inner necessity is drawn out by the preposterous indeterminist doctrine. Messrs. Fullerton and McTaggart have recently laid about them doughtily with this argument.

It may be good *ad hominem*, but otherwise it is pitiful. For I ask you, quite apart from other reasons, whether any man, woman or child, with a sense for realities, ought not to be ashamed to plead such principles as either dignity or imputability. Instinct and utility between them can safely be trusted to carry on the social business of punishment and praise. If a man does good acts we shall praise him, if he does bad acts we shall punish him,—anyhow, and quite apart from theories as to whether the acts result from what was previous in him or are novelties in a strict sense. To make our human ethics revolve about the question of 'merit' is a piteous unreality—God alone can know our merits, if we have any. The real ground for supposing free-will is indeed pragmatic, but it has nothing to do with this contemptible right to punish which has made such a noise in past discussions of the subject.

Free-will pragmatically means *novelties in the world*, the right to expect that in its deepest elements as well as in its surface phenomena, the future may not identically repeat and imitate the past. That imitation *en masse* is there, who can deny? The general 'uniformity of nature' is presupposed by every lesser law. But nature may be only approximately uniform; and persons in whom knowledge of the world's past has bred pessimism (or doubts as to the world's good character, which become certainties if that character be supposed eternally fixed) may naturally welcome free-will as a *melioristic* doctrine. It holds up improvement as at least possible; whereas determinism assures us that our whole notion of possibility is born of human ignorance, and that necessity and impossibility between them rule the destinies of the world.

Free-will is thus a general cosmological theory of *promise*, just like the Absolute, God, Spirit or Design. Taken abstractly, no one of these terms has any inner content, none of them gives us any picture, and no one of them would retain the least pragmatic value in a world whose character was obviously perfect from the start. Elation at mere existence, pure cosmic emotion and delight, would, it seems to me, quench all interest in those speculations, if the world were nothing but a lubberland of happiness already. Our interest in religious metaphysics arises in the fact that our empirical future feels to us unsafe, and needs some higher guarantee. If the past and present were purely good, who could wish that the future might possibly not resemble them? Who could desire free-will? Who would not say, with Huxley, 'let me be wound up every day like a watch, to go right fatally, and I ask no better freedom.' 'Freedom' in a world already perfect could only mean freedom to *be worse*, and who could be so insane as to wish that? To be necessarily what it is, to be impossibly aught else, would put the last touch of perfection upon optimism's universe. Surely the only *possibility* that one can rationally claim is the possibility that things may be *better*. That possibility, I need hardly say, is one that, as the actual world goes, we have ample grounds for desiderating.

Free-will thus has no meaning unless it be a doctrine of *relief*. As such, it takes its place with other religious doctrines. Between them, they build up the old wastes and repair the former desolations. Our spirit, shut within this courtyard of sense-experience, is always saying to the intellect upon the tower: 'Watchman, tell us of the night, if it aught of promise bear,' and the intellect gives it then these terms of promise.

Other than this practical significance, the words God, free-will, design, etc., have none. Yet dark tho they be in themselves, or intellectualistically taken, when we bear them into life's thicket with us the darkness *there* grows light about us. If you stop, in dealing with such words, with their definition, thinking that to be an intellectual finality, where are you? Stupidly staring at a pretentious sham! "Deus est Ens, a se, extra et supra omne genus, necessarium, unum, infinite perfectum, simplex, immutabile, immensum, aeternum, intelligens," etc.,—wherein is such a definition really instructive? It means less than nothing, in its pompous robe of adjectives. Pragmatism alone can read a positive meaning into it, and for that she turns her back upon the intellectualist point of view altogether. 'God's in his heaven; all's right with the world!'—*That's* the real heart of your theology, and for that you need no rationalist definitions.

Why shouldn't all of us, rationalists as well as pragmatists, confess this? Pragmatism, so far from keeping her eyes bent on the immediate practical, foreground, as she is accused of doing, dwells just as much upon the world's remotest perspectives.

See then how all these ultimate questions turn, as it were, upon their hinges; and from looking backwards upon principles, upon an *erkenntniss-theoretische Ich*, a God, *a Kausalitätsprinzip*, a Design, a Free-will, taken in themselves, as something august and exalted above facts,—see, I say, how pragmatism shifts the emphasis and looks forward into facts themselves. The really vital question for us all is, What is this world going to be? What is life eventually to make of itself? The centre of gravity of philosophy must therefore alter its place. The earth of things, long thrown into shadow by the glories of the upper ether, must resume its rights. To shift the emphasis in this way means that philosophic questions will fall to be treated by minds of a less abstractionist type than heretofore, minds more scientific and individualistic in their tone yet not irreligious either. It will be an alteration in 'the seat of authority' that reminds one almost of the protestant reformation. And as, to papal minds, protestantism has often seemed a mere mess of anarchy and confusion, such, no doubt, will pragmatism often seem to ultra-rationalist minds in philosophy. It will seem so much sheer trash, philosophically. But life wags on, all the same, and compasses its ends, in protestant countries. I venture to think that philosophic protestantism will compass a not dissimilar prosperity.

W. James, "The One and the Many," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

It is possible to imagine alternative universes to the one we know, in which the most various grades and types of union should be embodied. Thus the lowest grade of universe would be a world of mere *withness*, of which the parts were only strung together by the conjunction 'and.' Such a universe is even now the collection of our several inner lives. The spaces and times of your imagination, the objects and events of your day-dreams are not only more or less incoherent *inter se*, but are wholly out of definite relation with the similar contents of any one else's mind. Our various reveries now as we sit here compenetrate each other idly without influencing or interfering. They coexist, but in no order and in no receptacle, being the nearest approach to an absolute 'many' that we can conceive. We can not even imagine any reason why they *should* be known all together, and we can imagine even less, if they were known together, how they could be known as one systematic whole.

But add our sensations and bodily actions, and the union mounts to a much higher grade. Our *audita et visa* and our acts fall into those receptacles of time and space in which each event finds its date and place. They form 'things' and are of 'kinds' too, and can be classed. Yet we can imagine a world of things and of kinds in which the causal interactions with which we are so familiar should not exist. Everything there might be inert towards everything else, and refuse to propagate its influence. Or gross mechanical influences might pass, but no chemical action. Such worlds would be far less unified than ours. Again there might be complete physico-chemical interaction, but no minds; or minds, but altogether private ones, with no social life; or social life limited to acquaintance, but no love; or love, but no customs or institutions that should systematize it. No one of these grades of universe would be absolutely irrational or disintegrated, inferior tho it might appear when looked at from the higher grades. For instance, if our minds should ever become 'telepathically' connected, so that we knew immediately, or could under certain conditions know immediately, each what the other was thinking, the world we now live in would appear to the thinkers in that world to have been of an inferior grade.

With the whole of past eternity open for our conjectures to range in, it may be lawful to wonder whether the various kinds of union now realized in the universe that we inhabit may not possibly have been successively evolved after the fashion in which we now see human systems evolving in consequence of human needs. If such an hypothesis were legitimate, total oneness would appear at the end of things rather than at their origin. In other words the notion of the 'Absolute' would have to be replaced by that of the 'Ultimate.' The two notions would have the same content—the maximally unified content of fact, namely—but their time-relations would be positively reversed.

After discussing the unity of the universe in this pragmatic way, you ought to see why I said in my second lecture, borrowing the word from my friend G. Papini, that pragmatism tends to *unstiffen* all our theories. The world's oneness has generally been affirmed abstractly only, and as if any one who questioned it must be an idiot. The temper of monists has been so vehement, as almost at times to be convulsive; and this way of holding a doctrine does not easily go with reasonable discussion and the drawing of distinctions. The theory of the Absolute, in particular, has had to be an article of faith, affirmed dogmatically and exclusively. The One and All, first in the order of being and of knowing, logically necessary itself, and uniting all lesser things in the bonds of mutual necessity, how could it allow of any mitigation of its inner rigidity? The slightest suspicion of pluralism, the minutest wiggle of independence of any one of its parts from the control of the totality would ruin it. Absolute unity brooks no degrees,—as well might you claim absolute purity for a glass of water because it contains but a single little cholera-germ. The independence, however infinitesimal, of a part, however small, would be to the Absolute as fatal as a cholera-germ.

Pluralism on the other hand has no need of this dogmatic rigoristic temper. Provided you grant *some* separation among things, some tremor of independence, some free play of parts on one another, some real novelty or chance, however minute, she is amply satisfied, and will allow you any amount, however great, of real union. How much of union there may be is a question that she thinks can only be decided empirically. The amount may be enormous, colossal; but absolute monism is shattered if, along with all the union, there has to be granted the slightest modicum, the most incipient nascency, or the most residual trace, of a separation that is not 'overcome.'

Pragmatism, pending the final empirical ascertainment of just what the balance of union and disunion among things may be, must obviously range herself upon the pluralistic side. Some day, she admits, even total union, with one knower, one origin, and a universe consolidated in every conceivable way, may turn out to be the most acceptable of all hypotheses. Meanwhile the opposite hypothesis, of a world imperfectly unified still, and perhaps always to remain so, must be sincerely entertained. This latter hypothesis is pluralism's doctrine. Since absolute monism forbids its being even considered seriously, branding it as irrational from the start, it is clear that pragmatism must turn its back on absolute monism, and follow pluralism's more empirical path.

This leaves us with the common-sense world, in which we find things partly joined and partly disjoined.

W. James, "Pragmatism and Common Sense," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

But the scientific tendency in critical thought, tho inspired at first by purely intellectual motives, has opened an entirely unexpected range of practical utilities to our astonished view. Galileo gave us accurate clocks and accurate artillery-practice; the chemists flood us with new medicines and dye-stuffs; Ampère and Faraday have endowed us with the New York subway and with Marconi telegrams. The hypothetical things that such men have invented, defined as they have defined them, are showing an extraordinary fertility in consequences verifiable by sense. Our logic can deduce from them a consequence due under certain conditions, we can then bring about the conditions, and presto, the consequence is there before our eyes. The scope of the practical control of nature newly put into our hand by scientific ways of thinking vastly exceeds the scope of the old control grounded on common sense. Its rate of increase accelerates so that no one can trace the limit; one may even fear that the *being* of man may be crushed by his own powers, that his fixed nature as an organism may not prove adequate to stand the strain of the ever increasingly tremendous functions, almost divine creative functions, which his intellect will more and more enable him to wield. He may drown in his wealth like a child in a bath-tub, who has turned on the water and who can not turn it off.

and

The most fateful point of difference between being a rationalist and being a pragmatist is now fully in sight. Experience is in mutation, and our psychological ascertainments of truth are in mutation—so much rationalism will allow; but never that either reality itself or truth itself is mutable. Reality stands complete and ready-made from all eternity, rationalism insists, and the agreement of our ideas with it is that unique unanalyzable virtue in them of which she has already told us. As that intrinsic excellence, their truth has nothing to do with our experiences. It adds nothing to the content of experience. It makes no difference to reality itself; it is supervenient, inert, static, a reflexion merely. It doesn't *exist*, it *holds* or *obtains*, it belongs to another dimension from that of either facts or fact-relations, belongs, in short, to the epistemological dimension—and with that big word rationalism closes the discussion.

Thus, just as pragmatism faces forward to the future, so does rationalism here again face backward to a past eternity. True to her inveterate habit, rationalism reverts to 'principles,' and thinks that when an abstraction once is named, we own an oracular solution.

The tremendous pregnancy in the way of consequences for life of this radical difference of outlook will only become apparent in my later lectures.

W. James, "Pragmatism's Conception of Truth," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

When Clerk-Maxwell was a child it is written that he had a mania for having everything explained to him, and that when people put him off with vague verbal accounts of any phenomenon he would interrupt them impatiently by saying, 'Yes; but I want you to tell me the *particular go* of it!' Had his question been about truth, only a pragmatist could have told him the particular go of it. I believe that our contemporary pragmatists, especially Messrs. Schiller and Dewey, have given the only tenable account of this subject. It is a very ticklish subject, sending subtle rootlets into all kinds of crannies, and hard to treat in the sketchy way that alone befits a public lecture. But the Schiller-Dewey view of truth has been so ferociously attacked by rationalistic philosophers, and so abominably misunderstood, that here, if anywhere, is the point where a clear and simple statement should be made.

I fully expect to see the pragmatist view of truth run through the classic stages of a theory's career. First, you know, a new theory is attacked as absurd; then it is admitted to be true, but obvious and insignificant; finally it is seen to be so important that its adversaries claim that they themselves discovered it. Our doctrine of truth is at present in the first of these three stages, with symptoms of the second stage having begun in certain quarters. I wish that this lecture might help it beyond the first stage in the eyes of many of you.

Truth, as any dictionary will tell you, is a property of certain of our ideas. It means their 'agreement,' as falsity means their disagreement, with 'reality.' Pragmatists and intellectualists both accept this definition as a matter of course. They begin to quarrel only after the question is raised as to what may precisely be meant by the term 'agreement,' and what by the term 'reality,' when reality is taken as something for our ideas to agree with.

In answering these questions the pragmatists are more analytic and painstaking, the intellectualists more offhand and irreflective. The popular notion is that a true idea must copy its reality. Like other popular views, this one follows the analogy of the most usual experience. Our true ideas of sensible things do indeed copy them. Shut your eyes and think of yonder clock on the wall, and you get just such a true picture or copy of its dial. But your idea of its 'works' (unless you are a clockmaker) is much less of a copy, yet it passes muster, for it in no way clashes with the reality. Even though it should shrink to the mere word 'works,' that word still serves you truly; and when you speak of the 'timekeeping function' of the clock, or of its spring's 'elasticity,' it is hard to see exactly what your ideas can copy.

You perceive that there is a problem here. Where our ideas cannot copy definitely their object, what does agreement with that object mean? Some idealists seem to say that they are true whenever they are what God means that we ought to think about that object. Others hold the copy-view all through, and speak as if our ideas possessed truth just in proportion as they approach to being copies of the Absolute's eternal way of thinking.

These views, you see, invite pragmatistic discussion. But the great assumption of the intellectualists is that truth means essentially an inert static relation. When you've got your true idea of anything, there's an end of the matter. You're in possession; you *know*; you have fulfilled your thinking destiny. You are where you ought to be mentally; you have obeyed your categorical imperative; and nothing more need follow on that climax of your rational destiny. Epistemologically you are in stable equilibrium.

Pragmatism, on the other hand, asks its usual question. "Grant an idea or belief to be true," it says, "what concrete difference will its being true make in any one's actual life? How will the truth be realized? What experiences will be different from those which would obtain if the belief were false? What, in short, is the truth's cash-value in experiential terms?"

The moment pragmatism asks this question, it sees the answer: *True ideas are those that we can assimilate, validate, corroborate and verify. False ideas are those that we can not.* That is the practical difference it makes to us to have true ideas; that, therefore, is the meaning of truth, for it is all that truth is known as.

This thesis is what I have to defend. The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication*. Its validity is the process of its valid-*ation*.

W. James, "Pragmatism and Humanism," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

What hardens the heart of every one I approach with the view of truth sketched in my last lecture is that typical idol of the tribe, the notion of *the* Truth, conceived as the one answer, determinate and complete, to the one fixed enigma which the world is believed to propound. For popular tradition, it is all the better if the answer be oracular, so as itself to awaken wonder as an enigma of the second order, veiling rather than revealing what its profundities are supposed to contain. All the great single-word answers to the world's riddle, such as God, the One, Reason, Law, Spirit, Matter, Nature, Polarity, the Dialectic Process, the Idea, the Self, the Oversoul, draw the admiration that men have lavished on them from this oracular rôle. By amateurs in philosophy and professionals alike, the universe is represented as a queer sort of petrified sphinx whose appeal to men consists in a monotonous challenge to his divining powers. *The* Truth: what a perfect idol of the rationalistic mind! I read in an old letter—from a gifted friend who died too young—these words: "In everything, in science, art, morals and religion, there *must* be one system that is right and *every* other wrong." How characteristic of the enthusiasm of a certain stage of youth! At twenty-one we rise to such a challenge and expect to find the system. It never occurs to most of us even later that the question 'what is *the* truth?' is no real question (being irrelative to all conditions) and that the whole notion of *the* truth is an abstraction from the fact of truths in the plural, a mere useful summarizing phrase like *the* Latin Language or *the* Law.

Common-law judges sometimes talk about the law, and schoolmasters talk about the latin tongue, in a way to make their hearers think they mean entities preexistent to the decisions or to the words and syntax, determining them unequivocally and requiring them to obey. But the slightest exercise of reflexion makes us see that, instead of being principles of this kind, both law and latin are results. Distinctions between the lawful and the unlawful in conduct, or between the correct and incorrect in speech, have grown up incidentally among the interactions of men's experiences in detail; and in no other way do distinctions between the true and the false in belief ever grow up. Truth grafts itself on previous truth, modifying it in the process, just as idiom grafts itself on previous idiom, and law on previous law. Given previous law and a novel case, and the judge will twist them into fresh law. Previous idiom; new slang or metaphor or oddity that hits the public taste;—and presto, a new idiom is made. Previous truth; fresh facts:—and our mind finds a new truth.

All the while, however, we pretend that the eternal is unrolling, that the one previous justice, grammar or truth are simply fulgurating and not being made. But imagine a youth in the courtroom trying cases with his abstract notion of 'the' law, or a censor of speech let loose among the theatres with his idea of 'the' mother-tongue, or a professor setting up to lecture on the actual universe with his rationalistic notion of 'the Truth' with a big T, and what progress do they make? Truth, law, and language fairly boil away from them at the least touch of novel fact. These things *make themselves* as we go. Our rights, wrongs, prohibitions, penalties, words, forms, idioms, beliefs, are so many new creations that add themselves as fast as history proceeds. Far from being antecedent principles that animate the process, law, language, truth are but abstract names for its results.

Laws and languages at any rate are thus seen to be man-made things. Mr. Schiller applies the analogy to beliefs, and proposes the name of 'Humanism' for the doctrine that to an unascertainable extent our truths are man-made products too. Human motives sharpen all our questions, human satisfactions lurk in all our answers, all our formulas have a human twist. This element is so inextricable in the products that Mr. Schiller sometimes seems almost to leave it an open question whether there be anything else. "The world," he says, "is essentially $\upsilon\lambda\eta$, it is what we make it. It is fruitless to define it by what it originally was or by what it is apart from us; it *is* what is made of it. Hence . . . the world is plastic." He adds that we can learn the limits of the plasticity only by trying, and that we ought to start as if it were wholly plastic, acting methodically on that assumption, and stopping only when we are decisively rebuked.

This is Mr. Schiller's butt-end-foremost statement of the humanist position, and it has exposed him to severe attack. I mean to defend the humanist position in this lecture, so I will insinuate a few remarks at this point.

Mr. Schiller admits as emphatically as any one the presence of resisting factors in every actual experience of truth-making, of which the new-made special truth must take account, and with which it has perforce to 'agree.' All our truths are beliefs about 'Reality'; and in any particular belief the reality acts as something independent, as a thing *found*, not manufactured. Let me here recall a bit of my last lecture.

'Reality' is in general what truths have to take account of; and the *first* part of reality from this point of view is the flux of our sensations. Sensations are forced upon us, coming we know not whence. Over their nature, order and quantity we have as good as no control. *They* are neither true nor false; they simply *are*. It is only what we say about them, only the names we give them, our theories of their source and nature and remote relations, that may be true or not.

The *second* part of reality, as something that our beliefs must also obediently take account of is the *relations* that obtain between our sensations or between their copies in our minds. This part falls into two subparts: 1) the relations that are mutable and accidental, as those of date and place; and 2) those that are fixed and essential because they are grounded on the inner natures of their terms. Both sorts of relation are matters of immediate perception. Both are 'facts.' But it is the latter kind of fact that forms the more important subpart of reality for our theories of knowledge. Inner relations namely are 'eternal,' are perceived whenever their sensible terms are compared; and of them our thought—mathematical and logical thought so-called—must eternally take account.

The *third* part of reality, additional to these perceptions (tho largely based upon them), is the *previous truths* of which every new inquiry takes account. This third part is a much less obdurately resisting factor: it often ends by giving way. In speaking of these three portions of reality as at all times controlling our beliefs formation, I am only reminding you of what we heard in our last hour.

Now however fixed these elements of reality may be, we still have a certain freedom in our dealings with them. Take our sensations. *That* they are is undoubtedly beyond our control; but *which* we attend to, note, and make emphatic in our conclusions depends on our own interests; and, according as we lay the emphasis here or there, quite different formulations of truth result. We read the same facts differently. 'Waterloo,' with the same fixed details, spells a 'victory' for an Englishman; for a Frenchman it spells a 'defeat.' So, for an optimist philosopher the universe spells victory, for a pessimist, defeat.

What we say about reality thus depends on the perspective into which we throw it. The *that* of it is its own; but the *what* depends on the *which*; and the which depends on *us*. Both the sensational and the relational parts of reality are dumb; they say absolutely nothing about themselves. We it is who have to speak for them. This dumbness of sensations has led such intellectualists as T. H. Green and Edward Caird to shove them almost beyond the pale of philosophic recognition, but pragmatists refuse to go so far. A sensation is rather like a client who has given his case to a lawyer and then has passively to listen in the courtroom to whatever account of his affairs, pleasant or unpleasant, the lawyer finds it most expedient to give.

Hence, even in the field of sensation, our minds exert a certain arbitrary choice. By our inclusions and omissions we trace the field's extent; by our emphasis we mark its foreground and its background; by our order we read it in this direction or in that. We receive in short the block of marble, but we carve the statue ourselves.

This applies to the 'eternal' parts of reality as well: we shuffle our perceptions of intrinsic relation and arrange them just as freely. We read them in one serial order or another, class them in this way or in that, treat one or the other as more fundamental, until our beliefs about them form those bodies of truth known as logics, geometrics, or arithmetics, in each and all of which the form and order in which the whole is cast is flagrantly man-made.

Thus, to say nothing of the new *facts* which men add to the matter of reality by the acts of their own lives, they have already impressed their mental forms on that whole third of reality which I have called 'previous truths.' Every hour brings its new percepts, its own facts of sensation and relation, to be truly taken account of; but the whole of our *past* dealings with such facts is already funded in the previous truths. It is therefore only the smallest and recentest fraction of the first two parts of reality that comes to us without the human touch, and that fraction has immediately to become humanized in the sense of being squared, assimilated, or in some way adapted, to the humanized mass already there., As a matter of fact we can hardly take in an impression at all, in the absence of a preconception of what impressions there may possibly be.

When we talk of reality 'independent' of human thinking, then, it seems a thing very hard to find. It reduces to the notion of what is just entering into experience and yet to be named, or else to some imagined aboriginal presence in experience, before any belief about the presence had arisen, before any human conception had been applied. It is what is absolutely dumb and evanescent, the merely ideal limit of our minds. We may glimpse it, but we never grasp it; what we grasp is always some substitute for it which previous human thinking has peptonized and cooked for our consumption. If so vulgar an expression were allowed us, we might say that wherever we find it, it has been already *faked*. This is what Mr. Schiller has in mind when he calls independent reality a mere unresisting $\upsilon\lambda\eta$, which is only to be made over by us.

and

In many familiar objects every one will recognize the human element. We conceive a given reality in this way or in that, to suit our purpose, and the reality passively submits to the conception. You can take the number 27 as the cube of 3, or as the product of 3 and 9, or as 26 *plus* 1, or 100 *minus* 73, or in countless other ways, of which one will be just as true as another. You can take a chess-board as black squares on a white ground, or as white squares on a black ground, and neither conception is a false one.

You can treat the adjoined figure as a star, as two big triangles crossing each other, as a hexagon with legs set up on its angles, as six equal triangles hanging together by their tips, etc. All these treatments are true treatments—the sensible *that* upon the paper resists no one of them. You can say of a line that it runs east, or you can say that it runs west, and the line *per se* accepts both descriptions without rebelling at the inconsistency.

We carve out groups of stars in the heavens, and call them constellations, and the stars patiently suffer us to do so,—*though* if they knew what we were doing, some of them might feel much surprised at the partners we had given them. We name the same constellation diversely, as Charles's Wain, the Great Bear, or the Dipper. None of the names will be false, and one will be as true as another, for all are applicable.

In all these cases we humanly make an *addition* to some sensible reality, and that reality tolerates the addition. All the additions 'agree' with the reality; they fit it, while they build it out. No one of them is false. Which may be treated as the *more* true, depends altogether on the human use of it. If the 27 is a number of dollars which I find in a drawer where I had left 28, it is 28 minus 1. If it is the number of inches in a board which I wish to insert as a shelf into a cupboard 26 inches wide, it is 26 plus 1. If I wish to ennoble the heavens by the constellations I see there, 'Charles's Wain' would be more true than 'Dipper.' My friend Frederick Myers was humorously indignant that that prodigious star-group should remind us Americans of nothing but a culinary utensil.

What shall we call a *thing* anyhow? It seems quite arbitrary, for we carve out everything, just as we carve out constellations, to suit our human purposes. For me, this whole 'audience' is one thing, which grows now restless, now attentive. I have no use at present for its individual units, so I don't consider them. So of an 'army,' of a 'nation.' But in your own eyes, ladies and gentlemen, to call you 'audience' is an accidental way of taking you. The permanently real things for you are your individual persons. To an anatomist, again, those persons are but organisms, and the real things are the organs. Not the organs, so much as their constituent cells, say the histologists; not the cells, but their molecules, say in turn the chemists.

We break the flux of sensible reality into things, then, at our will. We create the subjects of our true as well as of our false propositions.

We create the predicates also. Many of the predicates of things express only the relations of the things to us and to our feelings. Such predicates of course are human additions. Caesar crossed the Rubicon, and was a menace to Rome's freedom. He is also an American schoolroom pest, made into one by the reaction of our schoolboys on his writings. The added predicate is as true of him as the earlier ones.

You see how naturally one comes to the humanistic principle: you can't weed out the human contribution. Our nouns and adjectives are all humanized heirlooms, and in the theories we build them into, the inner order and arrangement is wholly dictated by human considerations, intellectual consistency being one of them. Mathematics and logic themselves are fermenting with human rearrangements; physics, astronomy and biology follow massive cues of preference. We plunge forward into the field of fresh experience with the beliefs our ancestors and we have made already; these determine what we notice; what we notice determines what we do; what we do again determines what we experience; so from one thing to another, altho the stubborn fact remains that there is a sensible flux, what is *true of it* seems from first to last to be largely a matter of our own creation.

We build the flux out inevitably. The great question is: does it, with our additions, *rise or fall in value*? Are the additions *worthy* or *unworthy*? Suppose a universe composed of seven stars, and nothing else but three human witnesses and their critic. One witness names the stars 'Great Bear'; one calls them 'Charles's Wain'; one calls them the 'Dipper.' Which human addition has made the best universe of the given stellar material? If Frederick Myers were the critic, he would have no hesitation in 'turning down' the American witness.

Lotze has in several places made a deep suggestion. We naïvely assume, he says, a relation between reality and our minds which may be just the opposite of the true one. Reality, we naturally think, stands ready-made and complete, and our intellects supervene with the one simple duty of describing it as it is already. But may not our descriptions, Lotze asks, be themselves important additions to reality? And may not previous reality itself be there, far less for the purpose of reappearing unaltered in our knowledge, than for the very purpose of stimulating our minds to such additions as shall enhance the universe's total value. *'Die Erhöhung des vorgefundenen Daseins'*[202] is a phrase used by Professor Eucken somewhere, which reminds one of this suggestion by the great Lotze.

It is identically our pragmatistic conception. In our cognitive as well as in our active life we are creative. We *add*, both to the subject and to the predicate part of reality. The world stands really malleable, waiting to receive its final touches at our hands. Like the kingdom of heaven, it suffers human violence willingly. Man *engenders* truths upon it.

No one can deny that such a role would add both to our dignity and to our responsibility as thinkers. To some of us it proves a most inspiring notion. Signore Papini, the leader of Italian pragmatism, grows fairly dithyrambic over the view that it opens of man's divinely-creative functions.

The import of the difference between pragmatism and rationalism is now in sight throughout its whole extent. The essential contrast is that *for rationalism reality is ready-made and complete from all eternity, while for pragmatism it is still in the making, and awaits part of its complexion from the future.* On the one side the universe is absolutely secure, on the other it is

[202] H. C. von Baeyer corrected the capitalization of this from James's original, and translated it as, "the enhancement of what is found to exist."

still pursuing its adventures.

We have got into rather deep water with this humanistic view, and it is no wonder that misunderstanding gathers round it. It is accused of being a doctrine of caprice. Mr. Bradley, for example, says that a humanist, if he understood his own doctrine, would have to 'hold any end, however perverted, to be rational, if I insist on it personally, and any idea, however mad, to be the truth if only some one is resolved that he will have it so.' The humanist view of 'reality,' as something resisting, yet malleable, which controls our thinking as an energy that must be taken 'account' of incessantly (tho not necessarily merely *copied*) is evidently a difficult one to introduce to novices. The situation reminds me of one that I have personally gone through. I once wrote an essay on our right to believe, which I unluckily called the *Will* to Believe. All the critics, neglecting the essay, pounced upon the title. Psychologically it was impossible, morally it was iniquitous. The 'will to deceive,' the 'will to make-believe,' were wittily proposed as substitutes for it.

The alternative between pragmatism and rationalism, in the shape in which we now have it before us, is no longer a question in the theory of knowledge, it concerns the structure of the universe itself.

On the pragmatist side we have only one edition of the universe, unfinished, growing in all sorts of places, especially in the places where thinking beings are at work.

On the rationalist side we have a universe in many editions, one real one, the infinite folio, or *édition de luxe*, eternally complete; and then the various finite editions, full of false readings, distorted and mutilated each in its own way.

W. James, "Pragmatism and Religion," in his *Pragmatism, a New Name for Some Old Ways of Thinking: Popular Lectures on Philosophy*, (Longmans, Green and Co., New York, 1922).

Let us apply this notion to the salvation of the world. What does it pragmatically mean to say that this is possible? It means that some of the conditions of the world's deliverance do actually exist. The more of them there are existent, the fewer preventing conditions you can find, the better-grounded is the salvation's possibility, the more *probable* does the fact of the deliverance become.

So much for our preliminary look at possibility.

Now it would contradict the very spirit of life to say that our minds must be indifferent and neutral in questions like that of the world's salvation. Any one who pretends to be neutral writes himself down here as a fool and a sham. We all do wish to minimize the insecurity of the universe; we are and ought to be unhappy when we regard it as exposed to every enemy and open to every life-destroying draft. Nevertheless there are unhappy men who think the salvation of the world impossible. Theirs is the doctrine known as pessimism.

Optimism in turn would be the doctrine that thinks the world's salvation inevitable.

Midway between the two there stands what may be called the doctrine of meliorism, tho it has hitherto figured less as a doctrine than as an attitude in human affairs. Optimism has always been the regnant *doctrine* in European philosophy. Pessimism was only recently introduced by Schopenhauer and counts few systematic defenders as yet. Meliorism treats salvation as neither necessary nor impossible. It treats it as a possibility, which becomes more and more of a probability the more numerous the actual conditions of salvation become.

It is clear that pragmatism must incline towards meliorism. Some conditions of the world's salvation are actually extant, and she can not possibly close her eyes to this fact: and should the residual conditions come, salvation would become an accomplished reality. Naturally the terms I use here are exceedingly summary. You may interpret the word 'salvation' in any way you like, and make it as diffuse and distributive, or as climacteric and integral a phenomenon as you please.

Take, for example, any one of us in this room with the ideals which he cherishes and is willing to live and work for. Every such ideal realized will be one moment in the world's salvation. But these particular ideals are not bare abstract possibilities. They are grounded, they are *live* possibilities, for we are their live champions and pledges, and if the complementary conditions come and add themselves, our ideals will become actual things. What now are the complementary conditions? They are first such a mixture of things as will in the fullness of time give us a chance, a gap that we can spring into, and, finally, *our act*.

Does our act then *create* the world's salvation so far as it makes room for itself, so far as it leaps into the gap? Does it create, not the whole world's salvation of course, but just so much of this as itself covers of the world's extent?

Here I take the bull by the horns, and in spite of the whole crew of rationalists and monists, of whatever brand they be, I ask *why not?* Our acts, our turning-places, where we seem to ourselves to make ourselves and grow, are the parts of the world to which we are closest, the parts of which our knowledge is the most intimate and complete. Why should we not take them at their facevalue? Why may they not be the actual turning-places and growing-places which they seem to be, of the world—why not the workshop of being, where we catch fact in the making, so that nowhere may the world grow in any other kind of way than this?

Irrational! we are told. How can new being come in local spots and patches which add themselves or stay away at random, independently of the rest? There must be a reason for our acts, and where in the last resort can any reason be looked for save in the material pressure or the logical compulsion of the total nature of the world? There can be but one real agent of growth, or seeming growth, anywhere, and that agent is the integral world itself. It may grow all-over, if growth there be, but that single parts should grow *per se* is irrational.

But if one talks of rationality—and of reasons for things, and insists that they can't just come in spots, what *kind* of a reason can there ultimately be why anything should come at all? Talk of logic and necessity and categories and the absolute and the contents of the whole philosophical machine-shop as you will, the only *real* reason I can think of why anything should ever come is that *some one wishes it to be here*. It is *demanded*,—demanded, it may be, to give relief to no matter how small a fraction of the world's mass. This is *living reason*, and compared with it material causes and logical necessities are spectral things.

In short the only fully rational world would be the world of wishing-caps, the world of telepathy, where every desire is fulfilled instanter, without having to consider or placate surrounding or intermediate powers. This is the Absolute's own world. He calls upon the phenomenal world to be, and it is, exactly as he calls for it, no other condition being required. In our world, the wishes of the individual are only one condition. Other individuals are there with other wishes and they must be propitiated first. So Being grows under all sorts of resistances in this world of the many, and, from compromise to compromise, only gets organized gradually into what may be called secondarily rational shape. We approach the wishing-cap type of organization only in a few departments of life. We want water and we turn a faucet. We want a kodak-picture and we press a button. We want information and we telephone. We want to travel and we buy a ticket. In these and similar cases, we hardly need to do more than the wishing—the world is rationally organized to do the rest.

But this talk of rationality is a parenthesis and a digression. What we were discussing was the idea of a world growing not integrally but piecemeal by the contributions of its several parts. Take the hypothesis seriously and as a live one. Suppose that the world's author put the case to you before creation, saying: "I am going to make a world not certain to be saved, a world the perfection of which shall be conditional merely, the condition being that each several agent does its own 'level best.' I offer you the chance of taking part in such a world. Its safety, you see, is unwarranted. It is a real adventure, with real danger, yet it may win through. It is a social scheme of co-operative work genuinely to be done. Will you join the procession? Will you trust yourself and trust the other agents enough to face the risk?"

Should you in all seriousness, if participation in such a world were proposed to you, feel bound to reject it as not safe enough? Would you say that, rather than be part and parcel of so fundamentally pluralistic and irrational a universe, you preferred to relapse into the slumber of nonentity from which you had been momentarily aroused by the tempter's voice?

Of course if you are normally constituted, you would do nothing of the sort. There is a healthy-minded buoyancy in most of us which such a universe would exactly fit. We would therefore accept the offer—"Top! und Schlag auf Schlag!" It would be just like the world we practically live in; and loyalty to our old nurse Nature would forbid us to say no. The world proposed would seem 'rational' to us in the most living way.

Most of us, I say, would therefore welcome the proposition and add our *fiat* to the *fiat* of the creator.

Max's First Reply

I finally received my copy of *Pragmatism* and have been reading it with much enjoyment ever since. I found myself nodding in agreement many times as I was reading along. I also enjoyed James' prose and choice of words. I like the idea of a world that might be pluralistic at its core, a world whose future is not yet hammered out, a world that is not subsumed into a monistic, eternal, static One. I like the open-minded, non-dogmatic and practical (yet not mundane) nature of the pragmatist approach. Among many things, reading the book has further cemented my ever-growing antipathy to Everett's many worlds. Not a bad outcome, I think, given that I used to be quite partial to this type of interpretation. Well, once I've finished the book, I hope to be able to communicate some more concrete thoughts and comments.

Also, I just re-read your paper "Subjective probability and quantum certainty," and I feel it has helped me in coming to grips with the idea of a purely subjective nature of certainty. I suppose my initial slight unease with this issue had something to do with the question you address toward the end: "Isn't not asking for a further explanation a betrayal of the very purpose of science, namely, never to give up the quest for an explanation?" I think that, for most of us, the idea of "explanation" is usually indeed intuitively linked with notions of causality and (moreover) determinism. And hence the initial discomfort when our feeling of certainty is not further related to some underlying mechanism that "indeed" (as we would then say) ensures an objective basis for our certainty. To some extent, much of the spirit of science may be built on the quest for such a mechanism: we observe patterns, regularities, and want to have them set in stone, in form of mechanistic physical laws, for example. But the upshot of QM may indeed be to look no longer for such mind-soothing tracks to underlie the journey of our experiences.

I'm very curious to see how your thoughts on "decoherence in QBist terms" will evolve. I too have sometimes found myself contemplating the precise role decoherence could assume in such an interpretive framework, and what the particular input of decoherence would be here.

As with similar questions about the implications and views of decoherence in the various interpretations of QM, it is apparent that, on the one hand, decoherence is simply an application of the QM formalism and hence cannot really "solve" any foundational riddles; but also that, on the other hand, decoherence may sharpen or inform or lessen certain worries of foundational relevance. I understand that you believe that decoherence has rather little (if any) input for q-foundations, which is a fair point (though I'd add that its force may also in turn depend on one's interpretive commitments and thinking, and thus on the particular form one chooses to state and rank foundational problems).

Of course, QBism in no way keeps us from applying the full machinery of decoherence analysis. I guess we may say that decoherence teaches us something like this: In order for the agent to improve his predictions of the consequences of future actions on the system, he ought to take into account the openness of any realistic quantum system. He could take actions on the environment and on the composite system-environment complex to bring about facts that in turn help him update is predictions of what he may (or, more often in the case of decoherence, may NOT) expect to experience if he interacts with the system. Or, in absence of access to most environmental degrees of freedom, he might just adjust his prior according to some good guess about what the environment is like and what its interactions with the system are likely to be. In turn, I suppose he could also use his experiences of interactions with the system to update his state assignments to the environment.

These are just a couple of first, painfully vague attempts at wrapping my head around the decoherence + QBist complex. Maybe I've missed the target. At any rate, if you think it may be beneficial/worthwhile for you too, I would certainly enjoy any discussions surrounding QBist decoherence while you and Rüdiger work on the draft.

Max's Second Reply

I *finally* finished James' book today. I had read three quarters of it in one go (enthusiastically, in fact!), and then somehow got sidetracked by other things and didn't seriously pick it up again until today. The earlier preliminary assessment I had given to you in my email of July 28th (you got that one, didn't you?) still stands, though:

> "I found myself nodding in agreement many times as I was reading along. I also enjoyed James' prose and choice of words. I like the idea of a world that might be pluralistic at its core, a world whose future is not yet hammered out, a world that is not subsumed into a monistic, eternal, static One. I like the open-minded, non-dogmatic and practical (yet not mundane) nature of the pragmatist approach."

Most importantly, maybe, I have already applied some of the pragmatic ways of thinking to various issues that have come before me lately, and I've had some nicely head-clearing epiphanies in the process. So, thanks again for pointing me to this book. I shall certainly hear James' voice in the back of my head from now on.

Below are some of my favorite quotes – I haven't checked explicitly, but it's likely they'll have some overlap with the ones you sent me. Well, I guess that just proves the universal power and appeal of the Jamesian ideas …

The Present Dilemma in Philosophy

> Whatever universe a professor believes in must at any rate be a universe that lends itself to lengthy discourse. A universe definable in two sentences is something for which the professorial intellect has no use. No faith in anything of that cheap kind!
>
> **
>
> The history of philosophy is to a great extent that of a certain clash of human temperaments.
>
> **
>
> The more absolutistic philosophers dwell on so high a level of abstraction that they never even try to come down. The absolute mind which they offer us, the mind that makes our universe by thinking it, might, for aught they show us to the contrary, have made any one of a million other universes just as well as this. You can deduce no single actual particular from the notion of it. It is compatible with any state of things whatever being true here below.
>
> **
>
> The actual universe is a thing wide open, but rationalism makes systems, and systems must be closed. For men in practical life perfection is something far off and still in process of achievement. This for rationalism is but the

illusion of the finite and relative: the absolute ground of things is a perfection eternally complete.

**

The books of all the great philosophers are like so many men. Our sense of an essential personal flavor in each one of them, typical but indescribable, is the finest fruit of our own accomplished philosophic education. What the system pretends to be is a picture of the great universe of God. What it is—and oh so flagrantly!—is the revelation of how intensely odd the personal flavor of some fellow creature is.

**

But almost everyone has his own peculiar sense of a certain total character in the universe, and of the inadequacy fully to match it of the peculiar systems that he knows. They don't just cover HIS world. One will be too dapper, another too pedantic, a third too much of a job-lot of opinions, a fourth too morbid, and a fifth too artificial, or what not. At any rate he and we know offhand that such philosophies are out of plumb and out of key and out of 'whack,' and have no business to speak up in the universe's name.

What Pragmatism Means

So the universe has always appeared to the natural mind as a kind of enigma, of which the key must be sought in the shape of some illuminating or power-bringing word or name. That word names the universe's PRINCIPLE, and to possess it is, after a fashion, to possess the universe itself. 'God,' 'Matter,' 'Reason,' 'the Absolute,' 'Energy,' are so many solving names. You can rest when you have them. You are at the end of your metaphysical quest.

**

When the first mathematical, logical and natural uniformities, the first LAWS, were discovered, men were so carried away by the clearness, beauty and simplification that resulted, that they believed themselves to have deciphered authentically the eternal thoughts of the Almighty. (...)

But as the sciences have developed farther, the notion has gained ground that most, perhaps all, of our laws are only approximations. The laws themselves, moreover, have grown so numerous that there is no counting them; and so many rival formulations are proposed in all the branches of science that investigators have become accustomed to the notion that no theory is absolutely a transcript of reality, but that any one of them may from some point of view be useful.

**

Purely objective truth, truth in whose establishment the function of giving human satisfaction in marrying previous parts of experience with newer parts played no role whatever, is nowhere to be found. The reasons why we call things true is the reason why they ARE true, for 'to be true' MEANS only to perform this marriage-function.

Some Metaphysical Problems Pragmatically Considered

To treat abstract principles as finalities, before which our intellects may come to rest in a state of admiring contemplation, is the great rationalist failing.

**

When we look at what has actually come, the conditions must always appear perfectly designed to ensure it. We can always say, therefore, in any conceivable world, of any conceivable character, that the whole cosmic machinery MAY have been designed to produce it.

The One and the Many

I must therefore treat the notion of an All-Knower simply as an hypothesis, exactly on a par logically with the pluralist notion that there is no point of view, no focus of information extant, from which the entire content of the universe is visible at once. "God's consciousness," says Professor Royce, "forms in its wholeness one luminously transparent conscious moment"—this is the type of noetic unity on which rationalism insists. Empiricism on the other hand is satisfied with the type of noetic unity that is humanly familiar. Everything gets known by SOME knower along with something else; but the knowers may in the end be irreducibly many, and the greatest knower of them all may yet not know the whole of everything, or even know what he does know at one single stroke:—he may be liable to forget. Whichever type obtained, the world would still be a universe noetically. Its parts would be conjoined by knowledge, but in the one case the knowledge would be absolutely unified, in the other it would be strung along and overlapped.

Pragmatism and Common Sense

The scope of the practical control of nature newly put into our hand by scientific ways of thinking vastly exceeds the scope of the old control grounded on common sense. Its rate of increase accelerates so that no one can trace the limit; one may even fear that the BEING of man may be crushed by his own powers, that his fixed nature as an organism may not prove adequate to stand the strain of the ever increasingly tremendous functions, almost divine creative functions, which his intellect will more and more enable him to wield. He may drown in his wealth like a child in a bath-tub, who has turned on the water and who cannot turn it off.

**

But now if the new kinds of scientific 'thing,' the corpuscular and etheric world, were essentially more 'true,' why should they have excited so much criticism within the body of science itself? Scientific logicians are saying on every hand that these entities and their determinations, however definitely conceived, should not be held for literally real. It is AS IF they existed; but in reality they are like co-ordinates or logarithms, only artificial short-cuts for taking us from one part to another of experience's flux. We can cipher fruitfully with them; they serve us wonderfully; but we must not be their dupes.

**

Ought not the existence of the various types of thinking which we have reviewed, each so splendid for certain purposes, yet all conflicting still, and neither one of them able to support a claim of absolute veracity, to awaken a presumption favorable to the pragmatistic view that all our theories are INSTRUMENTAL, are mental modes of ADAPTATION to reality, rather than revelations or gnostic answers to some divinely instituted world-enigma?

Pragmatism's Conception of Truth

But the great assumption of the intellectualists is that truth means essentially an inert static relation. When you've got your true idea of anything, there's an end of the matter. You're in possession; you KNOW; you have fulfilled your thinking destiny. You are where you ought to be mentally; you have obeyed your categorical imperative; and nothing more need follow on that climax of your rational destiny. Epistemologically you are in stable equilibrium.

**

The truth of an idea is not a stagnant property inherent in it. Truth HAPPENS to an idea. It BECOMES true, is MADE true by events. Its verity is in fact an event, a process: the process namely of its verifying itself, its veri-FICATION. Its validity is the process of its valid-ATION.

**

The true thought is useful here because the house which is its object is useful. The practical value of true ideas is thus primarily derived from the practical importance of their objects to us. Their objects are, indeed, not important at all times.

**

Primarily, and on the common-sense level, the truth of a state of mind means this function of A LEADING THAT IS WORTH WHILE. When a moment in our experience, of any kind whatever, inspires us with a thought that is true, that means that sooner or later we dip by that thought's guidance into the particulars of experience again and make advantageous connexion with them.

**

Such is the large loose way in which the pragmatist interprets the word agreement. He treats it altogether practically. He lets it cover any process of conduction from a present idea to a future terminus, provided only it run prosperously. It is only thus that 'scientific' ideas, flying as they do beyond common sense, can be said to agree with their realities. It is, as I have already said, as if reality were made of ether, atoms or electrons, but we mustn't think so literally. The term 'energy' doesn't even pretend to stand for anything 'objective.' It is only a way of measuring the surface of phenomena so as to string their changes on a simple formula.

Yet in the choice of these man-made formulas we cannot be capricious with impunity any more than we can be capricious on the common-sense practical level. We must find a theory that will WORK; and that means something extremely difficult; for our theory must mediate between all previous truths and certain new experiences. It must derange common sense and previous belief as little as possible, and it must lead to some sensible terminus or other that can be verified exactly. To 'work' means both these things; and the squeeze is so tight that there is little loose play for any hypothesis. Our theories are wedged and controlled as nothing else is. Yet sometimes alternative theoretic formulas are equally compatible with all the truths we know, and then we choose between them for subjective reasons. We choose the kind of theory to which we are already partial; we follow 'elegance' or 'economy.' Clerk Maxwell somewhere says it would be "poor scientific taste" to choose the more complicated of two equally well-evidenced conceptions; and you will all agree with him. Truth in science is what gives us the maximum possible sum of satisfactions, taste included, but consistency both with previous truth and with novel fact is always the most imperious claimant.

**

Truths emerge from facts; but they dip forward into facts again and add to them; which facts again create or reveal new truth (the word is indifferent) and so on indefinitely. The 'facts' themselves meanwhile are not TRUE. They simply ARE. Truth is the function of the beliefs that start and terminate among them.

**

The rationalist's fallacy here is exactly like the sentimentalist's. Both extract a quality from the muddy particulars of experience, and find it so pure when extracted that they contrast it with each and all its muddy instances as an opposite and higher nature. All the while it is THEIR nature. It is the nature of truths to be validated, verified. It pays for our ideas to be validated. Our obligation to seek truth is part of our general obligation to do what pays. The payments true ideas bring are the sole why of our duty to follow them.

Pragmatism and Humanism

I read in an old letter—from a gifted friend who died too young—these words: "In everything, in science, art, morals and religion, there MUST be one system that is right and EVERY other wrong." How characteristic of the enthusiasm of a certain stage of youth!

**

'REALITY' IS IN GENERAL WHAT TRUTHS HAVE TO TAKE ACCOUNT OF; [fn: Mr. Taylor in his Elements of Metaphysics uses this excellent pragmatic definition.] and the FIRST part of reality from this point of view is the flux of our sensations. Sensations are forced upon us, coming we know not whence. Over their nature, order, and quantity we have as good as no control. THEY are neither true nor false; they simply ARE. It is only what we say about them, only the names we give them, our theories of their source and nature and remote relations, that may be true or not.

**

Lotze has in several places made a deep suggestion. We naively assume, he says, a relation between reality and our minds which may be just the opposite of the true one. Reality, we naturally think, stands ready-made and complete, and our intellects supervene with the one simple duty of describing it as it is already. But may not our descriptions, Lotze asks, be themselves important additions to reality? And may not previous reality itself be there, far less for the purpose of reappearing unaltered in our knowledge, than for the very purpose of stimulating our minds to such additions as shall enhance the universe's total value. "Die Erhöhung des vorgefundenen Daseins" is a phrase used by Professor Eucken somewhere, which reminds one of this suggestion by the great Lotze.

It is identically our pragmatistic conception. In our cognitive as well as in our active life we are creative. We ADD, both to the subject and to the predicate part of reality. The world stands really malleable, waiting to receive its final touches at our hands. Like the kingdom of heaven, it suffers human violence willingly. Man ENGENDERS truths upon it.

**

The essential contrast is that for rationalism reality is ready-made and complete from all eternity, while for pragmatism it is still in the making, and awaits part of its complexion from the future. On the one side the universe is absolutely secure, on the other it is still pursuing its adventures.

**

And this, exactly this, is what the tough-minded of that lecture find themselves moved to call a piece of perverse abstraction-worship. The tough-minded are the men whose alpha and omega are FACTS. Behind the bare phenomenal facts, as my tough-minded old friend Chauncey Wright, the great Harvard empiricist of my youth, used to say, there is NOTHING. When a rationalist insists that behind the facts there is the GROUND of the facts, the POSSIBILITY of the facts, the tougher empiricists accuse him of taking the mere name and nature of a fact and clapping it behind the fact as a duplicate entity to make it possible. That such sham grounds are often invoked is notorious. At a surgical operation I heard a bystander ask a doctor why the patient breathed so deeply. "Because ether is a respiratory stimulant," the doctor answered. "Ah!" said the questioner, as if relieved by the explanation. But this is like saying that cyanide of potassium kills because it is a 'poison,' or that it is so cold to-night because it is 'winter,' or that we have five fingers because we are 'pentadactyls.' These are but names for the facts, taken from the facts, and then treated as previous and explanatory. The tender-minded notion of an absolute reality is, according to the radically tough-minded, framed on just this pattern. It is but our summarizing name for the whole spread-out and strung-along mass of phenomena, treated as if it were a different entity, both one and previous.

Pragmatism and Religion

In short the only fully rational world would be the world of wishing-caps, the world of telepathy, where every desire is fulfilled instanter, without having to consider or placate surrounding or intermediate powers. This is the Absolute's own world. He calls upon the phenomenal world to be, and it IS, exactly as he calls for it, no other condition being required. In our world, the wishes of the individual are only one condition. Other individuals are there with other wishes and they must be propitiated first. So Being

> grows under all sorts of resistances in this world of the many, and, from compromise to compromise, only gets organized gradually into what may be called secondarily rational shape. We approach the wishing-cap type of organization only in a few departments of life. We want water and we turn a faucet. We want a kodak-picture and we press a button. We want information and we telephone. We want to travel and we buy a ticket. In these and similar cases, we hardly need to do more than the wishing-the world is rationally organized to do the rest.

30-06-09 *Disturbing the Solipsist, 2* (to H. M. Wiseman)

Wisemanism 41 : *Perhaps not this section, but the introduction states:*

> For, quantum mechanics-we plan to show in this paper-gives a resource that raw Bayesian probability theory does not: It gives a rule for forming probabilities for the outcomes of factualizable experiments (experiments that may actually be performed) from the probabilities one assigns for the outcomes of a designated counterfactual experiment (an experiment only imagined, and though possible to do, never actually performed).

Maybe I'm misunderstanding. I thought you were contrasting classical Bayesian reasoning with quantum Bayesian reasoning, and implying the latter had more structure. This would be wrong, IMO, for the reasons I gave earlier. But maybe you mean something else by "raw" Bayesianism.

You are indeed a friend in Australia: For who else would read the paper this thoroughly!

I don't want the passage to be misleading, but strictly speaking it remains correct and consistent with what I said yesterday. In a literal reading, the only thing this passage expresses is that the *judgment* $q(B) = (d + 1)p(B) - 1$ (for relating lower path to upper path in the diagram) is a judgment beyond raw Bayesian probability theory (i.e., beyond Dutch-book coherence). As you called me on in your earlier note, a judgment that $q(B) = p(B)$ would also be an addition to Bayesian probability theory, just a different one. And that is true. The passage you quote is simply silent on that.

There is a bit of discussion of this just after Eq. (169). And of course there is a bit of discussion just preceding Eq. (4). Finally some discussion on page 53 near the displayed equations.

I don't know that I should modify anything in the introduction, since as I say the passage is not inaccurate.

01-07-09 *Quick, Before Rüdiger Sees . . .* (to H. M. Wiseman)

Yesterday didn't work out the way I planned, and today Rüdiger says I can't write so much email (we need to work on our "Quantum-Bayesian Decoherence" paper in his last days in Waterloo). So, once again, you'll have to wait to learn whether you exist.

Wisemanism 42 : *Are you going to modify your introduction then, if you admit it misrepresents your position?*

It doesn't so much misrepresent it, as it just doesn't say it in any detail. (That's the way introductions are supposed to work.) My feeling was that I had covered myself with respect to *most* people by using the phrase "quick and dirty," by making the allusion to "EPR-criterion-of-reality considerations" along with Bell, and by explicitly putting "reads off" in quotes. We'll see. If I can find a pithy way to make it more accurate (i.e., without adding more than an extra sentence), I might try.

Wisemanism 43: *I'll have to read your response to Norsen, but in my correspondence with Norsen I disagreed with his claim that one can (and Bell did) derive his theorem from locality (in the strict sense Einstein and Bell used it) alone. I think it is clear that locality plus determinism, or local causality (which is slightly weaker) is needed. This was one thing I wanted to make clear in writing a review of RL & AT. So I suspect we agree on this.*

If I understand you correctly, then I think we agree to a large extent.

Wisemanism 44: *I'm not sure what your "WITHOUT RECOURSE TO" is supposed to mean.*

I'll come back to that when I tell you whether you exist.

Wisemanism 45: *Regarding who is a genuine realist, what I find puzzling is that you say you believe there is a world out there because you believe it can surprise you. But in cases where you believe the world can't surprise you, you don't believe it's real. That is, you seem to take predictability as the* counter-evidence *for reality, which is the opposite to the usual scientific argument for there being a real world.*

It probably helps explain my attraction to James, Dewey, and Schiller. But you mangle the point a bit: Predictability is not *counter*-evidence; it's just null. I hope I formulate the point relatively carefully in my "Anti-Växjö Interpretation of QM" pseudo-paper. Michel Bitbol makes the point pretty well in a letter responding to that paper. I'll paste it in below (point 4 in particular). [See preply to 10-12-03 note "First Meeting" with M. Bitbol.]

I'll be back when Rüdiger's not watching.

02-07-09 *Your Objections* (to G. M. D'Ariano)

Thanks for the extensive notes. They are good food for thought for me, and let us discuss at length when you come to Waterloo. At the moment, let me just comment on two of your items.

D'Ariano-ism 4: *1) You said (publicly) that you gave up the problem of deriving QM from "operational" principles.*

I'm very sorry to cause you distress on this. I can't exactly recall what I said, but I do remember this much: What I said was meant to be something of a joke (expressing that I have been failing so far), but it went way wrong. So much so, that you didn't know that I was joking! So please be less stressed: I still believe operational principles are the great starting point for our considerations.

D'Ariano-ism 5: *2) You mentioned possible "empirical" motivation for QM.*

Maybe I have not used the best word here, I don't know. I am discovering that it has connotations with many (like yourself) that I had not intended. I still very much want a great principle. But I believe that that great principle will express something about the character of our particular world. A different world would have had a different principle. I insert an excerpt from a note to Eric Cavalcanti below, where I reply to some points in his own correspondence with Howard

Wiseman. [See 29-06-09 note "Eric's Note" to E. G. Cavalcanti & H. M. Wiseman.] Perhaps these notes will help take some of the sting of my choice of words (at least they give the sense of what I meant by "empirical") … and perhaps I should modify my words to better ones in the future.

I look forward to seeing you soon … so I may better clear my name and restore your faith in me!

03-07-09 *Emailing: 0907.0416v1* (to G. L. Comer)

Comerism 23: *From your point of view, is it even necessary to talk about the "same quantum state"? Can't I just forget about the constituents, and say that there is a "widget" I'm gonna latch onto and that I'm gonna write down this wave function for? Then if I introduce "fundamental" constituents, it's only because I have some prior knowledge about their behavior that I want to take advantage of in trying to extract info from the widget?*

Yep. Things don't HAVE wave functions, we make them up. A line from my most recent paper:

> For, the essential point for a quantum-Bayesian is that there is no such thing as *the* quantum state. There are potentially as many states for a given quantum system as there are agents.

And that applies to anything whatsoever.

06-07-09 *The Verdict* (to H. M. Wiseman)

Wisemanism 46: *The discussion on p. 55 again opens you to accusations of solipsism. I don't care about your saying you're not a solipsist because the world can still surprise you. Tell me what your attitude to other people is! You talk about "categories of thought". That seems like a veil to hide behind. Do you believe other people are agents, equal to you, who exist even when you are not observing them, doing things when they are space-like separated from you?*

If yes, then you believe in hidden variables, despite your disparagement of them.

If no, then you are a solipsist in my book.

And I don't mean you as a hypothetical agent telling me the answer your quantum theory / philosophy tells you to say. I mean you, Chris Fuchs, the individual I believe in even when I'm not interacting with him. What do you believe about me?

I finally come to the reply you have been waiting for …

Of course you exist! If I didn't believe that, I wouldn't write you emails and expectantly await all the consequences your replies will bring about for me. You are no different in kind from any other quantum system I interact with. (I'm sorry if that is an insult. And by the way, you should note the similarity between the language in the second sentence of this paragraph and that used in Section 8.1—it is no accident.) Was this not already written so clearly in the paper?

But I continue to reject your statement, "If yes, then you believe in hidden variables, despite your disparagement of them." My understanding of "hidden variables" or, better, "ontic variables"—the sort of thing Bohmians and the like hope to reasonably show as undergirding quantum mechanics—is that these, when found, will give a *closed* description of the essential content of "quantum measurement" (which of course means a broad range of issues/things) without reference to the agent using the theory. For instance, the contextual hidden-variablists (Bohmians being one species) are happy to put a hidden variable in the device as well as the particle, and think that that takes all the trouble out of the quantum interpretation conundrum. But a device ain't no agent using the theory as a theory of gambling, and that is why I keep myself at a distance from the hidden-variable conception. I think we will have understood something deep when we understand why the agent is only so stubbornly removed from the conception of the theory. Frankly, I think it can't be done without incurring the even greater mysteries the advocates of those approaches simply ignore. (The endpoint of those researches, I am quite convinced, will be lifeless, block universes of one variety or another—ones that can never be resuscitated, no matter what amount of Dennettian-style sophistry.) Worse than that, I see the ontic variable program so conceived as a dead end for developing the next stage of physics.

Quantum mechanics (and all its babies, like quantum field theory or even, potentially, though not likely, string theory) cannot be the end of the story for our physical understanding. I tend to think it is only the beginning. But I don't see a development going forward that doesn't recognize the agent as the central point (the "center of narrative gravity") of any particular use of the theory.

I know these answers leave you deeply unsatisfied. Your worry was that I don't believe in your existence, but that's the easy part of the conundrum. The hard part is in assuring my own existence and efficacy, and that is what every physical theory up until quantum mechanics left out of its worldview. You shouldn't let the small corrective that quantum mechanics provides frighten you into thinking it gives the opposite of what it actually does!

Wisemanism 47: *See, I'm not really an anti-Bayesian ogre. :-)*

I never thought you were. You read our papers more *thoroughly* than anyone else on the planet, even in those moments when you are unsympathetic; surely that is revealing something.

06-07-09 *Emailing: 0907.0416v1, 2* (to G. L. Comer)

Comerism 24: *Now, for a little Devilishism:*

Is there not a uniqueness property for the Schrödinger equation? That is, for a given set of initial conditions, $\{\Psi(0,x),\dot{\Psi}(0,x)\}$, one gets a unique solution for Ψ. Sure, I can decompose in terms of a particular basis, but that doesn't change Ψ, right? For a specific example, take the classic Hydrogen atom problem. We specify boundary conditions, a coordinate system, energy eigenstates, and a solution is the result. Moreover, we get the Balmer, Lyman, etc series for the spectra, whose accuracy is without doubt.

Is there not a "best" wave function for this system, independently of the agent? I mean, if I'm a stupid agent—which is a pretty good assessment—I might place my bets based on the analogous solution for uranium. Obviously, the point is that the Schrödinger equation is a very good friend for those who want to make bets.

That is the perennial question, and it goes much deeper than quantum mechanics. It is a question about probability theory more generally. The Bayesian says, the apparatus of probability theory gives no means for saying whether one probability assignment is better than another (none that doesn't depend on a more primal probability assignment further back in the stream), just like the apparatus of mathematical logic gives no means for specifying which truth value a proposition ought to have (true or false). Truth-value assignments and probability assignments come from outside logic and probability *theory*.

It's a hard intuition to shake. Without opening a very long email exchange (which would surely be required), I'm afraid the best I can do is say to read Dennis Lindley's book *Understanding Uncertainty*: `http://books.google.com/books?id=z0ArJ_CDnssC&dq=Lindley+Understanding+Uncertainty&source=gbs_navlinks_s`.

The bottom line is, our common quantum-state assignments "work" in the way that a 50-50 assignment "works" for most coin tosses. There is nothing absolute in the statement. Only an expression of our doing our best to get along in a world that's not completely revealed to us.

06-07-09 *Test the Born Rule Another Way* (to R. Laflamme)

Now it is me who is a week behind on email!

Yes, of course, I'm still interested in an experiment. The final version of the paper I sent you can be found here: `http://arxiv.org/abs/0906.2187`. (It grew by a factor of two since the draft I had previously sent—the proposed experiment is now to be found in Figure 2 on page 20.) The paper just got an "invitation" from *Reviews of Modern Physics*—so that will be its final coordinates.

I'd be very flattered if you'd think about how it might be done with techniques readily available to your team. Aephraim Steinberg already has plans to do something similar—he tells me he's got two students on it, the equipment ordered, and plans drawn up. So I'm pleased that something serious will happen there. But I figure synergy, cooperation, and/or competition from you guys could only be a good thing. (De Martini, as well, seemed to take quite an interest in the idea when we met in Sweden, but that may mean nothing in the end.)

Anyway, if you'd like to talk, tell me when to come by.

06-07-09 *Refeeding Quantum Mechanics* (to B. C. van Fraassen)

Thanks for the notes, and indeed thanks for the interest! Thinking of your being in San Francisco, I should send you a picture of the painted lady my wife and I are making here in Waterloo. It is a shock to the system of the locals, though we think the color scheme is pretty standard/simple for such things.

I read your notes several times over to make sure I wasn't missing something: If you don't mind, you'll have to tell me! I think the main confusion might be localized in my usage of the label "Bayesian." Sorry to have caused you trouble over that.

First, responding to

van Fraassenism 14: *For the orthodox Bayesian, conditional probability on A, if* $p(A) = 0$ *has no sense.*

and

van Fraassenism 15: *I notice first of all that when you introduce conditional probability with equation (1):* $p(A, B) = p(A)p(B|A)$ *it is presented in a form that is satisfied also if* $p(A) = 0$*, provided only that* $p(B|A)$ *has some numerical value or other. But I guess you did not write it that way for that purpose*

I think this means I'm a heterodox Bayesian then. Because I had put (what I call) "Bayes Rule" in the given form for just that reason. And I do think $p(B|A)$ has sense when $p(A) = 0$. Funny, just the other day, Rüdiger Schack wrote me this:

Schackcosm 119: *I'll be waiting for you at arrivals, where you emerge from customs. If I am late, wait a little. We are only 8 minutes from Heathrow, so if I am not there (extremely unlikely, but we Bayesians can condition even on probability zero events), you can always call us at home.*

If the label is appropriate, maybe I should call myself something a "Richard-Jeffrey Bayesian". For he takes the same stance in his book *Subjective Probability, the Real Thing*. See the discussion in Section 1.4 (pages 18–20) of `http://www.princeton.edu/∼bayesway/Book*.pdf`.

Does that now fix everything up in your mind? Perhaps when we repost a new version of the paper, we should be more careful to define our particular flavor of Bayesianism (i.e., the Richard-Jeffrey style, etc.). What label would you give to the flavor? Is one already existent in the literature?

On one of your other points,

van Fraassenism 16 : *Really important: that coherence argument assumes that the events A are not just mutually exclusive but form an exhaustive list.*

you are absolutely right, and it was a mistake for me to leave that implicit.

I know I've invited you before, but any interest in giving us a visit in Waterloo sometime soon? (The wine list at our institute isn't half bad.)

07-07-09 *Deriving the Urungleichung from Fundamental Probability Theory* (to R. Adler)

Rüdiger and I have had a chance to study your note. Thanks again for the interest. I don't believe we've ever heard the compliment of anyone "obsessing over [our work] for months"! I assure you, it gives us a very pleasant feeling!

Anyway, to your point, it would be nice if it were so simple . . .

But you'll note that your x or Pr(state in the sky is $\Pi_i \,|\, \rho$) is not actually a probability distribution. If you sum over the index i, you do indeed get 1, but the individual x can go negative. For the extreme case of this, note that $Pr(\Pi_i$ observed on $\rho)$ can vanish if ρ is a density operator orthogonal to Π_i; thus x can go all the way down to $-1/d$. In fact, there is no good way to think of the Π_i as ontic variables (i.e., elements of reality).

It is true that for any *particular* measurement on the ground, one can scheme up a hidden variable explanation to go <u>from</u> $q(j)$ <u>to</u> $s(j)$ in our notation (though not the other way around). See Eqs. (52) and (53) in the paper. That's a little similar to your idea, though it takes the state $q(j)$ on the ground as more basic than the state in the sky $p(i)$. But that is only true for any *particular* measurement. When one stitches together all the urgleichung diagrams (like Fig 2) for *all possible* measurements, if full quantum mechanics does indeed arise from this process, then an all-encompassing hidden-variable interpretation (like the one just mentioned, but working for all measurements on the ground) must be excluded. We've been thinking hard about adding a subsection to the paper explaining this latter point (as we have already had to make it to Bacciagaluppi, Uffink, and Spekkens and think we'd now like to preempt its being asked again!). If we do write that and repost, I'll certainly let you know when the time comes.

You're a lucky person, living in New York. When I lived in New Jersey, I would always get such great inspiration by spending a day in the Village (and a morning in The Strand).

07-07-09 *Why Bell Is My Friend* (to B. Dreiss)

I apologize for keeping you waiting so long. Right after Sweden (where I wrote you from last), Schack came to Waterloo for two and a half weeks of collaboration, and all else got dropped. I'm only now catching up on emails.

The reason my paper confuses you is because I believe there is absolutely nothing wrong with Bell's derivation. And that remains true even if Bell himself wasn't clear-headedly Bayesian (he died too young). The derivation of Bell's original inequality and many other similar inequalities (like the Clauser-Horne-Shimony-Holt one or Hardy's, etc.) are just right, and Jaynes was wrong on this point. It is not that the derivation fails as soon as one takes into account a properly Bayesian understanding of probability.

And there are other, crisper ways to see this as well, without any inequalities at all. My favorite originally comes from Allen Stairs. I'll tell you about that, but first please reread the EPR section of Jaynes' "Clearing Up the Mysteries," particularly the first three paragraphs. The ultimate thing that powers Bell inequality violations in quantum mechanics is that EPR's reasoning reported there (and accepted by Jaynes) is just wrong. One can see that by not just considering two observables P and Q as EPR do, but by considering many overlapping ones, i.e., ones that have some (but not all) common eigenstates in an interesting interlocking fashion as one conceptually travels from one observable to the next. For *each* individual observable, under the assumption of locality, one can run through EPR's reasoning to assert that IT "must have had existence as [a] definite physical quantity before the measurement." But when you consider the whole lot, you find a contradiction—the observables are so interlocked that one and the same proposition ultimately has to be evaluated as both true and false, and that cannot be. So it is the EPR premise that is wrong—it is wrong to assert an observable "must have had existence as [a] definite physical quantity before the measurement." You can read this argument in more detail in Section V of my paper: `http://arxiv.org/abs/quant-ph/0608190` and also in the next mailing you can peruse one of my presentations of it. (The simplest proof of the statement can be had by contemplating two entangled "four level" systems rather than two qutrits; have a good study of Cabello's vectors in that presentation, and I think it will hit you over the head.)

Anyway, Bell's treatment is just (an earlier) probabilification of that kind of argument, and it is perfectly airtight: To be consistent with quantum mechanics, one either has to give up Einstein locality or preexistence of measurement values or some combination of the two. We QBists think that only preexistence need be given up—which is distinctly a positive for the William James side of my mentality.

Like I said, I'll send you one of my presentations. But also, let me attach two things to the present note. One is an old, very clear and nicely written article on the Bell point by David Mermin. I suggest reading it very carefully; it'll help get these things into your bones. The other article is also by Mermin. I attach it because it carefully attacks one of the more technical points Jaynes was trying to make in "Clearing up the Mysteries." Jaynes thought time-evolving hidden parameters would play a role in clearing everything up. But that path has since been developed extensively by Hess and Philipp and also by De Raedt, and it is still wrong (as Mermin shows in the HP case, others have shown for De Raedt).

In summary, here's where things stand between Jaynes and we QBists. Jaynes was absolutely right that quantum probabilities can only in the end be subjective, Bayesian probabilities, and that quantum states cannot themselves be elements of reality. He was also right when he said:

> Our present QM formalism is a peculiar mixture describing in part laws of Nature, in part incomplete human information about Nature—all scrambled up together by Bohr into an omelette that nobody has seen how to unscramble. Yet we think the unscrambling is a prerequisite for any further advance in basic physical theory

and

> Of course, the QM formalism also contains fundamentally important and correct ontological elements ... It seems that, to unscramble the epistemological probability state-

ments from the ontological elements we need to find a different formalism, isomorphic in some sense but based on different variables; it was only through some weird mathematical accident that it was possible to find a variable ψ which scrambles them up in the present way.

That is what our latest paper is indeed an attempt to do. Where we differ from Jaynes is in thinking that quantum measurement outcomes cannot represent features of some underlying unmeasured reality. That, we believe, is what Bell inequality violations (and the simpler Stairs style arguments) demonstrate. Jaynes held to his view because he thought that taking something like the QBist view would be tantamount to declaring "the Universe runs on psychokinesis." But we feel the Paulian Idea (Section 8 in our paper) is enough to block that extreme. The gambling agent is a crucial participant in the making or coming to be of a quantum outcome, but he cannot will the outcome he pleases. That is a crucial distinction, and with it one can have one's cake and eat it too.

I hope that helps.

So strange that you drove through Cuero. Not much to see there; it's great that you at least saw the one thing that's worth looking at. For some reason, I was almost about to say, "You're probably the closest to celebrity the town has ever seen — someone who has surfed both in Hawaii and Australia!" But then I remembered that John Wayne used to like to bird hunt at the Hamilton ranch. And when I worked at the corner gas station (where the McDonald's is now), one day I met a man who claimed to be Marlboro Man in their famous painting.

07-07-09 *PI Kids Quote?* (to N. Waxman)

Waxmania 1: *I thought that was a very neat observation from your daughter—may I put it in the newsletter some month (maybe this month?), in the little "PI Kids are Saying" section?*

I wrote it down as follows: ...

It was Katie, when she was 5. Sure, you can use it. But please change the quote to:

"Wow! It's bigger on the inside than it is on the outside!"[203]

Adding the "even" slightly takes away from the paradoxical nature of what she said, and in fact, I believe I have the quote exact.

08-07-09 *My Favorite Convex Set* (to R. Schack)

That boy wouldn't know the idea of simplicity if it bit him in the ass. It is clear as day why a SIC representation is to be preferred, and I have tried to express it to him countless times. He has never had a positive (or detailed) thing to say with regard to our program (though he professes to love Jaynes), and it starts to grate. Mostly it gets very old putting up with all these alpha-male wannabes—that is what his note was purely an expression of—and I thought it was time to express that clearly.

[203] At the time, we had no knowledge of "the doctor" and his TARDIS—now, the whole family and especially Katie are avid fans of the *Doctor Who* television series. We also now know that Katie's exclamation is where the resemblance between the TARDIS and Perimeter Institute ends: One represents an adventure in an infinitely surprising universe, whereas the other is a paean to a state of death, a.k.a. the "final theory" most of PI's residents yearn for. From today's standpoint, I wonder whether a better appropriation for Katie's wonderment might be to use it to describe quantum systems themselves: Even the minuscule qubit must have a huge interior. See 30-10-09 note "My Interiority Complex" to H. Barnum and other instances of the word "interiority" in the present document and "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.

09-07-09 *Trying to Make a New Start* (to C. Ferrie)

Ferrie-ism 2: *On that note, I'd like at some point to discuss the various approaches to subjective probability with you and hear the case for the Dutch book as opposed to the others, e.g. Jaynes' robot.*

Attached is one interesting thing to read with regard to this (and Hacking has many more than that). [I. Hacking, Brit. J. Phil. Sci. **16**(64), 334–339 (1966).] Hacking gives a review of Kyburg and Smokler's collection on subjective probability, and in it he is careful to make a distinction between two kinds of Bayesianism, the "conservative," and the "radical." The first roughly corresponds to Jaynes' thought, and the second to roughly de Finetti and Ramsey (and made somewhat more radical still by Caves, Schack, and me).

I found this little article very useful to read myself, and I have you to thank for that. Particularly, Hacking's small remarks on J. S. Mill being an "ancestor" of the approach. That made a light-bulb flash in me. For, though I had realized I was led to de Finetti's version of Bayesianism partially because it gave me a tool with which to preserve *locality* in quantum mechanics, I had not quite appreciated that I was probably also drawn to it by my predilection for a Jamesian version of indeterminism (the republican-banquet pluralism I write of in the paper). The latter, as James knew, has its roots in Mill. (James dedicates his book *Pragmatism* to Mill.) And it is always good to know where one's prejudices are coming from.

09-07-09 *Potentially Useful* (to C. Ferrie)

This one seems to be a potentially useful point of entry as well: Peter C. Fishburn, "The Axioms of Subjective Probability," Stat. Sci. **1**(3), 335–345 (1986).

09-07-09 *My Favorite Convex Set* (to C. Ferrie)

Ferrie-ism 3: *You may be interested in our recent paper:* `http://www.iop.org/EJ/abstract/1367-2630/11/6/063040/`*. It mentions SICs.*

I have added a citation to this, and will elevate the point to an enlarged discussion in the section introducing SICs. Rüdiger commented this when we first got your note:

Schackcosm 120: *From his paper, I suspect that this guy knows a lot about quasiprobabilities. From his perspective, our paper shows just one out of many possible ways of representing q.m. using probabilities. What he is asking is why this particular way of doing it and not another. Itamar asked a similar question. I am not too sure how best to reply.*

Here is the way I would reply. The point of all the various representations of quantum mechanics (quasi-probability reps, as well as things like Heisenberg vs. Schrödinger picture issues and even path-integral formulations), is that they give a means for isolating or emphasizing one or another aspect of the theory—they help bring a particular aspect into plain view, even if all the representations are logically equivalent. In our case, we want to bring into plain view (and try to make compelling) the idea that quantum mechanics is an *addition* to Bayesian probability, not a generalization of it. With that goal in mind, the SIC representation has always struck me as particularly powerful tool. With it, one can see the Born Rule as "really" a *function* of a usage of the Law of Total Probability in another context (one different than the actual). That feature, as far as I can tell, does not leap out in the same way from the more general "deformed probability representations" you explore in your papers with Joseph. That in a nutshell is the reason for my love affair with SICs.

09-07-09 *Articulation* (to C. Ferrie)

Ferrie-ism 4: *Here is what I was trying to say. The sentence "The only way anyone has seen how to do it is ..." is very uncharacteristic of your otherwise beautifully written exposition. It stuck out and then stuck in my mind as I read on. It called out to me "well ... this isn't quite right but it was the best we could come up with ..." It has a very apologetic tone which forced the skepticism I felt while reading on.*

I really would appreciate it—and this is not just apologetics now—if you would articulate in greater detail the source of your skepticism. What in detail, indeed, made you feel "It seems an ad hoc addition to coherence contrived to bridge the Peresian slogan to your functional relation between q and p." As I tried to express last night, part of the reason behind my bad reaction was not that you were talking about an isolated page or two in the paper (I have no illusions that the paper is perfect, I certainly know that it could be made much better, or even rewritten from the start). It was that you were referring to the very point of the paper with your sentence. (It only dawned on me after your reply yesterday that perhaps you didn't know this, that maybe you thought you were referring to some small thing rather than the paper as a whole.) So, it's important for me to get it straight. ... And it'll be good practice for you in articulation. ☺

13-07-09 *Articulation, 2* (to C. Ferrie)

Contrived is a very different word than "unnecessary and distracting." "Contrived," at least in my usage, usually conveys bad intent.

Anyway, there are a few reasons I resist the Coxian understanding of probability. The most important is this: In the usual way the system is developed (and maybe it is necessarily so), $\Pr = 1 \;\Rightarrow\;$ TRUTH. But it is utterly important for Caves, Schack, and me that this isn't so; there is an ultimate disconnect between probability statements and truth values for us, and that in particular allows us to preserve locality in quantum mechanics in spite of Bell inequality violations. And Dutch-book definition of probability allows for just what we need.

Still, let me think further about the notation.

Chris's Preply

I'm no gambler and perhaps this is why the Dutch book argument has never sat well with me. I found Jaynes' robot argument much more intuitive. So let me conjure up his ghost (perhaps a younger, more naive and better looking version).

A bookie asks the agent to commit to $p(B)$ given a conditional A-lottery. The agent gives $p(A)$ and $P(B|A)$ and is forced through coherence to bet on $p(B)$ as calculated through the law of total probability. But the bookie reveals that there is no A-lottery. You would say that the agent is no longer committed to $p(B)$ as calculated before but now is committed to $q(B)$ (which is $p(B)$ without the A-lottery). Now Jaynes would say this is *inconsistent* as the 1st law of robots is a robot may not injure a human being ... wait ... no, it's consistency (the Jaynesian equivalent of Dutch book coherence I presume). There is a unique value of $p(B)$ no matter how you calculate it: $p(B)$ is $p(B)$ is $p(B)$ ....

If the agent is told something new then he is now committed to $p(B|\text{something new})$ and not $p(B)$. For the bet, the bookie really asked for $B|C$, where $C =$"The A-lottery has been performed", and then he later reveals that he will take bets on just B. Now "$q(B) \neq p(B)$" is just "$p(B) \neq p(B|C)$". This is something which should be obvious and much more clearly formalizes the statement "measuring A matters even if we don't reason about the outcome".

This is why I think the conditional lottery is unnecessary and distracting.

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\begin{sarcasm}
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You didn't get that from "contrived"?
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\end{sarcasm}
```

13-07-09 *Quantum Randomness* (to K. Martin)

When Schack was visiting me last week, I got him to give me a copy of the paper he and Caves are writing on quantum random number generators (as seen from our quantum Bayesian perspective). Attached is—he tells me—a very ROUGH DRAFT. Still, I thought it might be useful to send it to you. Is that the sort of thing you were interested in?

Things are progressing somewhat on the SIC end. Schack and I spent most of the last two weeks hashing out our next paper—a companion piece to our "Quantum Bayesian Coherence." This one will be called "Quantum Bayesian Decoherence" and will be an attempt to put Zurek's decoherence in its place. Appleby is presently chasing down an idea in dimensions $1 \bmod 6$ and is as happy as fox in a henhouse. And get this, Aephraim Steinberg's group at University of Toronto are starting up a $d = 3$ SIC experiment.

15-07-09 *Some $d = 3$ Measurements* (to R. Laflamme)

Let $d = 3$ and $\omega = e^{\frac{2\pi i}{3}}$.

Set 1:

$$
\begin{bmatrix} 0 \\ -1 \\ 1 \end{bmatrix} \quad \begin{bmatrix} 0 \\ -\omega \\ \overline{\omega} \end{bmatrix} \quad \begin{bmatrix} 0 \\ -\overline{\omega} \\ \omega \end{bmatrix}
$$
$$
\begin{bmatrix} 1 \\ 0 \\ -1 \end{bmatrix} \quad \begin{bmatrix} 1 \\ 0 \\ \overline{\omega} \end{bmatrix} \quad \begin{bmatrix} 1 \\ 0 \\ -\overline{\omega} \end{bmatrix}
$$
$$
\begin{bmatrix} -1 \\ 1 \\ 0 \end{bmatrix} \quad \begin{bmatrix} -1 \\ \omega \\ 0 \end{bmatrix} \quad \begin{bmatrix} -1 \\ \overline{\omega} \\ 0 \end{bmatrix}
$$

Set 2:

$$
\begin{bmatrix} -2 \\ 1 \\ 1 \end{bmatrix} \quad \begin{bmatrix} -2 \\ \omega \\ \overline{\omega} \end{bmatrix} \quad \begin{bmatrix} -2 \\ \overline{\omega} \\ \omega \end{bmatrix}
$$

$$\begin{bmatrix} 1 \\ -2 \\ 1 \end{bmatrix} \quad \begin{bmatrix} 1 \\ -2\omega \\ \overline{\omega} \end{bmatrix} \quad \begin{bmatrix} 1 \\ -2\overline{\omega} \\ \omega \end{bmatrix}$$
$$\begin{bmatrix} 1 \\ 1 \\ -2 \end{bmatrix} \quad \begin{bmatrix} 1 \\ \omega \\ -2\overline{\omega} \end{bmatrix} \quad \begin{bmatrix} 1 \\ \overline{\omega} \\ -2\omega \end{bmatrix}$$

15-07-09 *Measurement in the Sky without Magic* (to C. Ferrie)

Dear CFe,

I know you want me to change my notation. Eventually I'll tell you why I resist that. The text surrounding the definitions as well as Footnotes 5 and 6, it seems to me, already capture everything I wanted to say.

Also I'm not quite sure what you want of the paper. (Your remarks, or at least your phraseology, do continue to put me off a little; perhaps I continue to misunderstand their goal.) My own goal isn't to be "surprising," only clarifying. My big dream is to say what the content of quantum mechanics is without ever making a priori mention of "positive semi-definite operators," or "homogenous self-dual cones," or the like, or any issues to do with nonlocal-boxology. Indeed I want quantum mechanics to shake out to be an absolutely trivial theory in the end, at least from the proper point of view. In that regard at least, I feel the urgleichung gives me some hope, and I feel we have made some progress by realizing it might be taken as one of the foundational principles. To the extent that I want there to be any surprise at all, it is that people eventually slap themselves on the head and say, you mean the essence of QM might be so very simple?!

> `\begin{relevant non-sarcastic mild modification of William James quote}`
> I fully expect to see the [quantum Bayesian view of quantum mechanics] run through the classic stages of a theory's career. First, you know, a new theory is attacked as absurd; then it is admitted to be true, but obvious and insignificant; finally it is seen to be so important that its adversaries claim that they themselves discovered it. Our doctrine ... is at present in the first of these three stages, with symptoms of the second stage having begun in certain quarters. I wish that this lecture might help it beyond the first stage in the eyes of many of you.
> `\end{relevant non-sarcastic mild modification of William James quote}`

If I understand your point, you appear to be using the language of Stage 2.

Confused regards,

rare-earth Chris (old and quite oxidized)

15-07-09 *Measurement in the Sky without Magic, 2* (to C. Ferrie)

Ferrie-ism 5: *I'm generally not a very excitable guy. But this stuff really gets me going. I guess I've adopted you as my sounding board. Perhaps that is not acceptable conduct for a student. I suppose I could understand if it wasn't. Your inbox is probably bombarded with half-baked ideas.*

To be honest, yes it is. Attached is another reaction to the paper. Here's an exercise for you: Tell me what's wrong with what the guy says.

But your own note wasn't half baked like that. I just didn't see that you were telling me something I didn't know ... so it was hard for me to put it in context as representing a breakthrough in your understanding. As far as I could tell, you wrote down Eq. (54) from the paper with changed notation:

- I.e., I wrote $q(j) = (d+1)s(j) - 1$
- You wrote $p(D_j|N) = (d+1)p(D_j|Y) - 1$

And I thought, yeah, that's what I wrote. (Where, though, it is key that Y and N don't refer to just any conditions, but an intermediate conditional lottery.)

Clearly—with hindsight—the changing of notation means something deep to you. I just haven't gotten what that something deep is.

I am *trying* to be a better person ...

15-07-09 *Bohmian Mechanics* (to J. Emerson)

Emersonia 2: *I am putting together a list of topics / lecturers for another edition of the interpretations/foundations course for this upcoming Winter. I'd like to put a bigger emphasis on de Broglie – Bohm theory and would appreciate your recommendations regarding potential lecturers.*

I would also like to hear your recommendations for many worlds and decoherent histories.

You might consider Wayne Myrvold for the many-worlds slot. He strikes me as the rare person who might be able to do this:

Emersonia 3: *I'm tempted to go with someone who will overview the approaches but who can also be critical of the conceptual and technical shortcomings of those approaches.*

What's rare is someone seriously knowing the details of one of these interpretations (to the last technical bone) without having been a full believer to begin with. I.e., belief most always comes first, details, if any, are far secondary.

16-07-09 *Some Things to Think About* (to C. Ferrie)

Let me give you something specific and mathematical to think about if you're interested. See pages 25 and 26 of the attached talk ["QBism at QTRF-5"]. I define something called a "maximal consistent" subset (MCS) of the probability simplex (over d^2 outcomes) there. It is not difficult to show that all maximal consistent subsets must be convex.

Also, all maximal consistent subsets have vaguely quantum-state-space-like properties. But how far does that go, I wonder? Thus let me pose a couple of specific questions to show the flavor of things that we might profit from exploring.

1) Let S be any MCS. Must its extreme points form a connected set? Or can one find a counterexample?

2) Let S be any MCS. Must its extreme points form a smooth manifold?

3) If so, what can be said about the dimensionality of that manifold? Can one place any bounds on the dimensionality?

These are the sorts of things Åsa and I are thinking about at the moment, and in the Fall when my students get here, I want to greatly expand the effort. Åsa has some intuition that the answer to 2) should be yes, but I've got no strong feeling, and certainly no idea how to prove anything in this regard. If you have any input, it'd be wonderful.

20-07-09 *Remembering Hans* (to H. C. von Baeyer)

The last couple of days I've been writing my intro for the CUP edition of my samizdat. Attached is where I've gotten so far. It formalizes a bit of the story I first told you and Marcus in the Black Hole Bistro one day (I had never told anyone else before):

> I should tell you the strange story of how I came to be a physicist in the first place. It had nothing to do with seeing or feeling some beautiful order in nature, like the kind of thing I would read about in the physics biographies. It was not like Einstein's childhood experience with a compass, where he came to the conviction that there had to be "something behind things, something deeply hidden."
>
> My career, instead, is to be blamed on the 1970s television stations of San Antonio, Texas, ninety miles down the road from my little hometown Cuero. In those days, there was a steady stream of science fiction and horror films coming from the antennas every Friday, Saturday, and Sunday. And every weekday there was an episode of *Star Trek* or *Lost in Space* waiting for me just as I got home from school. I gained a taste for two things: the image of the noisy, swirling atom that concluded each commercial break of Channel 5's late-late-night *Project Terror*,[204] and the idea that mankind knew no insurmountable bounds. I dreamed my friends and I could fly to the stars and back if we wanted. Television molded my idea of what the world ought to be: one of danger, constant adventure, and yet, with luck and hard work, one ultimately malleable to what we want to make of it.
>
> That thought led me into junior high school, where I decided life was too short: If I wanted to see myself flying to the stars with my loved ones (successfully) waiting behind to cheer my return, I had better get to the technical details of making it happen. So I read popular physics book after popular physics book looking for the ways and means. To my genuine surprise and deep sadness, no real-world method emerged; I was left with the science fiction I had started with. I only learned from the wisdom of physics that my dream would stay a dream.
>
> I suppose that is where my story might have ended, but it did not. Instead, it came to me slowly, and then more and more firmly, "Physics must be wrong!" I told myself that, and I was quite serious about it. I knew I had to become a physicist, not for the love of physics, but for the distrust of it.

There's much more to come in it, but I think I'll go ahead and send you the draft so far. As if you need more prodding, even in my autobiography you'll see this Pauli thing runs very deep with me. It was you who brought up the opportunity of writing something fresh; now I don't want to see it slip away!

[204] Look it up on YouTube.

22-07-09 *Charlie Stories* (to D. W. Leung)

Thanks for the comments. I cleaned up the Charlie story still a little more; I think it's reached its final form now. There's still so many stories to tell about everybody else, but I'll attach the latest draft for your amusement. It'll be a fun exercise when I finally get to John Smolin's story.

Leungism 1: *Saying teleportation marks the beginning of quantum information is a bit like saying that Shor's algorithm marks the beginning of quantum computation ☺ (guess what's the omitted punchline ...)*

That's right, I would say Shor's algorithm marked the beginning of quantum computation *as a field*. I.e., that's when outsiders widely started to pay attention to it. To help stave off confusion a bit, I have now italicized "as a field" in the footnote. Thanks for bringing this up to me.

23-07-09 *Fact Check?* (to B. W. Schumacher)

I've been working on an introduction for one of my books, and I just finished up a story on Brassard. In it I use an anecdote I heard from you, and I wonder if I can ask you to check its accuracy? I'm quite sure I screwed up some details—my memory is not that good. In any case, I'll certainly be tweaking the story (trying to get the best drama), but with this much down on paper, you can already see the kind of thing I'm aiming for. I just mostly need to get the facts straight. See attached file, starting page 6:

> Gilles Brassard, with Charles Bennett, is a father of quantum cryptography: The Bennett-Brassard 1984 quantum key distribution protocol started the field. But, from my perspective, he is dearest to me for all that he has enabled. Ben Schumacher once told me an anecdote worth repeating in this context. Ben says that when he first heard of the discovery of (Peter Shor's) quantum factoring algorithm, he thought it was a joke that Gilles had propagated—he didn't take it seriously. The reason he didn't is because a year earlier, at one of the "quantum information parties" Gilles regularly organized in Montréal,[205] Gilles joked around, "We're going to use quantum computers to factor numbers." This may have had no direct influence on Shor, but the tendrils of scientific discovery are sinuous, and one really never knows all the influences that come together in the making of an event. The greatest things happen not because of lone scientists in isolation, but because of communities.

Ben's Reply

> The anecdote agrees with my own memory. As I recall it, we were in Montréal and Dan Simon described the work he'd been doing there on quantum period finding. Gilles made this comment, grinning. I seem to recall that Shor had read Simon and was motivated thereby; I do not know if Shor had heard Gilles' joke (or if it had occurred to him independently).

[205]Like the one in 1993 that led to the discovery of the quantum teleportation protocol.

26-07-09 *Specifically Paulian* (to H. C. von Baeyer)

I'm sitting on my new front porch, waiting for a thunderstorm to roll in, and thinking Paulian thoughts. See attached photo; you can just see the chair I'm sitting in. (The porch isn't painted yet, but you'll get some indication of its ultimate colors from the second photo attached.)

... Storm just came, and I had to run inside. It's a fierce one! ... [Some time passes.]

The storm has mellowed a bit, and I'm now back on the porch. I'll finally reply to your note of yesterday. (Sorry that yesterday turned me into a liar.)

von Baeyerism 31: *I'm reading at this moment: H. Atmanspacher and H. Primas (eds.) Recasting Reality: Wolfgang Pauli's Philosophical Ideas and Contemporary Science, Springer 2009.*

Your writing this reminds me that I still do not have my copy. I purchased one at discount, while in Sweden, but Springer had to ship it to me. I will get on them.

von Baeyerism 32: *I find satisfaction in the overlap between some of its entries and my own essay, which is now on the web in the emag The Global Spiral, July/August 2009, search under Authors. (I'm not sure how to formulate a proper web citation.)*

You give a website? I have printed it out; it doesn't look so bad as you made it out to be. I haven't re-read it yet. Is it substantially different from the version I read in January (which I understood to be the finalized one)? In any case, I will read/re-read it at some point. I want to be better in your head, if nothing else to help anticipate the passages you might explore.

von Baeyerism 33: *The DO. The term "detached observer" does not merit an entry in the book's index. I think it is more important than ever to promulgate the idea, and have accordingly moved our joint project to the top of my writing agenda.*

Of course, I am very pleased to hear this! Interesting to hear that DO issues have very little representation in that book. I wonder why that's the case? If I recall correctly, I believe I did read one article there that *did* have some discussion on the subject. Could it be that it was just an oversight in the index?

It's probably worthwhile mentioning my self-analysis for why I don't completely remember the article, or even for sure whether it was in that book. The trouble is most everything these guys write is always the same and so becomes a blur. Nor, do they ever go an ounce beyond where Pauli went (or, at least, where the part of Pauli they've read has gone). Did you read the Enz entry in my Intro? The guy wrote 19 papers on Pauli, 19!—I read every one of them—and in the end didn't learn much more than I might have in three well chosen ones from the full list.

I had a similar experience with Folse on Bohr. 31 papers! They were all great on so many other details, but I never could get a sense of what Bohr really meant by "the quantum postulate." Folse used the term liberally. So I thought I would invite him to the "Shannon Meets Bohr" session I organized in Växjö in 2001 and get the issue settled. (This was the great meeting we had with Caves, Greenberger, Mermin, Peres, Schumacher, Smolin, etc., in attendance.) But to no avail. Folse said nothing new on that subject; he could say nothing beyond repeating the three word phrase. The thing that shocked me was that he seemed to have no sense that one should treat these things as *springboards* to deeper thoughts.

For once I'd like to see someone say (with reasonable analysis), "This is what Pauli would have made of the no-cloning theorem ... " Or, "Pauli would have understood that the no-cloning theorem didn't at all carry the essence of quantum mechanics." Or, "No, Pauli would have seen the deep statement of his unDO in the Kochen-Specker theorem, and here's why." Or, "Here's where quantum information theory might help us formalize a bit of what Pauli was after."

We can't go that far in our article, I know. But one thing I think would be nice is if we could push the reader to think of Pauli's ideas as the beginning of a bigger and better quest. Somehow give the reader the sense that there's something solid to do.

von Baeyerism 34 : *Pauli and religion. There is a lot of research on Pauli's legacy in such fields as philosophy, metaphysics, psychology, biology, cognitive science and the philosophy of mathematics, but I don't see much on religion per se. I will continue my interest in that.*

And I hope you will never stop keeping me apprised of what you're thinking even here. I try my best to gather data from all sources.

26-07-09 *A Memory* (to H. C. von Baeyer)

By the way, I remember you once writing me this about the CFS use of Kochen-Specker to establish the non-preexistence of measurement values:

von Baeyerism 35 : *I'm reading Caves, Fuchs, and Schack on quantum certainty, and thinking that I'm getting the drift, when I come up to an example – which is my way of understanding – and whammo, on page 10 you hit me with 33 states, which are (the cute little "of course" rubs salt in the wound) connected by 96 rotations and one shoulder separation. I can't envision that. Can't you use a simpler manifestation of KS like Mermin's very cogent* 3×3 *matrix, or maybe it was* 4×4*? As popularizer I'm always on the lookout for simpler versions.*

Did I ever make you aware of Cabello et al.'s version of KS? I well believe it can get no simpler. See attached file, page 11. Using it, the CFS-variation-of-Stairs argument is then powered via the use of two qudits, where $d = 4$.

29-07-09 *Pseudo-QBist State Spaces* (to Å. Ericsson)

I thought of a title for your paper. See above.

Also, thinking about your gingerly reactions yesterday, I thought I'd remind you of some of the words I put in the Mlodinow review:

> And the malaise of Professor Gardening (whose real identity is protected in the book), though I had not seen in it a professor, I had seen it in plenty of graduate students. The burdens of not just doing physics, any physics, but *important* physics, is sometimes too much—it is too elusive a phantom to even know how to get started.

The same goes with papers: Not every paper has to be important or conclusive, and in fact most aren't. Sometimes the only role of a paper is to get a problem started. Caltech tolerates elusive phantoms, I don't!

31-07-09 *Preskill Done* (to D. M. Appleby)

I finally got the Preskill story down. Boy that was hard! Here's an excerpt:

When we have discussed the interpretational issues of quantum theory, I have gotten the sense that what John finds most suspicious of the quantum Bayesian approach I promote is that he *thinks* it treats observers as unphysical systems. I say, "Not at all; John, you are a physical system to me." The real issue is one of inside versus outside. Contrary to the textbook exposition of quantum mechanics, a wave function that I write down about him is descriptive not of the outside, but of the inside, namely me: It captures what I believe will happen to me if I interact with him. If he knew what I believed, he'd probably do something to surprise me.

I think John's secondary worry is that unless one takes something like the Everettian approach to quantum mechanics, one cannot do quantum cosmology—the idea is that the observer must be excised from a fundamental role in the theory if one is going to address cosmological issues. (In a phrase, what is desired is a theory that is all outside and no inside.[206]) The Everettians think they do that, but a quantum Bayesian would say, "only in a way that leaves the observer's clothes behind." I tried to impress John once by writing down $|\,\Psi_{\mathrm{universe}}\,\rangle$ and saying, "Look, I can do that too." I don't think it worked.

Most recently, I've tried a new technique in my lectures. I draw a stick figure on the chalkboard along with a little blob near him, and say, "There's an observer with a small physical system in front of him: He can profitably use the formalism of quantum mechanics to estimate what might happen to him if he interacts with it." Then I say, "But there's nothing to keep us from talking about a larger physical system." I extend the blob, making it larger. I do this a couple of times, at first imperceptibly putting a bend in the blob, making it a little kidney-bean shaped with the observer at the center of its curvature. I make it more pronounced with each iteration and finally go for broke: The observer is completely surrounded by the physical system he is considering, and I declare in great triumph, "That's quantum cosmology!"

One of these days maybe I'll work up the nerve to show John.

Marcus's Reply

I realize that you have a lot to do so don't even think of trying to answer this now. But when you have the time I am curious: what is the criterion for being a physical system? What would Preskill have to do in order to convince you that he wasn't a physical system?

Also, how does this fit in with Pauli's synthesis of science and religion? Would Pauli agree that Preskill is a physical system? (just a physical system? [shades of vicious abstractionism]). And if he wouldn't, would he be wrong? Do you think?

I guess virtually every religion (absolutely every religion?) would say that a human being isn't just a physical system. I am not sure what they mean by this (I would ask of virtually every religion the question I just asked of you: how does one tell whether something is a physical system or not? What is the criterion?). But whatever they mean, I get the impression they do say it. I also get the impression it is an important feature of religion.

I am being a little disingenuous here, you see. I pretended not to know what is meant by saying a human being isn't just a physical system. And in one way that is true: I don't know how to explain what is meant (which is why I asked the question: perhaps

[206] On first pass, the Texan in me wanted to write "all hat and no cattle," but I repressed that.

you can explain?). But the statement kind of resonates. In another way (though not in a way I know how to explain) I do understand what is meant.

So, although very obscure, I do feel that this idea, that a human being isn't just a physical system, is an important feature of religion. Perhaps even an essential feature. So the question is: is this a feature of religion which has to be dropped in Pauli's synthesis? And, if it does have to be dropped, what is left? What kind of synthesis would it be which leaves out this feature of religion?

The old Soviet Union claimed to be a synthesis. Notionally Uzbekistan and the Russian Republic were equal partners. But that was, of course, a cheat. And I am thinking that a Paulian synthesis in which Preskill is just another physical system (vicious abstractionism again) would be something like that, with science playing the role of the Russian republic and religion playing the role of Uzbekistan.

Also, how does all this fit in with your wish to deconstruct physics? Given that aim, why do you say that Preskill is specifically a physical system? As opposed to just a system?

Perhaps we should say that Preskill is a deconstructed physical system. But in that case why not simply say that he is an unphysical system, and be done with it?

In short: what is the burden of the qualifier "physical" here?

Hans Christian von Baeyer's Comment

Marcus, the question seems to me to be related to reductionism — to "nothing buttery." A cathedral is a pile of bricks, but more. Even a shed is a pile of wood, but more. Steven Weinberg, the quintessential reductionist, admitted that he was not an uncompromising reductionist, but a compromising one. So you can learn a lot about a physical system, but you hit a snag of complexity when you want to understand what Preskill is.

The prettiest thought I learned in Phoenix the other day was this: Human nature is so exquisitely complex that you can't describe it, image it, limn it if you will, without recourse to a more complex language than that of mathematics. That language is poetry. So, to come to grips in any meaningful way at all with questions of morality, say, you need to read King Lear – neither legalese nor quantum mechanics have anywhere the resources required to do the job.

Preskill is a physical system all right — unfortunately he's so complex that this statement doesn't get us very far.

31-07-09 *Shelter Island and Bennett, Preskill, etc. Stories* (to D. Gottesman)

I learned the source of confusion over Shelter Island: The official name for the meeting was "Shelter Island Conference on the Foundations of Quantum Mechanics". On the other hand, a standard description of the conference goes:

> The general problem the conferees were asked to address was the impasse that elementary particle theory was perceived to have struck over the preceding decade and a half or so. Specific difficulties in quantum electrodynamics (QED) and in upper atmosphere meson phenomena were of particular interest, especially in light of Lamb's and Retherford's recent experimental findings on the fine structure of hydrogen, and

Rossi's experiments with cosmic rays. Both Lamb and Rossi were in attendance, and both were asked to report on their findings. Discussion of the problems raised by these experiments, and the papers that resulted from these discussions, produced significant advances in the development of QED.

And this is reflected in the list of participants:

Hans Bethe
David Bohm
Gregory Breit
Karl K. Darrow
Herman Feshbach
Richard Feynman
Hendrik Kramers
Willis Lamb
Duncan MacInnes
Robert Eugene Marshak
John von Neumann
Arnold Nordsieck
J. Robert Oppenheimer
Abraham Pais
Linus Pauling
Isidor Isaac Rabi
Bruno Rossi
Julian Schwinger
Robert Serber
Edward Teller
George Uhlenbeck
John Hasbrouck van Vleck
Victor Frederick Weisskopf
John Archibald Wheeler

I'd only consider Bohm, Feynman, von Neumann, and Wheeler as "quantum foundations" in the usual PI sense.

Speaking of history, you might enjoy some of the stories of people we know in the attached file. I just finished it; it's hot off the keyboard. (People stories start up on page 4.)

03-08-09 *Interpretation of Bayesian Probabilities* (to D. H. Wolpert)

Oh, I believe you: In practice people are most always "incoherent" in the Dutch book sense. I read Dutch book coherence (and hence the axioms of probability) normatively: It is what you should strive for.

The quote you refer to is not van Enk himself, but Frank Ramsey.

David's Preply

Skimming over your fun document, I came across your description of van Enk's view. I don't know what he precisely meant by saying that to measure "a person's belief . . .

propose a bet, and see what are the lowest odds which he will accept". But it is well-established experimentally that as far as bets are concerned, people have no consistent "degrees of belief" in events. In some situations, even if money is on the line, a person will at the same time take a bet that requires $P(a) > P(b)$ and also another bet that requires $P(b) > P(a)$.

Look up "Allais' paradox" on Google. Another good one is the Ellsberg paradox, if you want to be cured of the belief that people use Bayesian degrees of belief in the real world (as opposed to in philosophy land).

Here are some other great references:

- C. F. Camerer, *Behavioral Game Theory: Experiments in Strategic Interaction*, (Princeton University Press, 2003).
- C. Starmer, "Developments in Non-Expected Utility Theory: The Hunt for a Descriptive Theory of Choice under Risk," Journal of Economic Literature **38**, 332–382 (2000).

03-08-09 *Where I've Been* (to R. Schack)

Schackcosm 121: *Sorry, it isn't Logan, it's James Logue,* Projective Probability*.*

Oh, I see. Has it had any impact on reflection issues? What do you think of the main thesis of the book: "Based on a strongly subjective starting-point, with probabilities viewed simply as the guarded beliefs one can reasonably hold, the theory shows how such beliefs are legitimately 'projected' outwards as if they existed in the world independent of our judgements." ??

03-08-09 *Abner Story* (to A. Shimony)

I wrote a story about you: It's in the attached document, an introduction for the Cambridge U. Press re-issue of my original email samizdat. It'll be titled *Coming of Age with Quantum Information*. Your story starts on page 18, and I also say some things about you in the Zeilinger story on page 21. I hope you enjoy.

Let me also alert to a new paper of mine with Schack; it is my pride and joy from the last few months. The title is "Quantum-Bayesian Coherence," and you can find it here: `http://arxiv.org/abs/0906.2187`. In it, we show a new way of writing the Born rule, a way that makes it look very, very close in form to the Law of Total Probability. We use that then to argue that the Born rule should be viewed as an extra (contingent, empirical) addition to Dutch book coherence, to be used when one gambles on the outcomes of one's interactions with a quantum system. It is a way of having our Bayesian cake and eating it too. Section 8 addresses broader philosophical issues that may interest you: How (I see) to remain subjectivist Bayesian about quantum probabilities and still accept objective indeterminism as one of the deep conclusions of quantum theory.

I hope all is going well for you. I have not seen you in quite a while.

Abner's Reply

Your email novel (samizdat) is somewhat repetitious in plot, but as a compensation it is rich in characterization.

I was glad to be reminded of quoting Edward Lear's "They all went to sea in sieve, they did", which sums up my skeptical attitude towards a subjectivist interpretation of the probabilities of qm. Seeing the quotation reminded me that it was not original with Lear, since one of the witches in the fist act of *Macbeth* says, "But in a sieve I'll thither sail, And like a rat without a tail I'll do, I'll do, and I'll do" — wonderfully creepy!

04-08-09 *Counterfactualizable* (to R. Schack)

Schackcosm 122: *What do you think of this quote from Logue, attributed to L. J. Cohen's book* The Dialogue of Reason*:*

> ... people are naturally inclined to think in terms of counterfactualizable rather than non-counterfactualizable probabilities wherever possible, reflecting a general dominance of causal reasoning ... over reliance on bare statistics.

Nice. Does this mean the upper path in our diagram is the more basic?

04-08-09 *Personal Probability as ... Mode of Self-Attribution* (to R. Schack)

Schackcosm 123: *If you find it, maybe you could read those 1.5 pages. It's section 1, entitled "Personal Probability as Propositional Attitude and as Mode of Self-Attribution". I know nothing about the context or where in philosophy this kind of idea is discussed, but it sounds a lot like our conviction that one must not think of events or measurement outcomes as existing independently of the agent.*

Like usual, I have great difficulty *even* reading van Fraassen. I kind of doubt that that is what he is getting at, but I don't know. I did definitely note a kinship between us and Hacking, however. (At least if I read him correctly.) See section 6 of the attached on "personal possibility".

04-08-09 *Charlie and Sappho Sitting in a Tree, ...* (to C. H. Bennett)

Well, I'm not too proud to help you remember. Rüdiger sent me the note below, and I'm not ashamed to forward it on to you (to help you do so).

Rüdiger's Preply

> I had a long talk with Charlie Bennett at Dagstuhl, after his after-dinner speech on forgetting. I'd had quite a bit of that good Dagstuhl red wine, so although the discussion started with the reality of ψ, we then moved to logical depth, Homer, Borges, Kodaly, and Charlie's father's compositions in the octatonic scale which he played to me on the castle's grand piano.
>
> His talk was implying there is a sense in which Sappho's lost poems are not real. I said then that if I had to choose between the reality of Sappho's poems and the reality of ψ, I would choose the former. He clearly liked this formulation and said he would try to remember it (although when he tried to remember it the next day he misquoted it by stating the alternative as having a theory that does not give a complete picture of the world).

04-08-09 *Preskill and Sappho As Well* (to C. H. Bennett)

To rub a little salt in the wound, see the Preskill entry in the attached. Particularly, I hope you enjoy Footnote 18:

> In a phrase, what is desired is a theory that is all outside and no inside. [Footnote: On first pass, the Texan in me wanted to write 'all hat and no cattle,' but I repressed that.]

Charlie's Reply

> I like "all hat and no cattle" much better, and am glad you didn't repress it (putting it in a footnote does not constitute a successful instance of repression). I wish you wouldn't try so hard to persuade me of your interpretation of quantum mechanics. I think we can agree that the difference between preferring the reality of Sappho's lost poems vs. the reality of the wave function is a difference of interpretation, a matter of aesthetic or religious preference, not susceptible to being decided by rational argument or proof/refutation, since my interpretation logically contains yours and yours logically contains mine. Moreover, I don't think it would even be desirable for one or another interpretation to win out, because that would impede the intuitions, often leading to substantive progress, that are stimulated, at least in some people, by other interpretations. Perhaps one could say that the interpretations of quantum mechanics enjoy a complementarity reminiscent of quantum mechanics.
>
> I think Rüdiger's way of casting the question, as to which you would prefer if you couldn't have both, is an especially good way of explaining (to a lay audience likely to be bored by such arcane matters) what these different interpretations of quantum mechanics are all about, and why they are alternative interpretations, not alternative theories.

04-08-09 *Charlie and Sappho Sitting in a Tree, . . . , 2* (to C. H. Bennett)

Bennettism 36 : *. . . not susceptible to being decided by rational argument or proof/refutation, since my interpretation logically contains yours and yours logically contains mine.*

Aye, there's the rub: Because I do not believe that that is true.

I'm gonna go put on my hat now . . .

04-08-09 *The Mermin Challenge* (to C. G. Timpson)

I thought of you the other day when I was rereading through Mermin's paper, "Writing Physics." Particularly at this point where he writes:

> The puzzlement only arises when you try to combine what quantum mechanics tells you about the possible results of a group of mutually incompatible experiments. When you actually do any one such experiment you lose the ability to do any of the others. Why worry about what might have happened in the experiment you didn't do, if you no longer can do it? That's a problem for philosophers, not physicists.

> I'm inclined to agree. The problem is that most philosophers who do worry about quantum mechanics differ from the physicists who refuse to worry only because they worry and the physicists don't. The philosophers have, by and large chosen to embrace nonlocality as a natural phenomenon, rather than homing in on what bad habits of thought and expression make so implausible an inference so hard to resist. Uncharacteristically for philosophers, they ought to be more worried about the nature of language, how it can trap us into formulating questions that have no sensible answers, and whether it is possible to restructure ordinary language in a way that liberates us from those built-in errors that make it so hard to think clearly about quantum physics. They ought, in short, to be worried about writing physics.

I thought, "That's perfect for Timpson!" So, I'll call it Mermin's Challenge and pass it on to you. In that regard, let me also give you a little thing I just wrote (and the reason I was referring back to Mermin's article). See attached. The Benioff, Mermin, and Preskill entries have a little bit to do with Mermin's challenge, and so may be entertaining to you.

Looking forward to seeing you again finally!

10-08-09 *My Attraction to de Finetti's Side of the Force* (to P. G. L. Mana)

Attached are the two pieces of reading I was telling you about. Putting the two together, I think, starts to give some explanation for my attraction to the de Finettian side of Bayesianism. [See I. Hacking, "Subjective Probability," *The British Journal for the Philosophy of Science* **16**, 334–339 (1966) and the Introduction to C. A. Fuchs, *Coming of Age with Quantum Information*, (Cambridge University Press, 2011).]

11-08-09 *From Moore-ish Sentences to Bohrish Ones* (to C. G. Timpson)

In a better formulation, what is being claimed is this:

- "I am certain that if I interact with *it*, I will have experience x; but there is no intrinsic property possessed by *it* corresponding to x." Or,
- "I am certain that if I interact with *it*, I will have experience x; but my certainty is not correspondingly of an intrinsic property possessed by *it*."

Put this way, I feel the charge of our stating a Moore-ish sentence is diffused. Maybe you still disagree?

I hope we get a chance to discuss further today.

By the way, I'm not sure, but I think you may have taken my Mermin Challenge a bit too literally. My (unstated) focus was only on this part of his quote:

> for philosophers, they ought to be more worried about the nature of language, how it can trap us into formulating questions that have no sensible answers, and whether it is possible to restructure ordinary language in a way that liberates us from those built-in errors that make it so hard to think clearly about quantum physics.

Of which, I thought this of you: "If anyone is in a position to see that that is the central point, it is Timpson." Though, even I'm being a bit too loose here—for as I tried to express in the collection of stories I sent to you, it is not quite right that we need to "restructure ordinary language," but to find better choices of words for describing what is being talked about.

Have you by chance read Section 8 in my `http://arxiv.org/abs/0906.2187`?

12-08-09 *More on Moore* (to M. Schlosshauer)

Thanks for the remarks on the Moore-ish business. The way I further diffuse your (and Timpson's) higher level Mooresm in 1) is to have the agent admit in those cases, "Yeah, that's right, what I'm saying is, I can make it happen. I fully believe it." Yesterday at coffee break, the (probably over the top) imagery I used was that, in cases of certainty, the agent is viewing the quantum system a bit as a surrogate mother. He is confident that if he plants his seed there, the expected consequence will come about. (Of course, the world still has the right to surprise him infinitely.) There is a bit more on this on page 667 of the present build of "My Struggles" in my last reply to Timpson. (Maybe I was confused on other points there—I haven't reread it completely—but on this particular point I don't think I've changed my mind any.) [See 17-11-06 note to C. G. Timpson titled "Certain Comments."]

Anyway, remember the notion of indeterminism I am aiming for in Section 8 of the last paper (it is along the lines of that spelled out in the James quotes): It is only that each piece of the world truly has the power to make a contribution to the whole, a contribution that can't be seen from the outside ... but may well be surmised from the inside. And it is from the inside that the agent draws his probability assignments.

It'll be fun to read your choice James quotes (probably this weekend).

Max's Preply

Thanks for your further comments re: Moore-ism. It sounds like the key point of your argument is indeed, as I had suspected, to emphasize that bringing about the "outcome" requires the interaction of agent and system. In your note to Timpson you write:

> It just means that we should be a little more careful to say, "there is no fact in the object guaranteeing the outcome." Of course, there is no fact in the agent guaranteeing the outcome either; otherwise his certainty wouldn't be subjective certainty.

I understood the problem that's supposedly evidenced by Timpson's Moore-ish sentences as setting up a tension between "I am certain about what I will experience if I interact with the system" on the one hand, and "as a good Qbist, I must consciously proclaim that there's no fact guaranteeing what I will experience" on the other. Where could the fact reside? Per your quote above, it doesn't resides in either system or agent alone. Is there a fact in agent-plus-system? Plus the rest of the world? Is there ever a fact ANYWHERE that guarantees a particular experience to occur? I guess for the QBist the answer would be no; in your email you say: "Of course, the world still has the right to surprise him infinitely" – and wouldn't the agent, as a die-hard Qbist, have to take this insight to heart?

On the other hand, this just seems to be a completely generic issue. As long as the agent maintains that there are simply no facts, anywhere, before the agent/system interaction that guarantee a certain outcome, aren't we back to Timpson's original charge? (As I've said before, I don't believe this charge is a serious objection to begin with, but this whole discussion and attempt to rephrase the Moore-ish sentences is obviously built on the premise that it is indeed an objection, to an extent at least.) From Sec. 8 of your last paper:

> Whenever "I" encounter a quantum system, and take an action upon it, it catalyzes a consequence in my experience that my experience could not have foreseen.

Does my assessment of "could not have foreseen" take on any different flavor when I assign probability one (i.e., when I am certain about which consequence I will experience)?

(Well, I hope I'm not beating the proverbial dead horse here. Feel free to tell me if I do, and feel free to give this discussion a rest if you like.)

By the way, I like the ideas, however vague, spelled out in your note "Philosopher's Stone." Especially this one:

> Reciprocally, there should be a transmutation of the system external to the agent. But the great trouble in quantum interpretation–I now think–is that we have been too inclined to jump the gun all these years: We have been misidentifying where the transmutation indicated by quantum mechanics (i.e., the one which quantum theory actually talks about, the "measurement outcome") takes place. It should be the case that there are also transmutations in the external world (transmutations in the system) in each quantum "measurement", BUT that is not what quantum theory is about. It is only a hint of that more interesting transmutation.

I look forward to seeing these thoughts fleshed out; in the meantime, I'm just going to enjoy letting them play on my mind for a while.

12-08-09 *History Repeating Itself . . .* (to C. G. Timpson)

Timpsonism 13: *I'm stuck in bed with flu-like symptoms. I've emailed Philip to say I probably have to quarantine myself and stay in my room to avoid infecting everyone else, just in case it's swine flu; so I don't think I'm going to be able to give my talk tomorrow. Hopefully I'll be recovered to give it b4 the end of the conf, but I thought it might make a difference to your own talk preparation if I'm not going to be able to give mine before you tomorrow. I was just going to summarise my SHPMP paper on q Bayesianism (tho leaving out the Moore's sentence stuff as it takes too long to explain).*

I'm really crappy. I'm sorry to hear that. Do get better, and I really hope you can ultimately give your talk.

Indeed I was going to bank on it for a set-up on why I would seek an interpretation of the Born rule in terms of "an addition to coherence." But I'll survive!!

Do get better: I hope this is not a recurring thing *every* time we get together.

12-08-09 *History Repeating Itself . . . , 2* (to C. G. Timpson)

Rereading my note, I see I wrote in my first sentence: "I'm really crappy." That may be true! But I meant to say, "That's really crappy."

I think I'll stick with my present time, so as to get it over with. But I'll advertise your potential talk, and start off a bit like Bill Wootters did: "Suppose for whatever reason you ended up with an interpretation that looks like this. How could that interpretation *imply* any of the actual structure of quantum mechanics?" Then I'll just go down the SICkening path. With regard to why I cared to end up with an interpretation like that as my starting point, I'll refer to your (hoped for) talk on the strengths and weaknesses of the position.

If you feel well enough before going back to England, I'd like to have you and Marcus Appleby over for dinner (and wine and single malt) one evening. I'm very proud of my new library and liquor cabinet and like to show it off to the best people.

Get well!

15-08-09 *Roiling Mess* (to C. G. Timpson and R. W. Spekkens)

When you talked about the ontology of the roiling mess today, you brought my mind back to some lines in William James's essay "The Dilemma of Determinism." Maybe you've never seen them before:

> Let me, then, without circumlocution say just this. The world is enigmatical enough in all conscience, whatever theory we may take up toward it. The indeterminism I defend, the free-will theory of popular sense based on the judgment of regret, represents that world as vulnerable, and liable to be injured by certain of its parts if they act wrong. And it represents their acting wrong as a matter of possibility or accident, neither inevitable nor yet to be infallibly warded off. In all this, it is a theory devoid either of transparency or of stability. It gives us a pluralistic, restless universe, in which no single point of view can ever take in the whole scene; and to a mind possessed of the love of unity at any cost, it will, no doubt, remain forever unacceptable. A friend with such a mind once told me that the thought of my universe made him sick, like the sight of the horrible motion of a mass of maggots in their carrion bed.
>
> But while I freely admit that the pluralism and the restlessness are repugnant and irrational in a certain way, I find that every alternative to them is irrational in a deeper way. The indeterminism with its maggots, if you please to speak so about it, offends only the native absolutism of my intellect,—an absolutism which, after all, perhaps, deserves to be snubbed and kept in check. But the determinism with its necessary carrion, to continue the figure of speech, and with no possible maggots to eat the latter up, violates my sense of moral reality through and through. When, for example, I imagine such carrion as the Brockton murder, I cannot conceive it as an act by which the universe, as a whole, logically and necessarily expresses its nature without shrinking from complicity with such a whole. And I deliberately refuse to keep on terms of loyalty with the universe by saying blankly that the murder, since it does flow from the nature of the whole, is not carrion.

I thought you might enjoy the imagery of maggots. I'll cc this to Rob Spekkens for fun.

15-08-09 *Nice Word* (to R. W. Spekkens)

Well, I suspect our conversation didn't actually send you to wiki to learn about empiricism, but it did to me. Here's a nice word I just learned: `http://en.wikipedia.org/wiki/Hypokeimenon`.

I suppose I am a bit of a Lockean.

Rob's Reply

That is a good word, but too long to remember. Henceforth, I'll refer to it as your "Pokemon philosophy". :-)

17-08-09 *Footprints* (to M. Schlosshauer)

Schlosshauerism 4: *I just came across this sentence in Roberto Calasso's novel* The Marriage of Cadmus and Harmony*:*

"For every step, the footprint was already there."

A nice way of expressing the spirit of determinism, don't you think?

Yes, that's perfect!

And even "indeterminism" of the block-universe type that Timpson defended so strongly yesterday in our extended discussion. (There is a bit of that point of view in his talk at `http://pirsa.org/C09016`.)

17-08-09 *Timezones (a conversation)* (to M. Schlosshauer)

Yeah, it's a bit after 4:00 AM for me. I should get back to bed at some point; woke up at 2:30 and couldn't get back to sleep.

By the way, in that PIRSA talk, there are about 10 transparencies that you didn't see in Växjö. I drew them Wednesday in my PI office with your jazz streaming in the background. That's really very nice stuff. (I love jazz of all kinds, and have about 15,000 tracks of it in my iTunes folder.) Very impressive, that you perform and do serious physics as well. I'd like to see you and your group some day.

Schlosshauerism 5: *Thanks a lot for your kind words about my music! It's nice to hear that the tunes served as a soundtrack for your drawing session. I'll check out the video of your talk shortly and will try to figure out which transparency goes with what song.*

As you may have read on the website, all tracks are 100% live improvised. I.e., there's no planning, no sheet music, no pre-arranged chords or melody, nothing whatsoever, before we go on stage. So it's all created in the moment in an act of collective improvisation.

I've wondered in the past whether that might be a good metaphor for the kind of reality I'm trying to envision.

Schlosshauerism 6: *That's very nice!! Indeed it would seem like an apt metaphor.*

So once again it seems that the "temperament" (as James would say) informs quite consistently the way we each choose our favorites, be it in regards to our conception of reality, our style of music, or whatever else ...

Yeah, it's weak but see pages 495-496 in *My Struggles*. Also I'd like to think that's what I was thinking when I wrote the note to Garisto below ... but who knows. [See 13-08-01 note titled "Out of the Frying Pan ..." to Robert Garisto.]

In any case, you're much better positioned to develop the metaphor than me! Contemplate it.

Signing off for the evening ...

Schlosshauerism 7: *Thanks! I just had a look at pp. 495-496. I guess there were two notes of relevance? ("The Free City," on Christiania and "Inventory," on jazz and pragmatism.) Sweet little ideas and recollections in each of them, I thought.*

I'm sure you know that there's generally quite an affinity between physicists and music. Heisenberg and his piano playing come to mind, and of course our very own David Mermin. Somehow, though, there also appears to exist a bias toward classical music with most of these people! In some sense, classical music seems to me like the prototypical deterministic edifice, an "unrolling of the fabric of the past." (Don't get me wrong, I enjoy listening to this kind of music very much; it's just the spirit of actually playing it myself that never kindled for me.) To be sure, each musician brings his own "interpretation" to a piece, but the notes are all there and laid out already (could this be a metaphor for the usual interpretive programs of QM?!). Jazz, on the other hand, is spontaneous creation, as loosely constrained by rules and theory as one wishes.

I think I really like the jazz-and-QM metaphor ...!

Oops! I didn't realize the proximity of the Free City story. Oh well.

17-08-09 *Timpson* (to M. Schlosshauer)

Schlosshauerism 8: *I watched Timpson's talk this afternoon. Indeed a very nice and well-done presentation. I noticed that he made no mention of the Moore-isms. Did conversations with you convince him that there was maybe no serious problem after all?*

I think it shook his faith sufficiently that he preferred not to get into it in public. But of course I don't know for sure. I think that, at the end, he was still inclined to think he was right ... but couldn't vouch for it so confidently anymore.

I think the key point is, if one adopts a block-universe picture at the outset, the CFS claim will always look suspicious. For Timpson, there is just no sensible notion of "facts coming into being" and I think that might lie at the root of his continued worry. But that's a longer story than I feel like writing down at the moment. I wish I could have had a tape recorder for yesterday's 3-hour conversation between him, Appleby, Wootters, and me. I'll try to get it all down eventually, and then send you a draft at that point.

We just had a nice (but nearly trivial) result: Maximal consistent sets must be compact.

17-08-09 *Your Thoughts on Timpson* (to M. Tait)

You missed a really good meeting. Timpson's talk was excellent; I'd really recommend you watch it. All the talks can be found here: `http://pirsa.org/C09016`. I think my talk was fairly decent as well—unfortunately though, in this way of viewing things, you won't be able to see to where I'm pointing in the diagrams.

Timpson has another paper as well that should interest you. This one is directly about QBism. You can find it here: `http://arxiv.org/abs/0804.2047`. We had several discussions this week on the Moore-sentence point he brought up there. I'm pretty sure I've got that all safely wrapped up now. If you ever get to that point and need my notes; I don't mind sharing them. Sometime this year, Rüdiger and I will write a formal reply.

Sorry I haven't answered about your visit request. The second week of November is not so good for me; I have to go to North Carolina during that time. I'm free however Oct 27 – Nov 7, so I tentatively marked the time in my calendar. If that time is good for you, I'll send an official request in to the secretaries to check accommodation availability.

Attached are some stories I wrote about various characters in quantum information and quantum foundations. You might enjoy some of them—I don't know. A couple were also intended to carry philosophical messages as well (like the ones on Mermin and Preskill). [See Introduction to C. A. Fuchs, *Coming of Age with Quantum Information*, (Cambridge University Press, 2011).]

Morgan's Preply

> I wanted to say how sorry I am to have missed Chris Timpson's talk on Saturday (I had another commitment). I've read a couple of Timpson's papers on quantum Bayesianism:
>
> 1. 'Information, Immaterialism, Instrumentalism: Old and New in Quantum Information' forthcoming in A. Bokulich and G. Jaeger (eds.) *Philosophy of Quantum Information and Entanglement* (Cambridge University Press 2008)
> 2. 'Philosophical Aspects of Quantum Information Theory' forthcoming in D. Rickles (ed.) *The Ashgate Companion to the New Philosophy of Physics* (Ashgate 2008)
>
> and I would have dearly liked to see your reaction to his talk. I don't suppose anyone was filming the talk? If not, I hope we can have a conversation at some point about Timpson's take on your views (if in fact you think he is representing your view at all!).

17-08-09 *The Activating Observer* (to J. Rau)

It was good seeing you again. Sorry to be so very "thick"—I am a really slow thinker. I think the line of thought you (or you and Terry) are pursuing is a good one. After your prodding me yesterday, it does seem to me deeply connected to the point of view I'd like to see developed that unitaries be representable as sequences of measurement. You'll note for instance, in Section 8 of my recent `http://arxiv.org/abs/0906.2187`. I reserve the notion of an "action" on a quantum system to correspond to a POVM. I don't, for instance, think of unitaries as actions—which of course goes against the grain of almost everyone else in quantum information. (Where unitaries are usually viewed as the things you want to "do" to a system, and measurement is, well, something mysterious ... but more analogous to "looking.") What you guys are developing may help fill the gap for me.

I have always thought of the "engagement" of the observer in quantum phenomena as best expressed (or demonstrated) by the Kochen-Specker theorems (particularly Stairs's version of it, combining KS with considerations to do with entangled states). It would be interesting to see if a more direct connection between your considerations and KS could be fleshed out.

Anyway, I endorse. And at some point I'll come back and watch your talk again.

Let me now back up to the broad philosophical point. Attached are a couple of files that might entertain you. Maybe I gave you a paper copy of the "Resource Materials" file on your last visit? (If I haven't, maybe it will help give you further spiritual strength in wavering times.) But one file I know you haven't seen before—it has some funny stories in it, and some sad stories as well. [See Introduction in C. A. Fuchs, *Coming of Age with Quantum Information*, (Cambridge University Press, 2011).] The entry on Nielsen is directly relevant to our conversation above. (It makes a bit more sense if you read the Carl Caves entry first.)

18-08-09 *Question Regarding Your Preprint* (to B. R. La Cour)

La Courism 1: *I'm reading your paper, "Quantum-Bayesian Coherence," which you kindly gave preprints of during the Växjö conference this summer. In it you (and Rüdiger) speak of a "measurement in the sky" involving a SIC POVM. Now, in a d-dimensional Hilbert space, the measurement of any given observable can have at most d outcomes. How, then, do you construe that a SIC POVM may be used to perform, even hypothetically, a single measurement with d^2 outcomes?*

The measurement in the sky is a positive operator valued measure (POVM). POVMs are not restricted to d outcomes. A good place to start reading about this is:

`http://en.wikipedia.org/wiki/POVM`

and references therein.

19-08-09 *Question Regarding Your Preprint, 2* (to B. R. La Cour)

La Courism 2: *I understand that these are POVMs, so you assume that they may be measured simultaneously. But they are also (scaled) projection operators and, as such, may be thought of individually as standard, von Neumann measurements.*

The trouble is that, as a member of a SIC-POVM, they are assigned one probability and, as isolated measurements, they have another, larger probability. If you'll forgive the rhetorical question, how does one member of a SIC-POVM know that the others are being measured? Do you consider this a reflection of quantum contextuality?

You talk about a POVM as if it is a plural—your usage of "they" gives it away. Rather a SIC-POVM is a *single* measurement with d^2 possible outcomes. It is true that a POVM is written down mathematically as a set of hermitian operators, but those operators are only meant to *index* the outcomes of the measurement. (Much as the d eigenprojectors can be used to index the outcomes in a standard, von Neumann measurement.) In this context, the operators shouldn't be thought of as separate observables themselves.

If you want to think of a POVM in terms of the old von Neumann notion of measurement, you must make use of helper or ancillary systems. For instance, one way to instantiate a SIC-POVM on a three-level atom is to first let the three-level atom unitarily interact with a nine-level atom (i.e., 3^2), then take the nine-level off and perform a standard von Neumann measurement on it alone. That gives rise to a measurement with 9 outcomes and by way of the preceding interaction may be thought of as saying something indirectly about the three-level atom. (But the "indirect" here is a bit of a misnomer, since by this method, one probes the three-level atom more deeply than can be done by any standard measurement.)

So, in all, I would say you are contemplating two kinds of distinct measurement: One, a POVM with d^2 outcomes, $\frac{1}{d}\Pi_i$, $i = 1, ..., d^2$. The other, a POVM with two outcomes, Π_k and $I - \Pi_k$, say. How does the system know which is being measured on it? It takes two distinct kinds of interaction to enact them. In old terms, the first requires interaction with an ancilla, while second is made directly on the system (no ancilla needed).

Let me this time recommend something further than the wiki article. Have a look at John Preskill's notes referenced therein. As I recall, they were an excellent introduction to the nuts and bolts of POVMs.

Hope you are braving the Texas heat. I've been mowing the grass today and it's about killing me at 75 degrees!

19-08-09 *Utterly the Last Time* (to the QBies)

The more I think about it, the more I think I should make today the day of my concerted effort in the lawn. Kiki's trying to put a push to finish the porch today; so I'm going to put a push on getting the lawn ready for her parent's visit.

Marcus (sorry Åsa), if you need a break from pure writing, and want to talk pure philosophy for a while—i.e., how the world is not sentence shaped, but made of neutral stuff, etc., I can imagine that happening while I'm moving/stacking rocks and edging, as long as you're within ear distance and willing to sort of follow me around. If you get bored, feel free to drop by.

20-08-09 *More on the Cover Story* (to S. Capelin)

I took the words literally. It didn't say provide an *idea* for an image; it said provide an image. (One that would need to be approved, but a seemingly final image no less.) It didn't give me a sense that you have a design staff that does the real artwork.

Anyway, now that I understand that you are only seeking ideas, and that there is a real artist behind the scenes, I will try harder. The cover of Schumacher and Westmoreland's book is indeed very nice, beautiful even. And the meaning (I'd bet money) was not lost on me. The wave symbolizes a solution to the Schrödinger equation; the spots at the bottom left symbolize "its" (electrons, photons, whatever), the zeros and ones in the upper right symbolize "bits." The whole image is an encoding of John Wheeler's phrase "it from bit". Of course, many in the public won't know that, but it surely must be satisfying to Ben and Mike to have his deep idea encoded on the cover of his book in that way.

The two images I've sent you, amateurish though they were, were similarly infused with meaning for me. And in the hands of an artist with some imagination—I would well imagine—could be made to amount to something. You'd get your sales, and I'd go to bed in the evenings with a satisfied soul.

Capelinism 3: *If you really can't think of anything else, then we'll ask the designer to try to make something abstract using the psi image you sent. Is there any reason why what you supplied has the symbol three times? Or can we use just one?*

I don't know what to tell you at the moment, but I will try to think harder. I'd hope you'd listen harder as well, or put me in touch with your graphic artist directly so we could more efficiently bandy about ideas. What that image was trying to get at was a wave function $|\psi\rangle$ rising off of a physical object. That was the base idea I was trying to convey in some way. The wave function initially on the object, flies off of it, and moves still further away ... strengthening in color as it does. (I'm not wedded to that: I'd prefer some variation on my stick man if an artist with some imagination could make it more acceptable. But I already know it does nothing for you, so that idea is probably already dead in the water.) The symbol was there three times because I didn't know how else to give a sense of it flying off the object (the cube).

Could something be done with a $|\psi\rangle$ and an image of a Bohr atom? Maybe make a $|\psi\rangle$ fly out of a Bohr atom in some interesting way? It'd be easier if I could have a conversation with an artist directly. The image of $|\psi\rangle$ carries for me the very core of quantum mechanics. (It was a symbol invented in Cambridge, by the way, by Dirac.)

I'm not trying to cause trouble for you. If you want an image—as I now understand you to very much want—I'll try to work with you. (My anger stemmed only from having wasted a lot of time via the misunderstanding, trying to play a graphic artist when I am not.) If you just need hand sketches and ideas, I can shoot them out whenever they come. But it would be death to my spirit if the endpoint were to be something like the image on the book you mentioned—I really do dislike it, and its meaninglessness is very apparent to me.

20-08-09 *That Formulation* (to L. Hardy)

Boy did I botch that at lunch. Below is the true blue thing. [See 06-07-09 note "The Verdict" to H. M. Wiseman.] The key point is that previous to quantum mechanics agents had reason to doubt their own existence (at least in any deep sense). That is, it is not that quantum mechanics causes me to doubt your existence, but rather to have better reasons for believing in my own.

21-08-09 *Quantum Locality* (to R. B. Griffiths)

Griffithsism 1: *I have posted the item indicated below on* `arXiv:0908.2914`*; you will find the title and abstract at the end of this note.*

In addition to getting the science right I want to give proper recognition to other people's work, cite the most relevant material, present my own ideas in as clear a manner as possible, and remain courteous in disagreements. Any comments or suggestions in these or other respects are welcome.

> TITLE: Quantum Locality
>
> ABSTRACT: It is argued that while quantum mechanics contains nonlocal or entangled states, the instantaneous or nonlocal influences sometimes thought to be present due to violations of Bell inequalities in fact arise from mistaken attempts to apply classical concepts and introduce probabilities in a manner inconsistent with the Hilbert space structure of standard quantum mechanics. Instead, Einstein locality is a valid quantum principle: objective properties of individual quantum systems do not change when something is done to another noninteracting system. There is no reason to suspect any conflict between quantum theory and special relativity.

Thanks for the heads up. I'll read your paper with interest.

I'd like to think that I too have been a rather staunch one of the "some dissenting voices" you mention in the first paragraph of your introduction. Indeed it has been what has driven my epistemic (more particularly Bayesian) view of quantum states, expressed for instance in the opinion piece Peres and I wrote for *Physics Today* way back (that you and Todd commented on), more recently in `http://arxiv.org/abs/quant-ph/0608190` and most recently in `http://arxiv.org/abs/0906.2187`. Timpson also makes the point pretty clearly in his review of us: `http://arxiv.org/abs/0804.2047`.

So, with me you preach to the choir. But it is good indeed to see things from all angles, and it'll be fun reading your paper.

22-08-09 *A Stupid Question for Saturday* (to N. D. Mermin)

Merminition 180: *If it turned out that there is a dimension above which there are no SIC POVMs, would this wipe out their foundational significance?*

Yep, it certainly would. But as my wise student from Vietnam, Hoan Dang, tells me every time we find a new wonderful property for the SICs, "They have just become more expensive."

My opinion really is this: THEY HAVE TO EXIST. Too many things simply become too pretty (supposing only their existence) for them not to.

24-08-09 *The Penrose Tale* (to N. D. Mermin)

Merminition 181 : *Tell Hoan Dang to read Fermat's Last Theorem, by Harold M. Edwards, pp. 76–79, especially the last paragraph on the top of p. 78 for a cautionary historical parallel.*

Another cautionary tale, relevant to your own attitude. When I was a graduate student there was a story, no doubt apocryphal, about a guy who wrote a Ph.D. Thesis on analytic functions of a quaternion variable. He proved all kinds of spectacular properties — much more beautiful than analytic functions of a complex variable. When had written it all up, somebody discovered a proof that the only analytic functions of a quaternion variable are linear.

To be sure, you have many nontrivial examples, so the second cautionary tale is less frightening. But nevertheless, be wary! And challenge every mathematician you meet to find a proof that they exist in all d, or a counterexample.

Indeed I've been doing that for quite some time. Simon Kochen, John Conway, Elliott Lieb, Peter Shor, . . . The list is quite big. John Conway had a cute reaction — he had worked on the real-vector-space version in the late sixties and sometime in the seventies. There, the corresponding upper bound on the maximal number of equiangular lines is $d(d+1)/2$. But it is well known that the bound is saturated only in a few sparse dimensions. When I told John that it appears that the d^2 bound is always achieved in the complex case, he was quite delighted. He thought for just a couple of minutes and declared that he thinks the bound will indeed be achievable in all dimensions. But he quickly followed with, "But it will be very hard to prove it. There are too many of them." Unfortunately, I have no idea what he meant by that; we were interrupted immediately afterward, and I could never get back to him.

Anyway, I wanted to tell you a story about Roger Penrose. You probably know that the known $d = 4$ SICs are already quite nontrivial. See equations 26 to 29 in `quant-ph/0310075`. Here's a conversation I had with Penrose just before (and continuing immediately after) Harvey Brown's talk at last year's PIAF meeting.

> CHRIS: I have a mathematical problem that I think will interest you, and it's one I could use some guidance with. Here's the question. For a complex vector space of dimension d, what is the maximal number of equiangular lines you can have in it?
>
> PENROSE: You mean any two lines have the same angle between them?
>
> C: Yep, exactly that.
>
> P: Oh, that'd be a very hard question. [A bit of a grimace on his face.]
>
> C: Believe me! I and several others have been working on it for quite some time now. The interesting thing is the answer seems to be simply d^2.
>
> P: You mean order d^2.
>
> C: No, I mean precisely d^2.
>
> P: Oh! [His face lighting up like a child's.] That's very interesting. You mean the maximal number appears to be precisely d^2?
>
> C: Yep, out to dimension 47 at least. [Now we know it goes out to 67 at least.]

[The talk begins; we sit down. As the talk proceeds, I can see Penrose out of the corner of my eye; he's just sitting there politely, apparently listening to the talk. When the talk is over, he turns to me and says:]

P: Well, I can see that they exist in dimensions 2, 3, and 4. What's difficult to see is if one can construct a 3-dimensional set by suitably projecting a 4-dimensional one.

I was floored. When he started to tell me more details about the $d = 3$ case, I could tell he had used the same construction technique that Lane Hughston had used in his 2 months of work on the problem (Lane classified all solutions in $d = 3$ by studying cubic equations and Hessian forms or some such). In $d = 4$, Penrose said the same sort of thing generalized, but I remembered well that Lane had never been able to make it work. And Penrose did that much without pencil or paper!

26-08-09 *The Poetry of Chance* (to N. Gisin)

It was fun sparring with you yesterday as ever. And also as ever, I was struck that there are some points upon which we seem to agree on (or at least seem to feel as the right direction forward). One of them is the issue of will, if I understand you correctly. I want to send you two quotes of William James that I think are quite pretty and express the variant of chance and will that I was trying to convey to you at lunch. Maybe you will enjoy them, or maybe you will disagree. If it is the latter, I would like to understand your point.

> [Chance] is a purely negative and relative term, giving us no information about that of which it is predicated, except that it happens to be disconnected with something else—not controlled, secured, or necessitated by other things in advance of its own actual presence. ... What I say is that it tells us nothing about what a thing may be in itself to call it "chance." ... All you mean by calling it "chance" is that this is not guaranteed, that it may also fall out otherwise. For the system of other things has no positive hold on the chance-thing. Its origin is in a certain fashion negative: it escapes, and says, Hands off! coming, when it comes, as a free gift, or not at all.
>
> This negativeness, however, and this opacity of the chance-thing when thus considered *ab extra*, or from the point of view of previous things or distant things, do not preclude its having any amount of positiveness and luminosity from within, and at its own place and moment. All that its chance-character asserts about it is that there is something in it really of its own, something that is not the unconditional property of the whole. If the whole wants this property, the whole must wait till it can get it, if it be a matter of chance. That the universe may actually be a sort of joint-stock society of this sort, in which the sharers have both limited liabilities and limited powers, is of course a simple and conceivable notion.

> Why may not the world be a sort of republican banquet of this sort, where all the qualities of being respect one another's personal sacredness, yet sit at the common table of space and time?
>
> To me this view seems deeply probable. Things cohere, but the act of cohesion itself implies but few conditions, and leaves the rest of their qualifications indeterminate. As the first three notes of a tune comport many endings, all melodious, but the tune is not named till a particular ending has actually come,—so the parts actually known of the universe may comport many ideally possible complements. But as the facts are not the complements, so the knowledge of the one is not the knowledge of the other in anything

> but the few necessary elements of which all must partake in order to be together at all. Why, if one act of knowledge could from one point take in the total perspective, with all mere possibilities abolished, should there ever have been anything more than that act? Why duplicate it by the tedious unrolling, inch by inch, of the foredone reality? No answer seems possible. On the other hand, if we stipulate only a partial community of partially independent powers, we see perfectly why no one part controls the whole view, but each detail must come and be actually given, before, in any special sense, it can be said to be determined at all. This is the moral view, the view that gives to other powers the same freedom it would have itself,—not the ridiculous 'freedom to do right,' which in my mouth can only mean the freedom to do as *I* think right, but the freedom to do as *they* think right, or wrong either.

For myself, part of this vision of the universe as a "republican banquet" is to recognize that probabilities are not rigidly connected to nature, but are of the personalist Bayesian variety even in quantum mechanics. Nonetheless we do not have to agree on that to feel some overlap in our thoughts.

I have a fuller sketch of how I see the notion of chance above as fitting with my understanding of quantum mechanics, and also why I insistently call myself a "realist", in Section 8 of: `http://arxiv.org/abs/0906.2187`. You might find that section fun reading, even if you throw it in the bin afterward.

27-08-09 *'Empirical Coherence' and Invitation* (to I. Hacking)

You may not remember me, but we met at a meeting in the Carré des Sciences in Paris, June 2008. You seemed somewhat interested in the personalist Bayesian account of quantum probabilities that a few colleagues and I have been working out. And you also seemed somewhat intrigued by my remarks that we could still have (and quite desire) an "objective indeterminism" from quantum mechanics without ever once invoking objective chances (for instance Lewisian) or propensities. In that regard, let me call your attention to this paper that we've posted recently: `http://arxiv.org/abs/0906.2187`.

It is crucial to us that there is no such thing as a "right and true" quantum state. But on the other hand the Born rule for calculating quantum probabilities is indeed used all the time, and thus calls for an explanation in our terms. Our solution is not to think of it as defining a propensity, but something more along the lines of probabilistic coherence. In other words, the Born rule should be viewed as a normative principle for relating probabilities. The idea being that if one does not make sure his probability assignments (for the outcomes of various potential measurements) are related according to the dictum of the Born rule, Nature is liable to smack his hands. In contrast to Dutch-book coherence, however, the origin of the normative rule is not of a purely logical nature, but should be seen rather as dependent upon contingent features of our actual world.

Anyway, that's the sort of stuff in the paper, and I hope you will find parts of it entertaining and thought provoking. Particularly, any feedback you have for me on the Intro, the Dutch-book section, and the concluding Section 8 (on a William Jamesian "republican banquet" non-block universes), would be most appreciated. And I would be flattered to hear back from you. (Most recently I've been studying your "Slightly More Realistic Personal Probability" from 1967, and think parts of it are quite in line with the views we are working out, and crucial to us actually.)

That said, let me change subjects abruptly. I saw you at Nicolas Gisin's talk two days ago, but I was not able to catch you afterward to convey the following invitation. It would have saved me some writing!

Steve Weinstein (philosophy, U. Waterloo), David Wolpert (NASA), and I are organizing a meeting of physicists, philosophers, and mathematicians at the Perimeter Institute in Waterloo for next year titled, "Laws of Nature: Their Nature and Knowability." The target date presently is May 20-22, but we have some flexibility to modify that as needed to attract the best people. We envision about 20 external participants, plus 6 from PI. I have been assigned to contact you, Cheryl Misak, Huw Price, and Bas van Fraassen for a sort of pre-invitation before we send out other invitations more generally. I.e., we really want you to come, and think you'd be a great asset for attracting some of the others that we plan to invite. Of course, all your expenses would be paid by PI, and I personally am itching to have some refreshed face-to-face conversations with you.

Attached is a list of topics for the meeting written by my colleagues. I hope it will give you some sense of what the meeting will address.

I very much hope you will say yes, and I'll wait on needles until I hear back from you!

27-08-09 *Progress and Invitation* (to B. C. van Fraassen)

It is nice to have a reason to write you again, but unfortunately it is not yet to send you a draft of our paper "Quantum Bayesian Decoherence." Writing it has been painfully slow, but we do feel that we are really clarifying things with it. At the moment, we're working on an island within the paper that may become a paper on its own: We are making a strong endorsement of your reflection principle and want to give that a lot of care in presentation to ward off some of the evils we have seen you endure!!

Instead at the moment, I need to write you about something else. Steve Weinstein (philosophy, U. Waterloo), David Wolpert (NASA), and I are organizing a meeting of physicists, philosophers, and mathematicians at PI for next year titled, "Laws of Nature: Their Nature and Knowability." The target date presently is May 20–22, but we have some flexibility to modify that as needed to attract the best people. We envision about 20 external participants, plus 6 from PI. I have been assigned to contact you, Ian Hacking, Cheryl Misak, and Huw Price for a sort of pre-invitation before we send out other invitations more generally. I.e., we really want you to come, and think you'd be a great asset for attracting some of the others that we plan to invite. Of course, all your expenses would be paid by PI, and I personally am itching to have some refreshed face-to-face conversations with you.

Attached is a list of topics for the meeting written by my colleagues. I hope it will give you some sense of what the meeting will address.

I very much hope you will say yes, and I'll wait on needles until I hear back from you!

01-09-09 *Schumacher and Westmoreland Endorsement* (to S. Capelin)

Here you go:

> This is a fantastic book, with one of the authors no less than the very inventor of the word and idea of a qubit. When I opened the book for the first time, I found I couldn't stop reading through it and working out some of the problems. I should be ashamed to admit, but after 15 years as a professional quantum information theorist, I was still learning some elementary aspects of quantum mechanics. In presentation, the book strikingly reminded me of a conversation I had had with Schumacher several years ago. He told me that just after seeing the new *Star Wars* movie, he became quite depressed because he thought it was just a fantasy and he would never himself be a Jedi knight;

he would never know the ways of "the force." But one day it dawned on him, "Wait a minute, I am learning the ways of the quantum! I'm exploring its great mysteries, its real mysteries. I am a Jedi after all!" And so will be the reader who invests his mind in this book. There's no book out there I would recommend more for learning the mechanics of this quantum world.

If it is too long for you, you can trim some of it out, but please pass any changes you make by me before going to press.

01-09-09 *Interpretations, Closed Timelike Curves* (to C. H. Bennett)

Bennettism 37: *I would be interested to hear what you think of our (Debbie, John, Graeme, and me) new paper asserting the computational and discriminatory impotence of closed timelike curves. It involves questions about what is a reasonable way to formalize slippery notions like discrimination and (especially) computation. Such conundrums are very much up your alley, and I think you'll have interesting things to say about them, though I'm a little worried you'll go all theocratic again like you did in your emails about me and Sappho sitting in a tree. I'm not her type, anyway.*

I think your paper is indeed important for foundational issues. For instance, I think linearity of evolutions is absolutely required for an epistemic interpretation of quantum states. With nonlinearity, that interpretation goes right out the window.

Will study it with a fine-toothed comb. (And resist the temptation to infer that I'm devilishly referring to lice.) With regard to your remark on Sappho, oh I don't know: Maybe you don't watch the same movies I do.

02-09-09 *Sam Hurt's Eyebeam* (to N. Waxman)

Here are a couple of links that might jog your memory:

- `http://en.wikipedia.org/wiki/Eyebeam_(comic)`
- `http://eyebeam.com/`

02-09-09 *Inconsistencies* (to R. W. Spekkens)

Two little points of thought connected to yesterday's conversations.

1) With regard to your inter-theoretic pragmatism and intra-theoretic realism. I said I have a good bit of sympathy with that. But mulling over your particular choice of words, I'm not sure that one of the things you said makes sense. Intra-theory, you said, you could feel free to invoke a correspondence theory of truth. But I myself couldn't go that far. For I'd have to wonder, "correspondence with what *something* that's independent of the prior pragmatic choice?" There's nothing that the theory can give me a hook to by admission. So, really what I meant when I said I was in some agreement was something more along the lines of the following.

Once a theory like quantum mechanics is accepted (say for purely pragmatic reasons—i.e., for reasons that have directly to do with our practice and our survival in the world)—then one can step back and ask, "Well, what is implicit in this theory that is not directly to do with my practice?" That is, in positing the theory, I may be positing many things, not least of all the theorist's postulated limitations in interacting with the world, but I may also be positing things about the world itself (without the theorist). I wouldn't deny that, and that is our point of overlap. But those things are a distillate of the original pragmatic choice, and not girded up by a correspondence (that we could never know as actually holding or not).

Actually, my present position is decently close to F. C. S. Schiller's. See

`http://en.wikipedia.org/wiki/Ferdinand_Canning_Scott_Schiller`

or better, his paper "Axioms as Postulates" directly:

`http://en.wikisource.org/wiki/Personal_Idealism/Axioms_as_Postulates`

The outline at the beginning of this one, I think, is particularly useful. In the Wikipedia article, there is this nice explanation of one aspect of the thought. See below.

2) Yesterday, you said you preferred not to be political. What I am about to say is not an outright inconsistency, but I think it is an exhibition of expediency. It is about our conversation with Natasha on the cartoon. She wants some reference to many worlds because it will make quick connection to what little the readers may already know of the quantum foundations debate. But I think that, without finding a way to insult the view at the same time, that's a kind of cheap move, only further promoting an incorrect image of the theory in the public eye. I.e., on this issue I can see some real (albeit subtle and long-term) damage being done.

Anyway, thanks for provoking my thoughts. More on Schiller below. (Many typos fixed from the Wiki article.)

> In "Axioms as Postulates" Schiller vindicates the postulation by its success in practice, marking an important shift from *Riddles of the Sphinx*. In *Riddles*, Schiller is concerned with the vague aim of connecting the "higher" to the "lower" so he can avoid skepticism, but by 1903 he had clarified the connection he sees between these two elements. The "higher" abstract elements are connected to the lower because they are our inventions for dealing with the lower; their truth depends on their success as tools. Schiller dates the entry of this element into his thinking in his 1892 essay "Reality and 'Idealism'" (a mere year after his 1891 *Riddles*).
>
>> The plain man's 'things,' the physicist's 'atoms,' and Mr. Ritchie's 'Absolute,' are all of them more or less preserving and well-considered schemes to interpret the primary reality of phenomena, and in this sense Mr. Ritchie is entitled to call the 'sunrise' a theory. But the chaos of presentations, out of which we have (by criteria ultimately practical) isolated the phenomena we subsequently call sunrise, is not a theory, but the fact which has called all theories into being. In addition to generating hypothetical objects to explain phenomena, the interpretation of reality by our thought also bestows a derivative reality on the abstractions with which thought works. If they are the instruments wherewith thought accomplishes such effects upon reality, they must surely be themselves real.
>
> The shift in Schiller's thinking continues in his next published work, *The Metaphysics of the Time-Process* (1895): The abstractions of metaphysics, then, exist as explanations of the concrete facts of life, and not the latter as illustrations of the former [. . . .] Science [along with humanism] does not refuse to interpret the symbols with which it operates; on the contrary, it is only their applicability to the concrete facts originally abstracted from that is held to justify their use and to establish their 'truth.'

Schiller's accusations against the metaphysician in *Riddles* now appear in a more pragmatic light. His objection is similar to one we might make against a worker who constructs a flat-head screwdriver to help him build a home, and who then accuses a screw of unreality when he comes upon a Phillips-screw that his flat-head screwdriver won't fit. In his works after *Riddles*, Schiller's attack takes the form of reminding the abstract metaphysician that abstractions are meant as tools for dealing with the "lower" world of particulars and physicality, and that after constructing abstractions we cannot simply drop the un-abstracted world out of our account. The un-abstracted world is the entire reason for making abstractions in the first place. We did not abstract to reach the unchanging and eternal truths; we abstract to construct an imperfect and rough tool for dealing with life in our particular and concrete world. It is the working of the higher in "making predictions about the future behavior of things for the purpose of shaping the future behavior of things for the purpose of shaping our own conduct accordingly" that justifies the higher.

> To assert this methodological character of eternal truths is not, of course, to deny their validity [....] To say that we assume the truth of abstraction because we wish to attain certain ends, is to subordinate theoretic 'truth' to a teleological implication; to say that, the assumption once made, its truth is 'proved' by its practical working [....] For the question of the 'practical' working of a truth will always ultimately be found to resolve itself into the question whether we can live by it.

A few lines down from this passage Schiller adds the following footnote in a 1903 reprint of the essay: "All this seems a very fairly definite anticipation of modern pragmatism." Indeed, it resembles the pragmatist theory of truth. However, Schiller's pragmatism was still very different from both that of William James and that of Charles Sanders Peirce.

04-09-09 *Little Lemma, Big Theorem* (to N. S. Jones)

Thanks for the nice note. Have we ever met in person? Thanks too for telling me more of the story of "the remarkable theorem." That's what I actually call it in my presentations; in a few of them I've even promoted it to the stature of the no-cloning theorem—pointing out particularly that it's trivial to prove, but that it nonetheless says something very deep about the "shape" of quantum-state space. Also I always tell the story of how many big-time quantum information people simply weren't aware of it—it was a well-hidden little gem. I know because I have asked around very thoroughly. Here are the people I can recall who did not know the theorem until I pointed it out to them: Holevo, Lindblad, Lieb, Nielsen, Bengtsson, Calderbank, Ruskai, Gottesman, Kochen, Uhlmann, Mermin, Brukner, ... and there were surely others as well. It's a bit like the story I just wrote up on Wigner and the no-cloning theorem. See page 2 of the attached (barely started) draft of an upcoming paper.

I'm sorry to hear that you've left quantum information, but I hope you're happy in your new line of work.

Nick's Preply

> Dear Chris - I was just scoping around and found, in an interesting paper of yours, that you liked my little lemma. I always liked the lemma myself, but Noah and I

wrangled about whether it was worth hyping up more. From a mathematical perspective it's trivial to validate; yet (probably because I'm an incompetent) it took me two or three weeks to prove (because, when you only know the left hand side ...). I note Flammia apparently scooped me; I presented the idea in 2003 at the Informal Quantum Information Gathering, Max-Plank Institute for Quantum Optics 2003 and had proved it in the winter/spring of '03 I think (it was also in my thesis transfer report of that year). Noah then made me sit on it for a long time (to the frustration of a cocky graduate student) ... however, there doesn't seem a whole lot of point setting the record straight, as I suspect (like Noah) that, given the simplicity of the result, it must have been proved sometime in the 60's (and then yearly ever since) but we just don't know it. I think I remember being careful in the paper that we didn't explicitly claim that the result was new. Something I never quite seem to get around to is going to town with these equations from a Real Algebraic Geometry perspective.

[My recent favourite cute thing is that the digit sum of a random walk on the integers, shifted so that it never goes below zero, shows $1/f$ noise ... maybe all those papers on Self Organised Criticality and $1/f$ are just random walks represented wrong.]

You'll be pleased to hear that I left QI disheartened by my ability to make a scientific difference and am now something of a biologist (and also increasingly Bayesian)!

Btw—a smidgeon of flattery—it was some of your work that pulled me into the field of QI.

07-09-09 *Labor Day Recreations* (to R. W. Spekkens, D. M. Appleby, W. K. Wootters, and R. Blume-Kohout)

On this Labor Day morning (enjoying my new porch), I thought I'd spend a few minutes capping off our Friday lunchtime conversation. I never quite gave my own answer to Rob's question of "What is the purpose of science?" (Shy in a crowd, I am.) As usual, I'll answer the question with a couple of quotes written by someone far more eloquent than me. Here are two from William James that have struck me very deeply. The first answers the question straight out: [See James quote at the end of the 02-09-01 note titled "Intersubjective Agreement" to N. D. Mermin.]

I love the last two paragraphs especially. For in my own mind they capture the essence of the story. Our scientific theories are not descriptions of what the world *is*, nor are they positivist *prediction* devices predominantly—that idea fairly appals me—but they are tools for changing existing realities. They are like hammers in a very real sense: tools with which we can build and destroy, and on occasion finesse what we want to make happen.

The following quote probably paints my stripes still more brightly. I submit it with the understanding that you all tolerate me more or less.

> In many familiar objects every one will recognize the human element. We conceive a given reality in this way or in that, to suit our purpose, and the reality passively submits to the conception. You can take the number 27 as the cube of 3, or as the product of 3 and 9, or as 26 *plus* 1, or 100 *minus* 73, or in countless other ways, of which one will be just as true as another. You can take a chess-board as black squares on a white ground, or as white squares on a black ground, and neither conception is a false one.
>
> You can treat the adjoined figure as a star, as two big triangles crossing each other, as a hexagon with legs set up on its angles, as six equal triangles hanging together by

their tips, etc. All these treatments are true treatments—the sensible *that* upon the paper resists no one of them. You can say of a line that it runs east, or you can say that it runs west, and the line *per se* accepts both descriptions without rebelling at the inconsistency.

We carve out groups of stars in the heavens, and call them constellations, and the stars patiently suffer us to do so,—*though* if they knew what we were doing, some of them might feel much surprised at the partners we had given them. We name the same constellation diversely, as Charles's Wain, the Great Bear, or the Dipper. None of the names will be false, and one will be as true as another, for all are applicable.

In all these cases we humanly make an *addition* to some sensible reality, and that reality tolerates the addition. All the additions 'agree' with the reality; they fit it, while they build it out. No one of them is false. Which may be treated as the *more* true, depends altogether on the human use of it. If the 27 is a number of dollars which I find in a drawer where I had left 28, it is 28 minus 1. If it is the number of inches in a board which I wish to insert as a shelf into a cupboard 26 inches wide, it is 26 plus 1. If I wish to ennoble the heavens by the constellations I see there, 'Charles's Wain' would be more true than 'Dipper.' My friend Frederick Myers was humorously indignant that that prodigious star-group should remind us Americans of nothing but a culinary utensil.

What shall we call a *thing* anyhow? It seems quite arbitrary, for we carve out everything, just as we carve out constellations, to suit our human purposes. For me, this whole 'audience' is one thing, which grows now restless, now attentive. I have no use at present for its individual units, so I don't consider them. So of an 'army,' of a 'nation.' But in your own eyes, ladies and gentlemen, to call you 'audience' is an accidental way of taking you. The permanently real things for you are your individual persons. To an anatomist, again, those persons are but organisms, and the real things are the organs. Not the organs, so much as their constituent cells, say the histologists; not the cells, but their molecules, say in turn the chemists.

We break the flux of sensible reality into things, then, at our will. We create the subjects of our true as well as of our false propositions.

We create the predicates also. Many of the predicates of things express only the relations of the things to us and to our feelings. Such predicates of course are human additions. Caesar crossed the Rubicon, and was a menace to Rome's freedom. He is also an American schoolroom pest, made into one by the reaction of our schoolboys on his writings. The added predicate is as true of him as the earlier ones.

You see how naturally one comes to the humanistic principle: you can't weed out the human contribution. Our nouns and adjectives are all humanized heirlooms, and in the theories we build them into, the inner order and arrangement is wholly dictated by human considerations, intellectual consistency being one of them. Mathematics and logic themselves are fermenting with human rearrangements; physics, astronomy and biology follow massive cues of preference. We plunge forward into the field of fresh experience with the beliefs our ancestors and we have made already; these determine what we notice; what we notice determines what we do; what we do again determines what we experience; so from one thing to another, altho the stubborn fact remains that there is a sensible flux, what is *true of it* seems from first to last to be largely a matter of our own creation.

We build the flux out inevitably. The great question is: does it, with our additions, *rise or fall in value*? Are the additions *worthy* or *unworthy*? Suppose a universe composed of seven stars, and nothing else but three human witnesses and their critic. One witness names the stars 'Great Bear'; one calls them 'Charles's Wain'; one calls them the 'Dipper.' Which human addition has made the best universe of the given stellar material? If Frederick Myers were the critic, he would have no hesitation in 'turning down' the American witness.

Lotze has in several places made a deep suggestion. We naïvely assume, he says, a relation between reality and our minds which may be just the opposite of the true one. Reality, we naturally think, stands ready-made and complete, and our intellects supervene with the one simple duty of describing it as it is already. But may not our descriptions, Lotze asks, be themselves important additions to reality? And may not previous reality itself be there, far less for the purpose of reappearing unaltered in our knowledge, than for the very purpose of stimulating our minds to such additions as shall enhance the universe's total value. *'Die Erhöhung des vorgefundenen Daseins'* is a phrase used by Professor Eucken somewhere, which reminds one of this suggestion by the great Lotze.

It is identically our pragmatistic conception. In our cognitive as well as in our active life we are creative. We *add*, both to the subject and to the predicate part of reality. The world stands really malleable, waiting to receive its final touches at our hands. Like the kingdom of heaven, it suffers human violence willingly. Man *engenders* truths upon it.

No one can deny that such a role would add both to our dignity and to our responsibility as thinkers. To some of us it proves a most inspiring notion. Signore Papini, the leader of Italian pragmatism, grows fairly dithyrambic over the view that it opens of man's divinely-creative functions.

The import of the difference between pragmatism and rationalism is now in sight throughout its whole extent. The essential contrast is that *for rationalism reality is ready-made and complete from all eternity, while for pragmatism it is still in the making, and awaits part of its complexion from the future.* On the one side the universe is absolutely secure, on the other it is still pursuing its adventures.

We have got into rather deep water with this humanistic view, and it is no wonder that misunderstanding gathers round it. It is accused of being a doctrine of caprice. Mr. Bradley, for example, says that a humanist, if he understood his own doctrine, would have to 'hold any end, however perverted, to be rational, if I insist on it personally, and any idea, however mad, to be the truth if only someone is resolved that he will have it so.' The humanist view of 'reality,' as something resisting, yet malleable, which controls our thinking as an energy that must be taken 'account' of incessantly (tho not necessarily merely *copied*) is evidently a difficult one to introduce to novices. ...

The alternative between pragmatism and rationalism, in the shape in which we now have it before us, is no longer a question in the theory of knowledge, it concerns the structure of the universe itself.

On the pragmatist side we have only one edition of the universe, unfinished, growing in all sorts of places, especially in the places where thinking beings are at work.

On the rationalist side we have a universe in many editions, one real one, the infinite folio, or *édition de luxe*, eternally complete; and then the various finite editions, full of false readings, distorted and mutilated each in its own way.

Happy holiday.

07-09-09 *Awaits Part of Its Complexion from the Future ...* (to R. W. Spekkens)

You should be able to tell that I've been reading and thinking about your Friday diary entry this morning. You are an honest intellect, and I very much like that about you.

At the moment, I'm tumbling over the last paragraph in your entry (as I tumbled it over when you gave your brief introduction last Friday). There is something about the word "teleological" that I do *not* like; but there is something about the title of this note that I very much do like. Is that a contradiction? I'm not sure: So I keep tumbling these things about.

In the meantime I supply you of two more quotes that I think hint of why the word teleology (as often used) troubles me a bit.

The first is from William James:

> Our sense of 'freedom' supposes that some things at least are decided here and now, that the passing moment may contain some novelty, be an original starting-point of events, and not merely transmit a push from elsewhere. We imagine that in some respects at least the future may not be co-implicated with the past, but may be really addable to it, and indeed addable in one shape *or* another, so that the next turn in events can at any given moment genuinely be ambiguous, i.e., possibly this, but also possibly that.
>
> Monism rules out this whole conception of possibles, so native to our common-sense. The future and the past are linked, she is obliged to say; there can be no genuine novelty anywhere, for to suppose that the universe has a constitution simply additive, with nothing to link things together save what the words 'plus,' 'with,' or 'and' stand for, is repugnant to our reason.
>
> Pluralism, on the other hand, taking perceptual experience at its face-value, is free from all these difficulties. It protests against working our ideas in a vacuum made of conceptual abstractions. Some parts of our world, it admits, cannot exist out of their wholes; but others, it says, can. To some extent the world *seems* genuinely additive: it may really be so. We cannot explain conceptually *how* genuine novelties can come; but if one did come we could experience *that* it came. We do, in fact, experience perceptual novelties all the while. Our perceptual experience overlaps our conceptual reason: the *that* transcends the *why*. So the common-sense view of life, as something really dramatic, with work done, and things decided here and now, is acceptable to pluralism. 'Free will' means nothing but real novelty; so pluralism accepts the notion of free will.
>
> But pluralism, accepting a universe unfinished, with doors and windows open to possibilities uncontrollable in advance, gives us less religious certainty than monism, with its absolutely closed-in world. It is true that monism's religious certainty is not rationally based, but is only a faith that 'sees the All-Good in the All-Real.' In point of fact, however, monism is usually willing to exert this optimistic faith: its world is certain to be saved, yes, is saved already, unconditionally and from eternity, in spite of all the phenomenal appearances of risk.
>
> A world working out an uncertain destiny, as the phenomenal world appears to be doing, is an intolerable idea to the rationalistic mind.
>
> Pluralism, on the other hand, is neither optimistic nor pessimistic, but melioristic, rather. The world, it thinks, may be saved, on condition that its parts shall do their best. But shipwreck in detail, or even on the whole, is among the open possibilities.

> There is thus a practical lack of balance about pluralism, which contrasts with monism's peace of mind. The one is a more moral, the other a more religious view; and different men usually let this sort of consideration determine their belief.

The second comes from Richard Rorty:

> In this essay I shall focus on Whitman's phrase 'counts ... for her justification and success ... almost entirely upon the future'. As I see it, the link between Whitmanesque Americanism and pragmatist philosophy—both classical and 'neo-'—is a willingness to refer all questions of ultimate justification to the future, to the substance of things hoped for. If there is anything distinctive about pragmatism it is that it substitutes the notion of a better human future for the notions of 'reality', 'reason' and 'nature'. One may say of pragmatism what Novalis said of Romanticism, that it is 'the apotheosis of the future'.
>
> As I read Dewey, what he somewhat awkwardly called 'a new metaphysic of man's relation to nature', was a generalization of the moral of Darwinian biology. The only justification of a mutation, biological or cultural, is its contribution to the existence of a more complex and interesting species somewhere in the future. Justification is always justification from the point of view of the survivors, the victors; there is no point of view more exalted than theirs to assume. This is the truth in the ideas that might makes right and that justice is the interest of the stronger. But these ideas are misleading when they are construed metaphysically, as an assertion that the present status quo, or the victorious side in some current war, stand in some privileged relation to the way things really are. So 'metaphysic' was an unfortunate word to use in describing this generalized Darwinism which is democracy. For that word is associated with an attempt to replace appearance by reality.
>
> Pragmatists—both classical and 'neo-'—do not believe that there is a way things really are. So they want to replace the appearance-reality distinction by that between descriptions of the world and of ourselves which are less useful and those which are more useful. When the question 'useful for what?' is pressed, they have nothing to say except 'useful to create a better future'. When they are asked, 'Better by what criterion?', they have no detailed answer, any more than the first mammals could specify in what respects they were better than the dying dinosaurs. Pragmatists can only say something as vague as: Better in the sense of containing more of what we consider good and less of what we consider bad. When asked, 'And what exactly do you consider good?', pragmatists can only say, with Whitman, 'variety and freedom', or, with Dewey, 'growth'. 'Growth itself,' Dewey said, 'is the only moral end.'
>
> They are limited to such fuzzy and unhelpful answers because what they hope is not that the future will conform to a plan, will fulfil an immanent teleology, but rather that the future will astonish and exhilarate. Just as fans of the avant garde go to art galleries wanting to be astonished rather than hoping to have any particular expectation fulfilled, so the finite and anthropomorphic deity celebrated by James, and later by A. N. Whitehead and Charles Hartshorne, hopes to be surprised and delighted by the latest product of evolution, both biological and cultural. Asking for pragmatism's blueprint of the future is like asking Whitman to sketch what lies at the end of that illimitable democratic vista. The vista, not the endpoint, matters.
>
> So if Whitman and Dewey have anything interesting in common, it is their principled and deliberate fuzziness. For principled fuzziness is the American way of doing what Heidegger called 'getting beyond metaphysics'. As Heidegger uses it, 'metaphysics'

is the search for something clear and distinct, something fully present. That means something that does not trail off into an indefinite future ...

08-09-09 *Awaits Part of Its Complexion from the Future ..., 2* (to R. W. Spekkens)

Thanks, that's useful. Dennett's definition of "teleological explanation" makes it clear that what I'm talking about with the phrase "awaiting part of its complexion from the future"—whatever it is—is not that.

The world Dennett sets you to thinking about, the flavor of everything he says, is foreign to the landscape I see in James's writings. (It's certainly foreign to me.) "This exchange reveals one of the troubles with teleology: where does it all stop?" Part of the very point is that it doesn't. Nor is there one and only one all-inclusive chain (or 'hierarchy' in his terms) that one can imagine embedding the discussion in. An essential piece of what Dennett balks at is that no finite story can arise from what he views as the other side (i.e., teleological blah blah blah). To the extent that his other side coincides with my own landscape, *yes*, that is right: It shouldn't be any other way. But also, there is all this talk of "explanation" (in him and in you)—we just hit the rationalist/pragmatist (monist/empiricist) divide again. It is what sets us apart. He wants the world reduced; we say it cannot be. We say that to the extent that one can contemplate any toy reduction, one only does so by leaving essential things out. It is a divide that has plagued worldviews for a long time.

09-09-09 *Till Tomorrow* (to R. Schack)

I've done just a little to the abstract, intro, and acknowledgments. I'm going back to bed now. I'll try to get an hour more in before leaving for Niagara at 9:00 AM. Then I'll send you (what little) I have, and pick back up on it this evening.

One thing that confuses me (worries me!) is this line:

Schackcosm 124: *Quantum Bayesian state assignments are personalist in the sense that they are functions of the agent as well as the world outside the agent.*

Why the reversal from our now very infamous position?

10-09-09 *Phrases That Came to Mind* (to D. M. Appleby)

"Theory is a focal point for designing counterfactuals."

"Theory is a focal point that aids in designing counterfactuals."

10-09-09 *Euler and Classical Mechanics* (to P. G. L. Mana)

Marcus and I had a long discussion about your note yesterday as we were at Niagara (doing work-permit business and viewing the amazing falls). These are very important points you make.

Do you mind if I share your note with Rob Spekkens if the issue comes up in our on-going debate about the value (or unvalue in my opinion) of assuming a deterministic world?

Luca's Preply

I hope your research is going fine there! Unfortunately I am going to miss the new perspectives on the quantum state.

I wanted to send you the translation of Euler's passage and some comments on it a while ago, but I got the swine flu (no different from any other flu) and have been to Sweden and Rome afterwards.

Here are the passages from Euler that I found important. Recall that in that paper he is studying the condition for fluid equilibrium:

> Now, all comes to properly establishing the basic idea on which to base the reasonings to arrive at our goal: the idea of the nature of fluidity in general. For the equilibrium laws for fluids will not have to differ from those of solids but in the fact that the nature of fluids is different from that of solids. So the point is to know the genuine and essential difference that distinguishes the fluids from the solids — a question well bustled about amongst Philosophers and Physicists: but from all they have said about it we cannot deduce anything suitable to our plan. It is maybe true that the smaller elements of a fluid have no link with one another, and that they are in a perpetual movement; but this truth would be completely sterile for our researches . . . .
>
> Inasmuch as this essential property of fluids is to provide the principles of Hydrostatics, I find it only in this: that a fluid mass will not be in equilibrium unless it be acted upon all points of its surface by equal forces perpendicular to the surface. . . .
>
> Conceive an immaterial diaphragm in the interior of the fluid, cutting out a generic portion of it. Since this portion is in equilibrium, all the elements of the diaphragm will sustain forces of the same intensity . . . .

This paper is one of the most important in the history of classical mechanics, and it is full of gems. In the first paragraph, for example, Euler has very clear the difference between a general law ("the laws for fluids will not have to differ from those of solids") and a constitutive equation ("but in the fact that the nature of fluids is different from that of solids"). I know not a few physicists who do not have that difference clear today.

It is obvious that Euler is here looking for a model with which to describe and possibly deduce new properties of fluid equilibrium. He says clearly that he is not interested about "the real situation", because this may be too complicated and irrelevant for the phenomena under investigation. Rather, he wants a simple conceptual device from which to derive, mathematically and as simply as possible, the phenomena of interests; in this case, fluid equilibrium.

Note how his goal is limited to a particular range of phenomena, not *all* phenomena of this world; and how he adapts the means to the end.

And here 'conceptual' is in its original sense of something *conceived* or *conceivable*: something that can be taken in, imaginable. It does not matter if what is imagined contradicts "the real situation".

In fact, in this case Euler's device is to *imagine* any portion of a fluid (in fact, of any body) enclosed by an immaterial diaphragm, and to study the imaginary forces that would act on this diaphragm from the fluid outside it. A very simple thing to do with our imagination! But this obviously goes against the idea, of that time, of a fluid made of elements in perpetual motion and with no interaction. But who cares?

With this device all the equilibrium of fluids is summarised in a simple principle: the forces per unit area on any such diaphragm must be everywhere equal in intensity and perpendicular to it. With a very simple generalisation, from the same principle Euler obtains in the subsequent paper all phenomena of non-turbulent fluid motion. Isn't this genius?

This device was revolutionary and very fertile; indeed, every engineer learns it today: via Cauchy, it became our hodiern concept of stress, used even in general relativity. Thanks to it we can cross bridges, build very high buildings, make robust containers, fly from Canada to Australia, and do many other things (despite the fact that no such diaphragms nor stress forces appear in a molecular model, as every statistical mechanician knows).

It is also clear that the forces acting upon that imaginary diaphragm are hardly measurable. This was clear to Euler, and is clear to the engineer today. The way classical mechanics has always proceeded has been this: make some hypotheses about your model, work out the consequences, and if these match experience and experiment, feel more confident about your hypotheses.

Classical physics continually uses many fundamental hypotheses which *in principle* cannot be tested. Take the case of the Newton-Euler first law and inertial frames. The law states that, for an observer *in an inertial frame*,

$$\sum_i \mathbf{F}_i = m\mathbf{a} \ ,$$

the sum being extended to all forces acting on the body in question. How can we test whether we are in an inertial frame? Well, given all forces, the only way is to see whether the body's acceleration is proportional to their sum. But how can we be sure that we have not neglected some force? Well, given that we are in an inertial frame, we can check whether the sum of the forces considered is proportional to the body's acceleration. But how can we test whether we are in an inertial frame? We're going around in a circle. We cannot test both hypotheses, about the forces and about the frame: we have to *assume* the one and test the other. If and as long as everything works, we are confident about our untested assumptions.

So I think Euler's paper is one of the many which shows that:

- Classical physics is concerned about describing the world, but 'describing' does not necessarily mean 'saying how thing really are'. (In fact, after Wittgenstein we should know that 'how the world really is' has no meaning at all.)
- Classical physics is not based upon all-measurability and all-knowability: in it, hypotheses are everywhere made never to be proven, but to be held as long as their consequences agree with our experience, experiments, and taste.
- The models and hypotheses of classical physics are based on our imagination: the very concepts we use in it are extremely anthropomorphic, sublimations of our daily experience; sometimes they even contradict our thoughts about how things 'really' are.

What many people today say about 'classical physics', is not what it is and was, but what *they think* it *should* be and have been. Indeed, they very rarely present any single text of our past Masters (apart from the usual two lines by Laplace) to support their claims.

15-09-09 *The Primacy of Measurement* (to D. M. Appleby)

Fun reading on a depressing night. Rüdiger and I have a mistake in our paper and I've spent three days trying to fix it. (Luckily it doesn't infect my work with you and Åsa, but it remains a mistake in the public eye.) Anyway, we must talk much more of time when all this sickening paper-writing is done.

Marcus's Preply to Jochen Rau

I much enjoyed our discussions too. I also enjoyed your very stimulating paper.

I hope you won't take the following remarks as criticism. They are merely a few thoughts provoked by reading your paper. I believe that quantum mechanics is challenging us to develop a way of thinking about physical reality which is completely different from the one to which 300 years and more of classical physics have accustomed us (I say 300 years because that is the time elapsed since Newton's *Principia*—and I include the 20th century in that time span because it seems to me that, notwithstanding the mathematical innovations introduced by Schrödinger, Heisenberg et al, on a conceptual level we have hardly begun to get beyond the psychological cramp which seventeenth century science induced in us). I have the impression your attitude is similar to mine. If so you will agree that there is a long way still to go. And that is the motivation for these comments. I am thinking "what next?"

One of the obsessions of classical physics was the desire for what might be called *total* objectivity: a picture of the world from which every trace of the knowing subject has been removed. A picture of "things as they are in themselves", independent of them being conceived by us. I said something about this in my talk at Växjö. Like all simple-minded slogans it seems, on the face of it, an entirely reasonable demand. It seems (on the face of it) as though any qualification of the demand—any suggestion that things aren't quite as simple as it assumes—represents a surrender of the foundational principle of modern science, without which it wouldn't deserve to be called science. I think in many peoples' minds there is almost a moral dimension to this: that many people in physics regard (for instance) the Bayesian interpretation of the quantum state rather in the way that fundamentalist Christians regard liberal theologians. But the thing about fundamentalist Christianity is that it is doomed to failure for purely internal reasons. You cannot base yourself on a perfectly literal interpretation of the Bible because the Bible isn't consistent, either with itself or with the acknowledged facts (fundamentalist Christians often deny Darwin; not so easy for them, in these days of space travel, to deny Copernicus). And I think something similar is true of the fundamentalist demand for *total* objectivity. Its own internal inconsistencies mean that is simply not possible to carry the project through. This is obvious, really, just from the fact that it is a totally objective *picture* (or theory, or conceptualization) which is being demanded. For a picture (or theory, or conceptualization) is a human construction and it must inevitably bear the marks of that. Complaining that a theory has "subjective" elements (elements which reflect the fact that it is specifically *our* theory) is like complaining that a photograph is printed on paper, using ink—that it isn't literally identical with whatever it is a photograph of. As I said in my talk in Växjö I think quantum mechanics is finally forcing us to face up to this obvious point. Really it should have been obvious all along. However, there were features of classical physics which made it comparatively easy for those who wanted to conceal the truth from themselves. Nowadays it is much harder. Not that people don't still try. In fact, most of the effort in quantum foundations is devoted to the attempt to interpret quantum mechanics along objectivist lines. However, these attempts fail to carry conviction—which is why there continue to be all these arguments.

Anyway, that is by the way of preamble. Now for my first question. I am deeply interested in your idea, that maybe we should think of measurement as the primary thing. The contrary idea, that unitary evolution is the fundamental concept, and that the aim should be to explain measurement in terms of it, is closely connected with the classical ideal of *total* objectivity. Anyone who thinks in those objectivist terms must inevitably find the fact that measurement is treated in quantum mechanics (when interpreted along Copenhagen type lines) as an unexplained primitive, unacceptable. But if you reject that classical assumption the suggestion that we treat the concept of measurement as a conceptual primitive is no more objectionable than the suggestion that we treat the concept of logical inference as a conceptual primitive. Sure the idea of measurement makes essential reference to the agent who is doing the measuring. But so what? And since I take that view I am naturally very attracted to the idea that we should turn things round completely, and seek to explain unitary evolution in terms of measurement. However, it raises a question in my mind. It should, I hope, be clear that in rejecting the ideal of *total* objectivity I am not committing myself to a rejection of objectivity absolutely, in any shape or form. There is a (big) universe out there, and I believe it is the job of physics to say something about it. The fact that any statement we may make about a distant star (for instance) will contain subjective elements (will contain an essential reference to us, who are making the statement) doesn't mean that we simply can't make any valid statement. It only means that we need to be much more careful, and subtle, than classical physicists were wont to be. Objectivists would like to believe that unitary evolution is occurring in distant stars. I definitely don't believe that. And I suppose you don't believe it either. But the question is: what do we believe? Do we say that measurements are going on in distant stars? And if we do say that, what has become of the concept of measurement? Are they measurements without agents to do the measuring? Or what?

(Let me hasten to add that I am not expecting you to be able to answer the question. I certainly couldn't answer it myself. I am very uncertain what quantum mechanics says about distant stars. I am just wondering if you have any thoughts on the subject, however tentative and uncertain.)

My other question concerns your reference to "future attempts at constructing a truly timeless—and possibly measurement-based—theory of quantum gravity". I suspect that the classical demand for *total* objectivity has led to some deep-seated confusions about the nature of time. You talk about a "timeless" theory of quantum gravity. But it seems to me that there is an important sense in which the classical theory of relativity is already "timeless". That is, it leaves out some of the essential features of what we ordinarily call time. In the common sense conception of time the present plays a distinguished role. But in classical relativity there isn't really any concept of the present at all.

Now of course you might say that there is no reason to pay any attention to common sense on this. Copernicus simply ignored what was once the common sense assumption, that the Earth is *obviously* at rest. So why shouldn't Einstein ignore the common sense assumption, that the concept of "the present moment" is something more than a subjective illusion? In response to that question, I would agree that we are as free to ignore common sense on this as we are on anything else. But only if we can do it consistently. And I don't think modern physics does do it consistently. It seems to me that, on the contrary, although denying the concept of the present at the level of official theory (the 4 dimensional space time manifold), modern physics continually smuggles the concept in unofficially, at the level of actual thought and practice. Moreover, it does this surreptitiously, without comment, and mostly without anyone noticing.

To justify that statement I need to digress a little and say something about counter-factuality. I will come back to time in a moment. At least so far as its practical applications are concerned physics is a tool for answering counter-factual questions. For instance, an engineer wants to design a bridge. To do that s/he needs to ask themselves such questions as "if I put the girder *here* what will be the stress *there*?" Before arriving at the final design, which is actually built, the engineer runs through a multitude of other designs which never have more than a purely conceptual existence, inside the engineer's mind. In short, designing a bridge requires us to know the answers to questions about what *would* happen if one *were* to do something which, however, one never actually does do. In other words it requires us to answer counter-factual questions. And what goes for bridges also goes for just about any other technological application of physics. To a very large extent the practical utility of physics depends on its ability to provide answers to such counter-factual "what would happen if?" questions. I also think that to a considerable extent theoretical understanding depends on them too. For instance, understanding Newtonian gravity doesn't just mean knowing what the orbits of the planets *actually* are. It also means knowing what the orbits *would* be if they were (counter-factually) perturbed in some way. Similarly, you don't understand the geometry of a black hole if you don't know how (for example) to calculate how the geometry counter-factually would be affected if the black hole counter-factually did become more massive. So I would say that counter-factual reasoning is absolutely basic to physics. To a considerable extent it is what physics is about. But because such reasoning involves the consideration of situations which are in a certain sense imaginary it is felt that it is not really part of physics proper. (I use the prejudicial word "imaginary" because that is how physicists of a certain bent tend to regard it. However, I think one might question whether it is really the case that it is only true *in imagination* that this glass would have broken if I had dropped it [but didn't].)

Counter-factual reasoning is closely connected with the concept of possibility (it was *possible* for the engineer to span the river: meaning that s/he counterfactually *could* have built a satisfactory bridge if s/he had really tried), and that in turn is closely connected with the concept of free will (the engineer was *free* to build a bridge instead of a tunnel, if s/he had wanted to). But, notwithstanding the fact that one of the main practical uses of physics is to answer questions as to what is physically possible in various situations, these concepts are regarded with grave suspicion by what is still the dominant tendency in physics. Witness Bell's embarrassment (*Speakable and Unspeakable*, revised edition, p. 101 ff) when he was accused of postulating the existence of free will, and his strenuous attempts to explain away what is commonly called freedom as really something else. The reason that Bell didn't want to accept free-will (and the related concept of counter-factuality, which also plays a crucial role in Bell's argument) as a conceptual primitive is, I would suggest, the same as the reason he didn't want to accept measurement as a conceptual primitive. My own instincts are the exact reverse of his. Not only am I ready to embrace measurement as a primitive concept. I am also ready to embrace counterfactuality and free will as primitive concepts. And perhaps quite a few other things too. I think measurement is only the tip of the iceberg. I think the reason we still don't satisfactorily understand quantum mechanics is that we have hardly got started on the road it naturally suggests.

Which brings me back to time. I believe the ordinary, naive concept of time includes, as an essential element, the idea that we are free to alter the future, but not free to alter the past. In other words, the future is the domain in which we can use counter-factual reasoning to practical effect. Counter-factual questions can be interesting. For example, engineering students are taught how the designers of the Tacoma Bay bridge counter-factually *should* have designed it. Counter-factual questions are also highly relevant in a court of law. But the future is where counter-factual reasoning really comes into its own: because that is when we can do something about it.

Now, as I said, the fact that something forms an essential part of the common-sense conception is not in itself a reason for accepting it. However this way of thinking about time *also* plays an essential role in the application of modern physics (no one is currently trying to build a quantum computer in the past). Every modern physicist does *in fact* think in this way about time. It is just that they don't want to face up to the fact that they are thinking this way. Moreover, I strongly suspect that the reason they don't want to is nothing more than a philosophical prejudice, arising from the feeling that all talk of the observer and his or doings has no place in physics. I believe that does provide a reason for taking it seriously.

Though let me stress that I am not suggesting that we simply go back to common sense ideas about time. Quite the contrary. I would speculate that if we were to follow this line of thought seriously, and try to formulate relativity in a way that did proper justice to counter-factuality, and agency, we might arrive at new, and suprising physics.

In short I would suggest that perhaps what we need is a theory of quantum gravity in which, not only measurement, but also certain concepts such as counter-factuality and agency play a central role. I would also suggest that the theory which resulted wouldn't really be timeless. It wouldn't get rid of the concept of time. Rather what it would do is to conceive of time in a very different way from that to which we have become classically accustomed.

Which brings me to my question: does this make any sense to you?

16-09-09 *PIAF Quantum Foundations Conference – Late Reminder* (to M. Bitbol)

Bitbolism 2: *Thank you for your kind invitation. Unfortunately, I can't come. But I am delighted to see that this topic is now a top priority. The very word "state" is doubtful, and generates a lot of misunderstandings.*

Indeed I am in much agreement with your point. In connection with that, you may enjoy the little story I wrote about David Mermin (and quantum interpretation) in the attached file. It is partially on linguistical issues to do with quantum interpretation. See attached file, starting on page 21:

David Mermin: I cannot honestly call David Mermin a father of quantum information, but I can call him the godfather of it—he blessed the field through its formative years and continues to watch with pride as it approaches maturity. There are many reasons I say this, but one of them is told well in his own words in, "Copenhagen Computation: How I Learned to Stop Worrying and Love Bohr," `quant-ph/0305088`.

Then again, David always tells things well. N. David Mermin is the Horace White Professor of Physics Emeritus at Cornell University, and for years has been the writer extraordinaire of our field. I very much knew the latter when I first wrote to him, and believe me, it did take some effort to work up the nerve to critique his papers. But I did it for the reasons I have already explained. A further aspect that I had not completely appreciated at the beginning is something David argues so beautifully in his essay, "Writing Physics":

> Physicists traditionally replace talk about physics by a mathematical formalism that gets it right by producing a state of compact nonverbal comprehension. The most fascinating part of writing physics is searching for ways to go directly to the necessary modifications of ordinary language, without passing through the intermediate nonverbal mathematical structure. This is essential if you want to have any hope of explaining physics to nonspecialists. And my own view is that it's essential if you want to understand the subject yourself.[207]

I found that when I wrote to David, I wrote better—self assessment, of course—than I did to any of my other friends. It was a question of wanting to keep up with the Joneses, I suppose, but it had this lovely unintended consequence: I felt I understood things better and better just by touching my keyboard and having a new David-Mermin letter in mind to write. Better writing, not just any writing, made for better understanding.

I started to realize that the major part of the problem of our understanding quantum mechanics had come from bad choices of English and German words (maybe Danish too?) for various things and tasks in the theory. Once these bad choices got locked into place, they took on lives of their own. With little exaggeration, I might say that badly calibrated linguistics is the predominant reason for quantum foundations continuing to exist as a field of research. Measurement? I agree with John Bell—it is a horrible word and should be banished from quantum mechanics. But it is not because it is "unprofessionally vague and ambiguous" as Bell said of it. It is because it conveys the *wrong* image for what is being spoken of. *The* quantum state? That one is just about as bad. Who would have thought that so much mischief could be made by the use of a definite article? So many of these things came to mind as I would strive to be clear and entertaining in my writings to David.

I once had a piece of trinitite, that lovely green glass born in the heat of the Trinity test explosion, 16 July 1945. I was very proud of it and kept it displayed on a shelf in my library until the Cerro Grande Fire took it from me. It was a material reminder of Stanisław Ulam's words, "It is still an unending source of surprise for me to see how a few scribbles on a blackboard or on a sheet of paper could change the course of human affairs." But from my standpoint today, it is not those equations that changed the world. They were merely a center of attention for the layers and layers of verbal apparatus and practical action required to give them life. It is language that powers things, and in this thought David Mermin has been my leader.

[207] Mild paraphrasing from David's original.

16-09-09 *PIAF Quantum Foundations Conference – Late Reminder* (to J.-Å Larsson)

Larssony 3: *Sorry, I've booked a trip to idQuantique over just those dates.*

I've forgotten to remind you that I'd very much enjoy meeting you and Marcus at some point. (But the nearest that I can manage is probably November.)

Just let me know as your ideas firm up. Say hello to Nicolas Gisin for me. See the subtitle on page 3 of the attached file—it is a talk I gave at the meeting where Gisin won the John Bell Prize. [Title: Quantum Bayesian Coherence. Subtitle: The Anti-Nicolas-Gisin Interpretation of Quantum Mechanics.] I don't think he liked it! But in great part we see the world diametrically oppositely; so I thought why not be honest about it.

16-09-09 *The Primacy of Measurement, 2* (to D. M. Appleby)

Applebyism 36: *Do we say that measurements are going on in distant stars? And if we do say that, what has become of the concept of measurement? Are they measurements without agents to do the measuring? Or what?*

These are the crucial questions. My ideas for an answer have been forming since last New Year's Eve when I started to write you and Hans on the subject. I'll be curious to hear what Jochen answers you on this.

16-09-09 *QF Group Meeting, Thursday 4pm Bob Room* (to R. W. Spekkens)

Spekkensism 50: *This week's topic of discussion: Is the possibility of doing science contingent on the universe having a certain kind of locality? Einstein famously claimed something to this effect: "Without such an assumption of the mutually independent existence (the 'being-thus') of spatially distant things, an assumption which originates in everyday thought, physical thought in the sense familiar to us would not be possible. Nor does one see how physical laws could be formulated and tested without such a clean separation."*

Thanks for the quote. It reminded me of why I've been pushed to my rather radical Bayesian position about quantum probabilities. It's because over and over again, I always find myself agreeing with Einstein.

16-09-09 *Till Tomorrow* (to R. Schack)

Suppose we have already enforced the following: $nb = a-1$ and $am = n(b+1)$. (The latter condition being the one that arises from fixing the length of a posterior vector of an ISU measurement to be the length of the basis vectors.)

Now look at Eq. (163) in the RMP version of the paper. From the latter constraint it follows that we have m unit vectors with pairwise inner products $-b$. I'm going to make use of this in a moment. First though recall the precursor observation I made to you yesterday:

Take q normalized vectors $|f_j\rangle$ such that

1. $\sum |f_j\rangle = 0$, and
2. $\langle f_j|f_k\rangle = c$ for $j \neq k$.

Then c is fixed to $-1/(q-1)$. The converse is true as well: If $c = -1/(q-1)$ and 2), then 1) must hold necessarily.

But we have m unit vectors with pairwise inner products of value $-b$. Think of b as the free variable. Suppose $b = 1/(m-1)$. Then these vectors could not possibly all be proper probability distributions: For not all of them could live in the positive "quadrant" of the space and still give condition 1 above. On the other hand, if $b = 1/m$, it seems to me nothing obviously or immediately prevents us from augmenting our set of m supposed probability distributions with 1 more vector to get a simplex centered at the origin.

I.e., thinking of m as fixed, only $b = 1/m$ and smaller should be viable values.

I think this sketches the solution to our conundrum. Please think hard about it and see if you see any flaws. And see if you see a way to make it more rigorous—I'm going back to bed now. (Shockingly, if the reasoning above is correct, it vindicates most of the old argument, with a proper understanding of the direction of inequality now. But we should definitely scrap the old argument and use a rigorized version of the one above.)

May I call you at work after I reawaken (and get the kids off to school). Say about 10:00 AM my time?

16-09-09 *Ciao* (to G. Valente)

Many congratulations on your new position! That's wonderful to hear—though Alexei did clue me in some. I fully endorse your learning some true physics and very much sympathize with your sentiment about those "just pretending to know physics, as many philosophers do." To us on the inside, it is quite clear that many of them are indeed just pretending—they can't hide it.

I must admit that I now have Musil's book (two volumes) on my bookshelves, but I have not read it yet.

Let me point you to a new paper of mine: `http://arxiv.org/abs/0906.2187`. I hope some of the philosophy at the end (Section 8) will bring you back to some of our earliest discussions on the malleability of reality and alchemy. We've missed the real Giovanni on this side of the fence!

17-09-09 *Bumpy Yard* (to D. M. Appleby)

It's moles man! It's moles! Now, how do you get rid of moles? The bumpy yard has gotten much worse since you were here, and I've finally started to catch on to the pattern. It's most definitely moles.

Very depressing day of quantum foundations at PI. We had a big QF group meeting, and once again I realized I was orthogonal to most everyone in the audience. It's kind of a lonely and frustrating place.

18-09-09 *Happen?* (to the QBies)

Will either of you happen to go to the University today before coming to PI? If so, could one of you to pick up the following book while you are there?

Title: Conceptual problems of quantum gravity
Publisher: Boston : Birkhäuser, c1991.
Location: UW Davis. Book Stacks. Main Floor
Call Number: QC178.C63 1991

I think we should follow up thinking about yesterday's group meeting topic, and there is an important article by Stachel on the subject in that book.

19-09-09 *The Cabibbo Angle* (to R. Schack)

Cabibbo has an angle. Weinberg has an angle. Why can't we QBists too have an angle? Ours satisfies

$$\cos\theta = \frac{(n-m)}{(m-1)^2+(n-1)}$$

and we declare, for reasons "of a strong Bayesian flavor" that $\cos\theta = 1/2$.

But what are those reasons? We need something good. I can see at least three paths of attack, but beyond seeing that they're paths, I don't presently know how to proceed any distance down them. 1) is to try to supply a reason for your observation that adding an $(m+1)$'th $|\alpha_k\rangle$ gives a set that sums to the zero vector. 2) Is to try to supply a reason for the value $\cos\theta = 1/2$ outright. And here's a 3). Let $|r\rangle = \sum_k |p_k\rangle$. You can confirm that $\langle r|r\rangle = \frac{m^2}{n}$. Can we supply a reason that this quantity should be 1?

Note that $|r\rangle$ is not the mixture of the $|p_k\rangle$, but the sum. If we take the mixture, all useful information disappears because, no matter what m, the vector $\frac{1}{m}\sum_k |p_k\rangle$ is the uniform distribution over n outcomes.

2) very much looks like an extra symmetry assumption. Could we justify it from that angle? 1) really looks like a pretty direct statement about the spider-web character of Hilbert space. (See: `http://www.starmagic.com/MAGIC-LOOPS-Lotus-Flower.html` or do a search in Google Images of "flexi-sphere".) And 3) surely seems like a raw normalization condition of some variety.

Confused and confused. I wonder if Cabibbo was ever confused …

20-09-09 *One of the Essences of Things, Maybe?* (to R. Schack)

I spent a restless night alternating between dreams and meditating on $\cos\theta = 1/2$ and, sometimes, dreaming of meditating on the latter and meditating on dreaming of the latter. It was a wild mix of neuronal firings. The small residue left behind is perhaps more a vague feeling than anything else, but I'll share it nonetheless.

Previously we've been treating the *form* of the urgleichung as nearly the only empirical addition (i.e. nonBayesian) component in our reasoning. Certainly our emphasis was there. But important for this consideration, shouldn't we also include the scale of β? By considering measurements for which can have certainty over the outcomes, we are able to make a connection between m, n, and β, but the importance of the scale of β remains. I tend to think that that is the best way to think of it. What is interesting is that quantum mechanics takes β to be neither the smallest it can be 0, nor the largest $(n-2)/n$.

It's a bit like this maybe: Could one posit Schrödinger's equation as having empirical content, without at the same time as positing h to have such content as well?

Anyway, I wonder if we may have been off-base a bit, hoping to get a bit too much from "assumptions of a strong Bayesian flavor" as well as pure geometrical considerations. I.e., maybe indeed it is only empirics that gives $\cos\theta = 1/2$.

Or, am I giving up too easily?

Continuingly-Miserable Chris

20-09-09 *All Ways of Expression* (to R. Schack)

Maybe another way to put it is this. Would we really want an urgleichung so self-consistent that it had no adjustable parameters at all signifying *which* world we live in? Would we really want it completely fixed, save for the size of the system we were applying it to? I go up and I go down on this.

20-09-09 *A Way To Salvage Much of the Old* (to R. Schack)

I feel confident that what I am about to say is sound, but—warning!—I've had three glasses of wine, and I might not express myself as "clearly" as usual.

Suppose a device with m outcomes that achieves the ideal of certainty. Then that device defines m states in our proposed state-space. Let us make the restriction that a basis state can arise as the posterior consequent upon the outcome of such a measurement. I.e., we are in the usual setting of this last week, where $\beta = (n-m)/[n(m-1)]$. As we know, without further assumptions, there are no restrictions on m (other than that $2 \leq m \leq n$). BUT—and I'm only now starting to appreciate this—*we can still ask the question*: Are there any *further* potential states with the same symmetry as the first m of them? There's no claim here that such an extension *need* correspond to a measurement that achieves the ideal of certainty: By construction, there cannot be one. Instead, it is purely a question of symmetry. We are asking, is our space maximal in the sense of not allowing the addition of one more state with the appropriate relation with the previous ones? For given n and m, we know(?) we can do this if m is already sufficiently large in comparison to n. Thus what we ask is merely (humbly), what is the smallest m for which this symmetry property fails. To put it in still other words, what we are asking is that our ISU measurement that achieves the ideal of certainty generate a maximal set of states in the sense that adding even one more potential state must push it outside the valid state-space.

Now we use the mathematics from our previous argument (or an Åsa-ified version) to show that $m \geq$ blah blah blah. We choose m to be the extremal case as our postulate.

So the assumption is not one of impossibility of construction, but one of the symmetry properties of our space. Of course, why should one care about symmetry properties? I am staunchly belligerent to Jochen Rau whenever he invokes symmetry groups in his reconstructions of QM. But maybe we can really think of it more, again, as a maximality property. Our supposed measurements achieving the ideal of certainty have posteriors that "just fit into" the space.

OK, now four glasses of wine. Surely there will be problems above, but maybe it still indicates a productive path.

20-09-09 *PIAF Quantum Foundations Conference – Late Reminder* (to A. Plotnitsky)

Plotnitskyism 16: *On a different (or ever the same) subject, I have been reading some recent commentaries on the experimental verification of Hardy's paradox, which commentaries I found (unsurprisingly) confusing. My sense from Hardy's initial article and Mermin's article on it is that it is just a special and subtler case of quantum entanglement, which does not essentially change* *anything in terms of epistemology, although of course it is not about to stop the usual debates. I think more and more that nothing in any reasonably near future will stop them, and Hardy's paradox was used by Bohmians to support their view, just as the EPR experiment was.*

Yes, that's right: Just more of the same.

Plotnitskyism 17: *For instance, it seems to me one would still be easily able to maintain the Bayesian view you guys take, just as one can in the case of the EPR experiment. No? That is, does one need any special adjustments? Once again, sorry to miss your paper.*

No, not at all. The lesson of the Hardy paradox is as it is with the usual Bell inequality violation: If one assumes Einstein locality, then the outcomes of quantum measurements cannot pre-exist the act of measurement.

Sorry to hear you can't come. But I know we'll see each other again soon, somewhere, some sunny day.

21-09-09 *Wavering* (to R. Schack)

I'm starting to become disenchanted with the three-glass solution. I guess the transition is coming about because I started to appreciate tonight that it really seems to rely on the α_k representation for its statement. If you take the $|w_k\rangle$ representation, there doesn't seem to be an analogue. So, in what way is it an interesting symmetry assumption that there is no $(m+1)$'st state?

21-09-09 *Fantastic Talk* (to G. J. Chaitin)

Just wanted to let you know, I thought that was a fantastic talk today! Even though I knew most everything from having studied your papers in great detail in the mid '90s, I found it quite inspiring still.

Among other things, your remarks at the end on experimental mathematics struck a chord with my present situation. There's a certain kind of mathematical structure we (myself with Carl Caves and Rüdiger Schack) need to exist for our "quantum Bayesian" program in quantum foundations. But so far we only know it exists for spaces with dimension 2 to 67. Beyond that it has so far been too hard computationally. Nonetheless I plow ahead assuming these things exist in all dimensions, and I become amazed at how pretty the formal structure of quantum mechanics appears in terms of these structures. So as you say of the new axiom added to ZF set theory: Everyone believes these things exist, but we shouldn't let their lack of proof stand in our way. In case you're interested, here's a paper that gives an introduction to the formalism: `http://arxiv.org/abs/0906.2187`.

And yes, I fully believe "God plays dice" with the universe. Our QBism program takes that as the essence of quantum mechanics. See Section 8.1 in the paper cited above.

22-09-09 *The Great Hardy* (to R. Schack)

I just had a conversation with Lucien and he made a very keen observation. By construction, our $\cos\theta$ must be a rational number. Suppose we could give some desideratum so that it is a fixed number independent of n and m (exactly as we are doing with the Cabibbo angle postulate that it is 1/2). Then generally there will be no integer n and m solutions to the equation. He was guessing that $\cos\theta = 0$ and $\cos\theta = 1/2$ might be the only rational solutions.

But, Lord God, how would we prove that?

22-09-09 *Our Diophantine Equation* (to R. Schack)

Let $N = n - 1$ and $M = m - 1$. Then our demand is that there be integers N, M, q, r, with $q \le r$, such that

$$(q - r)N + rM + qM^2 = 0.$$

Wouldn't it be nice if that were a famous Diophantine equation that we could just look up?

Just solving the quadratic equation for M. We get that

$$r^2 + 4q(r - q)N$$

must at least be a perfect square. Which I think means that

$$4q(r - q)N$$

itself must be a perfect square.

Yow, this is too hard for me!! But I think it is almost surely our salvation!!

22-09-09 *Further Point* (to R. Schack)

I had previously said:

> Suppose we could give some desideratum so that it is a fixed number independent of n and m (exactly as we are doing with the Cabibbo angle postulate that it is 1/2).

But we wouldn't even need that further desideratum. The question is simply, when is there a solution. If the only strictly positive one is $\frac{1}{2}$, then we're done.

I've asked Peter Shor if he could help on the Diophantine equation front. No reply yet, but it's the sort of email that Peter actually replies to.

What a great turn of fortune. I'm so glad I had that off-hand conversation with Lucien today.

Cheers, as you wake up to a brighter morning.

22-09-09 *Router Bit* (to N. D. Mermin)

Merminition 182: *I'm happy to bring it along. Have it sent to me at 75 Hickory Road, Ithaca, NY 14850-9606.*

Ah, you mean the place made famous by the 75 Hickory Road Interpretation of Quantum Mechanics?

22-09-09 *Oxford-Austin Cuisine* (to T. Norsen)

I don't believe we've ever met, but I'm looking forward to some productive conversation with you at the PIAF meeting. I know Rob Spekkens says great things about you, and I have very much enjoyed the clarity in your Bell papers: They are great reading. Today I finally started (thinking about) preparing my talk for the meeting. I hope you at least enjoy the title ["Texas-Bavarian Home Cooking: A Quantum Bayesian Reply to Bell's (and Norsen's) la Nouvelle Cuisine," `http://pirsa.org/09090087/`], even if the content ends up appalling you.

I wonder if I might ask a favor of you, even before our first real hello? There are two talks on `pirsa.org` that I'd love you to have under your belt before we start talking in person: One by Chris Timpson, `http://pirsa.org/09080010/`, and one by me `http://pirsa.org/09080018/`. I think they much lay the groundwork for the particular way in which I will disagree with you in my PIAF talk, and I'm quite fearful that it is not something I can really convey properly in the 20 minutes allotted me. But if I could come to some productive conversation with you—learning and explaining and identifying subtle points—that's all I really care about. So, I hope you won't mind giving me this much of a head-start: I think it'd much help for building the necessary translation dictionary we'll need for comparing our worldviews.

23-09-09 *All QBbibbo Angles* (to R. Schack)

You were probably already aware of this, but let me record it since you didn't mention it.

You can find all valid QBibbo angles by rewriting the relation between n and m as such

$$n = m + \frac{2c}{1-c}\frac{m(m-1)}{2}.$$

In order for n to be an integer this means $\frac{2c}{1-c}$ must be an integer. (Take the $m = 2$ case if you have any suspicions.) This completely classifies all possible QBibbo angles. Letting k be that integer, we get

$$n = m + k\frac{m(m-1)}{2}.$$

The three lowest k-value cases give:

- $k = 0$, the classical law of total probability
- $k = 1$, something that looks like real-vector-space quantum mechanics
- $k = 2$, quantum mechanics

Discounting $k = 0$, it is darned frustrating that there is the $k = 1$ case in there below the beautiful quantum solution $k = 2$. Otherwise, we could have used an extremization-style axiom. I've been thinking all day in the back of my head how we might shut the RVS case out. Nothing so far, but 99% of my energy was thrown into Åsa's paper. Indeed we were wrong in our reasoning in the last version of the big paper, but we were oh so close to having the right answer. Is that meaningful? Or at least a hint about the next step.

On the upside, it is kind of cute that we get RVS-QM, when Hardy's axioms shut it out from the beginning, and have not so far found a way to be loosened so as to allow for it. (I know these things from lunch with Hardy and Wootters, the latter being quite interested in the real case.)

I think I remain with the conviction I outlined Sunday that this may be our most "physics filled" postulate. But given that it is nearly at the end of our scale above, maybe I'll reassess that in light of further, yet to be discovered, facts.

You might want to put some of the above in our paper; I don't know how much. In any case, we should thank Hardy at the end along with Myrvold. The conversation yesterday with him inspired much of this.

24-09-09 *Haute Cuisine* (to N. D. Mermin)

What exactly is it that you like so much about Bell's nouvelle cuisine article? In what way does it spook you? What is it in there that should really impress me or spook me? I read it, and I mostly felt nothing. Can you put it in words?

David's Response

> I don't have it with me now, but I'll try to send you some comments before I leave on Saturday. As I remember, it's the combination of the Jarrett argument, which always struck me as the most compelling way of isolating the assumptions behind the conditionally independent representation of joint distributions that underlies Bell inequalities, with Bell's talents as a writer, which I've always very much admired.
>
> More later, I hope. ...
>
> Took a quick look. It's really only two pages: Sections 6.9 and 6.10. Nothing about EPR arguments, determinism, probability 1 being certainty. Just a very general notion of what ought to count as an explanation of correlation. Lambda doesn't have to be EPR elements of reality. It can be attendant circumstances. As I remember my point was that this was the kind of argument that your critical remarks at the beginning of your paper ought to be addressing — not straw men.
>
> And of course he writes beautifully.

24-09-09 *That Bungling Mermin* (to N. D. Mermin)

Have you seen has how this guy `http://arxiv.org/abs/quant-ph/0607057` portrays all your expositions on Bell inequality stuff as bungled misunderstandings? How does his cuisine taste in your mouth?

As you can see, I've started preparing for my talk ...

24-09-09 *That Bungling Mermin, 2* (to N. D. Mermin)

The way he wants to argue (same as Bell but without the charm) is that "local causality" alone implies "local Mermin instruction sets" in an EPR scenario. Then Bell demolishes with his inequality local instruction sets. Consequently local causality all alone is demolished.

I want to say: no, no, no, local causality *alone* does not imply Mermin's instruction sets. There is a principle that you, Norsen (and I think Bell), take as so sacrosanct you don't even notice that it comes into play. It is that probability one assignments for a proposition should imply that the proposition has the truth value of TRUE. This is a significant component of what hides behind the EPR criterion of reality and a part that you accept without question. But we quantum Bayesians don't accept it at all. And therefore we don't have to accept that Bell inequality violations deny local causality. Instead they cap off our quantum Bayesian suspicions about the role of certainty (probability one).

24-09-09 *Timpson's Talk on Sept. 25* (to the QBies)

I hope you guys can come to Timpson's talk tomorrow. He's a really good speaker. Particularly, I thought he did a good job last month in this one `http://pirsa.org/09080010/` which might be a good lead in for tomorrow's talk.

> What would a consistent instrumentalism about quantum mechanics be? Or, why Wigner's friendly after all.
>
> Chris Timpson
> University of Oxford
>
> Instrumentalism about the quantum state is the view that this mathematical object does not serve to represent a component of (non-directly observable) reality, but is rather a device solely for making predictions about the results of experiments. One honest way to be such an instrumentalist is a) to take an ensemble view (= frequentism about quantum probabilities), whereby the state represents predictions for measurement results on ensembles of systems, but not individual systems and b) to assign some specific level for the quantum/classical cut. But what happens if one drops (b), or (a), or both, as some have been inclined to? Can one achieve a consistent view then? A major worry is illustrated by the Wigner's friend scenario: it looks as if it should make a measurable difference where one puts the cut, so how can it be consistent to slide it around (as, e.g., Bohr was wont to)? I'll discuss two main cases: that of Asher Peres' book, which adopts (a) but drops (b); and that of the quantum Bayesians Caves, Fuchs and Shack, which drops both. A view of Peres' sort can I, think, be made consistent, though may look a little strained; the quantum Bayesians' can too, though there are some subtleties (which I shall discuss) about how one should handle Wigner's friend.

25-09-09 *Some Geometry If You Want It* (to N. D. Mermin)

Merminition 183: *What is a Qbist?*

QBism – the foundational program of quantum Bayesianism. (See first sentence of Section 8 of the big paper.)

QBist – a practitioner of QBism.

The QBies – myself, along with my group of three grad students and Åsa our postdoc; maybe Appleby, our long-term visitor, as well.

The QBibbo Angle – 60 degrees; see Assumption 7 and equation 24 of my short paper with Rüdiger.

The QBicle – (a common meeting place for the QBies, yet to be determined)

David's First Reply

So I guess that would make me a QBissel?

David's Second Reply

Bissel is German, that's your problem. It means just a little. (As in: Sprechen Sie Deutsch? Nur ein Bissel.)

QBissel – One who is intrigued by QBism but not yet convinced.

25-09-09 *Why Such Radical Moves?* (to R. W. Spekkens)

It was interesting to watch your reaction again yesterday when I emphasized that in my picture of things, quantum measurement outcomes—the things the particular user of the theory, the agent, is gambling upon—are *personal* experiences, not happenings completely outside the agent. You showed a surprise as if you had never heard the idea before. Thinking about it tonight, I remember you having a similar reaction about a year ago, as if you had never heard of it before at that time either. Yet the personal aspect of the "dings" has been in my thought for at least four years, probably five. And I have presented it consistently so in my talks ever since. It is part of the content of the attached diagram which I know you've seen many times, and it was a significant part of my old tale of the two islands (one of nurturing wives, and one of bad girlfriends) in Konstanz and several talks later. Most recently I thought I gave a very careful statement at the reconstruction meeting, `http://pirsa.org/09080018/`, and I believe even Timpson did me right in `http://pirsa.org/09080010/` by emphasizing that the quantum Bayesians have an "extra layer" of agential involvement over the classical, radical Bayesian (say Richard Jeffrey, Brian Skyrms, etc.). You can confirm for yourself.

It's a learning experience for me: seeing this apparent potential barrier in you, and by extrapolation, to many, many of the ears I have been speaking to over the years. Come to think of it, there are only a few people who have challenged me on this aspect of the worldview. (One is Wayne Myrvold who tells me that I don't really believe it, and one is John Sipe who made the definition "consequence Bayesian" to try to capture the idea.) The lack of more and varied systematic challenges probably demonstrates that the idea isn't even getting to the level of awareness in people's minds where they can say, "ick!" if they want to. There's a conceptual potential barrier that's not even letting the idea get into people's heads. Somehow I'll use this information.

Anyway, yesterday, one of the things you prodded me about was: Why take such a radical stance on the character of measurement outcomes? I said to "preserve locality" among other things—but you talk too fast for me (and drop into attack mode too quickly sometimes), for me to give you more adequate responses. (I don't like that and wish it didn't have to be.) Locality—in my mind, in my usage—is a codeword for the idea that individual things have independent existences / independent essences of each other. It is as Einstein put it in part of the quote you sent out last week: "... it appears to be essential for this arrangement of things introduced in physics that, at a specific time, these things claim an existence independent of one another ..." I like that idea very much, that things "claim an existence independent of one another," so much so that I see Einstein's holy grail of a deterministic field physics—the thing he held to most dearly based on these considerations—as infringing on the very idea. He imagines a world where specifying a field on a boundary surrounding a spacetime point (a purported individual existence) completely determines the field at that point. I say, then the independent existence is only illusory: If you are *determined by*, you are not *independent of*. Period.

I see our quantum Bayesianism as the beginning of a correction in that regard; it embodies at its core the idea of individual existences closed off, in part, from the rest of the world. That is part of the lesson of quantum measurement outcomes as personal experiences. It gives me the means to identify something in physics that really is closed off from everything else. And all of this overconfident and little-justified talk of quantum mechanics *implying* "nonlocality outright" (Norsen's writing style does get under my skin) as very much a threat to the worldview of individual existences. But that is a bit of an aside.

When I set out to write this note, I simply wanted to say that there are more reasons for my adopting the radical move of measurement outcomes as personal experiences. And most particularly is the issue of a consistent account of Wigner's friend from the QBist point of view. I think that is part of the subject of Chris Timpson's talk tomorrow and hope you will go to it. Maybe that would be a good starting point for us to talk about it further some time.

There is a big, tightly knit package in my mind of thoughts on quantum things and a worldview arising from it that I am trying to think my way through. And it has a landscape so different from the quantum foundations chatter I hear around me all the time that it does make it very difficult to speak on the fly. I guess I am just asking for patience.

25-09-09 *The Scale of Engagement* (to H. C. von Baeyer)

Let me tell you the story of the last couple weeks: They have been quite miserable. Rüdiger and I were preparing a Ruedigerized version of Section 6 of our last paper, making it a stand-alone paper and hoping to linearize the thinking to make it clearer. Well, we linearized it indeed! So much so that we caught a flaw in our earlier derivation. Much, much gut wrenching followed, I can tell you, as we tried to salvage the result if not the derivation. And clarity only came very slowly. In the end, we had to face the inevitable—that our result would not be as powerful as it had appeared previously (giving something that looks identical to the quantum expression), but instead something that needs a further (possibly ad hoc) assumption to get to the quantum case.

Still with every cloud there is a silver lining! And now I think there is a greater opportunity than ever to frame the whole issue in terms of just how much the observer is engaged in quantum theory. Please look at the attached paper [`arXiv:0912.4252v1`]. I think you'll this one more of a pleasure to read than previously. (And I am now 99.9% confident that there are no mistakes in it other than grammatical ones!) Here is what leaps out that didn't before. The parameter β in the urgleichung

$$q(j) = \sum_{i=1}^{n} \Big(\alpha p(i) - \beta\Big) r(j|i)$$

signifies the degree to which the observer in quantum mechanics is undetached or engaged—for in the end, it signifies the amount of deviation there is from the law of total probability in this counterfactual games. When $\beta = 0$, we have a detached observer. The keen and interesting thing is quantum mechanics is not the very next step up, but rather the *second* step up in the hierarchy of possibilities. There is a potential theory with observer engagement more than classical but less than quantum. Then there are wild possibilities above quantum. See the discussion after assumptions 6 and 7.

I am now convinced that we ended up with a better playground than we had previously for our deeper foundational ideas. I hope reading this will inspire you, and I hope reinject you with some enthusiasm, so that maybe you'll re-pick back up on our AJP Pauli article. My guess it has slipped under the rug for a while.

I'd like to hear any thoughts you have (on any subject, much less the above).

25-09-09 *Beta is the Parameter to Rule them All* (to R. Schack)

But thank God there weren't nine parameters. See my story to Hans Christian below. [See note to H. C. von Baeyer titled "The Scale of Engagement," from 25-09-09.] In the order of ideas, I am fairly convinced now that it is β, a direct expression of the deviation from the Law of Total Probability, that is the key constant that we should be focusing on. Particularly I have always been reluctant to take m_0 (with the particular meaning as the number of distinguishable states) as the ultimate parameter. It evoked too much of a similarity to classical ideas.

Stream of thought. Associated with a system is a value of β. [Or maybe I could be convinced of α since α increases with dimension. But I sure like that minus sign in front of the β.] Wonder postulate: There are ISU measurements that achieve the ideal of certainty. That there can ever be certainty should remain a miracle from our point of view. β then determines the number of outcomes we can have in such a measurement. Self-consistency forces various rational and integer values. But β in the end rules them all. The constant q chooses which world we are in. Apparently one in which the observer is just a little engaged.

Student just came …

25-09-09 *More On Normativity and a Quantitative Measure on Degrees of Detached Observers* (to C. G. Timpson)

I woke up from my nap thinking I wasn't nearly as generous with you at lunch as I should have been. Your talk was just fantastic, and in point of fact, did in its way help me firm my thoughts on the Wigner friend issue. I very much liked the detailed exposition on "level discipline" (I think you called it) and that really is the key. Also you caused me to want to explore more thoroughly the tension between my "internalist"/"personalist" account of measurement and my respect for a kind of Copernican principle.

If you haven't seen it by now, I should tell you that I've always been in love with this paragraph of John Wheeler's, and with all these QBist explorations, every now and then I start to think I *might* know what it *might* mean:

> How did the universe come into being? Is that some strange, far-off process beyond hope of analysis? Or is the mechanism that comes into play one which all the time shows itself? Of all the signs that testify to "quantum phenomenon" as being the elementary act and building block of existence, none is more striking than its utter absence of internal structure and its untouchability. For a process of creation that can and does operate anywhere, that is more basic than particles or fields or spacetime geometry themselves, a process that reveals and yet hides itself, what could one have dreamed up out of pure imagination more magic and more fitting than this?

You'll see that John has that tension himself—on the one hand the "untouchability" and on the other the "anywhere." One of these days I so hope all of this will really fit together. Could I encourage you to write a proper paper on your talk? It would be so helpful for all of us and could give us a good focal point for thinking things further and pushing things along.

On another matter: Attached is one of my latest. It's an attempt to do much better on Section 6 from the last paper. There is a discussion on the idea of taking the Born rule normatively that might interest you. And also the way I now identify a parameter (q at the end of the paper) that is meant to characterize how "engaged" (meant to be the opposite of Pauli's "detached") an observer is in this theory. Classical has $q = 0$, quantum has $q = 2$. Why q is not 1 identically or strictly > 2, I do not know.

26-09-09 *Logo* (to H. C. von Baeyer)

von Baeyerism 36: *In my wanderings I came across a nice confirmation of our Möbius strip logo. It occurs in the book* Recasting Reality *(Atmanspacher and Primas, eds., Springer 2009) in the article entitled "Complementarity of Mind and Matter" by Primas. He defines: "Here, a Boolean description refers to a Boolean domain where all propositions are either true or false, characterized* *by the postulate of the excluded middle." And then, since a Boolean theory of matter is impossible, he goes on to say: "The most important example to get a globally non-Boolean description is to patch local Boolean descriptions together smoothly." How did we characterize the physics/psychic dichotomy? Locally contradictory, globally identical — Pauli's psychophysical reality combining mind and matter into one world (unus mundus).*

To my surprise I could only find one piece of email on the subject. [See note to D. M. Appleby, dated 26 April 2009, titled "Server Config."] It's below. I recall "locally different / globally the same." Possibly it was "distinct" instead of different. I'm not sure I like "contradictory." Are they contradictory? The thought I'd like to articulate or give some flesh to is that they're different categories (whatever that might mean).

30-09-09 *That Lovely Quote* (to Č. Brukner)

> The value of a [pluriverse], as compared with a universe, lies in this, that where there are cross-currents and warring forces our own strength and will may count and help decide the issue; it is a world where nothing is irrevocably settled, and all action matters. A monistic world is for us a dead world; in such a universe we carry out, willy-nilly, the parts assigned to us by an omnipotent deity or a primeval nebula; and not all our tears can wipe out one word of the eternal script. In a finished universe individuality is a delusion; "in reality," the monist assures us, we are all bits of one mosaic substance. But in an unfinished world we can write some lines of the parts we play, and our choices mould in some measure the future in which we have to live. In such a world we can be free; it is a world of chance, and not of fate; everything is "not quite"; and what we are or do may alter everything.
>
> — Will Durant, describing William James's philosophy

30-09-09 *37 Percent* (to R. W. Spekkens)

You piqued my paranoia: So I counted my transparencies from my talk yesterday. 18 of the 49 were completely new. Two others were major modifications to something old. Minor changes on about five others.

But no doubt, my core ideas are the same as they ever were. . . . same as they ever were . . . same as they ever were . . .

02-10-09 *Experience Primary, Events Secondary?* (to R. Blume-Kohout)

Events as intersections of experiences? Does that phrase or the one in the title bear any resemblance to what you were saying to me yesterday?

This is probably not your kind of reading, but let me put it on the table:

`http://psychclassics.yorku.ca/James/experience.htm`.

03-10-09 *From Dewey to Unger . . .* (to T. Homer-Dixon)

It was good meeting you—I'm back in my office at PI, immediately after the Unger talk . . . scratching my head, wondering what I heard. It could be that I'm just missing crucial vocabulary. In any case, my education in James, Dewey, Schiller, Putnam, Rorty, etc., didn't carry me to the brink of understanding Unger. Maybe better luck to me next time.

Still, one doesn't denigrate opportunities: I'm glad to have met another person in Waterloo interested in pragmatism. In case it might entertain you, let me offer one of my own spoutings on the subject. See section 8.1 of this paper: `http://arxiv.org/abs/0906.2187`. That section is much independent of the remainder and gives some orientation on how I think the physical world is put together along "fallibalist" lines.

06-10-09 *QF/Southwestern Ontario Philosophy of Physics Talk This Friday* (to D. Fraser & J. Berkovitz)

I'm so sorry I'm going to miss this. I don't get back to Waterloo from New Orleans until late afternoon. Good luck Jossi! The more de Finetti these guys are exposed to, the better!

Doreen's Preply

Jossi Berkovitz (U Toronto) will be giving a talk this Friday, October 9th at 11a.m. in the Alice Room:

> The World According to De Finetti
> Jossi Berkovitz (U Toronto)
>
> Abstract: Bruno de Finetti is one of the founding fathers of the subjectivist school of probability, where probabilities are interpreted as rational degrees of belief. His work on the relation between the theorems of the probability calculus and rationality is among the corner stones of modern subjective probability theory. De Finetti maintained that rationality requires that an agent's degrees of belief be coherent. I argue that de Finetti held that the coherence conditions of degrees of belief in events depend on their verifiability. On this view, the familiar constraints of coherence only apply to sets of degrees of belief that could in principle be jointly verified. Accordingly, the constraints that coherence imposes on degrees of belief are generally weaker than the familiar ones. I then consider the implications of this interpretation of de Finetti for probabilities in quantum mechanics, focusing on the EPR/Bohm experiment and Bell's theorem.

06-10-09 *New Orleans, RMP, and Capacities* (to N. D. Mermin)

Sorry I've kept you waiting so long for a reply. After that horrible meeting, I needed a couple of days away from anything professional. Then I jutted off to New Orleans for the next meeting. And I've been kept busy in all kinds of way since. Finally a moment to myself: I'm writing you from the poshest hotel bar I've ever written you from.

Just had dinner with Henry Folse (the Bohr scholar) and had a tour of his amazing house near the Garden District. It was the poshest house I ever visited before the poshest bar. An 1868 home with 14 ft ceilings and 10,000 square feet! The front hall was just amazing—about 12 feet wide and running all the way from the front of the house to the back. The dining room had two massive mirrors taken from an old New Orleans bordello. There was just so much character to the house and gardens. If I had a place like that, my thinking would be of such a higher order!

Anyway, keep the router bit, and thanks for the heads-up about the second referee. I hope to get you the revised Section 6 by Friday evening. I'll probably work on it mostly on Friday's flights (I got upgrades to business class).

On my flight over here, I read 90 pages of Nancy Cartwright's *The Dappled World*. Timpson was right: I do find much in her of value. I want to revise my language of what Hilbert-space dimension signifies—I rather like her terminology of "capacities." True enough it is "real" as I have emphasized before, but it should be thought of as a capacity like mass or charge. Its ontological role is of something of the same flavor. The way I want to emphasize things now is that what quantum mechanics has taught us is that associated with any piece of the physical universe is a previously overlooked capacity. Newton taught us that associated with any piece of the universe is a capacity to attract other bodies, quantified by the body's mass. I'm thinking something like that for Hilbert-space dimension. It's not like I haven't used language like this before, but now it seems to be more entrenched in my big picture of things. Don't know why, but the idea makes me tingle more than it had previously.

I wish you had been there for Friday's panel discussion. It was quite annoying but revealing. Psychologically, that is. This time Valentini called me a solipsist. What is it about these guys?!? Got a note from Norsen Saturday calling me a solipsist again too. I didn't bother to reply. If you want to stay on my good side, never call me a solipsist.

I'm glad you got something out of the meeting and (convinced yourself that) you enjoyed yourself. It meant a lot to me to see you again.

16-10-09 *The More and the Modest* (to L. Hardy)

I started writing the note below while in New Orleans, but only finished it today. It seemed a shame to rewrite the introduction, so I send it as is.

> It don't mean a thing if it ain't got that Zing!
>
> — Dean Rickles

Greetings from the land of the crawdad. This note is to say thanks again for your remarks at the panel discussion Friday. You did something that none of the other ones did—you caused me to think and clarify, rather than piss me off from a display of overt closed-mindedness. It amazes me the way Norsen and Valentini preach to me without understanding an ounce of the way I'm trying to see things. (I just got another note from Norsen yesterday accusing me yet again of solipsism.)

Your point on Hilbert-space dimension on the other hand was a productive one, and thinking about how I should reply to you has helped me realize that my language needs a significant overhaul. With better language, we inevitably get better understanding. Here goes.

I used to ask, "What is real of a quantum system?," and give "Zing" as a placeholder for the answer. What is Zing? The only thing I've been able to comfortably identify for it in the quantum formalism is Hilbert-space dimension. But that certainly can't be enough to get the world going—or at least, I think you would (and did) say something like this. John Sipe once wrote, "the quantum world of this interpretation is a fixed, static thing. It is a frozen, changeless place."

That is *at face value*, there's not enough engine, not enough gears and pinions, in this bare "reality" to account for motion and change and indeterminism and novelty and growth and evolution and . . . make the list just as big as you want . . . all the things we see around us. What is weird is that I agree. Such a minimalist world doesn't even track with what I myself believe. So it becomes a question of why I have doggedly resisted saying more.

I believe I have tracked down the troublemaker in my mode of expression, and it is this. My trouble is *not* that I believe that there is *nothing* in the world, or perhaps a minimalist number of things (too minimal), as the solipsism-chargers seem so obsessed with accusing me of. It is rather that my point of view admits *too many* things into the world—too many things of an independent and self-sustaining reality, things for which there are no equations; realities of which I am only willing to *point* to and say effectively, "Yeah the world includes that too."

This is because the ontology I imagine is the one of the humanist, rather than the trained or indoctrinated scientist. Take Democritus as an example: for him, the universe literally was "atoms and void." That simple phrase was meant to account for all that is. Similarly, the universe Einstein sought was literally meant to be a differentiable manifold supporting the solution to a very clever (though never found) differential equation—all else, even such as the flow of time, was simply illusion. (Recall Mermin's talk at PIAF.) The unifying theme in these two visions is that chairs and thumbs and bricks and all the like from our common experiences are secondary things—effective (operational) descriptions or illusions—having no real, primary existence of their own. For a most relevant example, consider David Wallace's universe. As far as I can tell, it is literally a single quantum state on a Hilbert space. That vector, timelessly unchanging in a rotating frame, is the universe's whole substance. The apparent indeterminism of the quantum world is subordinate to the greater monistic determinism and timelessness of the whole.

But here's the funny thing: It is exactly of Einstein's and Wallace's universes, not my own, that I would say, "the world of this interpretation is a fixed, static thing. It is a frozen, changeless place." To my mind, these are both completely barren visions for the world. I think our distaste for each other's proclivities comes from this: It is a very old philosophical divide, that between the rationalist attitude and the empiricist attitude. It has only become clear to me recently, but at my core I am an extreme empiricist.

Here is the way my friend William James put the distinction when he was in a relatively mild mood:

> By empiricism I mean the tendency which lays most stress on the part, the element, the individual, treats the whole as a collection, and calls the universal an abstraction. By rationalism I mean the tendency to emphasize the universal, and to make the whole prior to the part, in the order both of logic and of being. The temper of rationalism is dogmatic: it willingly claims necessity for its conclusions. Empiricism is more modest, and professes to deal in hypotheses only.

A more full-blooded statement of the issues involved, and just beautiful reading, can be found in a long quote I've plucked out for you from his essay "The Sentiment of Rationality." See subsection at the end of this note. If you find something worth reading in this note of my own composition, I hope you will take the time to read the longer quote of James. In the end, it has become very crucial to my thinking.

Let me give a briefer hint of the issue here. It has to do with the way James put the dangers of rationalism when he was in a fiercer mood:

> Let me give the name of 'vicious abstractionism' to a way of using concepts which may be thus described: We conceive a concrete situation by singling out some salient or

> important feature in it, and classing it under that; then, instead of adding to its previous characters all the positive consequences which the new way of conceiving it may bring, we proceed to use our concept privatively; reducing the originally rich phenomenon to the naked suggestions of that name abstractly taken, treating it as a case of 'nothing but' that, concept, and acting as if all the other characters from out of which the concept is abstracted were expunged. Abstraction, functioning in this way, becomes a means of arrest far more than a means of advance in thought. It mutilates things; it creates difficulties and finds impossibilities; and more than half the trouble that metaphysicians and logicians give themselves over the paradoxes and dialectic puzzles of the universe may, I am convinced, be traced to this relatively simple source. *The viciously privative employment of abstract characters and class names* is, I am persuaded, one of the great original sins of the rationalistic mind.

The way this is relevant to me is the following. Far from thinking the world is an empty place, a place only with me in it. I think it is full of things, overflowing with things. *All* distinct things, from head to toe. And literally so. It is not a world made of six flavors of quarks glued together in various combinations. It is not a world that maps to a single algorithm running on Rob Spekkens' favorite version of Daniel Dennett's mechanistic cellular automaton. It is a world of heads and toes and doorknobs and dreams and ambitions and every kind of particular. (And that is not a typo: It is a world in which even dreams and ambition have substance.) It is a world in which Vivienne Hardy is a distinct entity, not "constructed" of anything else, but a true-blue crucial piece of the universe as it is today—no less crucial than spacetime itself.

In modern parlance, I am not a reductionist. And when my world is judged as empty (of all but me), I claim it is because I am being interpreted from the standpoint of an (explicit or implicit) reductionist worldview. It is true that my envisioned world may be as cockamamie as James' heads of Borneo:

> Taken as it does appear, our universe is to a large extent chaotic. No one single type of connection runs through all the experiences that compose it. If we take space-relations, they fail to connect minds into any regular system. Causes and purposes obtain only among special series of facts. The self-relation seems extremely limited and does not link two different selves together. Prima facie, if you should liken the universe of absolute idealism to an aquarium, a crystal globe in which goldfish are swimming, you would have to compare the empiricist universe to something more like one of those dried human heads with which the Dyaks of Borneo deck their lodges. The skull forms a solid nucleus; but innumerable feathers, leaves, strings, beads, and loose appendices of every description float and dangle from it, and, save that they terminate in it, seem to have nothing to do with one another. Even so my experiences and yours float and dangle, terminating, it is true, in a nucleus of common perception, but for the most part out of sight and irrelevant and unimaginable to one another.

But it is not an *empty* world.

With this as a background, let me now return to Zing, Hilbert-space dimension, and the needed overhaul of my language. I ask, "What is real of a quantum system?" and answer, "its Hilbert-space dimension," but that is a very funny thing to say. There is *so much* that is real of a quantum system, why would I ever say that? I wouldn't say it of Vivienne Hardy, for instance,—I already declared this quite vocally—and she is a perfectly good example of a quantum system. So, what am I really up to?

It is a bit like this: Forget quantum mechanics, and think back to the days when the only physics known was basically Newtonian gravity. Would any Newtonian have ever answered the question, “What is real of a physical system?,” with the declaration, “Only its gravitational mass is real of it.”? Most definitely not. But that is because Newton merely/boldly taught us but a singular thing: That every body in the universe, that every thing that can be carved out from it, had a previously undisclosed capacity—a numerical capacity to (try to) attract every other body in the universe. On the one hand, I say “merely” because looked at in this way, it really is a very *modest* move—modest at least by the lights of the Steven-Weinberg-Stephen-Hawking-theory-of-everything generation: It was no theory of everything—never pretended to be. On the other hand, I say “boldly” because I certainly agree with my friend Hans Christian von Baeyer who wrote in one of his books:

> Great revolutionaries don’t stop at half measures if they can go all the way. For Newton this meant an almost unimaginable widening of the scope of his new-found law. Not only Earth, Sun, and planets attract objects in their vicinity, he conjectured, but all objects, no matter how large or small, attract all other objects, no matter how far distant. It was a proposition of almost reckless boldness, and it changed the way we perceive the world.

And therein lies the key, I think, to how I should modify my language. When I ask myself what have we learned with quantum mechanics, I want to say first and foremost that we have learned how we should more consistently gamble upon the consequences of our interactions with external physical systems. But why this new calculus for gambling? Because quantum mechanics is uncovering that every object in the universe has a previously undisclosed *capacity*.

I would have liked to have said “uncovered,” but at this stage of research I must still settle for “is uncovering”—it is an unfinished project to understand the significance of quantum mechanics in these terms. Hilbert-space dimension, like gravitational mass, is representative of some universal capacity. That’s the real idea. Dimension is a quality a body possesses in a way that, in a QBayesian understanding, it does not “possess” a quantum state. If we conceptually delete an agent gambling upon the consequences of his interactions with a quantum system—the QBist says—we also conceptually delete its quantum state. But there is no reason to believe the system and the system’s capacity are deleted as well.

Capacity for what? That’s where the hard part begins. Sometimes I say the capacity for birth and creation. Sometimes I say the capacity to entertain counterfactuals. Sometimes I say it is a capacity that can be harnessed to aid of computation, as in quantum computation. The truth is, I don’t yet know what I mean in any precise way. I only know that I have a strong inner tug to thinking that the SIC calculus will help reveal a precise idea.

Anyway, I write all this to put the key idea into perspective and to practice a way of speaking that I had not used to any great extent before. [Chris Timpson had pointed out that there are several similarities between the way I speak and the way the philosopher Nancy Cartwright speaks of “capacities.” And on this trip to New Orleans, I have confirmed it is very much so by starting to read her book, *The Dappled World*. So, at least I’m not alone in the world.] You were indeed right to suspect that Hilbert-space dimension doesn’t have enough gears and pinions to get things going, but neither did gravitational mass. Nonetheless, the disclosing of that old capacity—gravitational mass—“changed the way we perceive the world” (von Baeyer). And so I think of our new capacity.

Thus I end this instalment by saying thanks again for pointing out a great deficiency in my choice of words and for consequently causing me to think. I’ll leave you with a sentence that I wrote in this file, but could not quite find a way to use in the essay. It seems a shame to throw it away:

> Quantum mechanics calls out, “I will not be a representative of your monistic dreams!”

When Norsen, Valentini, and Wiseman call me solipsist (or worse), I think it is because they are working from the middle of a monistic dream.

Passage from William James' essay "The Sentiment of Rationality," (Compiled for Lucien)

Part of Immediate Interest

The facts of the world in their sensible diversity are always before us, but our theoretic need is that they should be conceived in a way that reduces their manifoldness to simplicity. Our pleasure at finding that a chaos of facts is the expression of a single underlying fact is like the relief of the musician at resolving a confused mass of sound into melodic or harmonic order. The simplified result is handled with far less mental effort than the original data; and a philosophic conception of nature is thus in no metaphorical sense a labor-saving contrivance. The passion for parsimony, for economy of means in thought, is the philosophic passion *par excellence*; and any character or aspect of the world's phenomena which gathers up their diversity into monotony will gratify that passion, and in the philosopher's mind stand for that essence of things compared with which all their other determinations may by him be overlooked.

More universality or extensiveness is, then, one mark which the philosopher's conceptions must possess. Unless they apply to an enormous number of cases they will not bring him relief. The knowledge of things by their causes, which is often given as a definition of rational knowledge, is useless to him unless the causes converge to a minimum number, while still producing the maximum number of effects. The more multiple then are the instances, the more flowingly does his mind rove from fact to fact. The phenomenal transitions are no real transitions; each item is the same old friend with a slightly altered dress.

Who does not feel the charm of thinking that the moon and the apple are, as far as their relation to the earth goes, identical; of knowing respiration and combustion to be one; of understanding that the balloon rises by the same law whereby the stone sinks; of feeling that the warmth in one's palm when one rubs one's sleeve is identical with the motion which the friction checks; of recognizing the difference between beast and fish to be only a higher degree of that between human father and son; of believing our strength when we climb the mountain or fell the tree to be no other than the strength of the sun's rays which made the corn grow out of which we got our morning meal?

But alongside of this passion for simplification there exists a sister passion, which in some minds—though they perhaps form the minority—is its rival. This is the passion for distinguishing; it is the impulse to be *acquainted* with the parts rather than to comprehend the whole. Loyalty to clearness and integrity of perception, dislike of blurred outlines, of vague identifications, are its characteristics. It loves to recognize particulars in their full completeness, and the more of these it can carry the happier it is. It prefers any amount of incoherence, abruptness, and fragmentariness (so long as the literal details of the separate facts are saved) to an abstract way of conceiving things that, while it simplifies them, dissolves away at the same time their concrete fulness. Clearness and simplicity thus set up rival claims, and make a real dilemma for the thinker.

A man's philosophic attitude is determined by the balance in him of these two cravings. No system of philosophy can hope to be universally accepted among men which grossly violates either need, or entirely subordinates the one to the other. The fate of Spinoza, with his barren union of all things in one substance, on the one hand; that of Hume, with his equally barren "looseness and separateness" of everything, on the other—neither philosopher owning any strict and systematic disciples today, each being to posterity a warning as well as a stimulus—show us that the only possible philosophy must be a compromise between an abstract monotony and a concrete heterogeneity. But the only way to mediate between diversity and unity is to class the diverse items as cases of a common essence which you discover in them. Classification of things into extensive "kinds" is thus the first step; and classification of their relations and conduct into extensive "laws" is the last step, in their philosophic unification. A completed theoretic philosophy can thus never be anything more than a completed classification of the world's ingredients; and its results must always be abstract, since the basis of every classification is the abstract essence embedded in the living fact—the rest of the living fact being for the time ignored by the classifier. This means that none of our explanations are complete. They subsume things under heads wider or more familiar; but the last heads, whether of things or of their connections, are mere abstract genera, data which we just find in things and write down.

When, for example, we think that we have rationally explained the connection of the facts A and B by classing both under their common attribute x, it is obvious that we have really explained only so much of these items as *is* x. To explain the connection of choke-damp and suffocation by the lack of oxygen is to leave untouched all the other peculiarities both of choke-damp and of suffocation—such as convulsions and agony on the one hand, density and explosibility on the other. In a word, so far as A and B contain l, m, n, and o, p, q, respectively, in addition to x, they are not explained by x. Each additional particularity makes its distinct appeal. A single explanation of a fact only explains it from a single point of view. The entire fact is not accounted for until each and all of its characters have been classed with their likes elsewhere. To apply this now to the case of the universe, we see that the explanation of the world by molecular movements explains it only so far as it actually *is* such movements. To invoke the "Unknowable" explains only so much as is unknowable, "Thought" only so much as is thought, "God" only so much as is God. *Which* thought? *Which* God?—are questions that have to be answered by bringing in again the residual data from which the general term was abstracted. All those data that cannot be analytically identified with the attribute invoked as universal principle, remain as independent kinds or natures, associated empirically with the said attribute but devoid of rational kinship with it.

Hence the unsatisfactoriness of all our speculations. On the one hand, so far as they retain any multiplicity in their terms, they fail to get us out of the empirical sand-heap world; on the other, so far as they eliminate multiplicity, the practical man despises their empty barrenness. The most they can say is that the elements of the world are such and such, and that each is identical with itself wherever found; but the question Where is it found? the practical man is left to answer by his own wit. Which, of all the essences, shall here and now be held the essence of this concrete thing, the fundamental philosophy never attempts to decide. We are thus led to the conclusion that the simple classification of things is, on the one hand, the best possible theoretic philosophy, but is, on the other, a most miserable and inadequate substitute for the fulness of the truth. It is a monstrous abridgment of life, which, like all abridgments, is got by the absolute loss and casting out of real matter. This is why so few human beings truly care for philosophy. The particular determinations which she ignores are the real matter exciting needs, quite as potent and authoritative as hers. What does the moral enthusiast care for philosophical ethics? Why does the *Æsthetik* of every German philosopher appear to the artist an abomination of desolation?

> Grau, teurer Freund, ist alle Theorie
> Und grün des Lebens goldner Baum.

The entire man, who feels all needs by turns, will take nothing as an equivalent for life but the fulness of living itself. Since the essences of things are as a matter of fact disseminated through the whole extent of time and space, it is in their spread-outness and alternation that he will enjoy them. When weary of the concrete clash and dust and pettiness, he will refresh himself by a bath in the eternal springs, or fortify himself by a look at the immutable natures. But he will only be a visitor, not a dweller, in the region; he will never carry the philosophic yoke upon his shoulders, and when tired of the gray monotony of her problems and insipid spaciousness of her results, will always escape gleefully into the teeming and dramatic richness of the concrete world.

Of Secondary Interest to the Present Discussion, but Worth Reading If You've Gotten This Far and Have Nothing Better To Do

So our study turns back here to its beginning. Every way of classifying a thing is but a way of handling it for some particular purpose. Conceptions, "kinds," are teleological instruments. No abstract concept can be a valid substitute for a concrete reality except with reference to a particular interest in the conceiver. The interest of theoretic rationality, the relief of identification, is but one of a thousand human purposes. When others rear their heads, it must pack up its little bundle and retire till its turn recurs. The exaggerated dignity and value that philosophers have claimed for their solutions is thus greatly reduced. The only virtue their theoretic conception need have is simplicity, and a simple conception is an equivalent for the world only so far as the world is simple—the world meanwhile, whatever simplicity it may harbor, being also a mightily complex affair. Enough simplicity remains, however, and enough urgency in our craving to reach it, to make the theoretic function one of the most invincible of human impulses. The quest of the fewest elements of things is an ideal that some will follow, as long as there are men to think at all.

But suppose the goal attained. Suppose that at last we have a system unified in the sense that has been explained. Our world can now be conceived simply, and our mind enjoys the relief. Our universal concept has made the concrete chaos rational. But now I ask, Can that which is the ground of rationality in all else be itself properly called rational? It would seem at first sight that it might. One is tempted at any rate to say that, since the craving for rationality is appeased by the identification of one thing with another, a datum which left nothing else outstanding might quench that craving definitively, or be rational *in se*. No otherness being left to annoy us, we should sit down at peace. In other words, as the theoretic tranquillity of the boor results from his spinning no further considerations about his chaotic universe, so any datum whatever (provided it were simple, clear, and ultimate) ought to banish puzzle from the universe of the philosopher and confer peace, inasmuch as there would then be for him absolutely no further considerations to spin.

This in fact is what some persons think. Professor Bain says —

> A difficulty is solved, a mystery unriddled, when it can be shown to resemble something else; to be an example of a fact already known. Mystery is isolation, exception, or it may be apparent contradiction: the resolution of the mystery is found in assimilation, identity, fraternity. When all things are assimilated, so far as assimilation can go, so far as likeness holds, there is an end to explanation; there is an end to what the mind can do, or can intelligently desire . . . . The path of science as exhibited in modern ages is toward generality, wider and wider, until we reach the highest, the widest laws of every department of things; there explanation is finished, mystery ends, perfect vision is gained.

But, unfortunately, this first answer will not hold. Our mind is so wedded to the process of seeing an *other* beside every item of its experience, that when the notion of an absolute datum is presented to it, it goes through, its usual procedure and remains pointing at the void beyond, as if in that lay further matter for contemplation. In short, it spins for itself the further positive consideration of a nonentity enveloping the being of its datum; and as that leads nowhere, back recoils the thought toward its datum again. But there is no natural bridge between nonentity and this particular datum, and the thought stands oscillating to and fro, wondering "Why was there anything but nonentity; why just this universal datum and not another?" and finds no end, in wandering mazes lost. Indeed, Bain's words are so untrue that in reflecting men it is just when the attempt to fuse the manifold into a single totality has been most successful, when the conception of the universe as a unique fact is nearest its perfection, that the craving for further explanation, the ontological wonder-sickness, arises in its extremest form. As Schopenhauer says, "The uneasiness which keeps the never-resting clock of metaphysics in motion, is the consciousness that the non-existence of this world is just as possible as its existence."

The notion of nonentity may thus be called the parent of the philosophic craving in its subtilest and profoundest sense. Absolute existence is absolute mystery, for its relations with the nothing remain unmediated to our understanding. One philosopher only has pretended to throw a logical bridge over this chasm. Hegel, by trying to show that nonentity and concrete being are linked together by a series of identities of a synthetic kind, binds everything conceivable into a unity, with no outlying notion to disturb the free rotary circulation of the mind within its bounds. Since such unchecked movement gives the feeling of rationality, he must be held, if he has succeeded, to have eternally and absolutely quenched all rational demands.

But for those who deem Hegel's heroic effort to have failed, nought remains but to confess that when all things have been unified to the supreme degree, the notion of a possible other than the actual may still haunt our imagination and prey upon our system. The bottom of being is left logically opaque to us, as something which we simply come upon and find, and about which (if we wish to act) we should pause and wonder as little as possible. The philosopher's logical tranquillity is thus in essence no other than the boor's. They differ only as to the point at which each refuses to let further considerations upset the absoluteness of the data he assumes. The boor does so immediately, and is liable at any moment to the ravages of many kinds of doubt. The philosopher does not do so till unity has been reached, and is warranted against the inroads of those considerations, but only practically, not essentially, secure from the blighting breath of the ultimate Why? If he cannot exorcize this question, he must

> ignore or blink it, and, assuming the data of his system as something given, and the gift as ultimate, simply proceed to a life of contemplation or of action based on it.

16-10-09 *Degree of Engagement* (to H. C. von Baeyer)

You might also contemplate this transparency from my last talk:

$$Q(D_j) = \left(\frac{1}{2}qd + 1\right) \sum_{i=1}^{n} P(H_i)P(D_j|H_i) - \frac{1}{2}q$$

with $q = 0, 1, 2, \ldots$ (character of the zing) and $d = 2, 3, 4, \ldots$ (value of a beable, how much zing).

For "character of the zing" read "degree of observer engagement" or "degree of observer activation". Or maybe the quantity q as "amount of alchemical potential." Mostly playing with words.

16-10-09 *The Sister Passion and the Shape of F-Theory to Come* (to R. W. Spekkens)

I wrote the note below with Lucien in mind, but I might send it to you as well. [See 16-10-09 note "The More and the Modest" to L. Hardy.] When composing it, I was struck by how much the first three paragraphs of the associated attachment sounded like what I deem of your philosophy of what makes good science. (Tell me if I'm right.) Also, in the actual note (i.e., below, not attached), I further flesh out some thoughts that really, rightly started in a discussion in your office. So, really you deserve some credit as well.

17-10-09 *The More and the Modest, 2* (to N. D. Mermin)

Some reading you might enjoy on a Saturday morning—see below. [See note to L. Hardy, titled "The More and the Modest" and dated 16-10-09.] I'll get back to fixing section 6 in the Revs Mod Phys paper Tuesday, after my visiting brother leaves. It's been hard to focus with the New Orleans meeting and then my brother's visit.

You never did say what you thought of that Weyl quote.

The word is out, by the way, that Grassl and Scott have now proven SICs to exist in dimensions 35 and 48. Two more dimensions in the list now! Infinity can't be all that far away.

17-10-09 *The More and the Modest, 3* (to N. D. Mermin)

I'm the youngest of five: two brothers and two sisters. The order was sister, brother, brother, sister, me. Weyl quote below:

> Finally and above all, it is the essence of the continuum that it cannot be grasped as a rigid existing thing, but only as something which is in the act of an inwardly directed unending process of becoming … . In a given continuum, of course, this process of becoming can have reached only a certain point, i.e. the quantitative relations in an intuitively given piece $\mathcal{S}$ of the world [regarded as a four-dimensional continuum of events] are merely approximate, determinable only with a certain latitude, not merely in consequence of the limited precision of my sense organs and measuring instruments,

> but because they are in themselves afflicted with a sort of vagueness ... . And only "at the end of all time," so to speak, ... would the unending process of becoming $\mathcal{S}$ be completed, and $\mathcal{S}$ sustain in itself that degree of definiteness which mathematical physics postulates as its ideal ... . Thus the rigid pressure of natural causality relaxes, and there remains, without prejudice to the validity of natural laws, room for autonomous decisions, causally absolutely independent of one another, whose locus I consider to be the elementary quanta of matter. These "decisions" are what is actually real in the world.

19-10-09 *Decisions* (to N. D. Mermin)

Merminition 184: *Calling the constituents of the real present "decisions" is a bit strange, though.*

Expand on that a bit if you can. I'm quite attracted to what I think I read of his idea.

19-10-09 *Cartwright* (to M. Tait)

Taitism 1: *I should also say that unlike Chris Timpson, I don't think that imposing a Cartwrightian metaphysics on QBism, or any other ready-made ontology, is necessarily helpful.*

Well, I'm with you on the "ready-made" part. I shouldn't think anybody's system will fit me perfectly. On the other hand, I have now read some of Nancy Cartwright's *Dappled World*, and I think Chris Timpson was right to perceive some similarities between Cartwright and me. You'll see what I mean if you read the note below and the attached. [See 16-10-09 note titled "The More and the Modest" to Lucien Hardy.]

Thanks for your proposal; it was good reading.

19-10-09 *Purple Haze* (to M. Tait)

Taitism 2: *I'm still a little hazy on the notion that the Born rule should be seen as an 'empirical addition' to Bayesian reasoning. It is clear that it is a normative rule that goes beyond Dutch book coherence; it less clear to me how we ought to interpret this additional constraint, though I'm still working through the meatier parts of your paper with Schack 'Quantum Bayesian Coherence'.*

Well, I'll make no bones about it: I'm hazy as well. There is however a slightly better discussion in the new (attached) paper. [See `arXiv:0912.4252v1`.] It'd be great if you could contribute to getting these ideas straight.

Also, there was quite a mistake (a technical one) in Section 6 of the bigger paper. That is fixed in the attached one. I still need to modify the big one and re-post. Anyway, fixing the mistake has led to some really good stuff: A new parameter crops up that I like to think of as quantifying the degree to which the observer is engaged in quantum mechanics. (By "engaged observer" I am aiming for a contrasting term for Pauli's "detached observer".) See other attachment for a quick illustration:

$$p(D_j) = \left(1 + \frac{1}{2}qd\right) \sum_i p(H_i)p(D_j|H_i) - \frac{1}{2}q \,,$$

where $q = 0, 1, 2, \ldots$ signifies the "character of the zing," and $d = 2, 3, 4, \ldots$ signifies the "value of a (local) beable, how much zing." [See also 25-09-09 note titled "The Scale of Engagement" to H. C. von Baeyer.]

20-10-09 *Howard vs Jammer* (to W. C. Myrvold)

Remember our discussion on Don Howard's versus Max Jammer's version of the Einstein-Ehrenfest conversation. See Don's discussion at:

`http://www.nd.edu/~dhoward1/Early%20History%20of%20Entanglement/sld026.html`.

I think you were right: Don definitely wins.

20-10-09 *My First Dean Rickles Quote* (to D. Rickles)

You'll now find yourself immortally samizdatized. You can find it here:

`http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`

starting at page 785 of text (page 813 wrt the raw pdf page numbering), in a note titled, "The More and the Modest." [See 16-10-09 note "The More and the Modest" to L. Hardy.]

Does this capture a little bit of (or have any overlap with) the way you were trying to defend me against Valentini?

Dean's Reply

> I'm deeply honoured.
>
> The 'more' versus 'modest' distinction hits the nail on the head actually. I was thinking about (what I think is) an analogous situation in modal logic, where you have your basic propositional calculus together with operators, including a modal operator. You define rules for these things (specifying when you have a well formed formula). Quantum mechanics can be set up initially like this: a formal schema. The job of interpretation is to latch this on to something. In modal logic the formal system can latch on to a range of things that are related only by their functional relations. For example, you might think in terms of possible worlds, of states in a computer program, of temporal instants, etc. These provide the semantics: the interpretation. They can all satisfy the basic formal schema, but they are obviously very different beasts. And indeed, some semantics (e.g. states in a computer program) seem more modest than others (e.g. possible worlds).
>
> This reminded me of what was going on in the debate you and Rüdiger were having with Anthony. You were saying: the formal system isn't latching on to the stuff "out there, hidden from view" (or it is only in some very general sense: Hilbert space dimension) and Anthony was saying, No, it Is precisely latching onto stuff out there and hidden from view. One is indeed more Modest and one is More: but both satisfy everything that needs to be satisfied, formally and empirically. One says QM is limited in what it can say about "the world"; one doesn't.

21-10-09 *Since It's My Birthday . . .* (to H. C. von Baeyer)

Since it's my birthday, and I'm feeling myself get older by the minute—very, very finite today—I'll feel free to try to stoke your coals again.

Adding to the note I had sent you last month [see note to H. C. von Baeyer dated 25-09-09, titled "The Scale of Engagement"], I'll remind you of that cool Pauli quote where he mentions a "degree of detachment of the observer." It is quite nice.

W. Pauli, letter to Niels Bohr, dated 11 March 1955, photocopy obtained from the Niels Bohr Institute via Henry Folse.

Dear Bohr,

I find your letter of March 2nd very youthfull, which is just the reason that it is not easy for me to answer. Although we have the same view "as regards the fundamental physical problems which fall within the scope of the present quantum mechanical formalism" and although I agree with some parts of your letter, the situation is now complicated by your use in a publication of a phrase like "detached observer" (without comment!) which I used already in some publications in a very different way. I believe that this should be better avoided to prevent a confusion of the readers[208] and I don't cling at all to particular words myself. I also felt, already before your letter arrived, that my brief characterisation of the observer in quantum theory as "non-detached" is in one important respect misleading. As is well known to both of us, it is essential in quantum mechanics that the apparatus can be described by classical concepts. Therefore the observer is always entirely detached to the *results* of his observations (marks on photographic plates etc.), just as he is in classical physics. I called him, however in quantum physics "non-detached", when he chooses his experimental arrangements.[209]

I shall try to make my point logically clear, by defining my concepts, replacing hereby the disputed phrase by other words. As I was mostly interested in the question, *how much informative reference to the observer an objective description contains*, I am emphasizing that a communication contains in general *informations on the observing subject*.

Without particularly discussing the separation between a subject and the informations about subjects (given by themself or by other persons), which can occur as elements of an "objective description", I introduced a concept "degree of detachment of the observer" in a scientific theory to be judged on the kind and measure of informative reference to the observer, which this description contains. For the objective character of this description it is of course sufficient, that every individual observer can be replaced by every other one which fullfills the same conditions and obeys the same rules. In this sense I call a referency to experimental conditions an "information on the observer" (though an impersonal one), and the establishment of an experimental arrangement fulfilling specified conditions an "action of the observer"—of course not of an individual observer but of "the observer" in general.

In physics I speak of a detached observer in a general conceptual description or explanation only then, *if it does not contain any explicit reference to the actions or the knowledge of the observer.* The ideal, that this should be so, I call now "the ideal (E)" in honor of Einstein. Historically it has its origin in celestial mechanics.

There is an important *agreement* between us that we find Einstein not consequent in this formulation of the "ideal E". Indeed, there is no a priori reason whatsoever to introduce here a difference between the *motion* of the observer on the one hand, and the realization of specified experimental conditions by the observer on the other hand. If Einstein were consequent he had to "forbid" also the word coordinate system in physics (as not being objective). That the situation in quantum mechanics has a

[208] An explaining remark about it in your *new* article would be most welcome!

[209] I still believe today that this more restricted use of my terminology is very good and that it has been unhappily obscured in your article in a non-logical way!

deep similarity with the situation in relativity is already shown by the application of mathematical groups of transformation in the physical laws in both cases.

In this way I reached the conclusion to distinguish sharply between the "ideal of an objective description" (meaning science) on the one hand (which I warmly supported just as you do) and the "ideal of the detached observer" on the other hand (which I rejected as much too narrow).

What really matters for me is not the word "detached", but the more active role of the observer in quantum physics, which is already implied in your [constatation?] of the "indivisibility of the phenomena and the essential irreversibility involved in the very concept of observation". According to quantum physics the observer has indeed a new relation to the physical events around him in comparison with the classical observer, who is merely a spectator: The experimental arrangement freely chosen by the observer lets appear *single* events *not* determined by laws, the ensembles of which are governed by *statistical* laws.[210] It is not relevant to me, if you say the same thing using *different* terminologies (but please use [essentially?] different words than I). They will only confirm my statements again as all these statements on the observer are part of an "objective description".

I confess, that very different from you, I do find sometimes scientific inspiration in mysticism[211] (if you believe that I am in danger, please let me know), but this is counterbalanced by an *immediate* sense for mathematics. The result of both seems to be my kind of physics, whilst I consider epistemology merely as a logical comment to the application of mathematics in physics.[212] Thus when I read a sentence as "how to eliminate subjective elements in the account of experience" my immediate association is "group theory" which then determines my whole reaction to your letter. Although the first step to "objectivity" is sometimes a kind of "separation", this task excites in myself the vivid picture of a superior common order to which all subjects are subjected, mathematically represented by the "laws of transformations" as the key of the "map", of which all subjects are "elements".

I hope that it will be possible to find a terminology which will turn out to be satisfactory for both of us, but it is no hurry with it. I propose to resume this discussion only when your new article will be ready, which I am eagerly awaiting. It will show me your terminologies in more general cases of objective descriptions, of which I am most interested in the application to biology, in connection with your new expression "natural evolution".

From March 16th till about 27th I am away in Germany and Holland and when I come back I hope either to see you or to hear from you (I wrote to Basel to get informations on your lecture there).[213]

Hoping that you will in the future (just as I do myself) enjoy the enrichment coming from the different kind of access to science by different scientists, expressed in different, but not contradicting terminologies, I am sending, also in the name of Franca, all good

[210] In this way we obtain just the logical foundations of an "*objective* description" of the incidents (Ein begriffe) which the quantum mechanical observer makes within his surroundings with his experimental arrangements. Attention: there is *no* logical contradiction between a word like "trouble" and a possibility of its objective observation and description.

[211] By the way: the "Unity" of everything has always been one of the most prominent ideas of all mystics.

[212] We are here *both* in our letter in a realm of information on the writing subject, which do *not* belong to the "objective content of the communications".

[213] Meanwhile I heard from P. Huber in Basel, [Fierz is in the United States], that your lecture there is on March 30. On this date I am very glad, because I shall be back from my trip by then. Paa Gensje!

wishes to yourself, to Margrethe and to the whole family,

as yours complementary old
W. Pauli

21-10-09 *All the Things that Got in My Way* (to R. Schack)

Here is the question that has been haunting me since yesterday's drive to Guelph. Suppose I judge two systems to be of Hilbert-space dimensions d_1 and d_2, and that they are each localized in space, a distance r from each other. Other than that, I make no commitments in what I think of them. I.e., whether they are spin systems, elaborate multi-level quantum dots, a combination of the two things, caffeine molecules, ion traps, etc., etc. Just "raw" Hilbert spaces. What further things should I judge of them, even with this minuscule, isolated information?

21-10-09 *PI/FQXI Workshop on Laws of Nature (May 2010)* (to S. Weinstein & D. H. Wolpert)

Sorry for the long absence.

I'm not as impressed with Unger as Lee is: I tried reading his *Pragmatism Unbound* and he just impressed me as an amateur who like to use big words and confusing constructions more than anything. In the end I got less than halfway through it. [You guys do know, or should know, I try to read everything there is on pragmatism.] I had a similar reaction at his recent talk at CIGI—I didn't understand a single word of it; I didn't even know what it was about ... though maybe that's just ignorance on my part. Maybe he does better in this video blog `http://www.cigionline.org/blogs/2009/10/roberto-unger-cigi09-video-blog`. Tell me what you think. STILL—with regard to my own leanings with regard to these "law without law" issues —I can certify that at least his heart is in the right place. Thus I don't see any harm in inviting him, particularly if his presence will help Lee be a more effective cheerleader.

Concerning Hacking, I'm writing him again right now.

Concerning Nancy Cartwright, I finally took the time to read her book *The Dappled World*—I have never read any of her, despite Timpson's urging. And I find her truly interesting, and I would guess that she's got quite a unique point of view with respect to the other participants. My sense of adventure says, "She should certainly be invited, and if we have the budget why not?"

22-10-09 *Coming of Age with Quantum Information* (to S. Capelin)

Capelinism 4 : *I was wondering where we stand with your book. the main thing we're still in need of before we can hand the book over to production is an idea for the front cover.*

I tried googling Quantum Computer Images and came up with some interesting pictures. You could try it. Given the problems we're having finding something striking and suitable, how would you feel about using something like that?

I came to a standstill precisely because of the issue of the cover. (And to be honest, because of several other things that have taken my time as well.) But having wine with my brother visiting the other night may have led to a potentially viable idea. What would you think about doing something with the image on my webpage. (Using it as a core with perhaps some other things around it or in the background or vice versa or something.) That's a photo my brother took of me when I was about 11 or 12. He still has the actual photo; so we could get a higher version. ... I know you'll be "brutally honest" so I'm bracing myself ...

22-10-09 *QBism House Draft* (to N. Waxman)

Attached is the result of my day's labor. My LaTeX program shows 920 words; that's probably not exactly right, but is probably pretty close. Be honest in telling me what you think: It won't hurt my feelings, but you may find me being a stubborn old fart if I have a vested interest in this or that phrase you'd like to see dashed. If you think I should cut it some I can work at it (cutting part of the details of Kiki's reconstruction, or our research group's projects, or some of the big quote). But, of course, I like the balance of the essay as it is now—wouldn't have written that way otherwise—and if I can get away with keeping it mostly intact, I'd like to do that. But I certainly understand your space limitations.

Thanks for encouraging me on this. It was satisfying to write, and your enthusiasm flatters me. (I like to be flattered.)

QBism House Opens for Business

Ideas, like children, need homes where they can be loved, nurtured, and raised to independence. PI researcher Chris Fuchs brought an idea to Waterloo that he feels finally has a proper home. The home is called QBism House, and the idea, if it can be put in a few words, is that a consistent foundation for quantum mechanics reveals our universe as no universe at all, but a *pluriverse* instead. It is the sort of world the philosopher William James described as a "republican banquet ... where all the qualities of being respect one another's personal sacredness, yet sit at the common table of space and time." It is a world of "partially independent powers [where] each detail must come and be actually given, before, in any special sense, it can be said to be determined at all." Will Durant wrote in his masterful history of philosophy:

> The value of a [pluriverse] as compared with a universe, lies in this, that where there are cross-currents and warring forces our own strength and will may count and help decide the issue; it is a world where nothing is irrevocably settled, and all action matters. A monistic world is for us a dead world; in such a universe we carry out, willy-nilly, the parts assigned to us by an omnipotent deity or a primeval nebula; and not all our tears can wipe out one word of the eternal script. In a finished universe individuality is a delusion; "in reality," the monist assures us, we are all bits of one mosaic substance. But in an unfinished world we can write some lines of the parts we play, and our choices mould in some measure the future in which we have to live. In such a world we can be free; it is a world of chance, and not of fate; everything is "not quite"; and what we are or do may alter everything.

Take an 1886 home with 10 and 12 foot ceilings, the willpower of Kiki Fuchs to restore it to its historical grandeur, a good carpenter to build a library of solid-oak bookshelves to house nearly 1,000 books on this subject, a covered porch and a chalkboard to discuss these matters at length in the warm summer air, and one has the base for some fantastic, new physics. The house is called QBism House in honor of its "quantum Bayesian" heritage—a certain point of view about quantum mechanics developed by Fuchs, along with Carlton Caves, Rüdiger Schack, Marcus Appleby, and Howard Barnum. From that view, a quantum state is not a real thing, like a rock or a tree or a quark, but "a biological function, a means of orientation in life, of enabling and facilitating action, of taking account of reality and dominating it." Without doubt, this is a soft-edged idea, but it is one with a sharply defined mathematical core, and it is the latter that the research program of quantum Bayesianism is about. Still, sharp cores and soft edges complement each other, much like the sharp glassed lines of Perimeter Institute and the curlicued Victorian corbels of QBism House.

Making the home young again, while preserving its memories and wisdom, has been a labor of love for Kiki Fuchs. The walls had to be opened to replace the rusting pipes and the early-century electrical wiring; the chimneys had to be reconstructed from scratch; the basement had to be closed to the elements it had been subjected for years; crushed sewer pipes dug up and replaced; walls stripped of wallpaper and repainted; floors refinished; kitchen modernized; the dull, crumbling paint on the external walls given full-life Victorian colors; fencing, landscaping, and gardening—the to-do list went on and on for two years. The home that emerged is now a Waterloo landmark.

Bolstered by a grant of $512,000 from the United States Office of Naval Research and PI's contribution of much-valued office space and computer support, Fuchs is building a group to plow ahead on the technical aspects of the QBist project: particularly, developing a good probabilistic representation of quantum states and dynamics by considering the so-called symmetric informationally complete (SIC) quantum measurements. At the moment, the group is busy exploring whether these structures exist in all finite-dimensional Hilbert spaces, what the geometry of quantum-state space looks like when written in these terms, how quantum mechanics itself might be derived from the supposition of their existence, and characterizing the convex theories (generalizations of quantum mechanics) this representation most naturally fits into. All told, the QBies—yes, the QBies—consist of PI visiting researcher Marcus Appleby, associate postdoctoral fellow Åsa Ericsson, University of Waterloo PhD students Hoan Dang and Gelo Tabia, and MSc student Matthew Graydon. The students' desks can be found in room 415 at PI: the QBicle, of course.

This Fall and Winter, the group will meet once a week at PI (at a day and time yet to be settled). But once the weather is warm, the weekly meeting will be on the porch of QBism House, where the ghosts of pragmatist philosophers past (William James, John Dewey, and F. C. S. Schiller) can listen in with satisfaction about how their ideas of an unfinished, malleable world take a more exact form with the help of modern quantum mechanics. Anyone at PI is welcome to join the meetings, research, and discussion. The QBies and QBism House are open for business!

23-10-09 *SICs and Reality* (to C. M. Caves)

Strange question, coming from you at least. I got the impression that you had really gotten the point of the big paper with Rüdiger. (I'm now depressed a little bit.) Let me attach another attempt at explanation: This one is predominantly from Rüdiger's beautifully crisp pen.

The role of a SIC in these diagrams is purely as a counterfactual (an "action" that could be taken on the system but isn't). The idea is that the Born rule *supplements* the usual rules of probability with a coherence-like (or Dutch-book-style) rule for: How probabilities should be related between a factual situation and a counterfactual one.

There is no implication here that the SIC elements should be thought of as representing potential "elements of reality" in the EPR sense. (I.e., that there is one TRUE one of the d^2 potential ones, maybe not known, but TRUE nonetheless.) Indeed, if they were to be thought of in that way, then one should relate the factual and the counterfactual via the usual law of total probability: One might say that usual Dutch-book coherence requires it.

The agent is involved in bringing about measurement outcomes in this picture, just as much as he has always been for me. The measurement "on the ground" is a factual *action* that the agent takes upon the system; the measurement "in the sky" is a counterfactual *action*. No self-supporting hidden variables here: Outcomes that come about, come about because of the agent's actions.

With regard to another of your points: One should not think of the SICs as supreme in any way over other informationally complete measurements—instead, only as convenient. They should be thought of as a convenient "coordinate system for the problem at hand." Here's the way I put it in an abstract once:

> As physicists, we have become accustomed to the idea that a theory's content is always most transparent when written in coordinate-free language. But sometimes the choice of a good coordinate system is very useful for settling deep conceptual issues. Think of how Eddington-Finkelstein coordinates settled the longstanding question of whether the event horizon of a Schwarzschild black hole corresponds to a real spacetime singularity or not. Similarly we believe for an information-oriented or Bayesian approach to quantum foundations: That one good coordinate system may (eventually!) be worth more than a hundred blue-in-the-face arguments.

And here's the way I explained a related point to a student here:

> The point of all the various representations of quantum mechanics (quasi-probability reps, as well as things like Heisenberg vs. Schrödinger picture issues and even path-integral formulations), is that they give a means for isolating or emphasizing one or another aspect of the theory—they help bring a particular aspect into plain view, even if all the representations are logically equivalent. In our case, we want to bring into plain view (and try to make compelling) the idea that quantum mechanics is an *addition* to Bayesian probability, not a generalization of it. With that goal in mind, the SIC representation has always struck me as particularly powerful tool. With it, one can see the Born Rule as "really" a *function* of a usage of the Law of Total Probability in another context (one different than the actual). That feature, as far as I can tell, does not leap out in the same way from the more general "deformed probability representations" you explore in your papers with Joseph. That in a nutshell is the reason for my love affair with SICs.

The SICs just emphasize and make clear the point that I want made. At the end of the day—after all our foundational worries are overcome—they can be thrown away as mere scaffolding.

Does that clarify anything for you? Did I miss something that you think is a more important point than I've addressed?

I think I lay out the whole program particularly clearly in these two talks: `http://pirsa.org/09080018/` and `http://pirsa.org/09090087/`. Probably much better than anything I could write to you. I'd be very flattered if you'd watch them, and even give me feedback (good and bad, though keep in mind the weight watcher's thing ... don't make fun of me). [Sometimes Firefox doesn't work so well with these files; if you have any trouble—like not having the pictures of my transparencies, etc—change to Internet Explorer.] Do they help clarify the worldview (that I partially ascribe to you, as you'll see)?

23-10-09 *QBism House Draft, 2* (to N. Waxman)

Waxmania 2: *Attached is an version of QBism House, with my proposed edits. These aim to focus the piece more on the ideas of QBism House, while still paying homage to the work of physical recreation involved. I also moved the Durant quote to become a stand-alone "sidebar" item. One other question I have is "authorship"—I'd think it quite logical to adjust the 3rd person references to first, and have you attributed as the author of the piece, unless you're uncomfortable with this in any way. Just let me know your preference.*

My first reaction is that it doesn't sound like me anymore. (First sentences and first paragraphs are always very powerful things for me; when I see changes there I get spooked right away.) When do you really need the final draft? I'd rather give you a reasoned reply, rather than an emotional reaction, and it'll probably take me a few hours to get my head in the right place. But I'll work by your deadlines.

24-10-09 *Dimension as Capacity* (to D. M. Appleby)

How are you feeling today? I hope better, and I hope you've taken my advice to relax some.

I was feeling sentimental this morning, so I had a look back at the introduction and concluding section of this paper: `http://arxiv.org/abs/quant-ph/0404122` where I first start thinking out loud of dimension as a kind of capacity. I also cryptically hint at a connection between dimensionality and gravitational concerns. As long as you're relaxing, you might join me in on these wild musings if you wish.

Get well!

Marcus's Reply

I am certainly feeling better than I did yesterday, perhaps because I did take your advice to relax. Though it had got to the point where I didn't have much choice: I couldn't have worked even if I had wanted to. But this afternoon I decided I felt sufficiently reinvigorated to come into PI to see if I could do some work. Which is when I looked at my email and found your two notes.

Let me say first of all that what you said about Hilbert space dimension and the holographic principle really struck a chord in me. I have the strong sense that there is something there. But what exactly? How to take it forward? —Here I struggle. What follows is just a few incoherent thoughts. I am going to ramble, in other words. Because rambling is all I can do.

I like your story about the guy who asked "what is energy?" It is a very good question. It is curious, however, that most people would instinctively agree with your assessment that this is an easier question than "what is Hilbert space dimension?" People think they know what energy is. At least I assume they do. It is, at any rate, an observable sociological fact that whole conferences will spend days agonizing over Hilbert space. But I have never heard of anyone agonizing about the meaning of energy.

I wonder why that is? It is a priori evident that Hilbert space is a much more obscure concept than energy?

Here I am reminded of my experience as a teacher. How do you explain what energy is to someone who has never met the concept before? —I used to find it very hard. What I usually used to do was to start off with light bulbs. Light bulbs are labeled with their wattage, so most kids have already met the idea of that a 100W bulb produces more light than a 60W one. I used to get them to calculate the relative costs of the different light bulbs: how many pence per hour. Then I would go on to computers, televisions, cookers etc, and the relative costs of those (amazing how few people realize that an electric cooker is vastly more expensive to run than a light bulb). Then I would introduce electric motors, and from that it would be a natural progression to something non-electrical such as car engines. And from that I would introduce them to kinetic energy. Then I would move on to cranes, and introduce potential energy. Then perhaps I would talk about bows and arrows, and medieval siege engines so as to introduce elastic potential energy (the medieval allusion being desirable because I don't want the concept to be tied in their minds to modern high-tech shiny boxes). Then maybe I would talk about Rumford's experiments with boring gun barrels. And so on. In short, I would adopt what in Jamesian language might be called a shrunken head approach. That is, I would introduce them to a richly variegated mass of diverse phenomena, all articulated by this mysterious number, the energy.

I say "mysterious". But is it mysterious? What makes it seem mysterious?

I suppose because one can't give a simple one sentence definition: energy is *this*. End of story. But then one can't give a simple one sentence definition of length. Is length mysterious? Well, actually, yes. It is to me. But clearly it doesn't seem mysterious (the sociological evidence suggests that it doesn't seem mysterious) to the average quantum foundationalist. There isn't a discipline called "length foundations". Or a discipline called "energy foundations". (Perhaps there should be?)

Like I told you, this is an unashamed ramble. I am just putting down thoughts as they occur to me, in a directionless way, without any idea of trying to build a coherent argument.

I suppose energy seems mysterious (not to the average quantum foundationalist, but to the crazed guy in the elevator at the University of Texas, or to me faced with the problem of giving an articulate, intellectually respectable account of it to a class of 16 year olds) because actually the only way I know of introducing it to someone who hasn't met the concept before is by using the kind of shrunken head approach I outlined above.

You said that I was an empirical mathematician: that I had a shrunken head attitude to mathematics. And there is some truth to that. But I am a recent convert. I used to be an out-and-out rationalist in such matters. I used to hate number theory, just because it is all shrunken heads, and no unifying rational theme. My conversion happened as a direct consequence of my work on the SIC problem, which forced me to think about number theory. On the other hand I have been teaching physics for 30 years, and for almost all of that time I was forced to say things which went clean against my rationalist conscience. In particular I was forced to teach energy in a shrunken head spirit, not because I liked it, but because there is no other way of doing it. At least no other way of doing it that I could discover.

I used to particularly dislike the way I found myself forced to start with electrical energy. I felt that logically one ought to begin with the simplest case: projectile motion and the like. Talking about light bulbs raises the question, where the energy is coming from. So logically one shouldn't talk to a physics class about light bulbs until one has introduced them to generators, and currents and voltages and ohm's law and so on and so on. I used to feel it was positively immoral to treat a light bulb as an unexplained primitive. But I had to do it because it works: the kids understand if you start off with light bulbs because that connects more directly with the things they already know.

I suppose what I am trying to say is that my rationalist instincts were pulling me in a reductionist direction: building the complex up out of the simple. But we are organic beings, and we do not learn reductively. Instead we learn by a process of assimilation. Learning is like eating: a process similar to the way a tree sucks nutrients up from its roots, and works them into its already existing structure.

Anyway, as I say, I used to have a really bad conscience about the way I taught energy. So after I had given them my shrunken heads spiel I would ask the class: "So what have I told you about energy?" Can you now say what energy is? —And, of course, they never could. For the very good reason that I couldn't myself. It is exactly because it is impossible to say what energy is that I was forced to teach it in the way I did.

It didn't usually bother them I hadn't told them what energy is. It was *me*—my rationalist conscience—that was bothered. Not them.

Kids are funny, in the questions they ask and the questions they don't. They would often complain that I hadn't explained why two bodies attract with a force proportional to the product of the two masses and inversely proportional to the square of their distance apart. And if I responded with the rhetorical question, whether the biology teacher had explained any of the innumerable number of individual facts of which biology consists, they would think I was cheating. I suppose they take it for granted that it is the job of a physics teacher to explain things, but not the job of a biology teacher.

Perhaps it is the influence of all those popular science programs on TV? All this nonsense about theories of everything has penetrated the consciousness of the average 16 year old, to such an extent that they come to the subject expecting the teacher to explain literally *everything*, with no bald assertions of empirical fact at *all*?

If so it is interesting that they don't seem to have picked up the reductivist disease. Because, it is of course the reductivist bacillus in me that makes me (I use the present tense here because I don't I think I am completely free of the disease even now) unhappy with a shrunken head approach to the concept of energy. But then now I come to think of it reductionism isn't the kind of thing that usually is stressed in popular science programs. I wonder why that is? —Perhaps it is just that claims to know the mind of God are sexy, in a way that reductionism isn't.

Anyway, the kids as they come into the educational system, with only the prejudices they have absorbed from the popular culture, do not, in my experience, usually have a bias towards reductionism. In fact I am not sure they even have the concept of reductionism. And for that reason a shrunken head approach to energy doesn't bother them.

Of course it doesn't bother them. Because when you come right down to it I don't believe an explanation can ever be anything but a shrunken head type of explanation. If Einstein had succeeded in his program he would have been able to say something along the lines of "there just is this object, the stress-energy tensor, and "energy" just is the name we give to the 0-0 component of it". And it is something like that the person with reductionist intellectual tendencies (such as I used to have, and to an extent still do have) always has in mind as the model of how a fundamental physical concept *ought* to be defined. But they are deluding themselves of course, and this I think should be obvious to anyone who has had the job of teaching fundamental physical concepts from the ground up (the word "should" needs to be stressed here: because I was doing that job for many many years before this obvious point actually did occur to me). If you went into a class of 16 year olds and gave them something along the lines of Einstein's ideal definition you would be met with blank stares. They wouldn't have a clue what you were talking about. The statement would be meaningless. And I really do mean *literally* meaningless. It would make no impact at all. It wouldn't even arouse a sense of puzzlement. It would be like presenting a musical score to someone who didn't know how to read music—who didn't even have the concept that it is possible to read music.

Einstein's ideal definition makes sense to someone like the mature Einstein. But it wouldn't make any kind of sense to the immature Einstein. Come to that it probably wouldn't make sense even to the mature Einstein, before Grossman had taught him tensor calculus.

There is only one way for the abstract statements of theoretical physics to acquire meaning, and that is by a long-drawn out process of assimilation, by which the abstract concept acquires numerous connections with the pre-existing body of knowledge and understanding. In short: shrunken heads are unavoidable. People who think otherwise (the majority of our colleagues, I guess) are deluding themselves.

(I say "pre-existing" body of knowledge and understanding because I think the tabula rasa idea of the 18th century empiricists is nonsense. A child grows from a fertilized egg cell. Not from nothing. And similarly with a child's understanding. And, what is more, learning really is a process of *growth*. Not imprinting. The process by which a human being learns is of an observably different nature from the process by which one programs a computer).

Anyway, what has all this to do with the holographic principle and Hilbert space dimension? Possibly nothing. Like I say I feel confused and all I aimed to do was to spout forth, and see what my fingers typed. But now that they have finished typing maybe I can see a connection.

For one thing I suddenly felt much more interested in your emphasis on Hilbert space dimension when you made a connection with entropy (I know you have been doing that for years, but this was the first time I really took it in). It is like the kids and the light bulbs: here is a connection with something I already know.

For another thing. There was a suggestion to my mind that you are introducing length alongside Hilbert space dimension as a kind of conceptual primitive (perhaps I have got you wrong?). Which is fine. I don't mind conceptual primitives at all. The only thing is that, as soon as I think of length, I find myself thinking of time, and as soon as I think of length and time together, I think of differentiable 4 manifolds and there I am, in two blinks of an eye, with the whole block universe nightmare. So length is OK. But I want something to hang on to, to keep myself from sliding down all the way down to the bottom of that horrible slope. So I guess I have had at the back of my mind the last few days the thought that, although it is OK to talk about length (of course it is OK), we have somehow got to find a different twist on it. A new angle, which doesn't descend into differentiable 4 manifolds. And that has kind of got me to asking: how do we *define* length? What is length? —which, of course, is similar to the question "what is energy?" So you can regard the foregoing as a meditation on the question: what sort of definition should we be looking for here? How do we set about defining a fundamental physical concept?

And, finally, for a third thing. I think one of my worries about (to quote from your note to Greg Comer) "just the single, lonely number—the dimension" has always been precisely the fact that it is lonely. I think what I would like is something like the shrunken head definition of energy. A richly textured, multi-faceted phenomenological account.

Maybe. Or maybe not.

I don't actually agree with James that it is *either* rationalism *or* shrunken heads. Can't one contrive to have both?

24-10-09 *Dimension as Capacity, 2* (to D. M. Appleby)

Applebyism 37: *I don't actually agree with James that it is either rationalism or shrunken heads. Can't one contrive to have both?*

I think he does contrive to have both. A simple piece of evidence is in the longer quote I had attached for Lucien. I think you said you hadn't read it. Here are a couple of small excerpts:

> A man's philosophic attitude is determined by the balance in him of these two cravings [rationalism and empiricism]. No system of philosophy can hope to be universally accepted among men which grossly violates either need, or entirely subordinates the one to the other.
>
> Each additional particularity makes its distinct appeal. A single explanation of a fact only explains it from a single point of view. The entire fact is not accounted for until each and all of its characters have been classed with their likes elsewhere. To apply this now to the case of the universe, we see that the explanation of the world by molecular movements explains it only so far as it actually is such movements. To invoke the "Unknowable" explains only so much as is unknowable, "Thought" only so much as is thought, "God" only so much as is God.

In summary: There is nothing wrong with invoking some rationalism. But it explains only so much as is rational.

I'm glad you're starting to feel better. I was actually starting to get a bit worried since I hadn't heard from you in so long.

24-10-09 *Detached Observer . . .* (to H. C. von Baeyer)

I'm not sure what you're asking. Can you make your question more specific?

The first part of the Pauli quote refers to what the philosopher Thomas Nagel famously called "the view from nowhere". Google his book of the same title.

With regard to the second part, it's funny but I found a bit of similarity between it and a quote I read in a James biography I just started reading. (It seems every two years or so, I feel the need to read a new biography of him.) The quote was this: "Individuality is founded in feeling and the recesses of feeling, the darker, blinder strata of character, are the only places in the world in which we catch real fact in the making, and directly perceive how events happen, and how work is actually done."

Does he explain why he finds the NO-view-from-nowhere view "more satisfactory"? That would be interesting to see.

I'm happy to hear you've been dragged back to the subject!!

Hans's Preply

Dear Chris, your persistent encouragement has had the effect of dragging me back to the subject. The price you have to pay is that occasionally I will ask for advice. Here is a snippet from a letter to Bohm — almost the last words before Pauli broke off the correspondence because he thought Bohm was a "fool" (his term in a letter to Fierz):

> Since Descartes it was the ideal of natural philosophy to conceive a system of laws in which an entirely loose and untied observer is looking from outside at a part of the world completely determined by these laws. For me, however, it is much more satisfactory if the laws of nature themselves exclude in principle the possibility even to conceive the disturbances in the observers (sic) own body and own brain connected with his own observations.

This was written in English, so if it's difficult to understand the fault lies with Pauli's command of the language, not with the translator.

Without going into the whole background of this statement, what do you make of it?

Hans's Reply

> Bohm says in the P.S. to his letter of 20 Nov. 1951:
>
> > Finally I shall say that even though we cannot at present correct for the disturbance due to the measuring apparatus, we can conceive of how the human being disturbs the apparatus in precise terms. It is true that such a conception presupposes that the person who conceives of this is left outside the system. ... This means that as in classical physics one can conceive of the part of the world under consideration as existing in a state that is substantially independent of what is going on in the mind of the man who is conceiving of it.
>
> To which Pauli replies on 3 December as I wrote you previously.
>
> I understand Bohm's claim, and Pauli's first sentence. But in his second sentence, why does he focus on a disturbance *in the observer*? Why doesn't he say that the laws of nature should exclude conceiving of the state of the system without including the effect of the observation on *the system* as well as *on the observer*?

25-10-09 *Metaphysical Club* (to D. M. Appleby)

Applebyism 38: *Is it true to say that only one of those guys (Peirce) had any appreciation of physics? It seems clear to me that neither James nor Holmes did. And I think that is significant. If you don't know physics then you are missing something important.*

I think that's probably true. James's real thing was biology/physiology, before psychology. James's father though had much wanted William to go into the sciences. And he at least seemed to have some amount of respect for physics: He once went to England to meet with Faraday, and he was taught by Joseph Henry in Princeton. (You probably don't recall, but John Wheeler was the "Joseph Henry Professor of Physics" at Princeton.)

25-10-09 *Another Factoid* (to D. M. Appleby)

James met with Mach in Prague for four hours of conversation in 1882. He also went one of Mach's lectures on gravity. He wrote to his wife, "I don't think anyone ever gave me so strong an impression of pure intellectual genius. ... He apparently has read everything and thought about everything, and has an absolute simplicity of manner and winningness of smile when his face lights up that are charming."

Sometime later that year or the next, James went to a lecture of Helmholtz on gravity and wrote that he heard Helmholtz, "give the most idiotic lecture I ever listened to."

25-10-09 *More Factoids* (to D. M. Appleby)

Of course, James was in no way a physicist like Peirce truly was. But more factoids from Richardson's biography of James:

p. 51,

> On the opening page of a notebook he started this fall of 1862, William copied down comments that show his new interest in the nature of force. William Grove's *Correlation of Forces* maintains ... William had also been reading Michael Faraday's *Experimental Researches in Chemistry and Physics*. ... [In his notebook] he quoted Faraday: "For my own part, many considerations urge my mind towards the idea of a cause of gravity which is not resident in the particles of matter merely, but conjointly in them and in all space." James, from now on, had a marked interest in physics, especially in questions of energy and force, an interest he encapsulated some time later as "matter is motion, motion is force, force is will."

p. 165,

> But it was science, especially new science and its procedures and assumptions, that now commanded his deepest, steadiest attention. He maintained his interest in Darwin and Darwinism; in 1875 he was reading in modern physics as well. ... In May 1875 James read and reviewed a book called *The Unseen Universe* by physicist and mathematician Peter Guthrie Tait ... and physicist and meteoriologist Balfour Stewart [a book on Maxwell, ether, etc].

[**NOTE:** The spelling of "Qubism" in the title of this note differs markedly from my standard QBism. I am ashamed of this (what surely was a) mistake, but I leave it for historical accuracy.]

Sorry for my tantrum the other day. Scientists with ambitions of writing are like little kids.

Attached is a modified version of my story. I used some of your suggestions, but in a way that made me feel like the whole thing was still coming from my voice. Overall, I was shooting for a kind of artistic effect, rather than journalistic effect. I hope that helps explain some of my extraneous phrases, like "all told," etc. Still, I was able to trim to my satisfaction, and I think to yours. Your version from late last week had 860 words (after all the instructions, etc., were stripped from it). The present version has 858, under the same conditions.

I think I would like to keep the article in third person, but signatureless. For some reason it feels more right to me that way.

Picture coming soon.

Thanks for putting up with me!

QBism House Opens for Business

Ideas, like children, need homes where they can be loved, nurtured, and raised to independence. PI researcher Chris Fuchs brought an idea to Waterloo that he feels finally has a proper home. The home is called QBism House, and the idea, if it can be put in a few words, is that a consistent foundation for quantum mechanics reveals our universe as no universe at all, but a *pluriverse* instead. It is the sort of world the philosopher William James described as a "republican banquet ... where all the qualities of being respect one another's personal sacredness, yet sit at the common table of space and time," a world of "partially independent powers [where] each detail must come and be actually given, before, in any special sense, it can be said to be determined at all."

Take an 1886 home with 10 and 12 foot ceilings, the willpower of Kiki Fuchs to restore it to its historical grandeur, a good carpenter to build a library of solid-oak bookshelves to house nearly 1,000 books on this subject, a covered porch and a chalkboard to discuss these matters at length in the warm summer air, and one has the base for some fantastic, new physics. The house is called QBism House in honor of its "quantum-Bayesian" heritage—a point of view about quantum mechanics developed by Fuchs, along with Carlton Caves, Rüdiger Schack, Marcus Appleby, and Howard Barnum.

In the QBist view, a quantum state is not a real thing, like a rock or a tree or a quark, but "a means of orientation in life, of enabling and facilitating action, of taking account of reality and dominating it" (Tilgher). Without doubt, this is a soft-edged idea, but it is one with a sharply defined mathematical core, and the latter is QBism's research program.

Yet sharp cores and soft edges complement each other, much like the sharp lines of Perimeter Institute and the curlicued Victorian corbels of QBism House. Making the home young again, while preserving its memories and wisdom, has been a labor of love for Kiki Fuchs. The walls had to be opened to replace rusting pipes and early-century electrical wiring; chimneys had to be reconstructed from scratch; floors refinished and the kitchen modernized; the dull, crumbling paint on the exterior refreshed to full-of-life Victorian colors—the work went on for more than two years. The home that emerged is now a Waterloo landmark.

Bolstered by a grant of $512,000 from the United States Office of Naval Research, Fuchs is building a group to plow ahead on the technical aspects of the QBist project, particularly, developing a probabilistic representation of quantum states through so-called symmetric informationally complete (SIC) quantum measurements. At the moment, the group is busy exploring whether these structures exist in all finite-dimensional Hilbert spaces, what the geometry of quantum-state space looks like in such terms, how quantum mechanics itself might be derived from the supposition of SICs, and characterizing the convex theories (generalizations of quantum mechanics) this representation most naturally fits into.

All told, the QBies (yes, the QBies) consist of PI visiting researcher Marcus Appleby, associate postdoctoral fellow Åsa Ericsson, University of Waterloo PhD students Hoan Dang and Gelo Tabia, and MSc student Matthew Graydon. The students' desks can be found in room 415 at PI: the QBicle, of course. This Fall and Winter, the group will meet once a week at PI, Fridays 1:00–3:00. But once the weather is warm, the meeting will be on the porch of QBism House, where the ghosts of pragmatist philosophers past (William James, John Dewey, and F. C. S. Schiller) can listen in with satisfaction about how their ideas of an unfinished, malleable world take a more exact form with the help of modern quantum mechanics.

Anyone at PI is welcome to join the meetings, research, and discussion; contact Chris Fuchs at cfuchs@perimeterinstitute.ca. The QBies and QBism House are open for business!

Boxed Sidebar: The Optimism of Pluralism and Indeterminacy

Will Durant, from his masterful *The Story of Philosophy* (1926):

> The value of a [pluriverse] as compared with a universe, lies in this, that where there are cross-currents and warring forces our own strength and will may count and help decide the issue; it is a world where nothing is irrevocably settled, and all action matters. A monistic world is for us a dead world; in such a universe we carry out, willy-nilly, the parts assigned to us by an omnipotent deity or a primeval nebula; and not all our tears can wipe out one word of the eternal script. In a finished universe individuality is a delusion; "in reality," the monist assures us, we are all bits of one mosaic substance. But in an unfinished world we can write some lines of the parts we play, and our choices mould in some measure the future in which we have to live. In such a world we can be free; it is a world of chance, and not of fate; everything is "not quite"; and what we are or do may alter everything.

Potential Boxed Sidebar: A Pretty Equation

$$q(D_j) = (d+1)\sum_i p(H_i)p(D_j|H_i) - 1$$

This equation is something of a logo for the QBists: It represents the Born Rule when written in SIC terms, rather than in standard quantum-state/measurement-operator terms. $q(D_j)$ is the usual quantum probability for the outcomes of a von Neumann measurement; $p(H_i)$ is the probability for the outcomes of a standardized SIC measurement; $p(D_j|H_i)$ are the conditional probabilities of one measurement outcome given the other; d represents the Hilbert-space dimension of the system in question. To Bayesian eyes, this form is particularly beautiful as it shows the Born Rule to be a subtle variation (with major consequences!) of the old Law of Total Probability.

27-10-09 *Partially Sipe Inspired* (to J. E. Sipe)

Sipesm 12: *Thanks for this! Very interesting ....* [See 16-10-09 note "The More and the Modest" to L. Hardy.]

To perhaps start a discussion on the points you raise here: I have not read anything by Cartwright yet, but how do I distinguish between the kind of "capacity" you are arguing for and the kind of Aristotelian idea of "potentia" that Heisenberg advocated in his later years, and saw embodied in the wave function. Are you, by chance, sleep-walking towards Copenhagen? ☺

Maybe I shouldn't have made the aside in that note to Lucien about Cartwright. Her name apparently carries lots of baggage (she seems to be the first thing everyone with a little philosophical background, like Healey, Timpson, etc., picks up on). Just think of it along the lines of gravitational mass as you normally understand it, as I tried to give an image of in the note.

28-10-09 *Gauge Freedom as Room for Art* (to R. Healey)

Sorry, my mind wasn't completely engaged in our group meeting yesterday: I couldn't seem to tear my mind away from an inequality I wanted to derive. But I did want to make a remark right at the end of the session: As usual I was too late. So here it is just to get it off my chest.

It struck me that the discussion of whether the "equivalence class" was the real thing or not was off the mark with respect to actual physical practice (i.e., actual physicists' practice). When *trying* very hard to solve one problem or another—i.e., not just pontificating on the nature of reality or the character of the world, but actually using the theory to do something—we are often very happy for gauge and coordinate freedoms. These freedoms give the practicing physicist some room for art—they help him solve problems that he might not have been able to solve otherwise. In other words, a theory, among perhaps other things, gives us a catalog of tools for our disposal—tools for actually doing something with the theory. Thinking of it that way, then, for instance, the spacetime manifold shouldn't be thought of as a topological space upon which, *if we wish* we can draw some coordinate charts, but rather is the other way around: It is more literally the collection of charts we might avail ourselves of when trying to work out some problem.

This thought has its origin in the opening lines of an abstract I wrote for a talk some time ago. I'll record part of it here for completeness.

> As physicists, we have become accustomed to the idea that a theory's content is always most transparent when written in coordinate-free language. But sometimes the choice of a good coordinate system is very useful for settling deep conceptual issues. Think of how Eddington-Finkelstein coordinates settled the longstanding question of whether the event horizon of a Schwarzschild black hole corresponds to a real spacetime singularity or not. Similarly we believe for an information-oriented or Bayesian approach to quantum foundations: That one good coordinate system may (eventually!) be worth more than a hundred blue-in-the-face arguments.

John Stachel makes a similar point (about the manifold issue) in one of his historical articles on Einstein, but I don't have it with me at home to quote from it. If anyone wants to see it, I have it in my office.

30-10-09 *Abelard Etc.* (to J. R. Brown)

Abelard doesn't exist anymore, just like Atticus. We went to BMV near the university, and one other one nearby, and then to Steven Temple Books on Queen. The latter guy said he was the last used-book store left on Queen. This is really a shame.

Nonetheless I managed to find 13 books that resonate somewhat with where I want to take things in physics. Attached is a strange little piece on my house, my research group, and my ambitions for the world, written for the PI newsletter. In it you'll find that Durant quote I put up at the very end of the meeting:

> The value of a [pluriverse], as compared with a universe, lies in this, that where there are cross-currents and warring forces our own strength and will may count and

> help decide the issue; it is a world where nothing is irrevocably settled, and all action matters. A monistic world is for us a dead world; in such a universe we carry out, willy-nilly, the parts assigned to us by an omnipotent deity or a primeval nebula; and not all our tears can wipe out one word of the eternal script. In a finished universe individuality is a delusion; "in reality," the monist assures us, we are all bits of one mosaic substance. But in an unfinished world we can write some lines of the parts we play, and our choices mould in some measure the future in which we have to live. In such a world we can be free; it is a world of chance, and not of fate; everything is "not quite"; and what we are or do may alter everything.

Thanks for the radio programs; I'll listen. And thanks for the opportunity for good interaction yesterday.

30-10-09 *My Interiority Complex* (to H. Barnum)

Here's that quote of William James on matter that really moved me:

> To anyone who has ever looked on the face of a dead child or parent the mere fact that matter *could* have taken for a time that precious form, ought to make matter sacred ever after. It makes no difference what the principle of life may be, material or immaterial, matter at any rate co-operates, lends itself to all life's purposes. That beloved incarnation was among matter's possibilities.

Attached are three further quotes by the man that connect to our conversation last night: The theme is that indeterminism is a manifestation of (some kind of notion of) interiority. I say further things on this in connection to QM in Section 8 of `http://arxiv.org/abs/0906.2187`.

Thanks; I had fun last night.

Howard's Reply

> Thanks. That is indeed a powerful quote. It captures something of what I think about the mind-ishness, or perhaps spirit, of matter. I'm not sure, that it is in itself opposed to some kind of compatibilism, or ephiphenomenalism, about mind. In my more happy-with-compatibilism-and-epiphenomenalism/reductionism moods, I used to say to myself that "I *am* that assemblage of matter, so even if it is the evolution of that matter according to deterministic laws that is my doing this, or experiencing that, it is still *me* doing it". I guess I still half-believe that. But still, on that picture, I find it hard to understand why there should be something it is like to be that assemblage of matter. And I think that there is probably something deeply wrong about confusing the picture drawn by the equations, with the thing. The deeper question, though, is whether consciousness and will — interiority — are essentially connected with that gap between the picture drawn by the equations and the thing. I'd like to think it is so, indeed I think it probably is, and that that has something to do with it.
>
> I used to also think that quantum indeterminism, or other probabilistic indeterminism involving probabilities prescribed by a scientific theory, could not help with e.g. the problem of free will, because a free choice is not supposed to be random. It's not an idealized computer-science coin toss. Although the idea that it's more like an idealized classical-physics coin toss—determined, but from the point of view of the theory that involves the probabilities, we just don't know what determines it—is interesting!

I now understand your quantum Bayesian point of view in its present incarnation as perhaps taking something like the point of view that last sentence suggests—that *from the outside point of view* it looks like probabilities, but *from the inside point of view* it looks like choice. That seems quite explicit in what you said last night about "every little thing having some ability to make choices" (apologies if the quote is garbled).

A long time ago, I used to worry that you rejected potentially realist interpretations of the quantum state just on the basis that you wanted room for free will. The idea that quantum probabilities were making room for free will seemed to me a bit dubious because of the view that choices shouldn't be random (see above), but I now think I may have been misguided about that. I also thought that the viability, even if unpleasant, of the relative-state interpretation made it clear that QM wasn't *forcing* us to make room for free will. Now, as you know, I'm more inclined to think relative states is not viable, and some of the above discussion makes me more willing to think that QM might be telling us that the limits to our attempts to take the Nagelian "outside" view are not just manifest in our sense that this is somehow incompatible with the fact that there is—*really is* an inside view—but are manifest in the scientific theory that actually results from our attempts to take this outside view: that theory itself has to make room for interiority. I would really like that to be right. A more fully developed version of this might end up involving a "sewing together of all the perspectives" of the sort I hoped for in my long SHPMP paper. But there I probably wasn't thinking of such a deep connection to metaphysical questions as your view of perspectivity in QM. I just wanted some sense of an overall picture that includes all the perspectives in a consistent way, but in which the "outside" perspective is just one of all these perspectives somehow coexisting, not a possibly nonsensical perspective similar to the ones of beings within the world but "from outside the world". I am not sure you are as concerned with the kind of overall consistency I would still sort of like, though.

31-10-09 *Dennerlein, Sacred Matter, Capacities* (to M. Schlosshauer)

I ran across someone playing the Hammond B3 in my (still largely unexplored) jazz mp3 collection – Barbara Dennerlein – and I thought of you. What I'm hearing sounds like a very different style than you, but it was enough to set the mental wheels in motion on this thoughtful, Fall-leaved morning. I've been up since 7:00 AM, reading on (yet) another biography of James—I seem to do a new one about every two years, following near upon a birthday. This one is Richardson's. Not remarkable so far, but it is helping to remind me of some old things and, here and there, introducing me to a turn of phrase that I might incorporate into my collection. For instance, here's a quote of James that I missed upon my (now ancient) reading of *Pragmatism*, but that very much strikes me now:

> To anyone who has ever looked on the face of a dead child or parent the mere fact that matter *could* have taken for a time that precious form, ought to make matter sacred ever after. It makes no difference what the principle of life may be, material or immaterial, matter at any rate co-operates, lends itself to all life's purposes. That beloved incarnation was among matter's possibilities.

I hope you are doing well and absorbing all the insights into life that only Copenhagen can give.

Below (with an accompanying attachment) is a recent further amble down the QBist path. [See 16-10-09 note to Lucien Hardy titled, "The More and the Modest."]

Just saying hello.

31-10-09 *Thinking of Our Friend . . .* (to R. W. Spekkens)

. . . as I read these words in Richardson's book this morning:

> Every university has around it somewhere one person who is more loved and listened to than most, who tutors and prepares his friends for their exams but who never takes his own, who is generally acknowledged as the most brilliant and gifted of all, who has every gift except the ability to use his gifts. He is the one who never quite finishes, never quite succeeds, never quite writes the great book — a person like Sherlock Holmes's older brother Mycroft (who solves Sherlock's thorniest problems by sheer force of mind without stirring from his chair), or like a member of Melville's choice circle of the Divine Inert who stands wholly, tragically superior to the working world and the arts the working world requires for success and fame. In the American Cambridge of the 1860s and 1870s this figure, the village Socrates who lived in the minds of his great students was Chauncey Wright.

02-11-09 *Furthermore* (to C. Ferrie)

Ferrie-ism 6: *Sciencewise, I've been thinking about a lot of things trying to find a thesis topic. Over the past few days I've been thinking about this quote from Jeffreys (from "Epistemology in Modern Physics", 1941):*

> But in accounts of quantum theory we are presented on the first page with at least $\sqrt{-1}$, and usually with matrices or numbers not satisfying the commutative rules of multiplication. These cannot be necessary. They may be convenient, but if so it should be shown that rules satisfied by the probabilities are such as to make them convenient.

I certainly agree with this. And as you know, I contend that the SICs will help us best in seeing why $\mathrm{GL}(d,\mathbb{C})$ gives the simplest form for the calculus.

Do you have that paper in electronic form? Can I get a copy from you? (I couldn't find it on Google Scholar.)

I have not thought about the Markov stuff before. So, sorry I can't give any guidance here.

By the way, see the flyer I wrote for this month's PI Newsletter. You're welcome to come any of these meetings you wish.

02-11-09 *One Good Rant Deserves Another* (to M. Schlosshauer)

Thanks for the further Hammond pointers. It'll be fun looking them up a little later in the week.

Schlosshauerism 9: *At a recent workshop in Utrecht I heard a talk by Carl Hoefer, who you may know? He quite vigorously defended a form of objective Bayesianism, which seemed motivated by his view that subjective probabilities are insufficient. Here is how he put it in a recent essay:*

> Now, I have nothing to say against the idealization of human agents as Bayesian subjective probability-bearing entities; I am sure it is useful for many purposes, and reasonably faithful to reality in a few. But the ontic weaknesses of subjective probabilities, I would say, make them an unsuitable bedrock on which to try to mount a program that aims to capture *all* probabilities. . . . The exquisite precise and reliable probabilities found

> in casinos and in physics laboratories deserve to be accounted for in *some* way or the other.

After reading de Finetti and some other texts, I became quite convinced that the subjective viewpoint is the only reasonable one to opt for. I wonder what your general take would be on a program like Hoefer's?

You asked some good questions. "How you would try to respond to those rationalist, hard-nosed scientists?" In the coming months, I plan to work on this, honing out a satisfactory response. One thing I intend to do for practice is read some things by (and about) Nancy Cartwright, just to get straight where we are the same, and where we differ, and study the shape of the tomatoes that have been thrown at her. The Carl Hoefer response is easier; I'd just need some time to compose it properly. The key to the story is this thing Rüdiger wrote me after reading Logue's book *Projective Probability*:

> Logue makes this interesting point. Often coherence is regarded as a minimalist constraint on probabilities. But coherence has incredibly far-reaching consequences. Coherence alone guarantees that two agents with exchangeable priors that are nonzero everywhere will converge to a joint belief. Coherence alone leads an agent to certainty that he is calibrated in the long run. I like this. I like the idea that Dawid's calibration result is a strength of subjectivism, not a problem for it.

The key point is that if one is coherent with as many of one's beliefs as one can, that leads to all kinds of constraints on one's isolated assignments. So many constraints as to start misleading one into thinking the end results must be objective. But as I say it'd take me some time to do this job right.

Finally, for that awaited rant. In the week before the PIAF meeting, I tried to do a really honest job of reading many of Travis Norsen's papers. Spekkens, you see, thinks Norsen is the clearest thinker since Socrates, and I thought it was my duty to try to understand this clarity ... so that I might find the small chink in the armor, if there be one. I didn't realize what I was in for: The end result wasn't enlightenment, just depression. A really very serious depression.

Check out `http://pirsa.org/09090098/`, the panel discussion that Norsen and I both participated in. One of the questions was, what would it take for you two to switch sides. I laid out a set of criteria for what would turn me Bohmian, and I was honest. When it came to Norsen's turn, he said, "Nothing!" Not surprising at all now that I know him.

03-11-09 *Noncolorable, Maximally Consistent Sets* (to A. Cabello)

I almost forgot to send you electronic versions of the papers we discussed yesterday. The two main ones on the `arXiv` are: `http://arxiv.org/abs/0910.2750` (this one I gave you) and `http://arxiv.org/abs/0906.2187` (this is the one with the most meat and motivation in it, for this whole way of looking at quantum mechanics).

Also, I gave you a paper that I have not yet posted. I will attach it to the present note.

Thanks for spending a little time with me yesterday. There are insights my QBist program needs that I think you are nearly the only person on earth to be consulted on! I look forward to any insights you might have. Part of the question is what is the best question to make progress on!

(Attached also, by the way, is a little blurb written for the PI newsletter. It reveals in a little more poetic way, where I think "QBism" is heading at the "metaphysical" level. See particularly the side bar near the end.)

04-11-09 *QK, QB, QR – First Read* (to H. Barnum)

I gave your "quantum knowledge" draft a *first* read through yesterday. (I'll be reading it again before saying anything too substantial.) At the moment, let me just quickly comment (in my usual time-saving way) on two points in the draft.

With regard to the first point, I'm not quite sure which piece of your text I'd like to pluck out for display with regard to what I'm responding to, but maybe this one will do well enough:

> I worry that they are underselling the extent to which we can have both all the objectivity anyone could reasonably want for at least some quantum state ascriptions—a degree of objectivity which, moreover, it seems to me would be unreasonable not to grant in some cases—while also not reifying the quantum state as an object out in the world, and especially, not as an object localized at the system whose state it is.

Please go to `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf` and find the 08-11-06 note to David Mermin titled "November 8th" on page 655 (page numbering at the bottom of each page). With regard to overall pdf page counting, that is page 683 in the file. The point put there is a mundane one: *Classical* thermodynamical heat tables might be described in your terms as "having all the objectivity anyone could reasonably want" (publishing houses, for instance, found it profitable to publish them because of their seemingly universal use), but in what way did physics *not* benefit by Jaynes's constant reminder that all these numbers have their origin in *subjective* probability assignments? These same tables would be of no use and no universality for a Maxwellian demon for instance. And I say it is good to recognize that.

The danger, it seems to me, is not in overemphasizing the subjectivity of probability assignments (and then backing off in those cases when agents, for whatever reason, have similar-enough priors as to potentially come into agreement later or even to be in agreement presently), but rather in *forgetting* their subjective origin. The usual physicist's knee-jerk reaction is to do just that, to reify something that shouldn't be … to the distraction of all the conundrums that mistake leads them to. I view the great lesson of Ed Jaynes' work to be this: Remind always of the principle (that probabilities are subjective), rather than of the convenience (that in many cases agents agree and then sloppily get in the habit of thinking a probability to be of the same logical category as a fact).

Read the piece to Mermin: It is better.

Now on to this one:

> I agree that *if one does* give a quantum description of the measuring device, the operation it performs will be dependent upon one's assignment of a quantum state to it—one's beliefs about it. But who says a quantum analysis of the apparatus is needed? From a point of view from which what we take to be quantum depends upon our purposes and the experiments we plan to do or other projects we plan to undertake, to suddenly require that we do a quantum analysis of the apparatus seems to put us in danger of becoming unwitting victims of the cult of the larger Hilbert space.

Please see page 694 in the file (page 722 by raw pdf accounting), picking up at the line starting with, "The logic goes like this." Please read that paragraph along with the referee's comments above it that I am addressing. It is true that Carl often seems to skirt quite close to being an "unwitting victim of the cult of the larger Hilbert space," but I do not.

That circuit argument is Carl's favorite one, but it is not mine. Mine is simply this: With respect to a fiducial informationally complete measurement, *any* other measurement can be represented as a collection of conditional probability distributions. Think, for instance, of my beloved urgleichung. If all probability assignments are subjective (in the sense of being functions solely of the agent rather than the object), then so are these conditional ones, and then so is the POVM associated with a measuring device.

I'll certainly read your draft again in a day or two (and I'll certainly have more to say).

04-11-09 *QK, QB, QR – First Read, 2* (to H. Barnum, D. M. Appleby, and M. Tait)

Rereading this note, and also some others of my old writings, let me also recommend the note "Prepare Yourself" on page 711 (page 739 by raw pdf accounting). It further expands on the subjectivity-because-of-input-state-in-quantum-circuit account of the subjectivity of POVM ascription, and it also says some things on Mermin's "disembodied-fact account of quantum states" which seems to have some similarity to what Howard is striving for.

06-11-09 *The Interiority of John Preskill* (to H. Barnum)

I had forgotten that I had tried to make a statement on the interiority business in my John Preskill story. "If he knew what I believed, he'd probably do something to surprise me." And, "In a phrase, what is desired is a theory that is all outside and no inside. [Footnote: On first pass, the Texan in me wanted to write 'all hat and no cattle,' but I repressed that.]" Have a look at the Preskill section, pages 26–28, of the attached:

> When we have discussed the interpretational issues of quantum theory, I have gotten the sense that what John finds most suspicious of the quantum Bayesian approach I promote is that he *thinks* it treats observers as unphysical systems. I say, "Not at all; John, you are a physical system to me." The real issue is one of inside versus outside. Contrary to the textbook exposition of quantum mechanics, a wave function that I write down about him is descriptive not of the outside, but of the inside, namely me: It captures what I believe will happen to me if I interact with him. If he knew what I believed, he'd probably do something to surprise me.

06-11-09 *Renouvier on Kant* (to M. Tait)

See entry 291 (pages 102–106) of "The Activating Observer" document I sent you. See also

`http://en.wikipedia.org/wiki/Charles_Bernard_Renouvier`.

And finally the attached files if you're interested. [See Shadworth H. Hodgson's "M. Renouvier's Philosophy–Logic," *Mind* **6**(21), 31–61 (1881) and "M. Renouvier's Philosophy–Psychology," *Mind* **6**(22), 173–211 (1881).] The one on Logic is particularly on Kantian things.

06-11-09 *Quotable Schopenhauer* (to M. Tait)

It was this passage of James from an 1870 diary entry that struck me as sounding a bit Schopenhauer-esque when you brought it to my attention:

> Hitherto, when I have felt like taking a free initiative, like daring to act originally, without carefully waiting for contemplation of the external world to determine all for me, suicide seemed the most manly form to put my daring into; now, I will go a step

further with my will, not only act with it, but believe as well; believe in my individual reality and creative power.

Morgan's Preply, "Quotable Schopenhauer"

Anticipating James, though (perhaps) with a less cynical moral:

> The discovery of truth is prevented more effectively, not by the false appearance things present and which mislead into error, not directly by weakness of the reasoning powers, but by preconceived opinion, by prejudice.

Take heart QBists:

> All truth passes through three stages. First, it is ridiculed. Second, it is violently opposed. Third, it is accepted as being self-evident.

On Hegel:

> ... a commonplace, inane, loathsome, repulsive and ignorant charlatan, who with unparalleled effrontery compiled a system of crazy nonsense that was trumpeted abroad as immortal wisdom by his mercenary followers ...

On Kant's early reception:

> Great minds are related to the brief span of time during which they live as great buildings are to a little square in which they stand: you cannot see them in all their magnitude because you are standing too close to them.

On Suicide:

> They tell us that suicide is the greatest piece of cowardice ... that suicide is wrong; when it is quite obvious that there is nothing in the world to which every man has a more unassailable title than to his own life and person.

09-11-09 *Vienna Indeterminism* (to A. Zeilinger)

Thank you for the very nice letter—nice in so many aspects. A week for a visit to Vienna is definitely doable; a month sounds very intriguing. What is the most beautiful period of Vienna's springtime? But maybe more practically, when would be the best time for the meeting you want to organize? Or more practical still, if you decide you have an interest in doing a SIC experiment, perhaps I should fly over earlier rather than later to talk about these things more deeply. (I could always come back for a longer visit later ... ?)

I'll write you more about the SICs in a moment, in a separate note.

Your mention of Laurikainen reminds me that I once read a note you and someone else wrote on his passing away. Do I have that right? I'm pretty sure it was you. If so, would it be possible for you send me a copy? I did a search for the article on the web, but could not find anything. Hans Christian von Baeyer and I are writing an article for *American Journal of Physics* on Pauli's notion of the detached observer (more particularly its opposite), and it'd be nice to have all possible resource materials available. (I think you know Hans Christian, but you may not have known that Markus Fierz's brother was Hans's godfather, and that Fierz himself was Hans's father's best friend in their college days.)

With regard to Exner, that's not a quote by him. It is from Cormac McCarthy, a novelist that Carl Caves likes. I only recently learned that Exner was my academic great-great-great-great grandfather, but I had known about his argument for indeterminism for a very long time. I learned of it from Paul Hanle's article "Indeterminacy before Heisenberg: The Case of Franz Exner and Erwin Schrödinger" (Historical Studies in the Physical Sciences, vol **10**, 225–269, 1979). I tried to get a copy of that from the web to send to you, but I couldn't. Still, I did find some newer tidbits on him that I had not read before, by an author named Stöltzner. I'll attach them for you—they look quite interesting.

09-11-09 *Seeking SICs* (to A. Zeilinger)

Let me now address this part of your letter:

Zeilingerism 1: *These SIC-POVMs: where can I learn more about them? Why do you think they are among the deepest structures of quantum mechanics? You probably mean deepest in a conceptual/foundational way.*

I make my most comprehensive statement in this paper "Quantum-Bayesian Coherence": `http://arxiv.org/abs/0906.2187`. Here's the way to read it. First, jump to page 50 to see the color figure, and read the figure caption. That sets the agenda. Then, jump to page 20 and look at that figure and caption. That is a schematic of the experiment I would like to see done: It could be billed as an experimental test of the Born Rule. The idea would be: First, to perform a set of runs with only one or another regular quantum measurement performed on the systems. But second, perform a set of runs with a SIC measurement first, before the regular quantum measurement at the end. Then, one just shows that the equation at the bottom of page 20 is satisfied to within experimental tolerance.

Where I think the experiment would be most interesting and dramatic would be for $d = 3$ and $d = 4$ (single qutrits and two qubits). Particularly, for $d = 3$, one can make easy comparisons to Gleason's theorem, and also my students would have the best chance of providing some visual aids to thinking (graphs and slices will more effectively depict lower-dimensional spaces). Equation 24 on page 15 shows an exact expression for a $d = 3$ SIC—performing that measurement would be the difficult part of the experiment.

Two things to beware of in the paper:

1. The introduction is going to be completely rewritten. The referee thinks it gives Feynman too much credit, that Feynman wasn't anywhere near the key idea of this paper, and that I undersell myself and the result by diverting so much attention upon him.

2. Section 6 has a very interesting, but isolated, mistake in it. I doubt you'll read that section, but just in case, I thought I should warn you. I say "interesting" because in fixing the mistake, one finds something really deep: an extra handle within quantum mechanics that I believe quantifies how much the observer is "attached" or "undetached" in this theory. I will soon fix the mistake and repost the paper, but in the meantime let me offer another paper where the mistake is fixed; it is attached. I also attach an image to show how the equation becomes modified with this new handle—the classical world is the case $q = 0$, the quantum world is the case $q = 2$. The case $q = 1$ is something else still, something between classical and quantum. A value $q = 3$ or above, means an even more "engaged" observer—quantum mechanics is nowhere near the top of the hierarchy! If you're interested, see a more philosophical explanation below that I had originally sent to Hans Christian.

So there, that's the way to learn more about SICs; the papers I pointed you to are the best I can do for saying why I think they're deep without talking to you directly.

10-11-09 *QF Group Meeting Tuesday at 4pm in the Alice Room* (to R. W. Spekkens)

Spekkensism 51: *Our next group meeting will be tomorrow in Alice at 4pm. The topic of discussion will be: what conceptual ingredients of quantum theory are responsible for its superiority for computation (to the extent that we believe that it is superior). There will be no official discussion leaders. Rather, those who have an opinion should come prepared to say a few words about it.*

Metaphors, it's always metaphors with me. My weakness, and hopefully (in longer term) my strength. I'm not sure I will be in the office this afternoon to be able to come to the meeting: Emma is at home with an ear infection and ongoing fever. But below is my take on the matter. See old note to Gottesman, pasted in below. [See note to D. Gottesman titled "Bristol, 3 AM," dated 25 January 2008.] It is a metaphor only, of course, but one that I feel captures the essence. QM—for the QBist—seems to be a calculus for calculating probabilities for *factual* situations from *counterfactual* situations. And what is most interesting is that the quantum rule generically gives tighter distributions than the corresponding classical rule would. "It gives a reward of more certainty for this or that when an intermediate step (in the intermediate reasoning) is not taken physically."

Take factoring. The purpose of building a computer is so that, after an appropriate number of steps, one is fairly certain that if one looks at it, one will find the unknown factors. One cannot gamble (better than a flat distribution) what the factors will be initially, but one *can* gamble with near certainty that one will obtain them by having a look at the computer's output at the appropriate time. That's the point of writing an algorithm. But in this way of putting things, factoring becomes more obviously a probabilistic question. Analyze the primitive steps, and then make a probability calculation: Have an actual look when the probability for finding what you want rises to nearly one. Now transfer to the metaphor of my beloved urgleichung. First analyze conceptually in classical probabilistic terms—using conditioning, the law of total probability, etc.—but at the end of the day, use the magical quantum modification to that classical reasoning. Generically, it will give more *certainty* for some aspect of something. And more certainty—so the hope goes—should signify the need for fewer "computational steps" in between.

Not without connection to this metaphor is Andrew Steane's (less metaphorical) paper, "A Quantum Computer Only Needs One Universe," `quant-ph/0003084`. I definitely recommend discussion of it, if someone has sufficient knowledge of it.

10-11-09 *Broken Ends* (to H. Barnum)

Coming back to a Toronto conversation: Heidegger was Husserl's student, as I had faintly remembered. I just read it in wiki as I was trying to get a feel for what "phenomenology" is. Still don't know the latter, but I was pleased to run across the former.

10-11-09 *Hippy Shack* (to D. B. L. Baker)

Bakerism 5: *Somehow, only you could turn what would be a nerdfest into something that could possibly be cool. Since the name "Quantum Cowboy" is already taken, I guess you qualify as a Hilbert Space Hippy. Perhaps it is you, not I, that needs a beret.*

Did I ever recommend Louis Menand's book, *The Metaphysical Club: A Story of Ideas in America*, to you? Look it up at `amazon.com`. A Pulitzer prizewinner. It's got lots of good stuff about Emerson, the Civil War, the 54th Massachusetts Regiment (Shaw's), Oliver Wendell Holmes, all intertwined with stuff on Darwin, my hero William James, and the philosophical vision I'm shooting for. It was the very best book I read in 2003.

Hilbert Space Hippy is a way cool moniker; I'll wear it proudly!

11-11-09 *The Peirce Quote* (to R. Healey)

> If you look into a textbook of chemistry for a definition of *lithium*, you may be told that it is that element whose atomic weight is 7 very nearly. But if the author has a more logical mind he will tell you that if you search among minerals that are vitreous, translucent, grey or white, very hard, brittle, and insoluble, for one which imparts a crimson tinge to an unluminous flame, this mineral being triturated with lime or witherite rats-bane, and then fused, can be partly dissolved in muriatic acid; and if this solution be evaporated, and the residue be extracted with sulphuric acid, and duly purified, it can be converted by ordinary methods into a chloride, which being obtained in the solid state, fused, and electrolyzed with half a dozen powerful cells, will yield a globule of a pinkish silvery metal that will float on gasolene; and the material of *that* is a specimen of lithium. The peculiarity of this definition—or rather this precept that is more serviceable than a definition—is that it tells you what the word lithium denotes by prescribing what you are to *do* in order to gain a perceptual acquaintance with the object of the word (2.330).

12-11-09 *Probability is Single Case, or Nothing* (to R. Healey)

Healeyism 1: *From the abstracts, I expect to be quite sympathetic to Appleby's line. I certainly don't defend a frequentist view of probability, and agree that probability applies to the single case. But I don't think that makes all probabilities subjective, just as I don't think there are objective chances. I'm still holding out for probabilities that are authoritative for a rational agent in a specific physical situation, in the sense that his degrees of belief should match these probabilities if he has good empirical reasons to accept a theory that prescribes them.*

"Oh, where are the lines of quantum theory?" (I think I want to use that line in a play someday.)

16-11-09 *A Happy QBism House* (to N. Waxman & E. Goheen)

Thanks a million for the work on putting that together. I never imagined we'd make the front page too—I'm very flattered and grateful. And what a great coincidence that Bill Wootters's favorite equation is the same equation as mine, just written in different form! (Or did you set that up Natasha?)

Is there any chance I could get an electronic copy of the newsletter (either Word or PDF formats)? I'd like to send the issue to my "watchers" at the Navy. Also, could I get one more hard copy? I'd like to send my mother. (It's kind of like being on the cover of *The Rolling Stone*.)

18-11-09 *In the Maelstrom of Modernism* (to J. E. Sipe)

Finally finished the book. I liked it OK, but I was struck at how abruptly it ended: It really skimped on the last 8 years of James's life, the period when he was actually the hardest working and most productive in his life. And that period when James turned to full-blown philosophical efforts is, of course, the most intellectually interesting one for me personally—so I felt like I invested a lot in reading the early parts of the book only to be let down at the end. I honestly got the sense that the author had just gotten tired of writing on this project.

Still, there was plenty for me in there. For one thing I gained an appreciation for how much I need to read James's three psychology books. Particularly, I gained an appreciation for how much his ontology (of "pure experience") is based on his model of the mind. ["Pure experience," by the way, I think is an unfortunate name—he only means by that something that is neither material nor mental, but both and neither.] Also, because of Richardson, I finally feel the need to read James's *Varieties of Religious Experience*. Finally, I gained an appreciation for the influence of James's wife Alice on some of his thoughts—I hadn't realized she played such a large part in his intellectual life, and I want to learn more about that.

So, thanks for recommending the book to me. Below are the points of interest I recorded while reading it. A note to you seems like a good depository for me to record them in.

Oh, one last thing: I picked up a quote from this book that I had certainly read before (in James's *Pragmatism*), but strangely it didn't strike me then (I have no memory of reading it before). Reading it now though makes my skin tingle and come alive. It is surely the right idea:

> To anyone who has ever looked on the face of a dead child or parent the mere fact that matter *could* have taken for a time that precious form, ought to make matter sacred ever after. It makes no difference what the principle of life may be, material or immaterial, matter at any rate co-operates, lends itself to all life's purposes. That beloved incarnation was among matter's possibilities.

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19-11-09 *Psychology and Physics* (to H. C. von Baeyer)

Psychology has been much on my mind the last two weeks. The note below [see note to J. E. Sipe, dated 18-11-09, titled "In the Maelstrom of Modernism"] gives some story of the cause: It was in reading Robert Richardson's biography of William James, where a lot of attention is paid to his construction of *The Principles of Psychology*. I also started to realize from the book that there may be a more continuous thread from James to Jung than I would have expected. For instance, there are already relatively explicit statements of the collective conscious in late James (pre Jung). There are so many veins I must still explore! Our key to physics (as Pauli was already guiding) really must be in all this stuff.

20-11-09 *Chris's Picks* (to R. Healey)

Needless to say, I enjoyed yesterday's discussion from beginning to end.

And I liked one of the things you said near the end, which I think you described as a "new attitude" about reading some philosophy, to mine it for ideas, and worry about the logic later. I think that's a healthy thing. (But maybe I only say that because I'm a physicist, and physicists are opportunists.)

For fun, I was thinking this morning that I might suggest a collection of readings. (The list started because I wasn't sure you'd remember the name of the James book I suggested, but then it gathered steam on its own.)

Anyway, here goes:

- Rorty, *Philosophy and the Mirror of Nature*.

- Rorty, *Philosophical Papers Volume 1: Objectivity, Relativism, and Truth*. This is the one that starts off with this spiel that I have recorded in my computer:

> The six papers that form Part I of this volume offer an antirepresentationalist account of the relation between natural science and the rest of culture. By an antirepresentationalist account I mean one which does not view knowledge as a matter of getting reality right, but rather as a matter of acquiring habits of action for coping with reality.
>
> Philosophers in the English-speaking world seem fated to end the century discussing the same topic—realism—which they were discussing in 1900. In that year, the opposite of realism was still idealism. But by now language has replaced mind as that which, supposedly, stands over and against "reality." So discussion has shifted from whether material reality is "mind-dependent" to questions about which sorts of true statements, if any, stand in representational relations to nonlinguistic items. Discussion of realism now revolves around whether only the statements of physics can correspond to "facts of the matter" or whether those of mathematics and ethics might also. Nowadays the opposite of realism is called, simply, "antirealism."
>
> This term, however, is ambiguous. It is standardly used to mean the claim about some particular true statements, that there is no "matter of fact" which they represent. But, more recently, it has been used to mean the claim that *no* linguistic items represent *any* nonlinguistic items. In the former sense it refers to an issue within the community of representationalists—those philosophers who find it fruitful to think of mind or language as containing representations of reality. In the latter sense, it refers to antirepresentationalism—to the attempt to eschew discussion of realism by denying that the notion of "representation," or that of "fact of the matter," has any useful role in philosophy. Representationalists typically think that controversies between idealists and realists were, and controversies between skeptics and antiskeptics are, fruitful and interesting. Antirepresentationalists typically think both sets of controversies pointless. They diagnose both as the results of being held captive by a picture, a picture from which we should by now have wriggled free.

- James, *Some Problems of Philosophy*. For me a wonderful little book because it pretty well expresses what I think quantum mechanics is indicating at least for the small philosophical scale ... how far one can carry it I don't know. It is that everything has an inside—a true inside—that cannot be mathematized from the outside. This is a bit of Pauli's thought as well.

- James, *Pragmatism*. There's nothing you'd call a rigorous argument in here, but it paints a picture quite beautiful to me, one that certainly changed my life. There are all kinds of beautiful gems in here, including this quote:

> To anyone who has ever looked on the face of a dead child or parent the mere fact that matter *could* have taken for a time that precious form, ought to make matter sacred ever after. It makes no difference what the principle of life may be, material or immaterial, matter at any rate co-operates, lends itself to all life's purposes. That beloved incarnation was among matter's possibilities.

- Ralph Barton Perrry, *The Thought and Character of William James, Volume II: Philosophy and Psychology*. This book is still the best of its type around; it is the book that caused me to "want to be" William James. If you want to know what you should watch out for, so as not to be sucked into the James cult, approach this one cautiously.

- Dewey. I know I should suggest something by Dewey, but it is hard, as I just cannot stay awake while reading this guy. Still I have the impression that he is "mostly James" (in contrast to Peirce, say), but rigorized. Flipping through Rorty will tell you what of Dewey to read if you want any suggestions—Dewey is Rorty's hero. Still, let me offer this much:

- F. Thomas Burke, et al, *Dewey's Logical Theory: New Studies and Interpretations*. The introduction gives some nice orientation.

- H. S. Thayer, *Meaning and Action: A Critical History of Pragmatism*. This is a really very good reference; it covers literally everything of the pre-Rorty as far as I can see.

- And finally, if you (late in the game, after you've read some of the stuff above) really want to have a look into the wild side—I reveal my biggest dreams here—have a look at some of F. C. S. Schiller (remember, mining, mining): F. C. S. Schiller, "Axioms as Postulates". (I can make a copy and send it to you.)

20-11-09 *Wheeler on Quine* (to R. Healey)

This was the Quine quote that Wheeler liked to use when arguing that ultimately physics should shed the continuum:

> Just as the introduction of the irrational numbers … is a convenient myth [which] simplifies the laws of arithmetic … so physical objects are postulated entities which round out and simplify our account of the flux of existence. … The conceptual scheme of physical objects is [likewise] a convenient myth, simpler than the literal truth and yet containing that literal truth as a scattered part.

It comes from his paper, "On What There Is".

And by the way, I told you that the Pragmatism Cybrary classifies Quine as a "recent pragmatist"; you can find their classification here: `http://pragmatism.org/`.

21-11-09 *SICs, Reason, and Faith* (to the QBies)

Continuing on with our discussion from Wine & Cheese yesterday: Particularly, on how *belief* itself can *sometimes* be the very cause of a reality. That, without a belief first, without a *faith* that something *ought* to be,—in some cases!—it simply would not be. I'm thinking of that part of our conversation. Anyway, on these things, I was led back to William James's speech "Reason and Faith." I attach the full thing, and excerpt below the part of it that is important to our little group.

I can do nothing but admit that in coming to the conclusion that the foundation of quantum mechanics should be built on SICs, I climbed a "faith-ladder" in the sense that James defines it below:

> SICs are *fit* to be true. It would be well if they *were* true. They *might* be true; they *may* be true; they *ought* to be true. They *must* be true! They *shall* be true! For me at least!!

And so with it, faith brought a little bit of reality into being. It is the ONR funding and our little group, and all the results positive and negative we shall find over the course of the next few years.

> Faith uses a logic altogether different from Reason's logic. Reason claims certainty and finality for her conclusions. Faith is satisfied if hers seem probable and practically wise.
>
> Faith's form of argument is something like this: Considering a view of the world: "It is *fit* to be true," she feels; "it would be well if it *were* true; it *might* be true; it *may* be true; it *ought* to be true," she says; "it *must* be true," she continues; "it *shall* be true," she concludes, "*for me*; that is, I will treat it as if it *were* true so far as my advocacy and actions are concerned."
>
> Obviously this is no intellectual chain of inferences, like the *Sorites* of the logic-books. You may call it the "faith-ladder," if you like; but, whatever you call it, it is the sort of slope on which we all habitually live. In no complex matter can our conclusions be more than *probable*. We use our feelings, our good-will, in judging where the greater probability lies; and when our judgment is made, we practically turn our back on the lesser probabilities as if they were not there. Probability, as you know, is mathematically expressed by a fraction. But seldom can we *act* fractionally—half-action is no action (what is the use of only half-killing your enemy?—better not touch him at all); so for purposes of action we equate the most probable view to 1 (or certainty) and other views we treat as naught.

22-11-09 *More Reason and Faith* (to H. C. von Baeyer)

von Baeyerism 37 : *I will gladly read "Reason and Faith" in the spirit you propose, with one reservation. James, in his context, speaks about religious faith, but I would like to distinguish between the concepts of faith and religion. In my Pauli speech I referred to a book by James P. Carse entitled* The religious case against belief *(2008). My brother, a psychologist in Saskatoon, was so taken with it when I told him about it that he gave a sermon to his Unitarian church (appropriate since James was speaking to Unitarians too.) The abstract of the sermon put it succinctly:*

> Belief systems are wilfully closed and certain, and are the enemy of religions, which are so open to debate, uncertainty, and reinterpretation that it takes thousands of years for these discussions to run their course. In his 2008 book, theologian James P. Carse explains how the essence of religion is poetry or mystery or a higher form of ignorance rather than a didactic set of claims or beliefs. This book turned my understanding of religion on its head.

The relevance of all this to your thinking is further complicated by the difference between the words "faith" and "belief", which I am not prepared to discuss here. All I want to do is to make sure that religion is properly decoupled from the conversation about SICs.

Thanks for your brother's abstract; I think I'd like to take a look at this book during the Christmas holidays. I don't think you have anything to worry about with regard to James pushing 'belief systems' in the sense you mention.

Particularly, the more I thought about it today, I think he might have been served well to use the word "decision" rather than "belief." This can be seen in the last essay I sent, but it can be seen even more clearly in the attached essay. (It is from his book *Some Problems of Philosophy*, but I picked it up from a badly scanned copy on the internet; then I spent some time cleaning it up in TEX. It struck me as the sort of thing I'd like to have a good copy of in my computer.) This particular essay is less about religion, but more about the idea that the world can be changed by our actions. The faith-ladder once again makes an appearance in it.

Faith and the Right to Believe

(Appendix from William James's book *Some Problems of Philosophy*)

'Intellectualism' is the belief that our mind comes upon a world complete in itself, and has the duty of ascertaining its contents; but has no power of re-determining its character, for that is already given.

Among intellectualists two parties may be distinguished. Rationalizing intellectualists lay stress on deductive and 'dialectic' arguments, making large use of abstract concepts and pure logic (Hegel, Bradley, Taylor, Royce). Empiricist intellectualists are more 'scientific,' and think that the character of the world must be sought in our sensible experiences, and found in hypotheses based exclusively thereon (Clifford, Pearson).

Both sides insist that in our conclusions personal preferences should play no part, and that no argument from what *ought to be* to what *is*, is valid. 'Faith,' being the greeting of our whole nature to a kind of world conceived as well adapted to that nature, is forbidden, until purely intellectual *evidence* that such *is* the actual world has come in. Even if evidence should eventually prove a faith true, the truth, says Clifford, would have been 'stolen,' if assumed and acted on too soon.

Refusal to believe anything concerning which 'evidence' has not yet come in, would thus be the rule of intellectualism. Obviously it postulates certain conditions, which for aught we can see need not necessarily apply to all the dealings of our minds with the Universe to which they belong.

1. It postulates that *to escape error* is our paramount duty. Faith *may* grasp truth; but also it may not. By resisting it always, we are sure of escaping error; and if by the same act we renounce our chance at truth, that loss is the lesser evil, and should be incurred.

2. It postulates that in every respect the universe is finished in advance of our dealings with it;

That the knowledge of what it thus is, is best gained by a passively receptive mind, with no native sense of probability, or good-will towards any special result;

That 'evidence' not only needs no good-will for its reception; but is able, if patiently waited for, to neutralize ill-will;

Finally, that our beliefs and our acts based thereupon, although they are parts of the world, and although the world without them is unfinished, are yet such mere externalities as not to alter in any way the significance of the rest of the world when they are added to it.

In our dealings with many details of fact these postulates work well. Such details exist in advance of our opinion; truth concerning them is often of no pressing importance; and by believing nothing, we escape error while we wait. But even here we often cannot wait but must act, somehow; so we act on the most *probable* hypothesis, trusting that the event may prove us wise. Moreover, not to act on one belief, is often equivalent to acting as if the opposite belief were true, so inaction would not always be as 'passive' as the intellectualists assume. It is one attitude of will.

Again, Philosophy and Religion have to interpret the total character of the world, and it is by no means clear that here the intellectualist postulates obtain. It may be true all the while (even though the evidence be still imperfect) that, as Paulsen says, 'the natural order is at bottom a moral order.' It may be true that work is still doing in the world- process, and that in that work we are called to bear our share. The character of the world's results may in part depend upon our acts. Our acts may depend on our religion,—on our not-resisting our faith-tendencies, or on our sustaining them in spite of 'evidence' being incomplete. These faith-tendencies in turn are but expressions of our good-will towards certain forms of result.

Such faith-tendencies are extremely active psychological forces, constantly outstripping evidence. The following steps may be called the 'faith-ladder':

1. There is nothing absurd in a certain view of the world being true, nothing self-contradictory;
2. It *might* have been true under certain conditions;
3. It *may* be true, even now;
4. It is *fit* to be true;
5. It *ought* to be true;
6. It *must* be true;
7. It *shall* be true, at any rate true for *me*.

Obviously this is no intellectual chain of inferences, like the *sorites* of the logic-books. Yet it is a slope of good-will on which in the larger questions of life men habitually live.

Intellectualism's proclamation that our good-will, our 'will to believe,' is a pure disturber of truth, is itself an act of faith of the most arbitrary kind. It implies the will to insist on a universe of intellectualist constitution, and the willingness to stand in the way of a pluralistic universe's success, such success requiring the good-will and active faith, theoretical as well as practical, of all concerned, to make it 'come true.'

Intellectualism thus contradicts itself. It is a sufficient objection to it, that if a 'pluralistically' organized, or 'co-operative' universe or the 'melioristic' universe above, were really here, the veto of intellectualism on letting our good-will ever have any vote would debar us from ever admitting that universe to be true.

Faith thus remains as one of the inalienable birth-rights of our mind. Of course it must remain a practical, and not a dogmatic attitude. It must go with toleration of other faiths, with the search for the most probable, and with the full consciousness of responsibilities and risks.

It may be regarded as a formative factor in the universe, if we be integral parts thereof, and co-determinants, by our behavior, of what its total character may be.

How We Act on Probabilities

In most emergencies we have to act on probability, and incur the risk of error.

'Probability' and 'possibility' are terms applied to things of the conditions of whose coming we are (to some degree at least) ignorant.

If we are entirely ignorant of the conditions that make a thing come, we call it a 'bare' possibility. If we know that some of the conditions already exist, it is for us in so far forth a 'grounded' possibility. It is in that case *probable* just in proportion as the said conditions are numerous, and few hindering conditions are in sight.

When the conditions are so numerous and confused that we can hardly follow them, we treat a thing as probable in proportion to the *frequency* with which things of that *kind* occur. Such frequency being a fraction, the probability is expressed by a fraction. Thus, if one death in 10,000 is by suicide, the antecedent probability of my death being a suicide is 1-10,000th. If one house in 5000 burns down annually, the probability that my house will burn is l-5000th, etc.

Statistics show that in most kinds of thing the frequency is pretty regular. Insurance companies bank on this regularity, undertaking to pay (say) 5000 dollars to each man whose house burns, provided he and the other house-owners each pay enough to give the company that sum, plus something more for profits and expenses.

The company, hedging on the large number of cases it deals with, and working by the long run, need run no risk of loss by the single fires.

The individual householder deals with his own single case exclusively. The probability of his house burning is only 1-5000, but if that lot befall he will lose everything. He has no 'long run' to go by, if his house takes fire, and he can't hedge as the company does, by taxing his more fortunate neighbors. But in this particular kind of risk, the company helps him out. It translates his one chance in 5000 of a big loss, into a certain loss 5000 times smaller, and the bargain is a fair one on both sides. It is clearly better for the man to lose *certainly*, but *fractionally*, than to trust to his 4999 chances of no loss, and then have the improbable chance befall.

But for most of our emergencies there is no insurance company at hand, and fractional solutions are impossible. Seldom can we *act* fractionally. If the probability that a friend is waiting for you in Boston is 1-2, how should you act on that probability? By going as far as the bridge? Better stay at home! Or if the probability is 1-2 that your partner is a villain, how should you act on that probability? By treating him as a villain one day, and confiding your money and your secrets to him the next? That would be the worst of all solutions. In all such cases we must act wholly for one or the other horn of the dilemma. We must go in for the more probable alternative as if the other one did not exist, and suffer the full penalty if the event belie our faith.

Now the metaphysical and religious alternatives are largely of this kind. We have but this one life in which to take up our attitude towards them, no insurance company is there to cover us, and if we are wrong, our error, even though it be not as great as the old hell-fire theology pretended, may yet be momentous. In such questions as that of the *character* of the world, of life being moral in its essential meaning, of our playing a vital part therein, etc., it would seem as if a certain *wholeness* in our faith were necessary. To calculate the probabilities and act fractionally, and treat life one day as a farce, and another day as a very serious business, would be to make the worst possible mess of it. Inaction also often counts as action. In many issues the inertia of one member will impede the success of the whole as much as his opposition will. To refuse, *e. g.*, to testify against villainy, is practically to help it to prevail.[214]

The Pluralistic or Melioristic Universe

Finally, if the 'melioristic' universe were *really* here, it would require the active good-will of all of us, in the way of belief as well as of our other activities, to bring it to a prosperous issue.

The melioristic universe is conceived after a social analogy, as a pluralism of independent powers. It will succeed just in proportion as more of these work for its success.

[214] Cf. Wm. James: *The Will to Believe*, etc., pp. 1–31, and 90–110.

If none work, it will fail. If each does his best, it will not fail. Its destiny thus hangs on an *if*, or on a lot of *ifs*—which amounts to saying (in the technical language of logic) that, the world being as yet unfinished, its total character can be expressed only by *hypothetical* and not by *categorical* propositions.

(Empiricism, believing in possibilities, is willing to formulate its universe in hypothetical propositions. Rationalism, believing only in impossibilities and necessities, insists on the contrary on their being categorical.)

As individual members of a pluralistic universe, we must recognize that, even though we do *our* best, the other factors also will have a voice in the result. If they refuse to conspire, our good-will and labor may be thrown away. No insurance company can here cover us or save us from the risks we run in being part of such a world.

We *must* take one of four attitudes in regard to the other powers: either

1. Follow intellectualist advice: wait for evidence; and while waiting, do nothing; or
2. *Mistrust* the other powers and, sure that the universe will fail, *let* it fail; or
3. *Trust* them; and at any rate do *our* best, in spite of the *if*; or, finally,
4. *Flounder*, spending one day in one attitude, another day in another.

This 4th way is no systematic solution. The 2d way spells faith in failure. The 1st way may in practice be indistinguishable from the 2d way. The 3d way seems the only wise way.

'*If* we do *our* best, and the other powers do *their* best, the world will be perfected'—this proposition expresses no actual fact, but only the complexion of a fact thought of as eventually possible. As it stands, no conclusion can be positively deduced from it. *A conclusion would require another premise of fact, which only we can supply. The original proposition per se has no pragmatic value whatsoever, apart from its power to challenge our will to produce the premise of fact required.* Then indeed the perfected world emerges as a logical conclusion.

We can *create* the conclusion, then. We can and we may, as it were, jump with both feet off the ground into or towards a world of which we trust the other parts to meet our jump—and *only so* can the *making* of a perfected world of the pluralistic pattern ever take place. Only through our precursive trust in it can it come into being.

There is no inconsistency anywhere in this, and no 'vicious circle' unless a circle of poles holding themselves upright by leaning on one another, or a circle of dancers revolving by holding each other's hands, be 'vicious.'

The faith circle is so congruous with human nature that the only explanation of the veto that intellectualists pass upon it must be sought in the offensive character to *them* of the faiths of certain concrete persons.

Such possibilities of offense have, however, to be put up with on empiricist principles. The long run of experience may weed out the more foolish faiths. Those who held them will then have failed: but without the wiser faiths of the others the world could never be perfected.

(Compare G. Lowes Dickinson: "Religion, a Criticism and a Forecast," N. Y. 1905, Introduction; and chaps. iii, iv.)

23-11-09 *CoE Proposal* **(to H. Price)**

Sorry to keep you waiting. That's a genuinely nice document ... even if I did cringe at the thought of being associated with something that says this: "A striking example is the attempt to understand quantum 'nonlocality' (the 'spooky action at distance' in quantum theory, as Einstein famously called it). Nonlocality was characterised mathematically by John Bell in the 1960s, and later confirmed experimentally by Aspect and others." (The implication is pretty clearly that 'spooky action at a distance' is 'confirmed'. And you know of course, that that's about the last thing I think Bell inequality violations confirm.) … Still, I was seduced back by the small homage to these, I would say, closer-to-correct words, "the real breakthrough will come when we start to realise the connections between reality, knowledge and our actions." … And finally overpowered by that beautiful closing argument, "The quest to understand reality is one of humanity's fundamental projects. Each generation inherits new challenges on the shoulders of the last. In our generation, some of the deepest puzzles require the joint skills of physics and philosophy. Our goal is to lead the world in directing the best available intellectual resources to these challenges."

I'd be proud to be associated with this project. And you can list me for a 20% time commitment as you wish.

One final thing, I was amused by this line in the proposal, "What is the status of probability in Quantum Bayesianism?" I would have thought that is the one thing that is a settled issue by now! Attached is a draft I'm writing at the very moment, where effectively I say so. Rue the day your funding agents ever see my own writings about this question! They might just take that portion of the money back.

23-11-09 *CoE Proposal, 2* (to H. Price)

By the way, I like that way of writing "Quantum Bayesianism" as you do in the proposal, with both "Quantum" and "Bayesianism" capitalized. I just adopted the convention myself!

23-11-09 *My Visit, James, Lotze, and the Malleable World* (to A. Zeilinger)

Then let's settle it. I will come for the week of Jan 17–23, with possibly some extra time around the front or back edge of that range.

Funny, just this morning (because of a further grant proposal our Australian partners in the PI-AF quantum foundations collaboration are working up), I read the *New Scientist* thing on you from last month. They write,

> Zeilinger specialises in quantum experiments that demonstrate the apparent influence of observers in the shaping of reality. "Maybe the real breakthrough will come when we start to realise the connections between reality, knowledge and our actions," he says.

Nice. With that in mind, I give you a little gift: One of my favorite discussions from William James's little book *Pragmatism*. I hope you enjoy the attached short file, and I am very much looking forward to January.

23-11-09 *GRW and Bayes* (to G. J. Milburn)

Milburnism 3: *I gave a 'reading' of your paper "Subjective probability and quantum certainty" to our PIAF group on Wednesday. Some way through the discussion the question of what to make of spontaneous localisations in GRW came up. Have you, Carl or Rüdiger thought about this from a quantum bayesian point of view?*

I would guess that neither Carl nor Rüdiger nor I have thought much about GRW in these terms. In my case, not only would I guess, but I know that I haven't! It's a symptom of the thing I described in the draft I sent you and Huw, etc., a couple of hours ago. GRW is usually thought of in terms of reified states doing the collapsing, i.e. the states *are* the dynamical variable. The main point of the quantum Bayesian movement is to explicitly remove the quantum state from the observer-INdependent world, leaving no trace behind. If GRW can be posed in such a way that the thing jumping is not a quantum state, then maybe there's a chance of analyzing it in our terms. But I don't know of such a way.

I hope that helps answer things, and that our old paper didn't give you or the other guys too much of a headache.

23-11-09 *While Translating* (to J. Berkovitz)

Thanks much for this remark in your paper:

> "Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading misconception, an illusory attempt to exteriorize or materialize our true [i.e. actual] probabilistic beliefs." (de Finetti 1974a, p. x) ... Here the translation from Italian seems incorrect. The word 'vero' could be translated as 'actual' or 'true', and it is clear that here it should be translated as 'actual'.

It cleared up a good mystery for me. I had never liked the seeming use of true there. But it prompts another question for me. In the fuller quote below, should it really be an "or Fairies and Witches" or rather an 'and'. The 'or' has always struck me as out of place.

> The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time, ..., or Fairies and Witches, was an essential step along the road to scientific thinking. Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading conception, an illusory attempt to exteriorize or materialize our true probabilistic beliefs.

I haven't forgotten that I want to comment extensively on your draft. I sure hope to do it in the coming couple of weeks.

Jossi's Reply

> As for the translation, the word 'o' in Italian is usually translated as 'or' but could sometime be translated as 'and'.

24-11-09 *New Apologies: Me, Me, Me* (to N. Waxman)

Another week has passed and I still haven't put the *Physics in Canada* paper in your hand. I feel completely ashamed. On the other hand, I can honestly admit to having put an embarrassing amount of time into writing. (You'd probably not believe me if I told you.) Trouble is I've been proceeding with my two paper concept being very careful with my word choices:

> *To wit, from my previous note to you and Rob Myers*: How strict is the 2500 word-count limit going to be for the *Physics in Canada* thing? I ask because I just reached that number and still have a ways to go with my draft. I think what I am going to do,

now that I'm on a roll and really liking what I've written so far, is just plunge ahead, writing what feels right internally, and then let Natasha go at it with an ample scalpel. (I justify this in my mind by thinking, at the end I'll have two articles for the price of one. I'll find some place to send the longer one off to, even if only `arXiv.org`.)

The manuscript is, at the moment, just at 4000 words. Hopefully, if I get it done right, a significant portion of that will be simply detachable, and then we can smooth it out thereafter. But there's no denying, I've been writing the big version for me, me, me—trying to say things in a way that will have lasting value and persuasiveness.

Anyway, I felt I should write you this morning because, actually, I must take two days off this week and I feel very guilty about that: today and Thursday. Today, because I have to go to U. Toronto to work with the experimental team that's doing an experiment on one section of my *Physics in Canada* article (something called a SIC). Then Thursday because of American Thanksgiving, which is a big family tradition for us. But Wednesday and Friday, I'll give the paper my all—I promise.

To make a long grovel short: Can you—*please*— wait until Monday for the finished product???

25-11-09 *Modest Theories (with a capital M)* (to L. K. Shalm)

OK, Krister, your turn. (I'll cc Aephraim in case he's interested in these wispier things too.)

Thanks for your continued interest in this "QM as a single-user theory business"—I really do appreciate your efforts to get it straight, whether you accept OR reject in the end. The endpoint doesn't matter as much as the effort; the effort alone is flattering. (Plenty of so-called professional philosophers of science, for instance, reject the view at the outset without ever making an ounce of effort to understand what is new in it.)

First, on your particular question from yesterday. For it, please refer to a long note I constructed for Lucien Hardy after the last PIAF meeting. It can be found `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf` starting at page 813 by raw pdf accounting (with regard to LaTeX page numbering, instead, that is page 785). The note is titled, "The More and the Modest". [See 16-10-09 note "The More and the Modest" to L. Hardy.] The idea argued for there is the sense in which Quantum Theory is a very modest theory. Not modest in the sense of saying it is merely about "black boxes"—as you were describing yesterday—but *Modest* in the way of Newton's theory of universal gravitation. I say this to contrast it with Weinberg, Witten, or Hawking's dream of a "theory of everything". Newton's theory is not that, but on the other hand it is still a sweeping statement about nature. So too I think for quantum theory—that though it is a single-user theory, it is nonetheless a sweeping and universal statement about the character of the things in the world. Here's the best way I know how to put the point: What I am aiming for is that quantum theory is a theory of ONE ASPECT of everything.

Read the note to Hardy (including the supplementary material in it); it'll do a better job than what I can write here in a single paragraph.

Further, let me attach a paper that I'm writing at the very moment. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] Sorry, it's only halfway finished, but the beginning material may do a better job of motivating the single-user aspect of the program than previously. I have been trying to write it in an entertaining way; so hopefully it will draw you in.

If this stuff shakes anything loose in your thinking, please let me know. Or, if there is an aspect of it all that still strikes you of solipsism (or sophism!), please try to articulate it, and I'll give it my best shot of a thought-out reply. From every challenge I take on, I learn a little more.

30-11-09 *QELS* (to Z. E. D. Medendorp & A. M. Steinberg)

- This is Bayes' Rule or Bayes' Theorem:
  `http://en.wikipedia.org/wiki/Bayes\%27_theorem`
  See the first equation in.

- On the other hand, this is the Law of Total Probability:
  `http://en.wikipedia.org/wiki/Law_of_total_probability`
  See the third equation in.

My rewrite of the Born Rule (using SICs) is an analogue of the latter rather than the former.

30-11-09 *NOT Recommended* AMS Notices *Article* (to M. B. Ruskai)

I'm sorry to take so long to get back to you. I read the Chernoff article, and the end of it is indeed horrible: "In addition, experiments have been done which suggest that influence from one system to the other propagates enormously faster than light. These experiments point toward instantaneous transfer of information." There was just no call for any of that kind of discussion in an article about Gleason. On the other hand, is it worth a battle? This kind of crap happens all the time, everywhere you look—Nicolas Gisin is guilty of it, Alain Aspect is guilty of it: The list of otherwise good physicists just goes on and on. I try to fight the fight when I have to—for instance, see the present article under construction (and required of me from PI for a special issue)—but then sometimes I just get tired of building the background, trying to get the uninitiated to see the trouble after they've already had an untrustworthy introduction. [See "Quantum Bayesianism at the Perimeter," `arXiv:1003.5182`.]

I don't know, I partially fear taking on another project that'll probably leave me unsatisfied and more frustrated with the community in the end, at this time when I'm already far behind on all the other things I need to do.

01-12-09 *Hawaii in April?* (to H. Price)

It dawned on me that if we're going to have that meeting in April, I probably ought to get on planning it before December is out. [. . .]

As I said when I volunteered for this, I'd like to make the theme of the workshop: Contextuality, Perspectivalism, Pragmatism, or some combination/variation of that. In that regard, what do you think about inviting any non-PIAFers? I ask mostly because Richard Healey visited us for a couple months, and you should see the change in this guy (to the far-more interesting): He is now calling his research project for the next few years something like "developing a consistent pragmatism". See this fantastic talk he gave `http://pirsa.org/09110136/` at the end of his visit. Anyway, I think I'd like to invite at least him into the mix if we've got the budget for it.

Once we set some dates, I'll probably get Holly onto taking care of the hotel booking and logistics, etc. But first I want to hear back from you, then I'll start testing the waters more broadly within PIAF.

By the way, did you know you can be found on YouTube? I found you as I was looking up pragmatism talks!

07-12-09 *Eric's Note, 2* (to E. G. Cavalcanti)

Cavalcanti-ism 5: *I found this message gathering some dust in my mailbox.*

Taking your cue, I think I'm going to have to let your own note gather some dust in my mailbox. I just don't have time for a proper reply at the moment. I'll try to come back to you more properly after the New Year!

Cavalcanti-ism 6: *I am still very confused about this, even after reading your paper and listening to your talk. If this "dressing" of raw probability theory has an empirical component, doesn't it have a fundamentally distinct origin as the rest of probability theory? The extra structure that is added by quantum mechanics seems to have, in this view, a contingent nature, dependent on the specific ontology or the laws of nature of our world, and it is not a pure "law of thought" as plain-vanilla Dutch-book coherence.*

I mean, I could, in a very similar way, propose a theory of "gravitational decision theory", which claims that, on top of Dutch-book coherence, we should add a normative principle about gambling on the outcomes of experiments involving the relative movement of massive objects. But really, the best way of thinking about this, of course, is that Dutch-book coherence is all there is to probability theory, and anything added on top is just an instance of a good-old law of physics. In this case it would be a category mistake to confound the two. What is essentially different in your view that makes it not the same type of category mistake?

Ok, I will partially answer my own rhetorical question: underlying the probabilities in the example above is a deterministic, noncontextual hidden-variable theory. And now comes a non-rhetorical question: is there something about probabilities which do not allow noncontextual hidden-variables that makes your kind of position immune against the category-mistake criticism above?

On another note, another important feature of quantum probabilities is that they refer to **fundamentally** *counterfactual (or complementary, in Bohr's words) measurements, as opposed to the classical ones which could always in principle be performed simultaneously (and complementarity is a necessary—perhaps sufficient?—condition for the failure of noncontextual hidden variables. That is, in a world where all measurements can be performed at once there is always a noncontextual hidden variable model). Do you think perhaps you could say something along the lines that the extra normative assumption you propose can be justifiably called a normative rule of decision theory (as opposed to a law of nature) in a world where complementarity exists?*

In the meantime, I know there is a lot of repetition with what you have already seen, but let me recommend these two presentations:

- `http://pirsa.org/09090087/`
- `http://pirsa.org/09080018/`

I explicitly address some of your questions about the interaction between normative rules and physical law there.

Hope things are going well for you.

I certainly liked reading this line of yours:

Cavalcanti-ism 7: *And perhaps even Leibniz's monadology can accommodate something like the "republican banquet" you (and I!) have in mind.*

And finally let me give you a hint of an answer to this:

Cavalcanti-ism 8:

> [CAF wrote:] The other night Ruediger and I were semi-jokingly saying that the dimension d of a quantum object quantifies how much "will" it has. But, maybe some variant of that will not turn out to be so much of a joke after all.

That sounds interesting. Can you expand on that?

See attached excerpt from "My Struggles ..." [See 16-10-09 note "The More and the Modest" to L. Hardy.]

10-12-09 *Lawless World / Malleable World / Pluriverse* (to N. Cartwright)

I was glad to see today that Steve Weinstein could send you an invitation to our meeting on laws. (I am only a minor player in organizing this meeting.) I thought I might tell you independently how much I really hope you can come. Sad to say, I've only recently discovered your work, but that said, now that I know it, I am greatly enthusiastic about it.

I offer evidence that our thoughts on laws, etc. are in some sympathy with a few pieces of my own writing (see attached):

1) The introduction to my Cambridge U Press book *Coming of Age with Quantum Information*—you might find some of my early life (especially my distrust of "laws of physics" at the age of 12 or so) reported in the first few pages amusing. You might also enjoy some of my stories in there about some people you surely know—for instance, Abner Shimony and Jeff Bub.

2) An excerpt from my samizdat *My Struggles with the Block Universe* where I describe my extreme empiricism and how I am starting to see the Hilbert-space dimension of quantum systems as a "capacity" in a sense (I think) quite similar to your own. [See 16-10-09 note "The More and the Modest" to L. Hardy.]

3) The beginning of a paper I'm writing for *Physics in Canada* describing my "Quantum Bayesian" program for interpreting quantum mechanics. Though, I don't have the section on "Hilbert dimension as capacity" written yet, it will give you some introduction to the way I am starting to think of quantum mechanics in "normative terms," and maybe you can use your imagination on how to fill in the last sections.

Anyway, I hope these things whet your appetite a bit—i.e., to see that there is a (practicing) physicist who very much believes that there is no law that cannot be broken, that the world is malleable, and that the concept of a capacity is just what the doctor ordered for understanding quantum mechanics.

Of all our participants, I feel I could learn the most from a few well-placed conversations with you. I very much hope you can come.

14-12-09 *Invitation to Lecture* (to J. Emerson)

If I understand correctly, my lectures are March 9 and 11. Assuming that's correct, I've marked my calendar.

Title: Quantum Bayesianism at the Perimeter

Abstract: Theses lectures give a summary of the Quantum Bayesian point of view of quantum mechanics, originally developed by D. M. Appleby, H. Barnum, C. M. Caves, A. Peres, R. Schack, myself, and others. It is a view that arose from and depends crucially upon the tools of quantum information theory. Work at Perimeter Institute continues the development and is focussed on the hard technical problem of a finding a good representation of quantum mechanics purely in terms of probabilities, without amplitudes or Hilbert-space operators. The best candidate representation involves a mysterious entity called a symmetric informationally complete quantum measurement. Contemplation of it gives a way of thinking of the Born Rule as an addition to probability theory, operative when one contemplates gambling on the consequences of one's interactions with a new universal capacity: Hilbert-space dimension. (Newton gave us gravitational mass as a universal capacity; with hindsight one can say that the founders of quantum mechanics gave us another universal capacity.) The lectures end by showing that the egocentric elements in this point of view represent no impediment to pursuing quantum cosmology and outlining some directions for future work.

(I stripped both title and abstract for a *Physics in Canada* article I'm just finishing this week.)

18-12-09 *Sorters and Jokes* (to D. Bacon)

Hey, you're a man who likes a good stiff joke. Tell me whether a couple of my anti-Everett snide remarks hit the right tone in this paper I'm writing at the moment. See attached. Number 1 can be found in the last paragraph just before the section "Quantum States Do Not Exist" begins:

> The Everettian world, on the other hand, purports to have no observers, but then it has no probabilities either. What are we then to do with the Born Rule for calculating quantum probabilities? Throw it away and say it never mattered? It is true that much effort has been made by the Everettians to rederive the rule, but to many in the outside world, it looks like the success of these derivations depends upon where they are assessed: for instance, be it Oxford [8] or Cambridge [9] instead.

Number 2 is in the mid paragraph of the right column of page 3; scan for the word "lizard":

> The point is, far from being an appendage cheaply tacked on to the theory, the idea of quantum states as information has a simple unifying power that goes a significant way toward explaining why the theory has exactly the mathematical structure it does. By contrast, who could take the many-worlds idea and derive the structure of quantum theory back out of it? This would be a bit like trying to regrow a lizard from the tip of its chopped-off tail: The Everettian conception never purported to be more than a reaction to the formalism in the first place.

29-12-09 *. . . And Happy New Year* (to H. C. von Baeyer)

. . . or, better, Happy New Decade

Thank you for your nice Christmas Day note. I'm sorry to say it has taken me this long to reply to you. The Saturday before Christmas I started coming down with (I guess) a sinus infection, general congestion, deep cough, chest congestion, muscle aches, etc. At first I thought it was the flu (perhaps brought on by the flu shot I got the day before), but since I never developed fever, my guess now is that it was just probably a badly placed bacterium. Anyway, I have been very slow to get over it, and the thoughtful/philosophical/reading-filled break I was hoping to have from PI is now starting to disappear. (Don't get me wrong, the holiday with the family was still quite good and satisfying family-wise; I just haven't had the thought-filled time I had planned for.)

I had dreams of writing you and Marcus all kinds of stuff on the "neutral stuff" that I'd like to start thinking of "quantum events" (called "measurement outcomes" when one starts to think of one term as an agent) as exemplary of. It was a theme of thinking I started last year at this time and has been in the background of my thoughts since … trying, I can tell, to solidify into some kind of articulable entity. Though it is not there yet, at least a little progress has been made. If I could just put what little bit I've got onto paper, I know that would accelerate the process.

As a poor substitute, let me attach another draft in progress. It at least helps lead up to the point I want to get to in personal conversation … in ways perhaps that I have not articulated before. Even though incomplete, I hope you enjoy. [A few warnings: 1) Figure 2 is not quite right; that should be $Q(D_j)$ in the box—it's an old picture and will be redrawn. 2) Most of the exposition is stabilized up until the last two paragraphs of the "capacity" section. Thereafter, all bets are off, including the references I cite. 3) If you print this, the hand-drawn illustrations, until better cleaned up, will come out much better on a color printer.]

Maybe too, I'll leave you with an intellectual challenge if you're looking for a little diversion. I'll give it the code name, "What Did He Read and When Did He Start Saying It?" Pauli, as you know, used the terms "psychophysically neutral," "neutral language" and "neutral" itself quite often. But where did he pick the term up from? Is it a self-invention? Or did he pick it up from Jung? Or in reading William James or Bertrand Russell or still someone else? Did Pauli read Russell's book *The Analysis of Mind* (1921) where the word is splattered everywhere?

James himself has these two passages in "Essays on Radical Empiricism":

> First of all, this will be asked: "If experience has not 'conscious' existence, if it be not partly made of 'consciousness,' of what then is it made? Matter we know, and thought we know, and conscious content we know, but neutral and simple 'pure experience' is something we know not at all. Say *what* it consists of — for it must consist of something — or be willing to give it up!

and

> In 'Does Consciousness Exist?' I have tried to show that when we call an experience 'conscious,' that does not mean that it is suffused throughout with a peculiar modality of being ('psychic' being) as stained glass may be suffused with light, but rather that it stands in certain determinate relations to other portions of experience extraneous to itself. These form one peculiar 'context' for it; while, taken in another context of experiences, we class it as a fact in the physical world. This 'pen,' for example, is, in the first instance, a bald *that*, a datum, fact, phenomenon, content, or whatever other neutral or ambiguous name you may prefer to apply. I called it in that article a 'pure experience.' To get classed either as a physical pen or as some one's percept of a pen, it must assume a *function*, and that can only happen in a more complicated world.

Did Pauli, by chance, read this book? It's not a very well written article, but one easy source for you to find some other literature that Pauli might have read is the Stanford Encyclopedia article on neutral monism: `http://plato.stanford.edu/entries/neutral-monism/`.

Anyway, there are the few thoughtful thoughts I can muster this morning. For the rest of the day my alter ego must come out and work on Marcus's latest masterwork. I'm giving it my final run-through, and with some luck we'll get it posted before the week is out—it should have been posted two weeks ago, except for my dragging things on like this. I'm just trying to catch typos at this stage, but I've got nearly 30 pages to go, and I'm finding it particularly taxing in this condition. In case you need a diversion for your brain's other hemisphere, I'll attach it as well.

January 15 to 25 I'll be in Vienna giving the Zeilinger group some lectures on SICs. It would sure be wonderful if you could be in town at the same time! Now that's a place to walk and talk and get some insight on quantum mechanics!

All the best to you and Barbara, and really a happy New Year.

Hans's Reply

> I'm looking forward to reading your *Physics in Canada* piece at leisure.
>
> In the meantime, I have done a quick check of my Pauli library to find what seems to be the first reference to "neutral language." Provisionally I believe it occurs in an unpublished 1948 essay reproduced on p. 179 of *Atom and Archetype: The Pauli/Jung Letters*. Do you have it to hand? "Neutral" appears on p. 182.
>
> Interestingly Pauli goes on to answer your question (What did he read?) on the bottom of p. 188. As is often the case, he throws us a curve! Pauli does not always look to higher authority, as most of us do, and as you suggest in your list of possible inspirations. Often he looks to *lower* authority, by which I mean that he reads something with which he strongly disagrees, but which he then corrects and extends. In this case it is by an author whom he batters into the ground before picking his brain. At the end he has the priceless comment: "Regardless of any justified criticism, however, we can again ascertain from the spontaneous manifestations of this author's unconscious that…" I love the pat on his own back implied by the word "justified", and the idea that the poor guy wasn't even *conscious* of what he taught Pauli.

29-12-09 *New Year, New Decade, New Physics!* (to M. Schlosshauer)

These coincidences keep happening to us, and it is a strange thing. The very day you wrote your note below, I was literally thinking about you as I was walking home from PI. I was thinking it would be nice to send you some of the latest foundational thoughts on quantum mechanics, jazz, nonreductionism, and metaphysical pluralism, all stirred into a soup—the time seemed ripe again. But as I walked home, I started to feel worse and worse and found myself in the beginning of a holiday-long sinus/cold/cough/congestion/flu sort of thing, and I'm only starting to recover decently now. BUT that evening when I opened my email at home, I saw your greeting and, indeed, noted the new round of synchronicity!

I hope you have a great coming year, and indeed a great new decade, filled with all the same things you wished for me.

It is a sorry substitute for saying something more personal, and particularly using this time to think more deeply about the context of jazz, but let me attach something that I hope to return to writing in the next few days after I really get recovered. Most of the document is pretty well stabilized, at least up to the last two paragraphs of the "capacity" section. Thereafter, I guess all bets are off. [And warning: Figure 2 is not quite right. That should be $Q(D_j)$ in the box—it's an old picture and will be redrawn eventually.] Comments for improvement certainly welcome, though if my health keeps improving I hope to have it posted next week.

Sorry to say, Rüdiger still hasn't come through on the promised decoherence draft. I'm dropping in on him for a day Jan 25, on my way between Vienna and Texas, and I'm hoping to tighten the screw to a very uncomfortable level then.

And by the way, we need to come back to the idea of a PI visit this Spring or Summer if you'd still like to do it. If you could flag potential dates, that'd be great. At the moment, it looks like I should be able to pay all your expenses.

Anyway, once again: Happy New Year and happy New Physics!

30-12-09 *Gentle Rib* (to H. C. von Baeyer)

von Baeyerism 38: *Like all Americans you are color-blind to the distinction between ie and ei. The lady's name is GIESER as in Fierz (which you always get right).*

That's funny: I tried to be so careful about that! (I am aware of my shortcomings.) I remember even looking at the cover of the book before writing my sentence "to make sure" I got it right. So much for surety.

Anyway, look at Gieser's book, page 45. There she says: "According to Mach direct experience is *psychophysically neutral*." Now a question on my mind is whether Mach himself ever used that kind of terminology.

BTW, I often check my spelling of Fierz too.

30-12-09 *Happy New Year Again* (to H. C. von Baeyer)

von Baeyerism 39: *Provisionally I believe it occurs in an unpublished 1948 essay reproduced on p. 179 of* Atom and Archetype: The Pauli/Jung Letters. *Do you have it to hand? "Neutral" appears on p. 182.*

Interestingly Pauli goes on to answer your question (What did he read?) on the bottom of p. 188.

Thanks, I had a look at that. From Gieser's book I find that he wrote Fierz on the subject at almost the same time, 21 August 1948. See PLC III [971] on page 561. It might be good to look at that.

But even earlier than that I note that Pauli writes Jung in a 28 October 1946 letter, "It appears that Fludd's voice, which was ignored at the time, is imbued with new meaning, since for the moderns the objectifying of space had only limited validity. The *neutral language of the 'Blond'* in the dream (he did not employ such terms as 'physical' or 'psychic' but just talks of people who 'know what rotation is' and those who do not know) seems to be reanimating that intermediary layer where the infans solaris used to be. The modern unconscious speaks here of a 'radioactive nucleus'."

Somehow I suspect though that it really must go earlier than that, but I've got no evidence. If you read the Preface and first 8 pages of the Introduction to E. C. Banks' book *Ernst Mach's World Elements: A Study in Natural Philosophy* (on Google Books) you'll see his take on Mach was that he was actually espousing a neutral monism with his world elements. And Pauli would certainly, I would think, have read Mach very carefully. But see Pauli's letter to Jung dated 31 March 1953—there he definitely describes Mach's elements as psychical rather than neutral. [By the way, doesn't Banks' book look fascinating?]

I might write Gieser eventually. She seems to have noted what books precisely were in Pauli's library. It would be interesting to know exactly which James books (if any), which Russell books (if any), etc., Pauli gives evidence of having read.

On another subject, it looks like we're going to get that Appleby masterwork (now finalized) on the `arXiv` this evening! With that, my 2009 work is done and I myself can look toward the new decade!

31-12-09 *Last Posting of the Decade* (to C. H. Bennett)

I've got to say I was jealous this morning when I saw that you had the last posting of the decade. That's because a few weeks ago I was dreaming of myself trying to nab that coveted spot. But I ended up pushing myself so close to the edge of the deadline that I didn't even finish the paper! (With another paper, I did at least get position #4 of the new year.)

Anyway, with the end of the day here, and the thing still not finished, I thought I might send you the present draft of the less fortunate one. It might give you a smile (and some significant horror) to see what might have, with more diligence, been in the place of the beautiful reverse Shannon theorem. (You might also particularly enjoy Figure 3.)

I hope you and Theo and all the kids and grandkids are doing well and perched for a happy and productive new decade. Greetings to you all from Kiki, the kids, and me. [The kids know of you from the (around our house, famous) Charlie and the Christmas-Tree Cookies story.]

31-12-09 *QB at the Perimeter of Understanding* (to H. C. von Baeyer)

von Baeyerism 40: *I have spent a very pleasant last afternoon of 2009 reading your draft. It is, of course, perfect for me in my role as Sagredo to your Salviati and your critics' Simplicio. Since the decade will be over in a few hours, I do have a wish. I think your argument has, indeed, stabilized, but it should not be allowed to fossilize. I read your accounts of Bayesianism and SICs with growing understanding. Nevertheless. I still don't have a truly internalized grasp of Figure 2. Seeing it yet again does not help me. So I have the hope and wish that in the new decade you will invent a new version that looks at the same issue from a slightly different point of view. (Feynman praised the ability to recast the same problem into different formulations.) Maybe your visit to Vienna will result in a simplified version of what Anton will actually DO to verify equation 6 – and what he would expect to find in a classical world. Or maybe there is a useful analogy to the double slit experiment.*

So that's the challenge: Your words are new and fresh, your formulas are immutable, can you come up with new pictures?

I will do my best, and you will probably find me consulting with you on the issue. (Maybe I need to be like a salesman in a dress shop.)

In the meantime, let me attach the figure of the Steinberg group qutrit SIC experiment (in an abstract submitted to the QELS meeting). It doesn't depict the meaning of the old diagram, but maybe it will give you some sense of one way of to do the experiment.

I send this to you, and then I go drink my first round of bubbles for the evening!

Hans's Reply

> I trust the bubblies were uplifting!
>
> Thank you for the Steinberg paper. Maybe it shook something loose in my brain. The attempt is to make *actual* SIC measurements, and this brings them down to Earth

for me. My trouble was precisely with the point you labeled "unheard of in physical science", namely the reference to counterfactuals. These have always been vaguely mysterious to me. First you called them measurements in the Bureau of Standards. Then they moved to the sky. For me this language was perhaps a little too mystical. Now the SIC measurements have moved down to Steinberg's or Zeilinger's lab, so you might call them "Toronto measurements" or, narcissistically, "Perimeter measurements."

I guess I had clothed them in magnificent raiments, when they should have been as naked as the emperor. They simply provide a unique universal characterization of a quantum system, usurping the role of the wave function. Is the word counterfactual a little too suggestive here? I mean, when I step on my bathroom scale, I am tacitly saying that "if I were to weigh myself, counterfactually, against the kilogram in Paris, this is what I would find." This statement is then transferable to any scale in the world. (Of course that's what you had in mind in the first place.) But I never use the word counterfactual in my bathroom.

I will try to find language, such as "standard description" or "naked description" or something, that I'm more comfortable with.

I'm sorry to be so obtuse and thank you for providing me with an AHA moment to start the decade with.

03-01-10 *New Decade, Three Days In* (to M. Schlosshauer)

I've had a wonderful afternoon with the Hammond B3. My three girls went out skiing, and I built a fire and put Larry, Larry, Barbara, Max, and all the Water Babies on in the background, just letting the music loop and loop. A very relaxing and thoughtful afternoon—exactly the sort of thing I had been hoping for all vacation . . . but just as PI is about to open up again! Too bad . . . but at least I got one day of it.

Schlosshauerism 10: *Thank you for the attached paper. . . . I was happy to see that you're including a little section on the Deutschian quantum-cosmology argument in your paper. I completely agree with you that the argument is "nonsense." Deutsch's quote breathes the same spirit as those shopworn claims that it's only the Everett picture that "takes QM seriously." Here's another statement by Deutsch – you probably know it already – that seems to fall into a similar trap:*

> But in fact, there is only one known interpretation of quantum theory. . . . Perhaps one reason why the dichotomy between 'formalism' and 'interpretation' has been accepted so uncritically is that the debate has been conducted almost exclusively among theorists. Thus it has revolved around the question, 'what exactly does quantum theory imply about reality?' Putting it that way can make it seem natural to try to separate the 'scientific' (mathematical, predictive) core of the theory from its explanatory structure, and to keep the former fixed while adjusting the latter to one's philosophical prejudices. But no good can come of such an exercise. The formalism of quantum theory did not come out of nowhere. It is the solution of a scientific problem and, as always in science, the problem was not primarily what mathematical formula best predicts the outcomes of experiments. It was what mathematical structures correspond best to reality. If we alter the 'interpretation' of the theory without regard to the second question, we can conjure up virtually any world we like. But it will not be the real world. The real world is the multiverse, and it does contain many universes.

And thanks for the long note, Deutsch thought, typo, and all the rest. Most especially for the news that you and your wife will be having a baby! (You look so young to me that I had never expected you to be married ... much less nearing fatherhood.) As David Mermin (I think!) told me just before our first was born nearly eleven years ago: Nothing in your life will be harder or nothing more rewarding. Your children will shape you just as much as you shape them. And if you're like me, they will inspire sentences and paragraphs in your papers, and all kinds of natural philosophy in your mind.

I think I doomed myself by writing you last week, "I hope to have it [my draft] posted next week." That means that, of course, the draft changed nearly none at all from the first version I sent you! And there's no way it will now be posted this week, as—first thing—I've got to stop working on the extended version and get to work on the smaller extract (long overdue and must be gotten in to the editor this week). But I did read through the whole thing again, tweaking things a little when I could. One substantial point though: There was something quite misleading in the quantum de Finetti discussion. Since you indicated you might be reading those early parts, let me send you the present draft even though I still haven't completed the damned thing (and, as I say, probably will have to leave developing for much of the week): I just want to divert as much potential damage as possible in your thoughts on de Finetti.

There are a few more lines at the end of the capacity and cosmology sections, but not too much. Particularly, though, I found myself playing with Shakespeare as I was driving home from the library today, and I just had to record it. And now apparently I just have to send it to you as well! (I get tickled with myself when I feel I have made a good phrase.) Somehow I will make those lines fit properly into the paper when it's all done:

> These our actors, as I foretold you, were all spirits
> And are melted into air, into thin air ...
> We are such stuff as
> Quantum measurements are made on.

At the moment, all of July seems to be open for me. (Jan Vienna, Feb Nagoya, Mar Portland, Apr Chapel Hill NC, May nothing, Jun Växjö, Jul nothing, Aug Leipzig possibly, ...) So, let's tentatively pencil in July. If you have any weeks to suggest, let me know, and at least that will provide some points of resistance if anything tries to intrude. (We don't have to set them in stone yet, but points of resistance are good for helping promote well-meant plans to reality.)

I did very much enjoy (finally!) learning about the two Larry's today—I'm sorry I put it off for so long. That is really good stuff. Larry Young's version of Monk's Theme, for instance, really took me. Please do give me more recommendations as you think of them. Tell me about some of the Jimmys I should be looking out for.

Congratulations again on the baby. That's really nice news. What's your wife's name? Where's she from, etc.

03-01-10 *Slightly Better Rhyme* (to M. Schlosshauer)

Well, the girls came home from their day of skiing, full of stories of grand adventure. That went on for some hours as their mother and I poured more and more wine, eating cheese, laughing, wincing, and smiling at their stories, enjoying our everlasting pride in them. (When I said "three girls" earlier, by the way, I meant their mother and the two younger ones—total of three.) But then they left me all alone tending this fire that simply won't die! (I have tried to speed it along, but it is as procrastinating as I am in writing papers.) Anyway, I had nothing to do in this stuporous state but write a little more on the *Physics in Canada* draft—and you are the guinea pig. Attached is the result of a drunken fit: I rearranged a rhyme! See the Shakespearian modification!

Danger becoming one of my correspondents, it is!

04-01-10 *Birth Notes* (to M. Schlosshauer)

More later on various subjects in your note, when I need a second break! For the moment, I'll just say I do know Bill Evans very well (love him), and let me comment on this:

Schlosshauerism 11: *Thanks for your congratulatory wishes on the baby! ... The assessment of "nothing in your life will be harder or nothing more rewarding" is wonderful. What I like about the whole prenatal period is this exhilarating mixture of certainty and uncertainty. One knows that there will be a child added to your life in a mere few weeks. Yet there's so little one can figure out in terms of what the whole experience will actually be and feel like, until it all happens. (Excuse the crude and somewhat geeky analogy, but it's almost like with quantum measurement: You know there will be an outcome, but you don't know what it will be.*

It's not geeky at all. You remember my story about Christiania? Below are some more respectable variations on the moniker that story was about. [See notes: 28-07-00, "The Role of Registered Phenomena," to David Baker; 03-09-01, "Subject/Object," to Michael Nielsen; 20-09-01, "Praise, Folly, Enthusiasm," to Bill Wootters; 19-07-03, "Definitions from Britannica," to David Mermin; 18-05-04, "Agents, Interventions, and Surgical Removal," to Jim Woodward; 17-06-04, "Preamble," to Hideo Mabuchi; 27-02-09, "Vivienne and the Universe," to Lucien Hardy.] My opinions have certainly changed on some things since the earliest notes, but the common thread, that quantum measurement and birth are identical in a very deep sense, seems to have weathered the storms.

Thanks for giving me the impetus for this little stroll down memory lane. I once read a book by Ludwik Fleck, *Genesis and Development of a Scientific Fact*. It was mostly a study of how the diagnosis and treatment of syphilis developed over the years. What was so interesting to me in it was how the science that developed did so because, long before there was any science at all to the matter, and long before anyone had a right to think so, syphilis came to be viewed as a "blood condition." And this of course suggested the idea of blood tests for syphilis, and treatments that had nothing to do with the outward symptoms, etc. You're wondering why I'm telling you this. Well, in this stroll down the memory lane of birth, creation, and measurement, I started thinking how I had thought I was writing something fresh in this new paper of mine! Calling Hilbert space dimension a capacity, etc., etc. *But no*, I was calling it a capacity already in 2004, long before I had a right to think so, and particularly long, long before Eq. (9) in the present paper was even dreamed of!

No lesson here yet, but it is a curiosity to me.

05-01-10 *Three Attachments ... No, Four* (to M. Schlosshauer)

Thanks for your musings on the psychology of quantum foundations. It is a vexing issue, and one that I come to in my own head from time to time. Appleby and I also discuss it quite often, usually in frustration over something Rob Spekkens, or Harvey Brown, or Jeremy Butterfield, or Matthew Donaldson, has said—it seems those four are very often the foci of our conversations on these matters. They are our friendly foils. Appleby gets particularly riveted—you should see him in action—going on about how they desire a dead universe or how they can't possibly *really* believe what they say. He claims that if one *really* believed in a deterministic world (despite one's going about life in a normal way), it would be tantamount to insanity; so he thinks deep down they're all speaking lies.

Sometimes, I tend to think the rationalists derive a kind of religious comfort from determinism. It takes all the responsibility off their shoulders at some very deep level.

I think you hit the nail on the head when you talk about the influence of the PhD advisors—certainly in the vast majority of the cases. Of course, I feel immune on this issue myself, as I tried to make clear in my "How to Stuff a Wild Samizdat," but I do really think it true for the vast majority of the cases. There was one Bohmian postdoc we had here at PI (actually you saw him speak at a PIAF meeting in Sydney when I first met you) who very clearly could have fallen into any line of physics if his PhD advisor had just been a different one. And when he was having trouble finding his next position, I told Kiki several times (she was concerned because she knew the wife) that, "If he had just had a better luck of the draw field-wise ..." Anyway, I definitely worry about this with my own students: I almost wish they would disagree with me more and try to articulate reasons for holding their ground. It worries me that they sometimes seem to accept QBism a little too easily.

Schlosshauerism 12: *This desire might be so ingrained in us, not only intuitively but also in terms of the general scientific mindset, that anything that departs from pursuing a deterministic program is akin to giving up on of the very ideals of science itself. (I bet you've heard people say that about your program!)*

Indeed I have! Many times over. Attached is a report to Asher Peres of the first time I can recall of it—it's from `Notes on a Paulian Idea`. It was the first time David Albert beat me up (though it wasn't the worst licking he ever gave me). If you look at the "disclaimers" page in NPI, you'll find these words:

> Various deletions of text have been made to the original letters. The purpose of the vast majority of these is to spare the reader of the "merely personal" in my life. A smaller fraction are for the sake of protecting the innocent, protecting the correspondents, and protecting the illusion that I am good-natured.

Well, I intentionally left one personal insult in that document (and one by accident). The intentional one was about Albert. (I'll tell you about his worst thrashing of me the next time we see each other—it's a story I don't feel like writing at the moment.) [...] And, yes, I rearranged the Shakespearian thing again—this time I finally got it right, and will make it a piece of the ending of the paper.

Schlosshauerism 13: *Basically, the upshot is this. Genomics tries to make us believe that life is basically nothing but DNA.*

And funny you bring that up; I used DNA among my examples today in the "nothing but" section of the paper. In the PiC draft, do a search on the word "Carbon"—you might enjoy some of things in my list that DNA has characteristics in common with.

Schlosshauerism 14: *How about yourself? I know — from your Samizdats — that your wife's (nick-?) name is Kiki and that you have two daughters. But that's about it.*

Yep, Kiki is a nickname. When she was a baby, her older brother couldn't pronounce Kristen, and it stuck. The happy story of how I met her is attached (written in a larger context of a rather sad story). [See 16-02-02 note titled "Some Things Should Not Pass."] Her present career is refurbishing this rotten old house, but she was an elementary-school teacher in the past (until Emma, aka 11 years ago). Final attachment(!) is a story on the interplay/intersection of our two careers.

OK, I should stop boring you! (And certainly don't read any of that crap if you don't feel like it. It's just my long habit of regurgitating stories—sometimes I can't stop. I'll not be bothered at all if you've got better things to do.)

Max's Preply, "Randomness, Temperament, Genomics"

Thanks so much for these very thoughtful and thought-provoking musings about birth. They're very inspirational, in the best sense of the word. (I.e., they're the proverbial opposite of the "inspirational" associated with the tacky tele-evangelist. Not that I'd ever confuse you with a member of that creed.)

Many thoughts came to my mind when I was reading these notes of yours. Two of them really stuck out. They're both, in a way, personal contemplations about why other people hold certain opinions, or, in particular, arrive at particular conceptions of the universe (or quantum mechanics, for that matter). I hope you don't mind if I just throw them at you. They probably amount to hardly anything new.

The first one is – and it's something that's occupied me for a while – the issue of determinism. During the era of classical physics, determinism may have seemed like an inevitable consequence of what science was telling us. And people – philosophers, scientists, and lay persons – tried to come to terms with this development. They did so either by trying to find an escape route from the confines of the block universe, or by accepting determinism while still trying to seek out a niche where our intuition about an open future that's in continual creation could continue to exist in some way.

But then quantum mechanics came along. And suddenly we had the strongest indicator (not proof, of course) of some form of intrinsic ("irreducible") randomness – indeterminism – that one could possibly dream up. (Really, could you imagine a *better* way? So simple, so abstract, so mind-boggling, and so far-reaching?)

And what seem many, if not most, people working in foundations these days engaged in? To me, it seems to be programs geared toward restoring determinism, may it be in form of Bohmian trajectories, a global Everettian "quantum universe," or whatever. I'm not inclined to judge these approaches, I simply and plainly find them puzzling. And so I wonder (a lot) why so many people pursue such programs.

I have no good answer. Right now, I can mainly think of one reason, namely, that the ideal of science itself all along has all been one of determinism. Not just one of causality, but one of linking the occurrence of event A to the definite consequence of event B. This desire might be so ingrained in us, not only intuitively but also in terms of the general scientific mindset, that anything that departs from pursuing a deterministic program is akin to giving up on of the very ideals of science itself. (I bet you've heard people say that about your program!) I know I'm probably not saying or observing anything new here; I'm just trying to make some sense of the sociology of foundational scholarship.

The other thought is actually something I was contemplating again earlier today, and your note struck the same chord. (Another one of those coincidences, it seems.)

More precisely, it was your quote of James (a quote that I highlighted, *and* later marked in the margins, when I was reading James' book):

> The history of philosophy is to a great extent that of a certain clash of human temperaments. Undignified as such a treatment may seem to some of my colleagues, I shall have to take account of this clash and explain a good many of the divergencies of philosophies by it. Of whatever temperament a professional philosopher is, he tries, when philosophizing, to sink the fact of his temperament. Temperament is no conventionally recognized reason, so he urges impersonal reasons only for his conclusions. Yet his temperament really gives him a stronger bias than any of his more strictly objective premises. It loads the evidence for him one way or the other, making a more sentimental or more hard-hearted view of the universe, just as this fact or that principle would. He *trusts* his temperament. Wanting a universe that suits it, he believes in any representation of the universe that does suit it. He feels men of opposite temper to be out of key with the world's character, and in his heart considers them incompetent and 'not in it,' in the philosophic business, even though they may far excel him in dialectical ability.
>
> Yet in the forum he can make no claim, on the bare ground of his temperament, to superior discernment or authority. There arises thus a certain insincerity in our philosophic discussions: the potentest of all our premises is never mentioned. I am sure it would contribute to clearness if in these lectures we should break this rule and mention it, and I accordingly feel free to do so.

If you look back at the set of questions I sent you for the interview book that I'm putting together, there was one question that asked precisely about this issue of predisposition, of temperament, of philosophical prejudice, with respect to one's choice of foundational program. This influence may start very plainly, very simply, at the level of one's PhD supervisor. As you may have observed yourself, there are many people in foundations who work on a particular interpretation that "happens" to also be the favorite interpretation of their adviser. But of course it goes much deeper than that. A Bohmian may not be a Bohmian simply because he likes so much the form of the guiding equation and the double-slit imagery of trajectories, but also because it fits a more deeply rooted fundamental affinity toward a classical particle-based world view. Again, I'm not stating anything new here, but I think that some of the emotional debates in foundations would have some chance of leveling off to some extent if everyone just clearly stated their philosophical convictions and these convictions' influence on their (i.e., the people's) attitudes.

This is a very long-winded way of saying that I'm very fond of the birth imagery in quantum mechanics. If I see a lifeboat that has promise of staying afloat, and may actually take me to a new island, why hold on to the old sinking ship of determinism?

OK, I think I'm entering the "rambling" territory, so I better stop before this ship is drifting even more off course.

PS. Against all better intuition, here's another off-topic addition to the theme of our conversation. Last night I finished reading a little book on Darwin, genomics, and the "book of life." (The book is in German, but it's not really relevant anyway.) Basically, the upshot is this. Genomics tries to make us believe that life is basically nothing but DNA. What we are, what makes us human, is encoded in this sprawling sequence of

ACGTs. Figure out the sequence, and what proteins it codes, and you've got it all. The author of the book argues, quite persuasively I thought (mostly by appealing to recent scientific evidence, but also via more philosophical insights), that this is a gross oversimplification, and that we ought to be ever-cautious of such simplifications. In a very vague sense, this reminded me of the debate in quantum mechanics: how we try to read the formalism as reality, as describing everything there is, thus taking reductionism to its extreme. (Witness the Deutsch quote I sent you.) In this terribly vague and surely oversimplified way, quantum states strike me as akin to the ACGT of molecular biology. Both are *our* ways of dealing with the world, of deciphering it and coming to terms with it and trying to manipulate it. But they are not the world (or the human, or life) itself.

05-01-10 *That Thoughts Matter* (to M. Schlosshauer)

I worked hard to find something in Marcus's email corpus that would instantiate/corroborate what I told you of his reactions to determinism-seeking psychologies. I don't feel I've been all that successful (I'm sure there's something better out there), but I give up for the time being. Anyway, attached are two things: 1) A draft of an article Hans Christian von Baeyer was writing at the time, and 2) Marcus's reaction to it. It's the latter that I wanted to exhibit to you, but I included the former for completeness.

Marcus's writing is in PauliNotes.doc. For entertainment, you might pick about at page 6 where he starts talking about believing positivism as having the potential to lead to suicide. And then again at the bottom of page 8, where he asks "why people find it so hard to accept that thoughts actually matter." Oh, here's a choice one: Look on page 10 where he writes, "Materialists affect to believe—believe themselves to believe—that we are nothing more than complicated machines." Thereafter, there's some interesting stuff up to page 13 at least. Actually, there's further good stuff throughout. Just food for thought.

Now, maybe, I go to bed!!

05-01-10 *Importance of Humanism and Dogs* (to M. Schlosshauer)

Wow! Maybe even more than for the music, let me thank you for this! Particularly for the parts between the 1st and 2nd diamonds:

Schlosshauerism 15 : *I was very pleased to see that you gave an explicit run-down of the Wigner's-friend scenario (page 6). I think most readers will agree that the worry over an assignment of different quantum states by Wigner and his friend is dissolved in the personalist framework.*

Still, your argument may not—at least not at this point of your paper—dissolve all the concerns people get tangled up in when thinking about this thought experiment. What they might continue to worry about may not have so much to do with quantum-state assignments but with "what is really out there." On the one hand, Wigner's friend is believed to have seen one outcome. On the other hand, Wigner could, in principle, perform an experiment demonstrating interference effects between different outcomes. So the casual reader will ask: "How can it be that the friend has observed one outcome, but Wigner can do an experiment that would seem to require that no single outcome has occurred?" And furthermore, "Is this mystery, after all, reducible to the subject of quantum-state assignments? Or do I need to go further and embrace a different worldview, like the one you outline in the 'Capacity' section? Or do these two questions come down to the same thing?" You do point to this new mindset already on page 6:

> *The point of view here is that a quantum measurement is nothing other than a well-placed kick upon a physical system—a kick that leads to unpredictable consequences for the very agent who made it.*

So things should be clear, worries dissolved, even for the casual reader, even at this point of the paper—but maybe they're not, and I wonder if the argument on page 6 could therefore be made even more forcefully? À la, "This outcome is mine only. If you want an outcome too, you have to come over and elicit one for yourself."

Just a thought.

I had already had a nagging fear that I wasn't doing that part quite right, and you've pushed me over the edge. It's true I need to do that better. This sentence: "This outcome is mine only. If you want an outcome too, you have to come over and elicit one for yourself." Is that one of mine from somewhere? Or is it your own? Either way, I love it and definitely want to use it! You are also quite right in the parts between diamonds 2 and 3:

Schlosshauerism 16: *In the context of discussing Wigner's friend, you also mention unitary evolution and describe it as something that adjusts "the beliefs with the flow of time." I think it would be useful to elaborate a little bit on this issue.*

After all, there will be people who, just as they've opened their minds and hearts and have readied themselves to embrace a personalistic notion of quantum states, are suddenly stopped dead in their tracks by asking themselves why, in the world, their personal beliefs should change (or be updated) according to the Schrödinger equation. They will point to the usual text-book understanding of unitary evolution as being given by the Hamiltonian, which in turn is usually thought of as something objective defined by the physical situation under consideration. And thus they will perceive a clash between subjective quantum states and the supposedly "objective" evolution of these states.

So maybe it would be helpful for the reader to hear exactly what the status of unitary evolution in the QBist framework is, and how and why consistency of the subjective viewpoint requires one to understand evolution operators (and, generally, quantum operations) as subjective entities, too.

I should put more about unitary evolution there (and/or also in the SIC section). At this point in the "extended" version (whatever it means for it to be "extended" over something when it is the original draft, and wherever it is going to go), there is not much point in being overly economical with words. [That, by the way, was my original reason for not broaching time evolution in any detailed way.] I'll fix it! Your first comment, I will probably pass on and take my chances:

Schlosshauerism 17: *Page 2:*

> The trouble with all these interpretations as quick fixes for Bell's hard-edged remark is that they look to be just that, really quick fixes. They look to be interpretive strategies hardly compelled by the particular details of the quantum formalism, giving only more or less arbitrary appendages to it.

I certainly agree with this assessment. However, I wonder what an Everettian would say in response to your charge that his interpretation depends on "more or less arbitrary appendages" to the formalism. I imagine him exclaiming, "My interpretation is the most economic of them all. In fact, I don't need anything else besides the bare formalism. So what are these appendages you're talking about?" You mention the need of making sense of probabilities, but I suppose the Everettian might dispute to what extent this need constitutes an "appendage," rather than just an exploration of the consequences and features of the formalism itself. Similarly for the "many worlds"—to an Everettian, they're not an ontological appendage, but a consequence of the formalism.

Trying to beat off a committed Everettian is like opening a can of worms or Pandora's box—it's not something that can be won. (Too many conversations, too many experiences … this is exactly the sort of thing we end up talking about when Marcus and I get riveted over Harvey Brown. The gulf is deeper than quantum interpretation, it's whole worldview definitely.) I'll just remain with my well-placed insult about Cambridge vs Oxford.

The sentence may not be quite right as it is presently, but it was meant to be "bald" (naked, bare, etc.)

Anyway, I really, really, really appreciate these comments. They were very constructive for me. I'll try not to let you down.

You know, your understanding of this business is certainly getting to the point where you could express aspects of it in your own voice and probably do a much better job than me. I was serious before, but I'm more serious now: You might try your hand at telling your own version of the story, or how it relates/contrasts to Everettism, etc., being critical and pointing out all the holes in QBist thinking wherever need be. Understanding criticism is far, far better than un-understanding criticism, and there is no doubt we QBists need continued stretching: Life is, and should be, a struggle.

BTW: Do you ever have any interaction with Ulfbeck there? I have always had some sympathy with this sentence from one of his papers with Aage Bohr:

> The click with its onset is seen to be an event entirely beyond law. … [I]t is a unique event that never repeats … The uniqueness of the click, as an integral part of genuine fortuitousness, refers to the click in its entirety, with all the complexity required for a break-through onto the spacetime scene. … [T]he very occurrence of laws governing the clicks is contingent on a lowered resolution.

From my one meeting with him, I suspect he is a recalcitrant sort, but there are definitely some interesting elements in his thought.

05-01-10 *If You Take Quantum Mechanics Seriously …* (to M. Schlosshauer)

Schlosshauerism 18: *I imagine him exclaiming, "My interpretation is the most economic of them all. In fact, I don't need anything else besides the bare formalism. So what are these appendages you're talking about?"*

By the way, one of the buzz phrases that bugs me the most from the Deutsch-Saunders-Wallace-Greaves set is, "if you take quantum mechanics seriously" (always, to my ears, delivered with immense smugness). Those words, even when nothing further is said, so get under my skin. Deutsch *et al.* act as if it is a completely unambiguous, value-free statement, containing no prejudice whatsoever, and I think, "Hey, I take quantum mechanics seriously too! **I** just take it seriously in the way **it** is pleading to be taken."

Attached are three slides from my (awful) talk "13 Quotes from Everettian Papers and Why They Unsettle Me" on `pirsa.org`. It really is an awful talk—quotes far too long, printed too small; it was duller than anything else I've ever done, and a lot of people fell asleep—but it did have a couple of zingers in it that I remain proud of … and I guess one of them is the title! One thing of interest with regard to yesterday's discussion is that I just found out by reviewing the first five minutes of it that I presented it as "a case study in psychology"—I had forgotten that.

Read the slides in the order: "Seriously," "Saunders," "Mirror-World." The last slide could have been done much better and made more effective with some work, but it says something of the point.

Seriously

Taking Quantum Mechanics Seriously, 1

> At the conference, the strongest "Everettian" was physicist David Deutsch of the Center for Quantum Computation at University of Oxford. Deutsch asserted that people have been wasting time for decades debating whether or not the Copenhagen Interpretation is meaningful because, "Only Everett's theory consists of taking quantum mechanics seriously."
>
> — Peter Byrne, "Many Worlds in Oxford," at `blog.sciam.com`

> It's sad enough when cranks churn out this tawdry old excuse for refusing to contemplate the implications of science [*i.e., the Everett interpretation*], but when highly competent physicists — quantum physicists — dust it off and proudly repeat it, it's a crying shame.
>
> — David Deutsch, posted on `Fabric-of-Reality@egroups.com`, 16 July 2000, commenting on a paper by Fuchs and Peres

> Let quantum be quantum. — Wojciech Zurek, his talk at this meeting

Saunders

Taking Quantum Mechanics Seriously
(Simon Saunders version, this meeting)

- The theory is to apply universally.
- Without any special mention of 'the observer' or 'measurement'.
- And without any special interpretative assumptions or additional [???] ...

Mirror-World

Taking Quantum Mechanics Seriously
(Quantum Bayesian Version)

- The decision theory should be a normative ideal for all physical agents, universally.
- Without any special mention of a speculative ontology behind the agent's experiences (at least before the first necessary moment).
- And without any additional ad hoc structures introduced for the fuzzy feeling of an *ontologized* dynamics (i.e., that which is usually called "unitarity").

05-01-10 *Out with the Old, In with the New!* (to C. H. Bennett)

... oh I can't resist. At first I was disappointed that I didn't get the last `quant-ph` number of the last decade, but learning this morning that I got the first one of the new decade, I got quite tickled. Maybe it means there's some hope for my quantum foundations program yet! ... dig, dig, dig.

05-01-10 *New Decade, Five Days In* (to D. M. Appleby)

I saw this morning that the toil of your paper-writing troubles turned out to be the first paper of the decade on the `quant-ph` archive (not number 4 as I thought it would be). And with that, I finally got happy about my screw-up, my long dragging of feet: Maybe it is actually a beautiful sign that we nabbed that spot. (The last paper of the last decade stars Charlie Bennett, a devoted Everettian, as its first author.) Maybe this means that our way of looking at things, or at least the one we are trying to develop, will find a prosperous home in this coming decade. As I'm learning from you and Hans and Pauli and Jung: Symbols are important.

Myself, I have been slaving to write something lasting: It started as a super-long extension of that PiC paper you saw me working on last month, but now I feel it is much more than that—that it will capture the hopes and dreams of where this program of ours can be taken and will serve as a focal point for people to develop this worldview-to-be. I hope to unveil the paper to you and Rüdiger, etc., by next week: We shall see.

But why I really write this note is not to tell you all that. I wanted to thank you: You have profoundly influenced me the last three years, and I thought you should know that. What struck me particularly as I worked through your Lie-algebra draft last week is your utter fearlessness. It is remarkable, really. The calculations were so elementary in this case, but it was *you* and you alone who had the bravery to push them through. You were the one fearless enough to step into the river of experience and see where it took you. And it dawned on me, you're always like that—that is what I have come to know of you: It permeates your whole being. There really is some hope that we're going to do big things this coming decade, but the hope is here because you are here.

Happy New Year, Happy New Decade.

06-01-10 *You Dog!* (to C. H. Bennett)

Bennettism 38: *Quantum Theory Needs No 'Interpretation' Besides Ours.*

I just found this on the web while I was trying to find something nasty that David Deutsch had said about me way back (so I could make a gibe at him to Max Schlosshauer). I find Charles Bennett instead! With friends like you, who needs ... How does that saying go?

06-01-10 *Deutsch's Proof* (to M. Schlosshauer)

Schlosshauerism 19: *By the way, what also irks me about Deutsch is that he claims flat-out that interference experiments PROVE the existence of parallel worlds – conveniently leaving out all the assumptions required for such a claim, not to mention the philosophical predispositions etc. Somehow I just don't find that, well, academically honest.*

Yes, indeedy, I was absolutely appalled when I got the impression of that in his book. Attached is the passage I'm thinking of (from my 13 quotes talk), and I'm pretty sure somewhere in the book he calls it a proof:

> **Deutsch "explaining" one-particle-at-a-time interference:**
>
> So, if the photons do not split into fragments, and are not being deflected by other photons, what does deflect them? When a single photon at a time is passing through the apparatus, what can be coming through the other slits to interfere with it? ...
>
> I shall now start calling the interfering entities 'photons'. That is what they are, though for the moment it does appear that photons come in two sorts, which I shall temporarily call *tangible* photons and *shadow* photons. Tangible photons are the ones we can see, or detect with instruments, whereas shadow photons are intangible (invisible) – detectable only indirectly through their interference effects on tangible photons. ...
>
> Thus we have inferred the existence of a seething, prodigiously complicated, hidden world of shadow photons. They travel at the speed of light, bounce off mirrors, are refracted by lenses, and are stopped by opaque barriers or filters of the wrong colour. Yet they do not trigger even the most sensitive detectors. The only thing in the universe that a shadow photon can be observed to affect is the tangible photon it accompanies. That is the phenomenon of interference. ...
>
> ... Thus we have reached the conclusion of the chain of reasoning that begins with strangely shaped shadows and ends with parallel universes. Each step takes the form of noting that the behaviour of objects that we observe can be explained only if there are unobserved objects present, and if those unobserved objects have certain properties.

Is that what you're referring to?

Does it shock you that I made an attempt to read his book? Well, the truth is my arm was twisted by circumstance. Deutsch was scheduled to speak at the QCMC conference in Cambridge, MA a few years ago, and I had never forgotten something that Michael Nielsen alerted me to in 2000. It was a chat at this place (just found it after that aside on Charlie Bennett): `http://groups.yahoo.com/group/Fabric-of-Reality/message/17`. Deutsch said, "They really need to read FoR, don't they?" So I read much of the damned thing (as much as I could stomach) with the intention of shaking his hand at QCMC, and introducing myself with, "Hi. I'm Chris Fuchs, and I read the Fabric of Reality." But he foiled me! He accepted his award at the meeting and gave his talk by video conference.

06-01-10 *13 Quotes* (to M. Schlosshauer)

By the way, that title was a play on Arthur Lovejoy's old essay, "The Thirteen Pragmatisms."

06-01-10 *13 Quotes, 2* (to M. Schlosshauer)

Schlosshauerism 20: *Aha! I hadn't spotted that. Thanks for enlightening me!*

Well, I would have been very surprised if you would have spotted it. I suspect I'm nearly the only physicist who's ever heard of it, and there might well be less than a handful of the (so-called) "philosophers of physics" who've ever heard of it themselves. (I remember Bill Demopoulos saying that he had never heard of it, or the book of the same title, even though he had great respect for Lovejoy and had read many of his books.) Particularly, I was tickled, amused, and appalled when

Chris Timpson apparently felt a need to include a footnote like this, defining pragmatism!, in his paper `arXiv:0804.2047v1 [quant-ph]`:

> Pragmatism is the position traditionally associated with the nineteenth and early twentieth century American philosophers Pierce[215], James and Dewey; its defining characteristic being the rejection of correspondence notions of truth in which truths are supposed to mirror an independently existing reality after which we happen to seek . . .

Mind you this paper appears in *Stud. Hist. Phil. Mod. Phys.*—a philosophy of science journal!

06-01-10 *New Decade, Five Days In, 2* (to D. M. Appleby)

May I forward this inspiring note (excluding my personal bit) to Max Schlosshauer, who I've been in a lot of contact with recently. Max is one who has made the transition away from Everettism (after even writing a book on decoherence and all that) and is now leaning very much in our direction. And he too is very much concerned with "what will come next," now that he finally feels something WILL come next.

I see you're very late, but I guess I should expect no less of you.

Marcus's Preply

> I felt it was significant that we were the first paper of the decade. Though actually I don't think we need rely on Jung and Pauli and symbolism here. I think it's automatic. At least, I think it is automatic that Everett (Hawking, Turok, etc etc) will go out of fashion some time soon ("soon" as measured on a generational timescale: in this context 20 years would count as "soon"). The only question is what will replace them? That, of course, is a question of will. It depends on us, and it is far from automatic. It is in the sphere of human freedom. In this case your freedom, and mine.
>
> When I am faced with the dominant orthodoxy as it presents itself during (for example) lunch-time conversations in the Black Hole Bistro I always think of the court of the Bourbons in the 1760's or 1770's. A visitor to the French court in the second half of the eighteenth century would doubtless have been very impressed by the surface magnificence. Superficially it might have looked as though the monarchy would last for ever. But in fact it collapsed more or less overnight, almost without bloodshed. There was lots of bloodshed afterwards, of course, but that was the revolutionaries fighting each other. In 1789 the power of the monarchy simply evaporated, with no significant fighting at all.
>
> I think something similar applies to the situation which faces us. In conversations at conferences, the Black Hole Bistro etc etc it always seems as though you and I are in a tiny minority. Which, I guess, we are. But if one looks a little more closely I think one can see that the dominant orthodoxy in early 21st century physics, like the 18th century Bourbons, is supported by virtually nothing. When the time comes it will be blown away like smoke.
>
> When I was talking to Åsa's office mate (I have forgotten his name: the post doc who came up to you, me and Howard at wine and cheese, just before you left) he strongly emphasized (a) his atheism and (b) his strong belief in Everett. Afterwards I got to wondering what exactly was the difference between his belief in Everett and the religious

[215] And note the mispelling of Peirce.

belief he rejects. He cannot claim that his belief in many worlds is better supported by the empirical evidence than a belief in God. Everettianism, being expressed in terms of equations, may superficially look more scientific than the Bible, or the Koran. But when it comes to the crucial test, of empirical support, it fails completely. There is no more empirical reason for believing in many worlds than there is for believing that God created the world in seven days. Moreover, when you ask an Everettian what their belief does rest on they will typically appeal to simplicity and beauty. I think someone might give similar grounds for believing in religion. Fundamentalist religion is certainly simple. And, although religion does not appeal to the aesthetic sensibilities of the average PI resident, it surely does appeal, very strongly, to the sensibilities of the devout (and even the non-devout: many aetheists like to listen to Gregorian chants, or Verdi's requiem mass). In these respects Everettianism is very like a religion. However, there is one important difference. Religious belief speaks to something deeply rooted in the human soul, and it is very tenacious. Everettianism is an altogether more superficial psychic phenomenon. I can't see people giving up a belief in the approximate validity of Newtonian mechanics within its domain of application, because of the weight of empirical evidence, and because of its practical utility. Nor can I see them giving up a belief in religion any time soon, because of its deep-seated psychic attractiveness. But Everettianism lacks both the practical usefulness of science, and the psychic appeal of religion. It is nothing more than a transient intellectual fashion. Sooner or later the fashion will change, as fashions always do.

So I don't think we need worry about defeating the current orthodoxy. That will take care of itself. As in 1789 the important question is, not the revolution itself, but what comes afterwards.

I am looking forward to reading your new paper.

07-01-10 *Writing Is So Very Hard!* (to H. C. von Baeyer)

I can't get papers finished; I can't even get emails finished! How on earth could you make writing such a large part of your career without having committed suicide by now?

Below is a note I started writing you some time back. (It was originally titled, "New Decade, Three Days In," if that gives you a hint.) I'd like to finish writing that tonight or tomorrow, or at least pick up where I left off. At the moment, though, I don't really want to break my train of thought of trying to reduce the big paper to the little one. (Read the note below to see what I'm talking about.) And I thought: If Hans is out there with nothing better to do (yeah, right), maybe I could ask your advice (with all your experience of editing). Of course, feel free to say "Go away pest!" and in the harshest terms!

Anyway, attached is the big paper and the latest partial reduction of it.

At the moment, I've managed to cut it down from 16 pages (12,217 words) to 7 pages (4,886 words). And my understanding is, I really should reduce still $\frac{3}{4}$ of page more text-wise. The trouble is, at this stage, I can't see where to trim it anymore!! Already the smaller document feels lifeless, soulless to me. But maybe as an outside (relatively uninitiated) reader you can more easily see some large chunk that can be lopped off without affecting the drama, conceit, and essential content of the message. Maybe at this stage, at some point in the draft, I'm still fighting battles from philosophers past—battles that don't need to be aired to the general public in this venue.

Just a thought: If you see something you can suggest to lop off wholesale, please let me know what you think.

More on counterfactuals later.

Dear Hans,

Thanks for the notes. I'm certainly glad to give you an AHA moment when I can. This morning, I've got to get back to work on the articles—with the "extract" version taking top priority. It absolutely must be finished, or I will be in deep trouble with PI. That article will only cover the stuff *before* the capacity section, leaving the parts less friendly to the usual physics prejudices (i.e., the thoughts of those who say, "We're going after a theory of everything! That's what physics is all about!") for another venue. Contrast that with the parts in the longer version, where I write some lines like this:

> Everything experienced, everything experienceable, has no less an ontological status than anything else. You tell me of your experience, and I will say it is real, even a distinguished part of reality. A child awakens in the middle of the night frightened that there is a monster under her bed, soon to reach around and grab her arm—that experience has no less a hold on onticity than a Higgs-boson detection event would if it were to occur at a fully operational LHC. This is because the world of the empiricist is not a sparse world like the world of Democritus (*nothing but* atom and void) or Einstein (*nothing but* unchanging spacetime manifold equipped with this or that field), but a world overflowingly full—full beyond anything grammatical (rule-bound) expression can articulate.

Yep, that's not going into an issue of *Physics in Canada*, true as it is, and true as every practicing physicist should recognize it to be.

Anyway, I attach the latest version of the big one for the heck of it. The last three sections are still in transition, far from finished, but I guess mostly I send it to you because of the earlier part that you did read: There was something quite misleading in the discussion of the quantum de Finetti theorem, and I have that fixed up now. You shouldn't be misled any longer than you have to be!

Speaking of which ...

von Baeyerism 41: *They simply provide a unique universal characterization of a quantum system, usurping the role of the wave function. Is the word counterfactual a little too suggestive here? I mean, when I step on my bathroom scale, I am tacitly saying that "if I were to weigh myself, counterfactually, against the kilogram in Paris, this is what I would find." This statement is then transferable to any scale in the world. (Of course that's what you had in mind in the first place.) But I never use the word counterfactual in my bathroom.*

I think this may indicate a little confusion, and I want to see if I can clear it up. You're absolutely right that one would not normally speak in the manner of, "If I were to weigh myself, counterfactually, against the kilogram in Paris, this is what I would find"—the point of oddity being the word "counterfactual." But that is not how I am using it when I am using it.

07-01-10 *The Question Is: What comes next?* (to M. Schlosshauer)

Since you and I have been in so much contact recently, I asked Marcus if I could forward you the note he sent me last night. [See 06-01-10 note to D. M. Appleby titled "New Decade, Five Days

In, 2."] I thought it hit the mark, and in an eloquent way: What comes next? That's the only really important question.

I'm feeling doubly good today. I feel I had a very deep insight this morning just as I was getting into the shower. It is the inevitable conclusion to draw from this QBist trek I've been taking, but I only feel that I really saw it with clarity this morning. In a single sentence, it goes like this: Just as quantum theory is not in conflict with probability theory, only an *addition* to it, quantum mechanics is not in conflict with the normal, empirically perceived world of common experience (sometimes called the classical world); it is only an *addition* to it. It is not that the classical world undergirds quantum mechanics, as I read Bohr as saying; it is not that the classical world should be *derivable* from quantum mechanics (a thought that you are very familiar and probably still quite sympathetic with). It is that one is an *addition* to the other, just as quantum theory is an addition to Bayesian probability. It is an overpowering feeling I have in my chest, a feeling like I've only had a few times in my career—my whole body is most definitely reacting to this thought.

Anyway, I pray (metaphorically) that I can articulate this properly in some short number of paragraphs. We shall see—I will try my best and whatever the result, it will be incorporated into the manuscript you have been reading over. I feel I have to say it now and not wait to write another paper.

In lesser progress, I have the distillated version down to 6 pages now. Somehow I've got to get rid of still one more, even though it already feels like a dead, lifeless document in comparison to its parent. The consolation is that it will be in a journal that no one reads. But the antithesis to this is that many of people who will read this particular issue are my PI colleagues (those who wouldn't normally read me, cosmologists, string theorists, etc.)—and I would much rather them read the long version!

08-01-10 *Oh Editorship* (to H. C. von Baeyer)

Thanks again for helping me out. I followed through on nearly all of your suggestions I believe, save maybe 2 or 3. Some of your suggestions too, weren't just about size, but flow—I'm particularly grateful for those things, especially when they were with regard to sentences that I could just never get right. For instance, that very first sentence of the paper—I must have rewritten it a 100 times, with all kinds of little variations.

Attached is the version of the paper I finally turned in. It didn't cover nearly as many subjects as I had originally intended for it. (For instance, since I knew PI people would be reading it, I wanted to make sure I said something about quantum cosmology ... but alas.) Still, for what it does cover, I don't think it does a bad job.

Really, thank you very much for the help. The experiences I've had with your editing have been quite new to me—they're the first to ever give me respect for the trade: My writing is visibly better after your input. I would be hard pressed to think of a time I've ever thought that about anyone else's input (even in isolated instances).

Funny too, that this has happened at a time when I had read this article in the *New York Times*: `http://www.nytimes.com/2010/01/03/opinion/03galassi.html?scp=1&sq=electronic%20 books&st=cse` on the value of a good editor.

Tomorrow, I'll try to write you an extended version of the clarifying stuff on counterfactuals if you still need it. For the moment, I'll just write the definition from `dictionary.com` and put it into context: *Counterfactual: a conditional statement the first clause of which expresses something contrary to fact, as "If I had known."*

"If I had thrown the system up to the sky and let it cascade through that device back down to the one on the ground, I would have gambled this way (usual Law of Total Probability)." But that is contrary to fact: I do not actually throw it up to the sky, I throw it directly to the one on the ground. Quantum theory says, "Don't waste your old calculation. Just stretch it a little, and subtract a bit off afterward (Eq. 5 in the present draft)." The claim is, that's the meaning of the Born Rule—it tells us how we ought to rewrite our counterfactual probabilities.

That said, could I ask you to try to articulate what you do not like about Figure 2, or what confuses you about it? [Recall, it's still a bit inaccurate—I haven't redrawn it yet to fix the new notation. The quantity in the lower left box should be $Q(D_j)$ rather than $P(D_j)$.]

Have a look at the new conclusion as well. It gives the hint that I'm going to get downright alchemical before the bigger version is all over with. Eventually, you'll see what I mean.

Lucky Barbara, to be in Paris for as long as she can. I've been trying to convince Emma she should go to college there so I could come visit her often!

09-01-10 *The Duke of Chartres* (to H. C. von Baeyer)

One last point to address then,

von Baeyerism 42: *So, forgive me, but I don't like your "sky" nomenclature any more than the NBS. But unlike Mermin with his qbits I'm not going to fight you, as long as I and others understand you.*

Indeed I wouldn't want a fight. But I wouldn't mind suggestions either. Following your advice, I should not fossilize. There is nothing about my previous attempts at expression (sky, NBS, etc.) written in stone — they were just the best that came to mind at the point.

My student Matthew Graydon once gave a presentation to our group, where the SIC was placed near the bottom of a wishing well. You throw the quantum system down the well and, after interacting with the SIC, it is blown back up to ground level by a fan. We all had a good giggle.

You want a more drastic change than that. I'm open: I just need a better understanding of how the present imagery is ineffective and some suggestions for what should be put in its place. What part of what is going on is not being captured adequately?

At times, I've toyed with making a modification of my usual activating observer image (Fig 1 in the paper): One frame showing him giving a 1-2 punch with his measurement-device hands, and one frame showing him giving a real whammy with just one hand, a knockout punch. But that's probably nothing like you're thinking.

In your Swing book, I read this:

> If I were as rich as the Duke of Chartres, I would sponsor an international competition for the design of a modern atomic logo. The jury would include writers, painters, sculptors, physicists, chemists, and teachers. Somewhere, I feel, there must be a mind creative enough to come close to translating the well-understood mathematical language of quantum mechanics into visual terms ...

and ever since then, whenever I see a Bohr atom (as I have multiple times recently on some cable channel), I think back to it and wonder how I could do better.

You've got the bully pulpit secured; use it!

09-01-10 *Challenge* (to H. C. von Baeyer)

von Baeyerism 43: *That's quite a challenge but I'll give it a shot. Before I do, though, I need to go through a check list of properties of SIC measurements, strictly in words. Is it correct that:*

von Baeyerism 44: *The quantum system does not include the agent or his instruments.*

True.

von Baeyerism 45: *The quantum system comes with a description of the apparatus that has prepared it*

False. At this moment, I'm thinking of a particularly big, particularly interesting quantum system: Hans Christian von Baeyer. I mentally delimit him as a system (a decently defined piece of the world), yet have not at all made any attempt to write down all the things I believe might come about if I were to interact with him. (I.e., give him a well-placed kick of one sort or another.) Thus I hold the thought of him in my mind separately from any quantum state I might ultimately write down for him.

In my last few papers, you probably won't find the word "prepared" anywhere in them. This will be connected with one of the further answers below.

von Baeyerism 46: *It may be simple or complicated*

True.

von Baeyerism 47: *A spin 1/2, after passing through a bunch of magnets, is a simple system with dimension 2*

Without having seen you for several months, I posit that you are about 160 pounds. As a hypothesis, let's say you're exactly 160 pounds. I didn't need to put you on a scale to make that hypothesis—if I wanted to check it, I might put you on a scale, etc., but that is secondary in the logical order of things. Your weight is something you possess independently of any of my manipulations of you. Similarly with an ejectum of a hot piece of tungsten—I posit that, with regard to the part of it I might tweak with a Stern-Gerlach "hand" (Fig 1), it's about 2-d worth. If I wanted to check that, I could put a SG device near the tungsten and see whether I get two stripes, or three, or more, but that is secondary in the logical order of things.

Think this way: "system" is akin to a "rock", "HS dimension" is akin to the "rock's mass", "quantum state" is akin to "the chance I think this rock will break that window, if I throw it as hard as I can."

von Baeyerism 48: *An electron, with all its properties, is an example of a complicated quantum system*

More complicated at least. Better imagery is simply, "has more stuff." Has more dimension.

von Baeyerism 49: *A SIC measurement is often difficult, but can ACTUALLY be done anywhere*

True. Anywhere, anytime.

von Baeyerism 50: *A system can be prepared in the same way over and over again*

Nothing wrong with that so far as it goes. But this word "preparation" often carries with it a way of thought that is not-so-Bayesian. *Sometimes* quantum states do come about because of "preparations"—precise laboratory procedures—and for those cases, I might be willing to write down the same state over and over after each new instance of the procedure.

More on this below.

von Baeyerism 51: *If a complete set of SIC measurements is done on this system, and statistics have been collected, an agent has as much information as can possibly be gathered.*

Sounds very unBayesian. Think first of simple probability assignments: Bayesians leave unanalyzed where the priors come from—the answer to the question of why one possesses this or that prior is outside the apparatus of probability theory.

The same we say for a quantum state: It is only (and you have to take "only" very seriously here) a state of belief. Writing a quantum state down is tantamount to expressing what one believes of the d^2 possible outcomes to a single SIC measurement. My beliefs with regard to what will happen just once. As I can have a belief about whether the world will end or not tomorrow, I can hold a belief about a singular SIC measurement. And if I do hold such a belief, THAT is a quantum state assignment.

I don't use the word "information" much anymore to say what quantum states are, except to make the transition to speaking of quantum states as beliefs. So, also, for instance, I would not speak of a pure-state assignment to your spin-1/2 as "having as much information about the system as I can have". A pure state is a belief state of a certain variety, but I might hold that belief state for religious reasons rather than "having gathered statistics". Bayesians leave the origins of priors unanalyzed; the calculus kicks in once one has been set. Similarly of quantum mechanics.

von Baeyerism 52: *"Thinking of a quantum state as literally an agent's probability assignment for the outcomes of a potential SIC measurement" implies making many measurements for each i on the system as prepared. Why do you use the singular of the word measurement?*

See discussion above, for von Baeyerism 51. To make the point extreme, consider the pulsar PSR B1919+21. Astronomers have had their eyes on it since 1967. Wiki says it has a period of 1.3373 seconds and a pulse width of 0.04 second. And I'm sure lots of other things are known about its spectra. My friend Greg Comer, who works on the fluid and superfluid mechanics of neutron stars, could take that data, make various assumptions, and impose a "maximum entropy" calculation à la Jaynes, to come up with a quantum state for the star's core. There's no preparation here, no really significant statistics in the usual probabilistic sense, and no *unique* way to turn the observational data into a quantum state assignment. But with the tools of MaxEnt, one can give *some* quantum state assignment, and that's what people usually do. The QBist says, "Ah, what's going on here is that the theorist is expressing his full catalogue of beliefs about that system." And that is tantamount to posing the question of what the theorist believes will come about if a super-duper-big SIC were brought up to the neutron star and allowed to kick it. There would be one outcome, and only one, and the quantum state the extent to which one believes it will be this or that or that.

Maybe this is why I habitually put the SIC in the sky—it makes it inaccessible. I don't want anyone to think of it as a measurement that need be performed in order to have a quantum state assignment in the first place. An initial quantum state assignment is a *prior* from the point of view of quantum theory. A prior about what? About my SIC up in the sky.

von Baeyerism 53: *For a spin 1/2 a SIC requires finding the probabilities for pointing up or down in the four directions defined by a tetrahedron*

Yes.

von Baeyerism 54: *Equation 5 relates the probability of finding a spin 1/2 pointing up in the direction defined by the geographical coordinates of Waterloo, say, to the measurements along path 2.*

Yes, if I understand correctly.

von Baeyerism 55: *For a spin 1/2 system only one kind of Von Neumann measurement (a Stern-Gerlach) is possible, but for a more complicated system all sorts of conventional measurements are possible – but only one set of SIC measurements (except for a little bit of choice).*

If by "one kind" you mean the class of all Stern-Gerlach orientations. SICs start there: I.e., if you give me one SIC, I can mathematically produce a continuous infinity of other SICs by acting on the original with an arbitrary unitary operation. $\Pi_i \longrightarrow U\Pi_i U^\dagger$. But it's even more interesting than that. There are even more SICs than that. By that I mean, not all SICs are unitarily equivalent. But for the Bureau of Standards, you only need one. Pick any one you want and don't change it. I.e., pick your coordinate system and stick with it.

Thanks for the book review; I'll read it tomorrow morning in my comfy library chair and my coffee. Now I'm off to build a fire and a chance to beat Kiki at Scrabble. (It happens once every blue moon.)

10-01-10 *SIC* (to G. Zauner)

Thank you for your kind note. It is quite nice meeting you, and seeing that you still have some interest in this baby of yours. Perhaps you would like to visit our group at Perimeter Institute sometime and become even more involved? We could easily bring you in for 1 week, 2 weeks, or even a month and pay your expenses if you have the time.

I am most curious about your last remark on the quantum Bayesian program. Would you expand a little? What sort of thing are you thinking? The reason I am most interested in SICs is because 1) they are 2-designs, and thus give me a way of thinking of the Born Rule as a kind of "quantum law of total probability", but 2) give rise to a probability space with the same dimensionality as the space of density operators. Any 2-design with a larger number of outcomes would have the valid quantum probability distributions confined to a proper subspace within the probability simplex. It's for this reason that I have thought of SICs as "closer to the truth" and emphasized using them for the purpose of the QBism program. But I wonder what sort of criterion you have in mind?

Attached is a kind of semi-popular account of the QBism program just finished for the magazine *Physics in Canada*. [See "Quantum Bayesianism at the Perimeter," `arXiv:1003.5182`.]

11-01-10 *Analogy?* (to H. C. von Baeyer)

von Baeyerism 56: *Is the following analogy too misleading?*

SIC measurements are like Fourier analysis. An experimentalist launches a square wave pulse of light at a window. He can either predict directly what he will see, or theoretically *decompose his square wave into sine waves and then launch the sine waves at the window. The sine waves are universal, in that they work for any input signal whatsoever. In order for this to work he needs an equation that relates the two paths to each other. That this relationship is far from trivial is demonstrated by the Gibbs phenomenon (discovered by Wilbraham).*

I realize that this analog pays no attention to Bayesianism, but it seems to me to reproduce certain features of your scheme. Not the least of which is that it justifies an experimental check of Equation 5, just as the Gibbs phenomenon only gained traction when it was investigated experimentally.

It's not misleading, it's exact. Eq. (5)

$$Q(D_j) = (d+1)\sum_{i=1}^{d^2} P(H_i)P(D_j|H_i) - 1$$

comes about because of Eq. (3)

$$\rho = \sum_{i=1}^{d^2}\left((d+1)\,P(H_i) - \frac{1}{d}\right)\Pi_i\,,$$

and Eq. (3) simply expresses that any quantum state (pulse) can be decomposed into a linear combination of SIC elements (sine waves). The sine waves are a basis for the vector space of all continuous functions; the SIC elements are a basis for the vector space of $d \times d$ matrices.

And like launching your pulse at the window: If an agent knows how he would gamble on the ground outcomes *given* a "preparation" Π_i, and he also knows how he will gamble on the Π_i's themselves (up in the sky), then he will also know how to gamble on the ground outcomes directly. The analog of reintegrating the Fourier analysis is Eq. (5).

So that much of what say is exact. Maybe the Gibbs phenomenon part of what you say is a bit trickier. The Gibbs phenomenon comes about because one is trying to decompose a function outside of the vector space (a discontinuous function) with a basis for the continuous functions. I'm not sure that I know an analog of that for our SICs.

von Baeyerism 57: *Not the least of which is that it justifies an experimental check of Equation 5, just as the Gibbs phenomenon only gained traction when it was investigated experimentally.*

Most recently in my correspondence (describing Steinberg's or Zeilinger's interest), I've gotten in the habit of calling it a "demonstration" rather than "experiment." I think the word "experiment" should really only be used, say, as with the LHC, when one genuinely does not know what the outcome will be (whether the Higgs will be seen or not). In the case of "checking" Eq. (5), there is no doubt it will be confirmed.

Still, my nitpicking aside, you are completely right about the traction thing. My feeling has been that if Anton were to do a demo, it would bring a lot of attention to the SICs. That's really my most significant motivation for him to play with them.

By the way, Marcus, Steve, and I got a very nice letter from Gerhard Zauner yesterday saying he had read our most recent paper, some of the quantum Bayesian papers, and watched the talks from our "Seeking SICs" conference from a couple years back. Zauner, you probably don't know, was the *real* inventor of SICs! His 1999 thesis

`http://gerhardzauner.at/documents/gz-quantumdesigns.pdf`

was on them, though I didn't know of it until many years later. And in fact, until yesterday, I always thought he had fallen off the planet (he works for a financial firm or something). I always credit Caves too with the invention of the idea (summer of 1999), but we were really taking baby steps in comparison to what Zauner had already done. Zauner had already had the connection to the Weyl-Heisenberg group conjectured and proven their existence up to dimension 5 when we only knew about dimension 2 and 3 and didn't about the Weyl-Heisenberg connection at all (til Robin Blume-Kohout discovered it in 2002 or 2003). Zauner had also already had Marcus's (2004) order-3 symmetry conjecture way back then. (Not that any of us have ever read the thesis; we just know these things from Markus Grassl.)

Just as I write all this, it finally registers in me that that PhD was done in Vienna!! This is perfect for my talk next week! I'll definitely use it.

I've always wondered what Zauner might have been up to when he was studying these things—why was he interested in the question. And just now, I see from some of his references (Gudder, Accardi) that he had some knowledge or interest in quantum foundations. I'm glad I looked up the thesis to give you a link! If you've got some time, would you mind snooping through it to see what he gives as a motivation for what he's doing. Clearly almost all of the thing is about mathematics, but because of the references there must be something about foundations. (For some reason, I can't seem to do a word search in the document, otherwise I'd try to give you some page numbers.)

In our case, the original motivation came from trying to prove the quantum de Finetti theorem. Before we had a proof of it, at some point Carl thought we would need these structures in our proof—that's what motivated him to think of the idea, and he knew they existed in $d = 2$ because Feynman had mentioned them in a paper, but he didn't know whether they existed in any other dimension. Luckily though we did not actually need them—I figured out how to make the de Finetti proof work with *any* minimal informationally complete POVM with rank-1 elements. So, the symmetry was an extra condition that was not needed. But then the SIC question took on a life of its own—for no good reason really. They were just so damned pretty. And their elusiveness made them that much more desirable. I got the feeling (for no good reason again) that they had to be very deep structures, revealing something very important about quantum mechanics. And that thought ever so slowly carried me all the way to the urgleichung.

But maybe Zauner was already there, long, long ago.

Further note attached below (written to Max Schlosshauer) on the mysterious byways that lead to scientific discovery. [See 04-01-10 note, "Birth Notes," to Max Schlosshauer.]

11-01-10 *Born, Heisenberg, and the QBies* (to H. C. von Baeyer)

Have a look at this nice article:

`http://philsci-archive.pitt.edu/archive/00004759/01/SHPMP_paper_07_10_09.pdf`

by Guido Bacciagaluppi and a student. I was knocked over with a Wow! when I read it a little while ago. Particularly, see the discussion right before and right after Eqs. (2) and (3) in it. It was Born and Heisenberg's terminology that took me: "[I]t should be noted that this 'interference' does not represent a contradiction with the rules of the probability calculus."

Compare that to my discussion around Eq. (7) starting with "But beware:" in the present draft of my longer paper (attached).

Maybe it's the ghosts of Heisenberg and Born that have been haunting me all along, and not Pauli after all!

11-01-10 *Psi-ontologists* (to R. W. Spekkens)

In the PiC paper I sent you earlier, there is a line that goes:

> What considerations like this tell the objectifiers of quantum states is that, far from being an appendage cheaply tacked on to the theory, the idea of quantum states as information has a unifying power that goes a significant way toward explaining why the theory has the mathematical structure it does.

And the extended version of the paper, the corresponding line goes:

> What considerations like this tell the objectifiers of quantum states—i.e., those who to attempt to too-quickly remove the observer from quantum mechanics by giving quantum states an unfounded ontic status (status as a state of reality)—was well put by Spekkens: [A quote of you follows, then I pick up with the "far from being ...."]

These strike me as the perfect place to make a joke. I'm toying with the idea of coopting your term, if you'll let me, in place of "objectifiers". If I were to use the term, and put a footnote right at it giving you credit for inventing the term, would you mind? I'd probably write it as ψ-ontologists, and the footnote would read something like \footnote{Not to be confused with the Scientologists. This term was coined by R. W. Spekkens blah, blah, blah.}.

If you want to reserve the term for one of your own papers, I won't be hurt. But it is such a cool term, and so fits my pre-existing sentences, the desire to use it is just getting the better of me.

11-01-10 *... and the Smell of Postmodernity* (to M. Schlosshauer)

[Replying to Max's note "The Spell of Modernity" ...]

Sorry to take so long to get back to you! I've listened to Brad Mehldau, Jimmy Smith, Dr. Lonnie Smith, and Jazz Side of the Moon now. All really great stuff!

Attached is the final version of the essay for *Physics in Canada*. Nothing new in there for you to read, but I send it to you for completeness since I thank you in the back.

I'm going to hold off on personally answering your questions on classicality for the moment, and just try to do it really well in the draft. If I don't address everything, you can take another shot at me at that point.

12-01-10 *... and the Smell of Postmodernity, 2* (to M. Schlosshauer)

Schlosshauerism 21: *I retrospectively thought my from-the-gut reply to your ideas on "addition" and "classicality" wasn't very pointed, and I apologize if it came across as either not too bright or as just plain insufficient in responding to your enthusiasm.*

There is absolutely nothing to apologize for! I was very flattered to see your immediate response that day. I've just been swamped taking care of PI things, getting ready for Vienna, spending time with my students, trying to figure out how to get an internet link at my mom's house while I'm visiting her (after Vienna), etc. I was just saving myself time writing, and I knew I'd be addressing all your points in a later draft of the paper. They were exactly the questions that needed asking—it's not that they were not too bright, but TOO bright! And I knew it would take me some work. So I took the easy way out until I could recover my forces.

I'll probably be more in touch once I get to Vienna and get away from family duties etc. Already got one experiment on the old SICs going (see attached). If I can get Zeilinger interested in showing a demo in his own way that would be really cool—probably a fat chance, but it's worth a shot.

Send that balanced Everettian paper when you've got it done. It's about my turn to read something of yours!

12-01-10 *May 2010 Laws of Nature* (to R. Couban)

Thanks for your intriguing note. I had never thought of efforts to understand quantum mechanics as being clouded or thwarted by capitalism before! It does make me smile, but on the other hand I am intrigued as well. What could he mean? I snooped on the web a bit and saw that your professor has some interesting credentials—for instance, a senior thesis with H. S. Thayer, who wrote the masterful *Meaning and Action*. It's a history of pragmatism that sits on my bookshelf.

I also think there is a lot to learn from DeLeuze's metaphysics of "difference." I'm very much intrigued by his idea that difference goes all the way down. (I might also recommend that you read the beginning parts of Nancy Cartwright's *The Dappled World* and John Dupré's *The Disorder of Things*.)

About the meeting, I'm sorry, it's an invitation only thing, and we have it in an already tight room. The idea was to put everyone in each other's face, and I think we succeeded at that. However you can watch all the talks on `pirsa.org`—they'll be taped with good quality, showing up on the website the very next day after they're given live. I hope that will be a good substitute for you.

Rachel's Preply

> I was just curious about the upcoming conference, where I would be interested in keeping my ears open and my mouth shut. I don't belong to any websites, blogs or societies.
>
> I work at a research institute, and last year a new post-doc was seated in the cubicle next to mine and conversed with me about some theoretical problems of statistics. These conversations reminded me of George Caffentzis' arguments on the knowability of natural laws ("physics is not only 'about' Nature and applied 'just' to technology, it's essential function is to provide models of capitalist work"), which Prof. Caffentzis explained to me are influenced by his 1978 thesis that "we will not be able to have a coherent conception of quantum physics until humanity transcends capitalism." (!)
>
> Meanwhile at my home in Oakville I have been cleaning the basement and sent home movie film and other ephemera from the 1920's to the Tuxedo Park Historical Society in New York. (One of my predecessors had a place in Tuxedo Park.) I read about the Loomis Laboratory at Tuxedo Park, and it reminded me of the Perimeter Institute. I think Caffentzis arguments may be answered by Gilles Deleuze's series of discussions of paradox in his book *The Logic of Sense*, but I lack the cognitive ability to formalize this opinion. I am going to order the 1978 thesis (*Does Quantum Mechanics Necessitate a Theoretical Revolution in Logic?*) through InterLibrary Loan. Maybe reading this will provide some traction for my idea of the Deleuzian (non-mystical) paradox.

16-01-10 *The Tetragrammaton* (to H. C. von Baeyer and D. M. Appleby)

Greetings, dear alchemical friends, from Vienna!

Earlier today I sent Anton that quote from Heisenberg's essay "Wolfgang Pauli's Philosophical Outlook" that I like so much:

> In the alchemistic view "there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work,

> which succeeds only ‘Deo concedente’.” The governing symbol for this magical view of nature is the quaternary number, the so-called “tetractys” of the Pythagoreans, which is put together out of two polarities.

and since then I’ve been walking around the area of Franz Joseph’s imperial palace thinking all kinds of crazy thoughts. Just stopping into a coffee shop to write you.

Lovely imagery: The SIC of a qubit (“two polarities”) has four outcomes (“tetractys”). How much closer to the unspeakable name of God could we come with this!

With a smile, and a hunger for schnitzel …

16-01-10 *Paulian Alchemy, Sunday Reading* (to A. Zeilinger)

You asked me last night what I have been working on. I said, “SICs,” but something like these lines from Werner Heisenberg’s essay “Wolfgang Pauli’s Philosophical Outlook” might have been closer to the mark!

> In the alchemistic philosophy, he had been captivated by the attempt to speak of material and psychical processes in the same language. Pauli came to think that in the abstract territory traversed by modern atomic physics and modern psychology such a language could once more be attempted …
>
> In the alchemistic view “there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature’s course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher’s stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only ‘Deo concedente’.” The governing symbol for this magical view of nature is the quaternary number, the so-called “tetractys” of the Pythagoreans, which is put together out of two polarities.

The connection between what I said last night and what I say today is that (a little tongue in cheek, but only a little): Performing a SIC would “release the spirit that dwells in matter.” (It is interesting too that a SIC for a qubit has four outcomes—a SIC is its tetractys!)

Anyway, if you want some light reading for Sunday (and it actually is), see the attached draft. It is an essay I am hoping to finish in Vienna—to give it the right spirit so to speak. All that’s left really is finding a respectable way of saying this crazy alchemical stuff in “Hilbert-Space Dimension as a Universal Capacity” section!

A very pleasant day today walking around the city and thinking.

17-01-10 *Oh Translator* (to H. C. von Baeyer)

This passage of Pauli is now taking on such an important role in my newest version of that damned paper that I’m wondering if it is translated correctly:

> The objectivity of physics is however fully ensured in quantum mechanics in the following sense. Although in principle, according to the theory, it is in general only the statistics of series of experiments that is determined by laws, the observer is unable,

> even in the unpredictable single case, to influence the result of his observation—as for example the response of a counter at a particular instant of time. Further, personal qualities of the observer do not come into the theory in any way—the observation can be made by objective registering apparatus, the results of which are objectively available for anyone's inspection. Just as in the theory of relativity a group of mathematical transformations connects all possible coordinate systems, so in quantum mechanics a group of mathematical transformations connects the possible experimental arrangements.

There's something awkward about the English in a couple of places of it. For instance, "the observer is unable, even in the unpredictable single case."

The translation comes from pp. 117–123 in *Writings on Physics and Philosophy.* You think there's any way we can dig up the original text and check? Any idea how we can get our hands on that old journal? So far I haven't had any luck with Google Scholar, but maybe I'm not being too smart.

The longer passage of interest to me (but not all of it is used in the present paper) is below.

> Einstein's opposition to it [the so-called "Copenhagen interpretation"] is reflected in the papers which he published, at first in collaboration with Rosen and Podolsky, and later alone, as a critique of the concept of reality in quantum mechanics. We often discussed these questions together, and I invariably profited very greatly even when I could not agree with Einstein's views. "Physics is after all the description of reality," he said to me, continuing, with a sarcastic glance in my direction, "or should I perhaps say physics is the description of what one merely imagines?" This question clearly shows Einstein's concern that the objective character of physics might be lost through a theory of the type of quantum mechanics, in that as a consequence of its wider conception of objectivity of an explanation of nature the difference between physical reality and dream or hallucination become blurred.
>
> The objectivity of physics is however fully ensured in quantum mechanics in the following sense. Although in principle, according to the theory, it is in general only the statistics of series of experiments that is determined by laws, the observer is unable, even in the unpredictable single case, to influence the result of his observation—as for example the response of a counter at a particular instant of time. Further, personal qualities of the observer do not come into the theory in any way—the observation can be made by objective registering apparatus, the results of which are objectively available for anyone's inspection. Just as in the theory of relativity a group of mathematical transformations connects all possible coordinate systems, so in quantum mechanics a group of mathematical transformations connects the possible experimental arrangements.
>
> Einstein however advocated a narrower form of the reality concept, which assumes a complete separation of an objectively existing physical state from any mode of its observation. Agreement was unfortunately never reached.

Hans's First Reply, 17-01-10

> OK, here's the story. On 7 January 1958, in the middle of his fight with Heisenberg about their doomed theory, Pauli gave an address at the ETH on the occasion of the unveiling of an Einstein bust. The talk was entitled "Albert Einstein in der Entwicklung der Physik." Five days later it was published in the Neue Zürcher Zeitung, a national daily newspaper. It was collected in *Aufsätze und Vorträge über Physik und Erkenntnistheorie* (Viehweg 1961 and 1984) *which I do not own.* This book was translated by

Robert Schlapp into the volume you cited. Schlapp, whose name means "slack", was a decent translator, but not a great one. I think checking is a good idea.

Pauli's German book is quite prominent, so there are many ways to get a copy. I will ask my distant cousin Klaus Hepp, emeritus at the ETH (and Planck medal winner!) to send me a scan of the article. In Vienna there are copies galore. I will also try interlibrary loan. U. of Toronto might well own a copy!

If you find the passage in the institute library or somewhere – maybe even Anton's private library – please send me a copy. I will proceed at this end.

Hans's Second Reply, 19-01-10

With the German and the English texts in front of me I can try to answer questions. In general the translation is good, but you are talking about nuances of meaning that I can't disentangle by changing a few words here and there. Instead, you have to tell me what's bothering you.

According to the paragraph preceding, Einstein thought that QM sacrifices objectivity. To which Pauli answers: Objectivity is vouchsafed in the following sense:

1. Theory can determine only the statistics of a series of experiments.
2. However, the observer cannot influence the unpredictable outcome of a specific, single measurement. (This sounds like "a kick that leads to unpredictable consequences ...")
3. Furthermore, the theory contains no references to the observer's personal characteristics or properties. The measurement can even be made by an apparatus.

How are the Viennese reacting to your views?

17-01-10 *Holocaust Memorial* (to H. C. von Baeyer)

BTW, I went to see the memorial today. Stirring, as all such memorials are. It was an empty square on a grey day, and the name kept ringing in my ear: Judenplatz.

I liked your imagery. I'm full tilt on quantum mechanics while here and getting close to writing the section in the paper where I say that each piece of the world has something within it that the rest of the world cannot get. (Your words start to work a little for me: It is not hidden variable, but something like soul.) Each piece of the world is a little like your box inside out—it contains a universe within.

17-01-10 *Oh Translator, 2* (to H. C. von Baeyer)

You can see why I want to get the passage right: I disagree with it! See words at top left corner of page 7:

> *Whose information?* "Mine!" *Information about what?* "The consequences (for *me*) of *my* actions upon the physical system!" It's all "I-I-me-me mine," as the Beatles sang.
>
> The answer to the first question surely comes as no surprise by now, but why on earth the answer for the second? "It's like watching a Quantum Bayesian shoot himself in the foot." Why something so egocentric, anthropocentric, psychology-laden, myopic, and positivistic as *the consequences (for me) of my actions upon the system*? Why not

simply say something nice and neutral like "the outcomes of measurements," or fall in line with Wolfgang Pauli and say,

> The objectivity of physics is … fully ensured in quantum mechanics in the following sense. Although in principle, according to the theory, it is in general only the statistics of series of experiments that is determined by laws, the observer is unable, even in the unpredictable single case, to influence the result of his observation—as for example the response of a counter at a particular instant of time. Further, personal qualities of the observer do not come into the theory in any way—the observation can be made by objective registering apparatus, the results of which are objectively available for anyone's inspection.

To the uninitiated, our answer to *Information about what?* surely appears to be a rash abandonment of realism. But it is the opposite. The answer we give is the very injunction that keeps the potentially conflicting statements of Wigner and his friend in check (Pauli's wouldn't have done that), and, more importantly, gives each agent a hook to the external world in spite of QBism's egocentric approach.

You see, for the QBist, the real world, the one each agent is embedded in—with its objects and events—is taken for granted. What is not taken for granted is each agent's access to the parts of it he has not touched.

Gotta give my talk tomorrow, but, of course, haven't prepared anything specific on it yet (having such a bank of old talks). First thing tomorrow I've got to get some new shoes—with the coming of REAL winter in Waterloo, I had forgotten that I had a hole in my more casual shoes. Then I'll get ready for my talk!

But sleep? Maybe a little later.

19-01-10 *Translation* (to H. C. von Baeyer)

von Baeyerism 58: *How are the Viennese reacting to your views?*

Probably a better reaction than Caves would give me!

More details tomorrow when I'm sober again. Just back from my time with Gerhard Zauner. Apparently—and I believe it!—he had proven the existence of MUBs in prime-power dimensions before (and not knowing of) Wootters. And SICs? He first got the idea of the concept around 1991 (mentioned in his master's thesis)! Though he (mistakenly, stubbornly) believed them to *only* exist in prime-power dimensions—he simply thought they could not exist otherwise. In 1996 he got the idea to check in composite dimensions, and (nearly trivially with a quick check) they existed there too, so he decided to follow through with a PhD on it (despite his initial advisor's discouragement). He kept going on about how two-designs (SICs and MUBs and a set of similar objects) were his objects of study because they had the potential to express quantum mechanics in a non-algebraic way. I didn't understand what he meant, but I definitely felt he was on the tip of something.

There is no doubt, he knows things that Marcus does not know, but he only sporadically thinks about all these things. For instance, in 2004 when in a hospital for 6 weeks with a head injury; he had amnesia for a bit, but the first thing that came back to him was a two-design question that he had first come to him in the early 1990s. I am much, much intrigued. Tomorrow, when I write, I'll tell you about Rankin-Bass and the "Island of Misfit Toys": I swear, Marcus, Zauner, and I all come from that place. Zauner never published (not even one paper!) because when he solicited the level of interest of anyone he could think of that should have been interested, they yawned. It is a tragedy for the development for quantum mechanics: People could have been thinking about SICs since 1991 if someone (probably nearly anyone) had just given some encouragement to the guy!

More tomorrow, as I say. Too much beer! I've had a great time here this visit. It is good to literally get in the lab (fascinating actually): I felt so alive this morning when I saw their Kochen-Specker experiment. I joked that I always talk of reality creation, but these guys, with that equipped lab table, are actually doing it. Anton said, "Well, you yourself are doing it all the time, you just don't know it." I agreed, and offered my daughters as examples.

I'll try to make my questions on the Pauli article pointed when I come back.

19-01-10 *The Reason I Like Your Axiom 1* (to Č. Brukner)

... particularly the second part, "All systems of the same information carrying capacity are equivalent" ... has to do with the ideas I'm trying to formulate on "dimension" as a capacity analogous to (or in the spirit of) gravitational mass. Somewhere in your axiom 1 is an analogy to the Eötvös experiment.

See the two elevator stories in: `http://arxiv.org/abs/quant-ph/0404122` (i.e., see the introduction and conclusion of this paper, skipping everything else). Platinum and magnalium were the two elements in the Eötvös experiment.

The yet-to-be-written capacities section will have some of that in it and some of the attached rant, originally written to Lucien. [See 16-10-09 note titled "The More and the Modest" to L. Hardy.] I'll definitely be citing your new paper too.

20-01-10 *Bohr was Bayesian?* (to C. Ferrie)

Good to hear from you. I'm in Vienna trying to get some SIC demonstrations going with the Zeilinger group, and trying to write this damned paper. Since you seem to need some good Bayesian-support-group amens, I'll go ahead and attach the paper as it stands. The last three sections still have a have a good ways to go, but much of the rest of it is stabilized.

I suspect there'll be aspects you like, and aspects you hate ... but I can't do much about that. The key, for your respect though, I think, is that I am a realist—there is no doubt about it—I am just not a hidden-variable realist. (And the same statement is true of Bohr.)

Anyway, such as it is, see attached. And hold tight through those quantum foundations classes! I'll see you in a couple weeks.

Chris's Preply

> The usual attacks were made on Bohr at the latest lecture of the interpretations course. Those with enough intellectual responsibility to read the Bohr quotes without joining in on the laughter saw that what Bohr actually said and the "Copenhagen Interpretation" philosophy which is attributed to him are in fact polar opposites. I sat there thinking Bohr was really a Bayesian but didn't know it. Then I thought that you must have come to this realization as well, provided you had read any Bohr, which I assumed you did. So I started searching your PDFs for "Bohr" and found that exact phrase!
>
> It is now obvious to me that you are far more well read in Bohr than I am. Ironically, the only things of Bohr I have read are cherry-picked quotes intended to support the

claims that Bohr was either schizophrenic or an evangelist for the "Copenhagen Interpretation", which I now doubt ever even existed. Fortunately, I committed to neither claim then and re-reading them now (after my Bayesian enlightenment) his words are more clear than unclear.

I felt sorry for Bohr that you were not there to defend his legacy. But at the time I could only sit in silence as I tried to think of a coherent defense of Bohr coupled with a rebuttal to the inevitable charges of solipsism or antirealism.

20-01-10 *Bohr was Bayesian?, 2* (to C. Ferrie)

And yes, the letters may not completely indicate it (all that crap is too sporadic), but I have tried to read everything Bohr wrote on quantum interpretation pretty darned carefully. (Also I've read plenty of things by the "Bohr scholars" Folse, Faye, Plotnitsky, etc.) He is not my hero in the game (Pauli is), but there is plenty in Bohr's gropings to think hard about and try to make clear "in the new modern way" (from a country song, Tom T. Hall). Attached is a picture of one of my visits to the old man himself. Below is a story about his desk. [See 03-02-04 note titled "The Land of Bohr" to G. L. Comer.]

I've got to say, I had the greatest experience the other day. At some point in a big discussion in Anton Zeilinger's office, I got up to write something on the chalkboard. The chalkboard was pretty rough, not nearly as nice as the one Kiki installed on our porch (it was more like the plywood she had once painted with chalkboard paint). . . . AND . . . I was just about to say something, making some smart remark that would come off as clever and sound like a joke—that's the thing I try to do all the time. Bad habit. But just in time I saw a brass plaque at the bottom of the board . . . and caught myself! It said it was Ludwig Boltzmann's chalkboard! I almost fell over. I was so excited; my heart skipped a beat, and I acted like a little boy. Thereafter I tried to find every excuse I could to write some more on it. Anton caught on, and that turned out to be my big joke.

Look, hold on tight. You've survived my bastardly wrath—you're a strong guy. Your Bayesianism is on the right track. And the very fact that you've had an epiphany (one of any sort, any sort whatsoever), already sets you apart from the rest of the foundations pack. Few foundations folk are ever really stirred by anything that takes them by surprise. Just learn to articulate yourself, defend yourself, and the battles themselves will lead to clarity. Crown yourself emperor, and you may get a nation.

20-01-10 *Translation, 2* (to H. C. von Baeyer)

I guess it's only the phrase "even in the unpredictable single case." It's the "even" that confuses me. Is it really part of his construction? Is it a term of contrast? For instance, who might have thought he could "influence" the "unpredictable single case"? "Nope, even in that case, I can't do it." That's the sort of thing that's confusing me and making me wonder whether it is an artifact of the translation or whether he really meant it that way?

Hans's Reply

OK – here we go. I have added some emphases.

> Pauli: **Obwohl** nach der Theorie im Prinzip im allgemeinen nur die Statistik von Versuchsreihen gesetzmässig bestimmt ist, kann der Beobachter **auch** im nicht voraussagbaren Einzelfall das Resultat der Beobachtung – wie zum

> Beispiel das Ansprechen eines Zählers in einem bestimmten Zeitmoment – nicht beeinflussen.
>
> Schlapp: **Although** in principle, according to the theory, it is in general only the statistics of series of experiments that is determined by laws, the observer is unable, **even** in the unpredictable single case, to influence the result of his observation—as for example the response of a counter at a particular instant of time.

The word **auch** could be translated as even, or as also. But the initial **although** demands the stronger contrast provided by **even**.

It seems to me that the fault is Pauli's, not in the word "auch" but in the phrase "kann… nicht" which is translated as "is unable." Maybe he should have said "does not" rather than "is unable." What I have in mind is the difference between classical and quantum. In a series of coin tosses the beautiful law emerges without the help of the observer. But the observer certainly *does* influence a single throw. If we knew enough about it, we could predict the throw. In QM, on the other hand, the observer does not influence the throw even in principle.

This uncertainty in principle was not Pauli's emphasis here. He wanted to establish the objectivity of physics – almost by introducing a detached observer who is at the mercy of the objective world out there. He can't even influence a single observation. And of course the bell curve is not his creation either.

Once your head is clear I would be grateful for your keen analysis.

20-01-10 *Translation* (to H. C. von Baeyer)

OK, I guess I've got nothing to say: So much for my "keen analysis." I will let it stew in the back of my head however—maybe your point about the classical coin toss is a good one.

Tonight is my last dinner with Anton, and tomorrow the taxi arrives at the hotel at 5:45 AM—I'm off for a day and an evening with Rüdiger Schack in London. Then it's to the hicksville of Texas, where I hope the cellular modem I had shipped there will work with my computer … otherwise I'm going to be in a world of cybermisery.

21-01-10 *Coffee and Spirits* (to Č. Brukner)

I think I'm going to go work in that café of the building where Exner, Hassenohrl, and Schrödinger worked. I'll show up at the institute early afternoon. Writing is a very painful thing for me to do. I'm Exner's spirit might give me a bit of guidance.

21-01-10 *God Likes Qutrits* (to C. Schaeff & the Vienna Qutrit Guys)

I just set a tab in my address book titled "The Vienna Qutrit Guys". If there's anyone out there who doesn't want to be in this list, let me know. Also if I've missed anyone who should be here, let me know that too.

I've been trying to articulate why the qutrit is a really important state space, and the best I have at the moment is this: Gleason's theorem shows that if you can characterize the set of probability functions allowed by the structure of quantum measurement in $d = 3$, then you can characterize it in all dimensions. So all the weight of the probabilistic interpretation of quantum mechanics bears down on the qutrit. From the experimental perspective, if an experimentalist wants to say "our experiments have mapped out the essence of quantum theory" then he has to be ready for the rejoinder, "Well then, do you have the capability of performing every kind of measurement in dimension 3, including POVMs?" "Unless you can do that, you have not fully explored the features of that state space." I want to say something like that, and find a way to make it pithy and convincing.

21-01-10 *On Realism* (to C. Ferrie)

Here are some notes I took on Henry Folse's papers on Bohr. I'd be curious to hear what you think of his reading of Bohr.

You're absolutely right on the six billion varieties of realism. When I said about myself that "I am a realist" I meant something somewhat close to the attached Martin Gardner quote—that the world is made of something that doesn't a priori require the existence of human minds. That's all I meant. But as you point out, you surely wouldn't be able to figure that out by looking at wiki.

Ferrie-ism 7: *It seems that what is overlooked in all these debates about "interpretations" is the assumptions that quantum theory is either "The Truth" or the next logical step to it. But what if that assumption is wrong?*

Well, even with all my love for trying to get the meaning of quantum theory straight, you would not hear me say that quantum theory is the Truth or the next logical step to it. Do you know the old Dr. Hook song, "On the Cover of the Rolling Stone"? If not, look at: `http://www.youtube.com/watch?v=-Ux3-a9RE1Q`. I just had a copy of the *Oxford Dictionary of American Quotations* sent to my mother so that we can flip through it when I visit her for her 81st birthday. One of the best merit badges I've ever attained in my life—they actually quote me! Here it is, I think (haven't actually seen the precise page yet):

> "There is no one way the world is because the world is still in creation, still being hammered out."

If there is not one way the world is because the world is still in creation, there is no everlasting scientific theory either. Quantum mechanics will one day meet its demise in one way or other. The point for me for interpreting it (in a way that it ought to be) is to get a safer suggestion for how to take the next step.

22-01-10 *Is the Big Bang Here?* (to A. Zeilinger)

See attached for the Wheeler quotes I promised you. Most of the quotes are from his presentation (which in their own way discuss the question above), but the actual question came from Richard Elvee at the end of the talk. Wheeler answered, "A lovely way to put it—'Is the big bang here?' I can imagine that we will someday have to answer your question with a 'yes'."

> It is difficult to escape asking a challenging question. Is the entirety of existence, rather than being built on particles or fields of force or multidimensional geometry, built upon billions upon billions of elementary quantum phenomena, those elementary acts

of "observer-participancy," those most ethereal of all the entities that have been forced upon us by the progress of science?

At first sight no question could seem more ridiculous. How fantastic the disproportion seems between the microscopic scale, of the typical quantum phenomenon and the gigantic reach of the universe! Disproportion, however, we have learned, does not give us the right to dismiss. Else how would we have discovered that the heat of the carload of molten pig iron goes back for its explanation to the random motions of billions of microscopic atoms and the shape of the elephant to the message on a microscopic strand of DNA? Is the term "big bang" merely a shorthand way to describe the cumulative consequence of billions upon billions of elementary acts of observer-participancy reaching back into the past?

Stepping stone though this question may be to a new outlook, it is beset before and beyond by traps. One trap is a misjudgment of the role of "consciousness." The other is an exaggerated estimate of the category of "time."

and

Guided by these warnings and encouraged by the view that there is nothing at all of physics more elementary than "elementary quantum phenomena," are we destined some coming century to see all of existence derived out of this utterly primitive unit? And on the way; are we not surely destined to find some single simple idea that will lend itself to statement in a single sentence, so compelling that we will all say to one another, "Oh, how simple!" and "How stupid we all were!" and "How could it have been otherwise?"

Have I been making the world sound like a very mysterious place? It is! But amid all mysteries, we remember those great words of Leibniz, "Although the whole of this life were said to be nothing but a dream and the physical world nothing but a phantasm, I should call this dream or phantasm real enough, if, using reason well, we were never deceived by it."

There are many sources of energy. There is solar energy and the energy of fossil fuels, coal and oil. There is the energy of uranium, but a source of energy greater than any of these is the energy of the human heart, the energy that makes us reach for an understanding of the universe and our place in it, our responsibilities, our opportunities, our hopes. No questions are more pertinent to this comprehension than "How did the world come into being?" and "How is the world constructed?" I know of no clue more likely to allow us someday to grasp an understanding of these questions than the quantum.

and

ELVEE: Dr. Wheeler, who was there to observe the universe when it started? Were we there? Or does it only start with our observation? Is the big bang here?

WHEELER: A lovely way to put it—"Is the big bang here?" I can imagine that we will someday have to answer your question with a "yes." If there is any conclusion that follows more strongly than another about the nature of time from the study of the quantum nature of space and time, it is the circumstance that the very idea of "before" and "after" is in some sense transcended.

There are two aspects of this idea. First, Einstein's theory of space and time tells us that in order to predict all of space and time for time to come, we have to know what the conditions of space are now and how fast they're changing. Only then do we have enough information to predict all the future. The uncertainty principle of quantum theory tells us that if we know the condition of space now, we cannot know how fast it's changing. Or if we know how fast it's changing now, we cannot know what the geometry is now. Nature is so built with this complementary feature that we cannot have the information we need to give a deterministic account of space geometry evolving with time.

That deterministic account of space evolving in time is what we mean by spacetime. Everything that we say in everyday language, about time is directly built on that concept. And with determinism out, the very ideas of before and after are also out. For practical everyday matters, this indeterminism, this indefinability of spacetime is of no concern. The uncertainties only show up effectively at distances of the order of 10^{-33} cm. Nobody at present has equipment fine enough to reach down to a distance so small.

What does all this have to do with the big bang? At the very beginning of time we know that—according to Einstein's account—the universe was indefinitely small. Things were indefinitely compact. When we talk about time when the universe itself is so fantastically small, we deal with a state of affairs where the very words "before" and "after" lose all meaning. This circumstance puts one heavy restriction on the usefulness of the word "time." There is another.

When we do our observations in the here and the now on photons, quanta of light, hunks of energy coming from distant astrophysical sources, we ourselves have an irretrievable part in bringing about that which appears to be happening. We can put it this way: that reality is, in a certain sense, made up of a few iron posts of definite observation between which we fill in, by an elaborate work of imagination and theory, all the rest of the construction that we call reality. In other words, we are wrong to think of the past as having a definite existence "out there." The past only exists insofar as it is present in the records of today. And what those records are is determined by what questions we ask. There is no other history than that. This is the sense in which we ourselves are involved in defining the conditions of individual elementary quantum phenomena way back at the beginning of the big bang.

Each elementary quantum phenomenon is an elementary act of "fact creation." That is incontestable. But is that the only mechanism needed to create all that is? Is what took place at the big bang the consequence of billions upon billions of these elementary processes, these elementary "acts of observer-participancy," these quantum phenomena? Have we had the mechanism of creation before our eyes all this time without recognizing the truth? That is the larger question implicit in your comment. Of all the deep questions of our time, I do not know one that is deeper, more exciting, more clearly pregnant with a great advance in our understanding.

25-01-10 *Silly Name Play* (to A. Zeilinger)

Just playing around with words on my flight to Texas. Here are some potential names for your Copenhagen conference.

- Breathing New Life into Bohr
- Breathing New Life into the Copenhagen Interpretation

- Quantum Information's Impact on the Copenhagen Interpretation
- New Perspectives on the Copenhagen Interpretation
- Lifting of the Fog from the North: New Perspectives on the Copenhagen Interpretation
- Reading Bohr, Heisenberg, and Pauli in the Language of Quantum Information
- Bohr's Relevance Today
- The Copenhagen Interpretation of Quantum Mechanics as a Modern Interpretation of Quantum Mechanics
- Teleporting Bohr to Modern Times: Quantum Information and the Copenhagen Interpretation
- Beam Me Up, Professor Bohr!
- Seeking Bohr's Guidance Afresh: How the Copenhagen Interpretation Enlightens Quantum Information
- Seeking the Soul of Quantum Information Theory ... One Bohr, Heisenberg, and Pauli at a Time
- Searching Copenhagen's Soul: Quantum Information and the Future of Physics
- I Like the Copenhagen Interpretation, Why Don't You?
- Bohr 2, Einstein 1 (the poster could show Einstein and Bohr in football clothing kicking qubits)
- Protecting Bohr with Fault-Tolerant Quantum Error Correction
- If Bohr Knew Then What We Know Now (What Would He Say Differently)
- Bohring the Tears Out of Einstein

The "lifting the fog from the north" remark refers to a quote of John Wheeler: "It may be, as one French physicist put it, 'the fog from the north,' but the Copenhagen interpretation remains the best interpretation of the quantum that we have." (I think the French physicist was de Broglie.)

28-01-10 *Morning Time, Cuero* (to D. B. L. Baker)

Getting your note yesterday still strikes me as such a strange coincidence. Everything you said was exactly on my mind—both, about not going home and never really leaving.

Attached is a paper I've been working on (and still have a good ways to go before finishing). [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] It is an example of your point on "never really leaving"—there are Texanisms and Cueroisms sprinkled throughout. Little things from our youth. For instance, when I wrote Footnote 7, I kept thinking of [Cindy X]'s dad calling Terry "you common son of a bitch" the night he caught them together. And I thought of a beer joint jukebox when I wrote the "hell hath no fury" line on page 5. There are others, as well. Funny too you write me about the Bible just as I was putting in Figure 2.

And the Ike story. Before coming to Cuero, I had a bunch of books shipped here from Amazon etc. Things that I couldn't get in Canada without waiting a ridiculous time, or having to pay exorbitant shipping costs. One of them was the *Oxford Dictionary of American Quotations*, which I discovered only a couple of weeks ago to really have a quote of mine—I found my name in the index of the Amazon preview of it, as I was buffing up my resume, but that was all I could see. (The dictionary contacted me years ago, asking permission, but as time went on I had forgotten about it, until I was searching for merit badges to beef up my resume to try to get a promotion.) It's really very flattering being there with Jefferson and Emerson and Mark Twain, even if you know it was someone's arbitrary decision. I'm rather proud of it really. Anyway, for a brief few minutes I thought about writing Linda Henderson with the news. But I backed off, thinking something exactly like your Ike Eichholz story.

Tomorrow morning early, I go back to Waterloo to face the cold and the snow. But then in two weeks, I go to Japan for my 10th time. (Nagoya this time; never been there actually.) It still smarts to hear my old brother-in-law from years back, saying, "Why would you want to go over there?"

Funny the synchronicity between us. I do think of you often. As I walked the streets of Vienna and the quiet square of Judenplatz, where there is a little memorial for the 65,000 Austrian Jews killed by the Nazis, I was thinking of the time you brought the Blue Danube into our tent at Boy Scouts. It was one of my first introductions to any of that side of the world. You brought a perspective that I had never seen. You were the only culture I ever knew in Cuero, and there's no doubt the little bit you pulled me into, I fought kicking and screaming. I sometimes wish I had it all to do over again—to try to keep the good with the good from that little town, but reject the parts that gave me such a slow start.

I learned yesterday from my mom that Michael's dad died a few weeks back. I should send Michael a note this morning.

02-02-10 *Permission?* (to C. E. Granade)

I'm working on an article that reviews my research program, and there's a spot in it that one of your neologisms [psi-ontologists] would be perfect for. See footnote 2 (and the paragraph to which it refers) on page 3 of the attached draft:

> Not to be confused with the Scientologists. This useful descriptor was coined by Chris Granade, a PSI student at Perimeter Institute, and brought to the author's attention by R. W. Spekkens, who immediately understood its psychological value.

The article is not actually going to *Physics in Canada*, despite the formatting—I just haven't changed it yet. I'm not yet quite sure where I will send the thing. In any case though, do I have permission to use your beautiful word? If no, then I'll strip it out. If yes, then is there anything you want me to change in the way that I give you credit?

03-02-10 *Three Cheers for Measurement* (to J. Rau)

Thanks for the revised version of your paper. [See J. Rau, "Measurement-Based Quantum Foundations," `http://arxiv.org/abs/0909.1036`.] "We are such stuff as quantum measurement is made on." That is a line that will end one of my own forthcoming compositions. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] I'll attach it as it stands—it still has a good ways to go before being finished, but perhaps you can start to see the outline of things to come. At the end, it will agree with you that the notion of measurement is the very primitive.

03-02-10 *Giving QM and Pragmatism a Chance to Flirt with Each Other* (to R. Schack, D. M. Appleby, H. C. von Baeyer)

The Templeton Foundation has announced two funding opportunities that seem to fit our proclivities. See:

`http://www.templeton.org/what_we_fund/2010_funding_priorities/`

I will be applying for the quantum one, and it looks like the free-will one very much matches up with Rüdiger's discussion with me at Windsor Castle. Maybe everyone might want to take a shot at it?

I have no firm ideas for myself yet, but I think the direction I want to pursue is to find a way to get quantum foundational thinkers to mix and mingle with pragmatist philosophers. The thought I have is that it is the physicists who could use some education, and I want to think of ways to foster that. If anyone has ever seen a model of similar ilk work before or has any ideas along such lines, I'd enjoy hearing.

09-02-10 *Village Voice* (to D. B. L. Baker)

Bakerism 6: *Thanks for attaching a sample of your latest writing. I definitely heard your voice as I read it. I have to admit that the only science journalism I ever read is stuff you forward to me so it is kind of hard to compare it to anything else. How much of what is published is written in such a colloquial, conversationalist style? Even the hard science/technical writing that you send me sounds the same. Maybe it's just that I know you that I hear it so clearly. What do your Ivy League and foreign colleagues say?*

I don't mean to swell your already large sized head but I would compare the experience somewhat to reading Jack Kerouac. Perhaps Ada was wrong and it is not Thoreau you have a feeling for. Even when I read Ginsberg or Burrows it's Kerouac I hear. He just seems/sounds more real so theirs must be borrowed. Maybe it's just a Cuero thing, having to explain things to people who may not be interested or even understand; people who never wanted to leave town or think about anything that doesn't happen there. Hal Ketchum's "Small Town Saturday Night" definitely on the jukebox there. "Lucy, you know the world must be flat 'cause people leave town, they never come back."

PS. You can use "Quantum Kerouac" if you want, though I do enjoy being cited.

Thanks for the nice compliment—the idea of being on the road with Kerouac is nice. But there are lightyears between him and me. The very big difference is that his writing was natural: He typed his first book on a paper towel roll, as I recall! And I don't think it took him more than a few weeks. I pain over every sentence, changing some probably a hundred times before I get it right. It's completely fake with me. Kerouac was the real deal.

But thanks! I'll send you the final version of the paper when it's finished. I got sidetracked with lots of things once I got back to Waterloo. My collaborator Appleby is here from England, and we've been at the chalkboard pretty much nonstop. Those damned consternating SICs.

10-02-10 *Boyle* (to D. Kobak)

Thank you very much for that—it was thought provoking. Soon I will put a paper on the `arXiv` titled "The Perimeter of Quantum Bayesianism" where I expand in more detail on the character and origin of quantum indeterminism. It is a bit as you say, but in place of "divine will" I would write "divine wills". You will see what I mean when I finally release the paper.

Dmitry's Preply

> As I feel that I'd better introduce myself: we met at PI during one of the quantum foundations schools several years ago, and then last year had a brief communication about the infamous anti-Bell works of De Raedt. This time I have a much more pleasant topic.
>
> I have recently stumbled upon a short text about Robert Boyle – an extremely interesting account of his theological thoughts driving his scientific research. Here it is: `http://shkrobius.livejournal.com/189428.html` (don't be discouraged by the domain: it's a blog of a chemist and one of the most educated people I know). In short, he is saying that Boyle – who basically invented modern scientific method and was largely responsible for starting science as an enterprise as we know it – that Boyle did all that trying to disprove the idea of natural physical laws. He didn't believe in laws, he believed in Divine will, and so for example wanted to disprove, and did disprove the "law" that Nature "abhors vacuum". I will stop retelling this story and encourage you instead to read that post.
>
> The reason I think it might be interesting for you, is that it strikes me how similar Boyle's views would be to the Bayesian view of QM, where quantum laws are just conceits of mind and there seems to be no law whatsoever behind an outcome of quantum measurement – nothing but the pure Divine will. Would Boyle be a Bayesian?...
>
> I also wrote this in a bit more detail in the comments section of that post. Actually, before writing to you I made a search for "Boyle" in all your samizdats and found zero results. So I thought you might be interested. If you indeed are, I would be happy to know what you think about it.

10-02-10 *Three Cheers for Measurement, 2* (to J. Rau)

Rau-ism 1: *Given that you are such a philosophically minded person (after all, I owe to you my first encounter with American pragmatism), I was wondering whether you had ever come across the Vienna Circle? I first learnt about this group of philosophers (Schlick, Carnap, Neurath, Frank, ...) when I visited Časlav Brukner and Anton Zeilinger in Vienna last October, and I discovered that their "logical empiricism" is very much in tune with the Copenhagen interpretation of quantum theory. In fact, there was a fruitful exchange between members of the Vienna Circle and Niels Bohr. I attach a little paper discussing this exchange – maybe you'll enjoy it.*

Thank you for the Faye article. I have read him before and find him very interesting. Particularly, I read his book *Niels Bohr, His Heritage and Legacy* in very careful detail about 15 years ago. Particularly it was very important for me in learning about Høffding (which connects to my present infatuation with pragmatism, James and Høffding were correspondents). It would be so nice to read this all again, knowing (thinking) what I know (think) now. Faye and Henry Folse, by the way, are good friends. I visited Henry at his magnificent home in New Orleans in October, and he told me a story of the time Faye lived in his basement there. Anyway, though they are good friends, they have very different readings of Bohr. And for my money, I think there is more truth in Folse's reading—which of course can only mean operationally that Folse's reading agrees with my own reading of Bohr. (I may not reflect it in my writings, but I have put significant scholarly effort into Bohr himself.) Still, the healthiest reading involves both these countering opinions. I will study the article you sent me with care.

11-02-10 *Itamar* (to M. Hemmo)

This hits me like a hammer. It is a sad day. He was a great man, from whom I learned so much.

Meir's Preply

Dear friends,

I am very sorry to report that Itamar died last night. There are no words to express this great loss for all of us.

The funeral will take place tomorrow, Friday 12 February, at 10:00 in Jerusalem.

Meir

11-02-10 *Itamar* (to W. G. Demopoulos)

I had already heard about Itamar from Meir and sent my condolences. It is very tough. Itamar was a great man. I learned all the deep, secret details of Gleason's theorem from him.

My mind this morning keeps coming back to a quote of William James:

> To anyone who has ever looked on the face of a dead child or parent the mere fact that matter *could* have taken for a time that precious form, ought to make matter sacred ever after. It makes no difference what the principle of life may be, material or immaterial, matter at any rate co-operates, lends itself to all life's purposes. That beloved incarnation was among matter's possibilities.

I want say that Itamar's work in quantum theory was a foundation for the same conclusion, having come at it from a more rational side. Matter is sacred.

I will remember him all my life.

15-02-10 *Notes* (to M. B. Ruskai)

Yes, thank you, that would be nice. There is a quote of Primas's that I keep in my computer that I think is very deep:

> It is a tacit assumption of all engineering and experimental sciences that nature can be manipulated and that the initial conditions required by experiments can be created by interventions using means external to the object under investigation. That is, *we take it for granted that the experimenter has a certain freedom of action which is not accounted for by first principles of physics.* Man's free will implies the ability to carry out actions, it constitutes his essence as an actor. Without this freedom of choice, experiments would be impossible. *The framework of experimental science requires this freedom of action as a constitutive though tacit presupposition.*

I'm off to Japan early tomorrow morning.

Mary Beth's Preply

> Don't know if you're familiar with the work of Hans Primas on quantum theory. Going through some papers, I found a set of lecture notes from a course he gave at the ETH about 1970 on
>
> Problem der Interpretation der Quantnemechanik Grosser Molekular Systeme
>
> and I wonder if you might be interested. I also have a few preprints from that time.
>
> If you'd like them, I'll see if Bei Zeng can bring them back to Waterloo.

17-02-10 *Jet Lagged Titular Thought* (to R. Schack, D. M. Appleby, H. C. von Baeyer)

"Institute for Quantum and Freedom"
Better suggestions?

17-02-10 *Strong Radio Silence* (to M. Schlosshauer)

You weren't kidding about "radio silence": I'm the king of it! Not only am I not in Vienna anymore, but last night I arrived in Japan. In Nagoya. And to my surprise this morning, I learned that the meeting starts today, not tomorrow as I had thought! So my head is really in the clouds somewhere.

Let us shoot for early June with respect to my getting the interview paper in.

It must be getting very close to your baby's due date now. I hope everything is going OK in that regard.

On my flight over, I read your paper with Camilleri on Bohr's doctrine of classical concepts. It was quite good, and I learned a lot. Particularly, I did not know about the controversy between Bohr and Heisenberg on the movability of the cut/schnitt. I want to delve into that more carefully as I get a chance. I was quite surprised by this line of Heisenberg's too: "[A] dividing line must be drawn between, on the one hand, the apparatus which we use as an aid in putting the question and thus, in a way, treat as part of ourselves, and on the other hand, the physical systems we wish to investigate." I thought it was Pauli only who went so far as to take the measuring device as a prosthetic hand. That point is crucial for QBism in fact; so clearly I should learn more about its roots.

Attached finally is a bit more careful version of what I brought up with you after the New Year, my little epiphany. I am not completely pleased with the exposition—it remains too sketchy—but it is starting to take shape, and it gives me something to try to do better in a proper article later. The argument starts kicking in at page 5 with the discussion of Wigner's friend, comes back a little again at the end of the Seeking SICs section, and then climbs to a fevered pitch starting at page 16 in the Universal Capacity section.

You'll see that everything has grown a bit since the last version I sent you. I took into account almost all your comments, for instance. And indeed, your comments helped set me up for my New Year's epiphany—so I owe you a lot. One thing I haven't done yet that you suggested is to have a discussion on the subjective-Bayesian nature of unitaries—that's actually crucial now, so it's coming. In the present version, I am a bit harsh on the "quantum-to-classical transitionists."

I hope you will be provisionally tolerant of me and not get too angry. I can work harder to set the tone right and still make my point. Those paragraphs were written on the flight over here; so they're pretty raw. They are like Homer Simpson thinking out loud. Still, I thought I'd go ahead and show them to you now and take into account your reactions (and levels of anger) as they are for real, rather than try to anticipate them.

Scanning over your note again … On your question about Anton: Yes, it seems he is actually going to get one of his teams to do a qutrit SIC demo. So tugging on his shirt was a good thing. In fact, he's now quite interested in MUBs and SICs and will be holding a meeting in Vienna on the subject in July.

My apology again on the long silence. One should not go quiet on friends. But some not-good things happened right after Vienna (an old friend died), and I just ended up shutting up for a while.

17-02-10 *Oh, Repository* (to R. Schack)

Isn't your new "potential schism" the very core of our considerations already? The Law of Total Probability is operative when we expect to actually have an experience E_i. The Born Rule (urgleichung) tells us how to modify that old estimate into something relevant when we don't expect an intermediate E_i experience. It is a postulate of quantum mechanics, over and above coherence considerations.

I'm probably still not getting your point.

Random thought. Just recording it even it's not relevant to the present discussion. Decoherence is really just a cheap way of extending "the self" without recognizing it as a such. It tries to say, "that experience is really there waiting for me to have it" when in fact, all it is doing is lengthening the arm of the agent (with a complicated story in between). In this regard, William James's notion of self is important: "In its widest possible sense, however, a man's self is the sum total of all that he can call his, not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works, his lands and horses, and yacht and bank account."

Rüdiger's Preply

> I hope you made it safely to Japan!
>
> I read the first 10 pages of the paper carefully and have some comments (see below), but when reading the Wigner's friend bit and then the measurement in the sky story, I got hung up on a problem that may turn out to be of central importance.
>
> If I intend to perform the experiment in the sky, I expect to get outcome E_i with probability $P(E_i)$, and I expect that in the case I actually experience E_i my new probability for D_j will be my current $P(D_j|E_i)$. Reflection then gives me the law of total probability.
>
> But what if I only *imagine* performing the experiment? I.e., what if I believe there is decoherence (I can see you shudder!) and I want to use reflection to set my probabilities according to the law of total probability as above. I would have to say my beliefs are *as if* I expected to actually experience E_i. This sounds like vile as-if-ism to me. If I don't expect to actually experience E_i, how to I justify to myself setting $P(D_j)$ according to the law of total probability in one case, and according to the Urgleichung in the other case?
>
> Does this have the potential for a schism?

18-02-10 *Eliot* (to M. Schlosshauer)

This synchronicity thing between us seems to have no end. I was on a plane reading your paper, thinking about how you think, thinking about how you will react, and all kinds of things like that. Then, along the way (as I stop in Dallas), Rüdiger finally sends his QB-decoherence draft. Then, I learn that you and your wife were in Copenhagen participating in the biggest event of your lives.

Many, many congratulations. You will have a strong son; he was born in the land of Niels Bohr.

19-02-10 *Potentially Perfect Potential Aaron Problem* (to A. Fenyes & D. M. Appleby)

The PPPAP. [Notation and equations refer to D. M. Appleby, S. T. Flammia, and C. A. Fuchs, "The Lie Algebraic Significance of Symmetric Informationally Complete Measurements," `arXiv:1001.0004 [quant-ph]`.]

Marcus has known for some time that I keep hoping that the R_r will imply everything we need to know about the J_r. If the raw shape of quantum state space is more fundamental than anything else, then the R_r have to be more fundamental. I still hold out this hope.

This morning as I was putting together my talk for Japan, I was struck that we haven't asked the obvious question. What is the analogue of Theorem 7 for the Jordan algebra induced by the anti-commutator?

In other words, equip the vector space of Hermitian matrices with a symmetric product (the anti-commutator), and let Π_t be a basis for the algebra. (No assumptions about Π_t being a SIC or anything.) Now define a matrix R_r by Eq. (16) in the Lie algebra paper, and suppose the R_r have an analogue of the Q-Q^T property. Namely that the R_r can be written as in Eq. (103), where the $\|e_r\rangle\rangle$ are defined as EXACTLY as in Eq. (116) and the Q_r are rank $d-1$ projectors satisfying Eq. (105). Does it follow in analogue to Theorem 7 that a SIC must exist?

If it does, this will be immensely exciting. How are the Π_t related to this SIC, in analogue to Eq. (164)? And if such a theorem does not follow, then what does? What is the most one can say about the basis Π_t?

Aaron, I think this could be a PSI project for you (or the beginning of one) if you are interested. If you get a positive result, it may not have so many applications to the SIC existence problem as the Lie algebra version might (I say might), but for my money it'll be a deeper result about quantum mechanics itself.

Part of the point is, if there is a result like this, then one has a nice way to start with a Jordan algebra and then define a natural Lie algebra associated with it.

Let me know what you guys think? Marcus, what are your feelings/thoughts?

19-02-10 *Postscript* (to D. M. Appleby)

Applebyism 39: *Hope you are enjoying Japan.*

No enjoyment yet. I've just been writing, working on ONR things, trying to plan (unsuccessfully) my PI colloquium, and trying to get Belavkin and Ozawa to respect us. In the wee hours of the morning when I have my hot bath, I've been trying to think about gravity. The bold proposition is that simply the presence of a quantum system of any kind in an otherwise empty region of space must bend light around it. Just like those beautiful pictures we saw at the colloquium on gravitational lensing. No talk of coupling to the EM field (in the usual sense), no talk of the quantum state one ascribes to it, simply the presence of a system. There has to be an equivalence principle lurking here.

I am very intrigued by what you seem to have found. You are right about the flavor seeming to hint that all SICs are WH SICs or simple transformations of them.

Write as you wish, but there is no necessity. If you prove SIC existence, I'll buy you the next bottle of single malt! And a very, very rare one at that.

21-02-10 *Pierce Quote* (to V. P. Belavkin)

J. R. Pierce (IEEE Trans. Inf. Theory, vol **IT-19**, 3–8 (1973)):

> I think that I have never met a physicist who understood information theory. I wish that physicists would stop talking about reformulating information theory and would give us a general expression for the capacity of a channel with quantum effects taken into account rather than a number of special cases.

21-02-10 *Ways of Expression* (to D. M. Appleby)

Here's the way I expressed it tonight to Gen Kimura: If SICs exist, then there's a beautiful way of thinking of quantum state space that had not been thought of before. It is a union of a bunch of regular simplices, each over d^2 extreme points. "Quantum state space is a union of regular simplices."[216]

22-02-10 *Schelling, Quantum, Creation* (to I. Ojima)

I am very happy about our chance conversation today—now I have another refreshing thinker on nature's creativity to read and think about. Schelling strikes me as very interesting.

Attached is the essay I've been writing at this very meeting. It is still unfinished, but it already demonstrates why I was so interested in my conversation with you today.

Here are a few sentences from my paper that may whet your appetite:

- Page 6, figure caption)
  Measurement devices are depicted as prosthetic hands to make it clear that they should be considered an integral part of the agent. The sparks between the measurement-device hand and the quantum system represent the idea that the consequence of each quantum measurement is a unique creation within the previously existing universe.

- Page 8)
  QBism says when an agent reaches out and touches a quantum system—when he performs a quantum measurement—that process gives rise to birth in a nearly literal sense. With the action of the agent upon the system, the no-go theorems of Bell and Kochen-Specker assert that something new comes into the world that wasn't there previously: It is the "outcome," the unpredictable consequence for the very agent who took the action. John Archibald Wheeler said it this way, and we follow suit, "Each elementary quantum phenomenon is an elementary act of 'fact creation.' That is incontestable."

[216] See Footnote 218 for a technical, but remediable, mistake in the original statement of this.

- Page 14)
  The Quantum Bayesian, however, with a different understanding of probability and a commitment to the idea that quantum measurement outcomes are personal, draws a different conclusion from the theorem. And it is profound: Bell's theorem for the Quantum Bayesian is the deepest reaching statement ever drawn from quantum theory. It is that quantum measurements are moments of creation.

- Page 20)
  To put it still differently, and now in the metaphor of music, a jazz musician might declare that a tune once heard thereafter plays its most crucial role as a substrate for something new. It is the fleeting solid ground upon which something new can be born.

Anyway, those are the sorts of things you'll find argued for in the essay.

Below are my already favorite Schelling quotes:

> Has creation a final goal? And if so, why was it not reached at once? Why was the consummation not realized from the beginning? To these questions there is but one answer: Because God is Life, and not merely Being.
>
> — Schelling, *Philosophical Inquiries into the Nature of Human Freedom*, 1809

and

> Only he who has tasted freedom can feel the desire to make over everything in its image, to spread it throughout the whole universe.
>
> — Schelling, *Philosophical Inquiries into the Nature of Human Freedom*, 1809

22-02-10 *Hilbert-Space Dimension in Second-Quantized Theories* (to N. C. Menicucci & E. G. Cavalcanti)

Menicuccism 3: *Eric Cavalcanti raised a question that I've thought about, as well, in relation to your claim in "Quantum Mechanics as Quantum Information (and only a little more)" that the dimension of a Hilbert space is real: what happens if the dimension is not well defined, as is the case in QED? One can have a superposition of photon-number states, which results in a superposition of number of qubits available and thus a superposition of dimensions of the overall state space (e.g., H/V qubit encoding). Is the answer trivial (e.g., the overall dimension is infinite, which is a real property of the world), or is there something more subtle at work here? Have you (or anyone else that you know of) written about this?*

No I haven't written about this before, but I am just about going to! There'll be a discussion in the attached paper which I am in the process of writing at this very moment. I'll go ahead and send you this much, and sketch very briefly an answer to your present question. The paper helps give the setting, and the precise sense in which I'm thinking of quantum dimension now.

Anyway, to answer your question:

1. If quantum fields really did exist, the answer would be the one you call the trivial one: dimension $= \infty$.

2. Even if they don't exist, one might think of this case as like the moral equivalent of an infinite heat bath: a limiting case or approximation.

3. But in fact, I'm starting to suspect the nonexistence of infinite dim Hilbert spaces. This is because I am starting to take this holographic principle stuff more seriously. But I think these guys are misidentifying the principle as giving an entropy bound (how could you do that with a subjective quantity?). What I suspect it really gives is a dimension bound.

4. In any case, if such is the case, it keeps my "dimension as valence" thinking from collapsing. So, something like that ought to be the case.

These are the things I'll eventually write more thoroughly (and more poetically!) in the completed paper.

I hope you find some food for thought in the paper, even as it stands.

Best wishes from Nagoya, Japan (in the middle of a talk on "nonlocality" ... as if such crap were true).

23-02-10 *Kitaro and James* (to I. Ojima)

I have now read this article on Nishida Kitaro's conception of "pure experience" in contrast to William James's conception of it. [See D. Dilworth, "The Initial Formations of 'Pure Experience' in Nishida Kitaro and William James," *Monumenta Nipponica* **24**(1/2), 93–111 (1969).] Indeed—a bit as you warned—Kitaro does not look like such an interesting philosopher to me.

The article "Nature in American Philosophy," by Stefano Poggi, on Google Books looks to be very interesting. I have just started reading it, but it looks like it does a good job of tracing Schelling's influence on Fechner, and Fechner's influence in turn on James.

26-02-10 *Kitaro and James, 2* (to I. Ojima)

Thank you so much for the papers. I have so far read the first [I. Ojima, "Nature vs. Science," Acta Inst. Phil. Aesth. **10**, 55 (1992)] and 1/4 of the second on my flight back to North America. These are very deep papers, and we are very much on similar wavelengths. Technically, I enjoyed your argument on the nonexistence of (ontic) repeatability in the first paper. You gave repeatability a logical inconsistency (rather than an empirical establishment) that I had not appreciated before. To get repeatability you call for "approximation", and in my own naming scheme, I call it "judgment". But it is much the same thing—certainly our hearts are in the same place.

I look forward to finishing your longer paper as I have a chance.

I have gotten further in the writing of the paper I sent you last week. When it is finally finished, I will surely send it to you.

01-03-10 *My Lectures for You* (to J. Emerson & R. Laflamme)

I plan to work through the night putting the finishing touches on the attached (still slightly incomplete) essay. [See `http://arxiv.org/abs/1003.5209`.] Here's what I'd like to do for my lectures for your course. Let me check that you're OK with this.

Rather than give the students a post-lecture homework assignment of any sort, I would like to give them a pre-lecture suggestion or assignment: That they read the attached essay before I show up next week (tomorrow morning's version more precisely). Then through the week or over the weekend, they can email me any questions or discussion points they would like to be brought up in my lectures. In place of rigid lectures then, I'd try to spend much or most of the time replying to their questions, dodging tomatoes, etc. Similarly with any issues that arise spontaneously in the class. I think they'd get a lot more out of this than "yet another lecture on yet another point of view." I.e., it puts me on the spot, and they (and you) can try to pin me down as much as they (and you) want! The essay was written with an intention of being entertaining and provocative; so it should be quite easy reading for them.

If it's OK with you, I'd like to spend less than one minute tomorrow saying this is how I want to run things for my lecture slots. And I'd put a pile of these papers at the front so that they could pick a copy up. (How many students are there?) Then I'd quickly disappear so as not to interrupt your present lecturer.

Please, let me know, and I'll come prepared to tomorrow's lecture with a pile in hand. And I hope to give you some fun next week.

01-03-10 *Curiosity* (to C. Ferrie)

Thanks for this. It is a deeply thoughtful essay, and I enjoyed it very much. You have no need to worry that you cannot "articulate so well"—it simply is not true. Your message came across loud and clear. Are you a different person than when I met you? Or was I just stubbornly blind/deaf?

Just because I caught them, I'll point to some of your typos: ...

Now, not a typo, just a disagreement:

Ferrie-ism 8: *"Humans possess freedom of will. This is a privilege which the Earth, for example, does not seem to benefit from."*

Want to bet?

02-03-10 *Our Unfinished Universe* (to A. Zeilinger and Č. Brukner)

Well, I have finally finished (within epsilon at least), that essay I was writing on in Vienna—the one in which I try to capture my whole (present) worldview. I hope you will both enjoy and not find the ending sections (which were not yet written in Vienna) too outlandish! If you have any comments on anything you think I should change or add or delete!, please do tell me. I'm hoping to post the thing at the end of the week.

Časlav, in the footnote on Kofler, you'll see that our conversation the last days of the visit very much influenced me.

Brothers in arms for a creative universe!

02-03-10 *Wheelerism Full-Throttle!* (to B. W. Schumacher)

I'm nearly finished with an essay I've been pouring my soul into, and I thought I'd send you a draft before posting since it's chock-full of Wheelerish stuff. I attributed Wheeler's "great smoky dragon" to a paper by he and Warner Miller; do you happen to know whether that is the right one? Anyway, if you see anything that should be corrected or amended, please don't hesitate to say. I'll probably shoot for posting this Friday.

Hope you and Carol and the whole family are well.

Ben's Reply

> I won't be able to read this for a couple of days. Looks fun! The Great Smoky Dragon cartoon appears as Plate 27 (p. 399) of the 1988 *Between Quantum and Cosmos* book. The notes at the end (p. 619–20) give a reference that appears to agree with your Ref. 70.

02-03-10 *Nearly Final Product* (to H. C. von Baeyer)

Attached is the nearly final product; within epsilon it is finished! You will find it has changed substantially since the partial draft you read. Quantum measurement, for instance, is no longer a kick, but a birth! I hope you enjoy and not find the whole thing too outlandish.

From YOU (but only you), editorial comments always welcome! Don't be shy if you see something that really could be improved.

Hans's Replies

> Dear Chris – witnessing a birth is vastly more pleasant than giving or receiving a kick! I am reading carefully, and am only on page ten. Soon I will send you a list of small suggestions. . . .
>
> As I read that the world is not made of [Essence X] I recall a relevant quote by the American poet Muriel Rukeyser: "The world is not made of atoms; it is made of stories."

02-03-10 *Fatherhood/Paperhood* (to M. Schlosshauer)

How are you and your wife making it? Hopefully it's been relatively easy; my recollection is that babies are pretty easy at the beginning.

Attached is an actual (potentially) FINAL version of the essay. I bet you thought you'd never see that coming. I beefed up the quantum-to-classical transition discussion at bit—I hope it helps matters a small amount. Only one thing in your "to do" list that I did not do: I just couldn't bring myself to writing on the subjective meaning of unitaries. I lost steam, and couldn't find an easy place for it to fit into the present structure. If you do think the essay is really lacking because of this, please do tell me, and I'll rethink. A day or two of rest often brings a second wind. I value your input; I hope you know that. And I'll value your criticism as well: If I can't get it right with you, I won't be able to get it right with anyone.

I've got a big talk coming up next week that has me completely frightened: It's part of trying to establish myself a permanent position here. Who would think a 45 year old man could be scared like a little boy? But it happens.

I met one of your colleagues from Copenhagen in Japan, Teiko Heinosaari. He seems to be a good guy. He's even working on SICs now!

Please give my regards to your wife. And say hello to Niels from close up; he's been on my mind again lately.

02-03-10 *Another Attempt To Get You To Call Me a Realist!* (to H. M. Wiseman)

Since I implicate you in this essay, I should probably send it to you before posting! [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] I drew one of the figures for you. Seriously, thanks for the great challenges you've given me over the years. Any comments you have for improvement—so long as they're relatively easy to implement—will be welcome.

03-03-10 *My Worldview* (to A. Kent)

Since I implicate you in one of my sly insults, I probably ought to send you this essay before posting it:

> Or take the Everettians. Their world purports to have no observers, but then it has no probabilities either. What are we then to do with the Born Rule for calculating quantum probabilities, the very core of the theory from the experimental perspective? Throw it away and say it never mattered? It is true that much effort has been made by the Everettians to rederive the rule, but to many in the outside world, it looks like the success of these derivations depends upon where they are assessed—for instance, whether in Oxford [9] or Cambridge [10].

I hope you enjoy (some of) it. It is my latest attempt to form a worldview.

I hope you come to PI again soon; it's always a blander place when you're not around.

03-03-10 *A Jaume Stamp?* (to J. Gomis)

I finally finished that essay I was telling you about. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] It's attached. I didn't do as good a job connecting Hilbert-space dimension to mass/energy as I had hoped to, but I do believe there is something much deeper to the analogy between "fungibility of quantum information" and the equivalence principle than meets the eye. Anyway, the essay expresses the direction of thought I'll be exploring.

03-03-10 *Novelty, Creation, QBism* (to L. Smolin)

I haven't seen you in a long time, and I've just written something that I hope will intrigue you. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] It's an essay on my worldview, at least as it stands at the moment. See attached. Section V onward, particularly, is about how my QBism research program mounts onto the idea of a pluriverse in constant creation. I don't know to what extent my "interiority" has a connection to Verlinde's "our starting point was that space has one emergent holographic direction," but I am tempted to think something like this is going on . . . but from a completely different end of the philosophical spectrum. I feel it is a big research program ahead.

I hope to post the essay next week maybe. If you have any feedback on how the draft might be improved or made clearer, I'd much appreciate hearing your thoughts.

04-03-10 *My Worldview, 2* (to A. Kent)

Kentism 9: *Thanks. I haven't had a chance to read yet, beyond searching for the flagged implication. I have to be honest, I'm rather disappointed by it: it seems a cheap and lazy way of avoiding serious discussion, and quite inappropriate for an academic article.*

Guilty. And I misstated when I said "sly"—I forgot the extra *i* and *l*. Silly. But the cheapness of it was meant to be a direct comparison to John Bell's statement. The beginning of the paragraph announced it so. There are some things, we each of us, just do not take seriously. For instance a thoroughgoing epistemic interpretation of quantum states, as Bell didn't (and maybe you, don't). Anyway, much of the remainder of the article is meant to show how one can recover from the sting of Bell's original cheap shot.

The only thing I worry about now is that you won't look at the rest of the article, where I try to explain the mindset that would make Bell's remark a "cheap shot" rather than a "forceful way of conveying the essential point".

04-03-10 *Edits* (to H. C. von Baeyer)

von Baeyerism 59: *Figure 2 is unnecessary, and the inclusion of the ass among the Ten Commandments is wrong.*

See: `http://en.wikipedia.org/wiki/Ten_Commandments`:

> The firstborn of a donkey you shall redeem with a lamb, or if you will not redeem it you shall break its neck. All the firstborn of your sons you shall redeem.

And from another page:

> But the firstling of an ass thou shalt redeem with a lamb: and if thou redeem him not, then shalt thou break his neck. All the firstborn of thy sons thou shalt redeem. And none shall appear before me empty.

But you are right, strictly speaking, it is not one of the 10 Commandments. I'll fix the drawing and caption before posting. It was an attempt at humor; it definitely works in talks, but maybe it would not in the paper ... and it might even be offensive to some. Still, I don't think the discussion is unnecessary: You are the rare physicist who has ever heard of a normative rule. And generally, in both physics and philosophy of science, the Born Rule is thought to be of a character as Maxwell's and Einstein's equations: A statement of what is in the world. A quantum state IS, and the probability it carries with it IS. I want to hammer it home that it should be thought of as a recommendation, nothing more.

I keep drawing myself away from my talk preparations to look at your notes!

04-03-10 *Fuchsianism / Ones For Which* (to R. Blume-Kohout)

I finally put it all on paper, everything in my head. It's empty now. Anyway, you always say that whatever I believe of quantum mechanics is a mystery to you: I hope it won't remain so if you read this paper.

And don't forget:

1. Scaffolding paragraph.

2. "Hilbert-Space Dimension as a Universal Capacity" section

3. And I might as well point out these lines too:

> You see, for the QBist, the real world, the one both agents are embedded in—with its objects and events—is taken for granted. What is not taken for granted is each agent's access to the parts of it he has not touched. Wigner holds two thoughts in his head: 1) that his friend interacted with a quantum system, eliciting some consequence of the interaction for himself, and 2) after the specified time, for any of Wigner's own further interactions with his friend or system or both, he ought to gamble upon their consequences according to $U(\rho \otimes |\psi\rangle\langle\psi|)U^\dagger$. One statement refers to the friend's potential experiences, and one refers to Wigner's own. So long as it is kept clear that $U(\rho \otimes |\psi\rangle\langle\psi|)U^\dagger$ refers to the latter—how Wigner should gamble upon the things that might happen to him—making no statement whatsoever about the former, there is no conflict. The world is filled with all the same things it was before quantum theory came along, like each of our experiences, that rock and that tree, and all the other things under the sun; it is just that quantum theory provides a calculus for gambling on each agent's own experiences—it doesn't give anything else than that. It certainly doesn't give one agent the ability to conceptually pierce the other agent's personal experience. It is true that with enough effort Wigner could enact Eq. (1), causing him to predict that his friend will have amnesia to any future questions on his old measurement results. But we always knew Wigner could do that—a mallet to the head would have been good enough.

05-03-10 *Laws of Nature Conference* (to G. Musser)

I finally finished that essay that I had shown you part of. It's attached. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] Do you think you can cut it with meat cleaver and scalpel into something for Sci Am? Would you be interested? Section V is definitely the antithesis of that David Albert article you had.

05-03-10 *Torture* (to A. Kent)

... like a grain of sand in an oyster shell, you have been with me for the last 24 hours. It makes no pearl with me, but I do come to appreciate the annoyance the little fellow feels.

I do insist on insulting Everett, but your scolding (beside making me feel ashamed) did make me realize that I wasn't getting to the essential point. I try to do that in the present version. I will probably tweak it a hundred times more before posting, but the final result will be something more like this than yesterday's:

> Or take the Everettians. Their world purports to have no observers, but then it has no probabilities either. What are we then to do with the Born Rule for calculating quantum probabilities? Throw it away and say it never mattered? It is true that quite an effort has been made by the Everettians to rederive the rule from decision theory. Some sympathizers think it works [9], some don't [10]. But outside the *sprachspiele* who could ever believe? No amount of sophistry can make "decision" anything other than a hollow concept in a deterministic universe.

05-03-10 *Higher Road* (to R. W. Spekkens)

By the way, I did choose a (slightly) higher road with the insult. Now it targets the idea more than the people. You were right. I also rejigged the footnote on Granade and you to something a little less lame:

> Not to be confused with Scientologists. This neologism was coined by Chris Granade, a Perimeter Scholars International student at Perimeter Institute, and brought to the author's attention by R. W. Spekkens, who pounced on it for its beautiful subtlety.

If you're interested, latest version attached.

05-03-10 *A Wild Shock!* (to O. C. O. Dahlsten)

Thanks. On Dakić and Brukner, I personally don't think "information capacity" is the right idea—it is still too close to treating a quantum system as a bucket into which one can pour a certain amount of stuff—but I felt I should cite them in some way. Maybe I'll leave the reference, but take the quote out. I liked Hardy's (old) way of putting it better. It is just that no distinction of quantum theoretical properties can be made between two systems of dimension N.

I look forward to seeing your draft. In my own way, I might want to see systems dropped as well—that's partly what the mumbo-jumbo about "pure experience" was about at the end. But things are not clear in my mind yet. Extra stimulation would be good!

Oscar's Preply

> Thanks a lot, I enjoyed reading it!
>
> I also take PSI-epistemic view, that PSI represents our knowledge about something out there. I even agree that the Hilbert space dimension seems to be something real out there.
>
> A small remark on the Dakić and Brukner paper which you mention in a footnote, which I otherwise like, is that 'information capacity' may be a strange choice of word. Many other hypothetical systems, including gbits have the same information capacity as a qubit, i.e., one can only communicate one classical bit with them (in the sense that Bob can distinguish at most two of Alice's preparations after Alice has sent him the system). We (Roger, Renato and I) are writing something about these things at the moment, i.e., about different notions of how much information a system can be used to convey.
>
> In my way of thinking of things I am inclined to think that the notion of system itself should actually be dropped. Am writing this up at the moment and will send you a draft in case you have time/interest to look at it.

08-03-10 *Another Attempt To Get You To Call Me a Realist!, 2* (to H. M. Wiseman)

Wisemanism 48: *I've started to reading it, and I like the tenor of it so far. You seem to have taken a deep intellectual breath before writing this time.*

Did I ever share with you my idea of Escher's "Print Gallery" as a metaphor for the measurement problem? The boy is the observer, the picture is what he is observing, the town is the universe. The blur in the middle is the flaw which means we will never be able to solve the problem.

I'll keep my fingers crossed that that lasts! I don't believe you'd ever told me the Escher metaphor. My own favorite metaphor comes from a painting as well: Joan Vaccaro's "Broken Block" which she sent me, and I have hanging in my study at QBism house. It beautifully expresses all the cracks in the block universe idea. You can still see it at Joan's webpage, and attached is a little blurb on the house.

08-03-10 *Lee, from the Inside* (to L. Smolin)

Actually a Google search on the phrase "quantum cosmology from the inside" gives *Precisely* one hit ... and it was one of your papers!

10-03-10 *Meliorism* (to L. Smolin)

I read your essay and enjoyed it very much. We are both meliorists! See Footnote 31, page 19 in the essay I gave you for a bit on the term. (I'll attach a copy anew in case you don't have the essay in front of you.) [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] I also like this idea of "merging nature and technology"—it is long deep in my thinking. It is, in a way, why I always draw a quantum measurement device as a prosthetic hand.

I also enjoyed the section on democracy. It reminded me of something I wrote to Carl Caves once, when I sent him Louis Menand's book *The Metaphysical Club* and Richard Rorty's book *Philosophy and Social Hope* as a holiday gift (2003). Digging up the note, I ended it with:

> Anyway, I learned a lot about quantum mechanics from these books. And I learned a lot about true Americanism (i.e., the kind of Americanism we need to strive to get back to ... and actually mould into a more stable, long-lasting form). Done right, I think the two subjects are probably the same thing.

It very much fits with the "republican banquet" ontology I see quantum mechanics hinting at.

Anyway, just first thoughts. I'll read it again when things are less hectic.

10-03-10 *The Great Lotze* (to L. Smolin)

Thinking a bit more on your avowal of human agency as a real element in the world, I thought you might enjoy this passage from William James if you've never seen it. [See long William James quote near the beginning of the 05-01-09 note "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer & D. M. Appleby.]

10-03-10 *QBism on the Make* (to R. D. Sorkin)

I didn't feel I answered your questions very well today—neither to my satisfaction nor yours. I'd like to get a chance again if you've got some time to chat tomorrow. Attached is the essay I just wrote on the big picture of the QBism program. If you have time to give me some feedback on it, I'd sure appreciate it before I post it: I'm sure there are still plenty of places it can be tweaked for clarity, as well as to make sure the right message is coming across.

If you want a short introduction to the technical part about the SIC rewrite of QM, maybe I could recommend this one: `http://arxiv.org/abs/0912.4252`.

10-03-10 *Lighter Fare* (to L. Smolin)

I feel I didn't answer your questions very well today, at least not to the best of my ability. I'd like to get your questions straight. Would you have any time to chat further tomorrow? At the moment, I'm free all day except 2:00 to 4:00.

Attached is a lighter version of some of the "nonreductionism pieces" in the essay I gave you; it also encompasses some of the things I tried to say in answering your questions. [See 16-10-09 note "The More and the Modest" to L. Hardy.] The point being that QBism sees its rewrite of the Born rule as an identification of a new "active power" inherent in all matter. And that the form of the equations are simple enough that we have strong hope that they alone imply the formal structure of quantum mechanics. That should be first thing they do before we get to the next step beyond quantum mechanics, which I definitely see coming. I want to give a shot at conveying that to you.

Also, if you want a short introduction to the technical part about the SIC rewrite of QM, maybe I could recommend this paper to you: `http://arxiv.org/abs/0912.4252`.

11-03-10 *Lighter Fare, 2* (to L. Smolin)

Lee Smolinism 2: *I thoroughly enjoyed your talk and even felt like I understood most of it.*

Thanks; I was afraid you were put off by the talk.

Lee Smolinism 3: *I can put a query about what you mean by non-reductionism this way; to me strong reductionism is the claim that the properties needed to understand a composite system can always be translated completely into the language used to describe the properties of the parts. Non-reductionism denies this and allows the whole to have properties not expressible in the language used to describe the properties of the parts.*

Your proposal, if I understand it, is that the parts have no properties at all, ie you claim there is no realist microscopic description at all. This seems a denial of more than reductionism.

Am I misunderstanding?

Yes. Or it could be that I used some choice of words that accidentally conveyed this. I certainly would not have meant to! I think the two paragraphs below from my essay directly address your question. (And for fun, I'll leave in a footnote that I had opted to leave out of the actual paper.) Maybe a way of putting it is that the view here is that reality runs deep: It is to be found in a new and unique way at every level.

There may be some small similarity to something you said in your open-universe essay (or at least the wording of it). Let me dig up the line: "a lot of progress has recently been made on approaches to quantum cosmology in which time is fundamental rather than emergent." Reality runs deep in the sense that there is something nontranslatable from every level to every other level. And similarly from every "time" to every other "time". (Though I probably shouldn't be using the language of "level" at all: The view here is that there is no ordering or even overly broad partial ordering (though there may be within islands).)

Maybe another troublemaker is my slogan "Quantum States Do Not Exist." Maybe it is this that is giving you the impression that I am saying "the parts have no properties at all." All the slogan is really meant to convey is that quantum states are not microscopic properties, but rather macroscopic. Namely, a quantum state is a property of the agent who holds it. It is a property of the head of the agent, for it reflects how he will gamble on the consequences of the actions he contemplates taking upon a system.

Two paragraphs below. I hope this helps.

Physics—in the right mindset—is not about identifying the bricks with which nature is made, but about identifying what is *common to* the largest range of phenomena it can get its hands on. The idea is not difficult once one gets used to thinking in these terms. Carbon? The old answer would go that it is *nothing but* a building block that combines with other elements according to the following rules, blah, blah, blah. The new answer is that carbon is a *characteristic* common to diamonds, pencil leads, deoxyribonucleic acid, burnt pancakes, the space between stars, the emissions of Ford pick-up trucks, and so on—the list is as unending as the world is itself. For, carbon is also a characteristic common to this diamond and this diamond and this diamond and this. But a flawless diamond and a purified zirconium crystal, no matter how carefully crafted, have no such characteristic in common: Carbon is not a *universal* characteristic of all phenomena. The aim of physics is to find characteristics that apply to as much of the world in its varied fullness as possible. However, those common characteristics are hardly what the world is made of—the world instead is made of this and this and this. The world is constructed of every particular there is and every way of carving up every particular there is.

An unparalleled example of how physics operates in such a world can be found by looking to Newton's law of universal gravitation. What did Newton really find? Would he be considered a great physicist in this day when every news magazine presents the most cherished goal of physics to be a Theory of Everything? For the law of universal gravitation is hardly that! Instead, it *merely* says that every body in the universe tries to accelerate every other body toward itself at a rate proportional to its own mass and inversely proportional to the squared distance between them. Beyond that, the law says nothing else particular of objects, and it would have been a rare thinker in Newton's time, if any at all, who would have imagined that all the complexities of the world could be derived from that limited law. Yet there is no doubt that Newton was one of the greatest physicists of all time. He did not give a theory of everything, but a Theory of One Aspect of Everything. And only the tiniest fraction of physicists of any variety, much less the TOE-seeking variety, have ever worn a badge of that more modest kind. It is as H. C. von Baeyer wrote in one of his books [*Petite Leçons de Physique dans les Jardins de Paris*],

> Great revolutionaries don't stop at half measures if they can go all the way. For Newton this meant an almost unimaginable widening of the scope of his new-found law. Not only Earth, Sun, and planets attract objects in their vicinity, he conjectured, but all objects, no matter how large or small, attract all other objects, no matter how far distant. It was a proposition of almost reckless boldness, and it changed the way we perceive the world.

Finding a theory of "merely" one aspect of everything is hardly something to be ashamed of: It is the loftiest achievement physics can have in a living, breathing nonreductionist world.[217]

[217] Theories of everything, by contrast, belong to dead worlds, ones whose lives have already been completed. But perhaps I should not go so far as to say.

11-03-10 *Survival* (to R. Schack)

OK, I survived giving this: `http://pirsa.org/10030036/`. Now today, I give the follow on to this lecture (grilling) `http://pirsa.org/10030005/` for Laflamme and Emerson's class, and further write my report for the Navy (due Friday 4:00 PM). Saturday, I finally turn my attention to your reflection/decoherence draft and our Templeton proposal. I'm sorry for all these delays; it's just that I am so damned linear.

11-03-10 *New Footnote* (to C. E. Granade)

Thanks for the question yesterday. You'll find a better answer than I gave you in the discussion starting on page 23, and particularly footnote 44, of this nearly final draft of my essay. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] But that's not the "new" footnote I was referring to in the title. I've rejigged the footnote about you.

> Not to be confused with Scientologists. This neologism was coined by Chris Granade, a Perimeter Scholars International student at Perimeter Institute, and brought to the author's attention by R. W. Spekkens, who pounced on it for its beautiful subtlety.

The whole thing is getting very near posting now.

12-03-10 *Counterexample* (to J. Emerson & R. Laflamme)

Just to follow up on one of my remarks yesterday. Take some finite dimensional Hilbert space of dimension $d > 4$, and imagine performing (sequentially) two *projection* valued measurements on it. Suppose the first consists of n non-rank-1 projection operators P_i. For the second, let it be a PVM with d rank-1 projections Q_j. Further suppose that none of the Q_j commute with any of the P_i.

We can think of this as a single measurement with nd outcomes (i, j), and POVM elements $P_iQ_jP_i$. That is to say, a POVM with nd rank-1 elements: Let's call them G_{ij}. To further simplify notation, let's use lexicographic order to associate ordered pairs (i, j) with a single integer $k = 1, \ldots, nd$. In other words, let us write G_k for the POVM elements.

Now, suppose you hadn't read any of the above, and I walk up to you out of the blue and say, "Joseph, I have a POVM with a these rank-1 operators G_k as its elements. I'd like you to rig up a measurement of it for me." My guess is that your knee-jerk reaction would be to say, "Let me use the Neumark extension theorem to write this as a unitary interaction with an ancilla plus a projection valued measurement on an nd-dimensional Hilbert space."

And I would reply, "Yes, that's one way to think of it, but it is not how I described it to you above. Above, I mapped it out as a sequential measurement."

Similarly, a common technique used by the Zeilinger group and others, is to use extra timing information in their measurements. I would be resistant to call that the introduction of extra Hilbert space. The Steinberg SIC experiment is a bit of an example of that as well. I'll attach Medendorp's QELS abstract. There, a perfect SIC is enacted by a photon running around and around a loop and generally evolving unitarily, except encountering very weak partial beam splitters along the way. Zeilinger will also be doing a qutrit SIC experiment, and in the present plans, he will make use of the Neumark theorem and do it that way, i.e., as a PVM on a 9-dimensional system, but that is not the only way to do it.

I will take you up on those beers when I'm back from the APS meeting.

15-07-09 *To Contemplate* (to R. Laflamme & J. Emerson)

Attached is the vector I showed yesterday from a $d = 6$ SIC. The remaining 35 vectors are generated by acting $X^j Z^k$ on it, for all values of j and k. (X and Z are the usual shift and phase operators.)

And I'll remind you of the interest in dimension 6: No one has ever been able to construct a complete set of mutually unbiased bases in that dimension, and it is widely believed (though not proved) that no one ever will. This says the SIC is the only optimal measurement around in that dimension, and thus shows a divergence of the two concepts, SICs and MUBs.

The Singapore group has done a SIC in $d = 2$. Steinberg has done (something resembling) a SIC in $d = 3$, and Zeilinger will soon be doing a more honest-to-god SIC in $d = 3$ as well. It'd be nice to see IQC do the next interesting dimension, $d = 6$. And I suspect your techniques are much more tailored to the problem anyway.

I'll be back from the APS meeting on the 21$^{\rm st}$. Ray, I'll come by your office the following Wednesday.

27-07-09 *Under Your Belt* (to J. W. Nicholson)

I'm gearing up to write a story about you. See attached, page 11. In preparation for it, may I ask you how many publications you have now? I just want to make a small mention of the number, before I plunge into bar stories. And do you have a webpage? I couldn't find it.

Greetings from 4:58 AM,

28-07-09 *Red Naugahyde* (to J. W. Nicholson)

This still isn't a proper reply to your nice notes—I'll work on that soon. But let me send you the first draft of the Nicholson entry to try on for size. You'll find your entry on page 12 (close to the new one of Asher Peres).

If there's anything you want changed, let me know and I'll consider.

> **Jeff Nicholson:** Whether or not it is acceptable to say so in an academic publication such as this, I'll just record it: Jeff Nicholson was the best beer-drinking buddy I ever had in graduate school or since, and I was proud to be the best man at his wedding. Jeff is currently a research staff member at OFS Laboratories in Somerset, New Jersey and writing papers with titles like, "Continuum Generation Using a Femtosecond Erbium-doped Fiber Laser and a Silica Nonlinear Fiber." We started together as officemates at the University of New Mexico in 1993, and then found ourselves together again at Bell Labs many years later. Jeff is the taller, stronger, and faster of the two of us, with over 170 publications and patents. In the days that were, we had many a good round of philosophy at the ever atmospheric *Jack's Liquor and Lounge* in Albuquerque—red naugahyde seats, and plenty of talk of reality creation. I will well bet that J. W. Nicholson is the only person on earth to have handled a polarization maintaining, dispersion managed, femtosecond figure-eight fiber laser *and* read Richard Rorty.

Jeff's Reply

I love it. ☺

Did I ever actually read Richard Rorty? I can't recall ....

Also, you forgot my Nobel-cited paper of which I am waayyyyy too inordinately proud. ☺

- Brian R. Washburn, Scott A. Diddams, Nathan R. Newbury, Jeffrey W. Nicholson, Man F. Yan, and Carsten G. Jorgensen, "Phase-locked, erbium-fiber-laser-based frequency comb in the near infrared," Optics Letters **29**(3), 250–252 (2004).

12-03-10 *Portland!* (to the QBies)

I came downstairs grumbling about having to go to Portland, thinking, "Why in the hell did I commit myself to going so early in the meeting?" *But then* I Googled "used books Portland" and to my absolute surprise, shock, and pleasure found that the downtown area is chock full of bookstores! And Powell's City of Books (1 million in stock, and the building fills a whole city block!) is just four blocks from my hotel!!

I'm taking an empty duffle along with my suitcase—one would not believe it but there are still many books on the anti-block (universe) that I do not have. I suggest you all do the same!

... Now this trip is worth making.

13-03-10 *Conceptual Barrier!* (to the QBies)

Ever since I first exclaimed to some audience, "SICs are so ________ (beautiful? right? I can't remember the exact phrase) for quantum mechanics that, even if they DO NOT EXIST, they OUGHT TO exist!" ... ever since then, I have been fascinated with what my subconscious was trying to express. And it only got fueled one day when Bob Coecke said, "Yeah, there's some truth to that. It's a game that's worth playing. If you can't prove they exist, you just add them as an axiom." You both have heard my allusions to William James's faith ladder in this context (I reprise the story below), and the power of the SICs as a symbol like the Christian cross.

But today it finally all came together! And no less than in a philosophy of science seminar! I have many times said that I never get much of value from a philosopher of science, but this one was different. It was at Mark Wilson's seminar at UW. His abstract is below, but it gives no great hint of the depth of the talk. It seemed to be based on his book *Wandering Significance: An Essay on Conceptual Behavior*, which I am certainly going to buy.

His main lesson was this: There has never been any stabilized, finished thing called Maxwell's theory of electromagnetism. Instead, the meaning of all the terms in it have been subtly changing since the beginning (and even before). Meanings have been modified for "parts" of the "theory" in light of this and that for other "parts." Moving away from Maxwell's theory he had a nice example in Dedekind cuts. Finally he landed with the lesson of the definition of a Riemannian manifold. It was an analogy that I really took to heart. The point he emphasized was that one uses a structure that is not quite right as the basis for getting the structure one really wants. He had a beautiful picture of a curvy manifold with tangent spaces tacked on everywhere: The manifold is the limiting thing that the collection of tangent spaces is not.

[Next day. I started the note above before Kiki and I went to Jazz at the Bistro last night.]

Lesson: There is nothing wrong in using a structure that is not quite right to build up one that is more satisfactory for this or that purpose. So here is what I mean when I say we will *make* SICs exist: If SICs exist, the Born Rule

$$Q(D_j) \,=\, \mathrm{tr}\,(\,\hat{\rho}\,\hat{D}_j\,)$$

is allowed to take the form of the urgleichung

$$Q(D_j) \,=\, (d+1)\sum_{i=1}^{d^2} P(H_i)P(D_j|H_i) - 1\;.$$

We know that the Born Rule has that privilege in at least 67 dimensions; we know that the quantum-state space has the privilege of being a "maximal consistent set" in at least 67 dimensions. In 67 dimensions, the Born Rule represents the simplest modification one can imagine to the Law of Total Probability for taking into account the counterfactuality of the sky path vs the ground path. If living counterfactuality is the message quantum theory is trying to tell us, it has to find a simple expression somewhere in the theory. In at least 67 dimensions, quantum mechanics is isomorphic to "SIC mechanics," but if SICs don't exist generally within the usual quantum formalism, so much the worse for quantum mechanics: It means that quantum mechanics is not quite right. Close, but not quite right. There was never a commandment written in stone that the stories we tell of quantum phenomena must be written in the language of linear operators.

It is hard for me to express how liberating I found this thought yesterday. We have identified an essentially simple core to quantum mechanics in the urgleichung—and it is this that must be saved at all costs. For physics should be built on the conceptually simple. It is only our habit of thinking in terms of orthogonal bases that makes the SICs look difficult. If God had given us "SIC mechanics" to begin with, we would be finding it similarly difficult to establish "orthonormal bases" for representing the theory, and our nearby colleagues would be wondering why we would even want to. For though we would know that we could do it in dimensions 2 through 67 at least, we would not be able to hide the fact that the expression of these bases always look exceedingly complicated in terms of their original "SICs." It would just be our faith that for every extreme point in the set there should be a measurement for which it gives certainty that drives us forward and makes us slave to find such a structure.

One technical question for everyone to think about: Let us not try to build up a maximal consistent set one point at a time, but d^2 points at a time. That is, let us try to build it up one regular simplex at a time (what in quantum mechanical terms would amount to demanding that every pure state be an element in at least one SIC). Let us build a maximal consistent set by taking a maximal union of simplexes, subject only to the urungleichung. Is there now, modulo rotations, one unique set? If so, then under the assumption of SIC existence, that structure would be isomorphic to quantum state space.[218] And if SICs do not always exist, what physical phenomena would recommend that we not pass from the one theory to the other? Maybe we wouldn't be able to find a reason at all.

James's faith ladder and Wilson's abstract below.

> Faith uses a logic altogether different from Reason's logic. Reason claims certainty and finality for her conclusions. Faith is satisfied if hers seem probable and practically wise.
>
> Faith's form of argument is something like this: Considering a view of the world: "It is *fit* to be true," she feels; "it would be well if it *were* true; it *might* be true; it *may* be true; it *ought* to be true," she says; "it *must* be true," she continues; "it *shall* be true," she concludes, "*for me*; that is, I will treat it as if it *were* true so far as my advocacy and actions are concerned."

[218] We now know that, even if SICs do exist, quantum state space *is not* generally a simple union of regular simplexes. Rather, on the supposition of SIC existence, it is the *convex hull* of a union of regular simplexes. This is because there are quantum states for all $d \geq 4$ which are not expressible as a convex combination of SIC elements for any SIC.

Obviously this is no intellectual chain of inferences, like the *Sorites* of the logic-books. You may call it the "faith-ladder," if you like; but, whatever you call it, it is the sort of slope on which we all habitually live. In no complex matter can our conclusions be more than *probable*. We use our feelings, our good-will, in judging where the greater probability lies; and when our judgment is made, we practically turn our back on the lesser probabilities as if they were not there. Probability, as you know, is mathematically expressed by a fraction. But seldom can we *act* fractionally—half-action is no action (what is the use of only half-killing your enemy?—better not touch him at all); so for purposes of action we equate the most probable view to 1 (or certainty) and other views we treat as naught.

"How 'Wave Front' Found its Truth-Values"
Mark Wilson, Department of Philosophy, University of Pittsburgh

Scientific theories rarely serve as islands entire of themselves. Such hidden connections sometimes force novel readings upon venerable doctrines, dramatically altering our understanding of their basic "subject matter" in the process. Conventional Maxwellian electromagnetism underwent an upheaval of this type in the mid twentieth century. The case (which will be recounted in non-technical terms) offers interesting insights for both philosophy of language and philosophy of science, for it supplies a portrait of semantic stabilization unfolding in a different manner than conventional "natural kind" models suggest.

13-03-10 *All the More Reason* (to R. Schack, D. M. Appleby, Å. Ericsson)

Let me add one more reason for wanting the technical question in the last note to work out. It has to do with the axiom Rüdiger and I called "the availability of certainty" in the QB Coherence paper. I've never really liked it, and the footnote attached to our statement of the axiom explains why:

> In several axiomatic developments of quantum theory—see for instance [Goyal08] and [Hardy01]—the idea of repeated measurements giving rise to certainty (and the associated idea of "distinguishable states") is viewed as fundamental to the whole effort. The usual justification is that the existence of such kinds of measurement is nearly a self-evident necessity. However, from the quantum-Bayesian point of view where *all* measurements are generative of their outcomes—i.e., outcomes never pre-exist the act of measurement—and certainty is always subjective certainty [Caves07], the consistency of adopting a state of certainty as one's state of belief, even in what is judged to be a repeated experiment, is not self-evident at all. In fact, from this point of view, why one ever has certainty is the greater of the mysteries.

Couple that with something I said in the last note:

> If God had given us "SIC mechanics" to begin with, we would be finding it similarly difficult to establish "orthonormal bases" for representing the theory, and our nearby colleagues would be wondering why we would even want to. For though we would know that we could do it in dimensions 2 through 67 at least, we would not be able to hide

> the fact that the expression of these bases always look exceedingly complicated in terms of their original "SIC". It would just be our faith that for every extreme point in the set there should be a measurement for which it gives certainty that drives us forward and makes us slave to find such a structure.

That is, the reason we'd be driven is that we would be working to *establish* the availability of certainty as a *theorem*. And that is indeed the way it should be from a QBist perspective. That any certainty at all could come out of quantum theory *should* be a theorem. If something is the "greater of the mysteries" it really shouldn't have a comfortable spot as one of the very axioms.

So now I really find myself hoping the "union of simplexes" idea carries a lot of power.[219] It would be a very clean foundation for quantum theory.

13-03-10 *The Tetra Pak* (to H. C. von Baeyer and D. M. Appleby)

Yesterday I wrote these lines down:

> Ever since I first exclaimed to some audience, "SICs are so ________ (beautiful? right? I can't remember the exact phrase) for quantum mechanics that, even if they DO NOT EXIST, they OUGHT TO exist!" ... ever since then, I have been fascinated with what my subconscious was trying to express.

Today, it dawned on me that maybe there really is some kind of subconscious tug on me in this regard. As I was sitting on a bench in the shopping mall where Marcus gets his groceries here, I remembered my brother coming home one evening in 1968 or '69, when I was 4 or 5, with a small tetra pak of coffee creamer from a diner or Dairy Queen or something. (Look up `http://en.wikipedia.org/wiki/Tetra_Pak` if you don't know what I mean.) He put it in the refrigerator, but I walked behind him and took it out. I thought it was the most fascinating shape I had ever seen. In fact, it was in the refrigerator for some time, and whenever I would open the door, I would have a look at the little thing. Today was the first time my memory has ever gone back to that.

The Tetra Pak ... Pauli's fascination with the Tetractys ... and the hope that quantum state space is *nothing but* a grand union of simplices (subject only to the restriction of the urungleichung).[220] Today is one of those days when I can't seem to stop myself from thinking about the SICs and the Tetragrammaton in the same thought.

13-03-10 *Comments* (to H. C. von Baeyer)

von Baeyerism 60: *p. 22 footnote 37. Magnalium is showing off. I had no idea what it was, and it certainly doesn't matter. Didn't he use other, more common materials as well, like maybe brass?*

Cool! Just looked through; there is indeed new stuff. Thanks a million, million!

Magnalium isn't showing off—it is what he used, and I had never heard of the stuff either. In the first paper where I make a comparison between dimension and mass, I wrote in the acknowledgements:

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[219] See Footnote 218 for a technical, but remediable, mistake in the original statement of this.

[220] See Footnote 218 for a technical, but remediable, mistake in the original statement of this.

This paper is dedicated to Alexander Holevo on the occasion of his 60th birthday. I thank Osamu Hirota, Chris King, Masahide Sasaki, and Peter Shor for useful discussions, and especially Greg Comer for keeping me close to gravity. I thank Ken Duffy for teaching me about magnalium.

Hans's Reply

Many years ago I started a column on absolute zero (an impenetrable barrier in physics) this way: "The other day, as I was having morning coffee in my solarium, seventeen yellow waxwings crashed into the window and dropped dead into the flower bed below." This sounded ridiculous, so I changed it to seven, and then to three. That's how it was published.

But, Chris, seventeen was, indeed, the truth. Sometimes, in writing, you have to lowball in order to appeal.

14-03-10 *Sadness and Opportunity* (to G. Bacciagaluppi)

I'm so very sorry, but this time I'm going to have to miss the meeting for sure. It's a shame, I really would like to show my support for you and reach out to the UK audience once again. The meetings in Hull and Belfast were very important for me; they shaped my research life really. But July is just too full a month for me. I have to go to Brisbane for a 19 to 23 meeting, and Vienna for a 27 to 31 meeting. And in June there's the Växjö meeting as well, 13 to 18, of which I'm already committed to speaking.

On another subject, I can't tell you how much I enjoyed your recent article with someone (can't remember the name) on Heisenberg's schnitt argument. It was very thought provoking for me. I wish I had it in front of me while writing you (I'm on my way to Portland to show off my students at the APS March meeting): There was a quote by Heisenberg and I think Born on the meaning of probability surviving the transition from the classical venue to the quantum venue that might have come from my own mouth. Attached is a new essay where I say as much (and hopefully a lot more), that I plan to post Wednesday (before Thursday's talk in Portland). Time is short, but if you have any feedback I may be able to take it into account. At the very least—come to think of it—I may be able to cite your new article at some appropriate spot.

14-03-10 *Beginnings of Subconscious* (to the QBies)

Ev'rybody's talkin' 'bout
Bagism, Shagism, Dragism, Madism, Ragism, Tagism
This-ism, that-ism, ism ism QBism
All we are saying is give peace a chance ...

Sing it with me boys! Over the weekend, I've gotten quite taken with the technical question below. [See 13-03-10 note titled "Conceptual Barrier!"] Now a question for you guys to contemplate. Do you think there's any way we might check it numerically for $d = 3$? Or check any aspect of it? Could quantum state space be built up one simplex at a time?

See you in Portland.

14-03-10 *Shunning Certainty and Recovering It* (to L. Hardy)

I thought you might enjoy the notes below that I launched off yesterday to the guys in the crew. [See the 13-03-10 notes to the QBies titled "Conceptual Barrier!" and "All the More Reason."] I think they capture another distinction of direction where the path I'm following seems to take me away from your original axiomatic scheme. Ultimately I'd like to get away from taking the notion of distinguishable states as a primitive for the theory, and rederive that there are some as a consequence of other structures. Below starts to flesh out how that might go, and why I would want to go that way.

It seems to me a rather pretty fact that quantum-state space in SIC representation is exactly a union of regular simplexes.[221] And a maximal union as well, the addition of even one more point would make my urungleichung violated. Only recently has it struck me however that that might be a significant postulate in itself.

16-03-10 *Landed in Portland* (to M. Schlosshauer)

You flatter me through and through. And you make it hard for me to function in the REAL world, where no one likes the sound of anything I say. It's much easier to read Max's notes over and over and then take a nice nap.

Anyway, thank you, thank you so much for your feedback. And then thank Eli and Kari for me, for letting you go for a while to spend some time reading and composing notes. I fully realize that this is a very busy time of your life.

[I wrote the lines above Sunday evening; it is now Tuesday morning.]

But OK, beyond all the nicety, you pushed me: I couldn't stop myself without an effort to do your requests all the way through. Come to think of it, it was the opposite of reading your note and then taking a nice nap! You are subtle—a man who will go places. Let me rush this off to you so that it gets to you before you are asleep. I really want to post the thing tonight or tomorrow morning come hell or high water. Aside from making a couple cross references, and placing the figures more properly it should be done! See footnote 22 on page 13.

On another occasion (soon) we'll talk more about *measuring* dimension. As for the paper, I'll just leave it with the one reference to Scarani for the time being.

More later.

16-03-10 *Progress Report* (to Å. Ericsson)

Six books so far, but the two most notable are:

1) A picture book of alchemical symbols,

2) A book with a serious Jamesian style argument for human immortality

So far, I've only gotten through the first third (plus epsilon) of Powell's philosophy section. Following that I've got about four other bookstores to go to in town!

Haven't seen Hoan, Gelo, and Matthew yet. I'm staying at about the fanciest hotel I've ever stayed at (paid for by the APS)—beautiful lobby I'm writing you from, rich wood everywhere. The other guys on the other hand are probably staying at a dump. It makes me feel guilty.

[221] See Footnote 218 for a technical, but remediable, mistake in the original statement of this.

Not one note from anyone on my weekend epiphany—I thought I had made a great conceptual leap. It has kind of disappointed me. But still I have plodded on thinking about unions of regular simplices.

16-03-10 *Sadness and Opportunity, 2* (to G. Bacciagaluppi)

Bacciagaluppism 3: *Thanks for the appreciation of the Heisenberg article (my co-author is Elise Crull, currently finishing her PhD with Don Howard at Notre Dame). Is the quote the one about the 'quite usual probabilities'? It is actually not clear to me what picture of probabilities Born and Heisenberg have in mind (if they have a single one in mind!), and I suspect it is something like a propensity view (so I am actually wondering whether a quote would be out of context!).*

The issue is not Bayesian vs. Frequentist probability (Heisenberg and Born, at least in that quote, seem to be treating probability frequentistically, not propensitistically), but that whatever notion is chosen is stable under the transition from classical to quantum. Certainly not everyone believes that—they (and Feynman) did. Both emphasize a rule change for *calculating* probabilities when comparing a "factual" to a "counterfactual" (though they didn't say it that way). They both emphasized that the *notion* of probability itself does not change. What I particularly enjoyed about Born and Heisenberg is that they say explicitly this entails no break from the usual probability calculus: The quantum mechanical rule is rather an addition to it!

Attached is a modified draft where I reference you. See footnote 19 on page 12. Please share with Elise if you wish.

The final version of the paper should be posted Wednesday.

17-03-10 *We Are the Small Axe . . .* (to M. Schlosshauer)

I didn't get back to you today as I had hoped, and now I'm a bit drunk from the TGQI "executive" dinner (as if I could be the executive of anything). Blame John Preskill (even though he doesn't drink, funny all the causal connections). So I'll keep it short. It's no payment for all the help you've given me, but I thought I'd make you an "honorary" member of the group by spewing to you like I spew to all the rest of them. (I'm listening to Bob Marley as I write you this; do you know the song in the title? I am no executive, but I might be a small axe.) Below are two notes capturing a good experience I had at the end of last week. [See the 13-03-10 notes to the QBies titled "Conceptual Barrier!" and "All the More Reason."] I don't seriously believe that QM doesn't always contain the SICs, but even if it *does* contain them properly, I now understand that the urgleichung is truly the more basic structure: QM, at best, is secondary. Either way, the equations already tell us that we must *make* these damned SICs exist. I hope you enjoy James's faith ladder; I suspect that you more than anyone else in the group will feel it intuitively.

Just sharing with you, and trying to thank you in the deepest way I can for the ends you've pushed me to. The paper is way better because of you. I will finally post it tomorrow morning! It will be so good to get it off my chest.

I hope all is well with you and Kari and Eli.

17-03-10 *Bob Marley Words* (to Å. Ericsson)

Just heard a lovely phrase in a Bob Marley song, "mighty God is a living man." (I've known it for a long time, but hearing it again brought it to mind.) Anyway, isn't that what I've been saying myself?

I'll give you a better report tomorrow, but it looks like 17 books so far. More tomorrow!

18-03-10 *Very Final Version* (to R. W. Spekkens)

Here is the very final version of the paper, the one that'll be posted Friday. I looked at footnote 37 on Wootters' kid:

> That is, every piece of the universe had better be hard-wired for the contingency that an agent might experience it somewhere, somehow, no matter how long and drawn out the ultimate chain might be to such a potential experience. Does this mean even "elementary" physical events just after the Big Bang must make use of *concepts* that, to the reductionist mind, ought to be 15 billion years removed down the evolutionary chain? You bet it does. But a nonreductionist metaphysic need make no apology for this—such things are in the very idea. John Wheeler's great smoky dragon comes into the world biting its own tail.
>
> W. K. Wootters tells a lovely story of an encounter he had several years ago of with his young son Nate. Nate said, "I wish I could make this flower move with my mind." Wootters reached out and pushed the flower, saying, "You can. You do it like this." From the perspective here, this is an example of an interaction between two nonreductionist realms. Each realm influences the other as its turn comes. There is a kind of reciprocality in this, an action-reaction principle, that most reductionist visions of the world would find obscene.

One aspect of what it says is the very idea you were trying to defend against Tumulka: That "no operational distinction" should correspond to "no ontic distinction." Tumulka would say, "What business does ontology have in knowing what you, a high level agent, can or cannot distinguish?" "Ontology was written down before agents were ever thought of; their limitations are of no consequence for ontology." I imagine him saying. "You have the conceptual flow going the wrong way."

There is a bit of William James in you yet.

18-03-10 *We Are the Small Axe ..., 2* (to R. W. Spekkens)

Here are some of the other things I was telling you tonight, about how I should have answered Lee. [See 13-03-10 note "Conceptual Barrier!" to the QBies.]

Hierarchies of theories are all well and good, but when it comes time to modify quantum mechanics, I suspect it's going to be something much more subtle than just jumping to the next level in one's premade class. One of the things that struck me when I came across the thought below is that I actually found it believable—for the first time I felt the tug of why one would even think of modifying QM. Though as I wrote Max:

> I don't seriously believe that QM doesn't always contain the SICs, but even if it *does* contain them properly, I now understand that the urgleichung is truly the more basic structure: QM, at best, is secondary. Either way, the equations already tell us that we must **make** these damned SICs exist.

19-03-10 *Dimension Witnesses* (to V. Scarani)

Thanks. I was just about to post the thing today, but then was thwarted by some upgrade they've made to their system. Now, with my travels, it probably won't be until next week that I'll be able to figure the new `arXiv` system out. So I attach for you the near-posted version. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] The language near the end of the paper is very close to some things I note in Nicolas Gisin lately (quantum events as moments of creation, etc.), but I suspect he would never accept of any of my starting points or the reasoning that led me to these similar conclusions.

I enjoyed meeting you in Japan, and hope to see you again in the decent future.

Valerio's Preply

Here are a few articles beyond ours on dimension witnesses:

- `http://arxiv.org/abs/0812.1572` (where it is proved that even with binary outcomes you can have dimension witnesses for any d, just by changing the number of measurement settings);
- `http://arxiv.org/abs/0901.2542` (relating dimension witnesses to evolution)

19-03-10 `arXiv` *New submission* → *1003.3223 in* `physics.hist-ph` *from* `jgus@tds.net` (to J. R. Gustafson)

That's great news! Attached is something I'm just on the verge of posting myself. (I tried to post it today, but they thwarted me with a new system that's incompatible with my old version of LaTeX! So, now I'm the beginner again!)

John's Reply

I am enjoying your new "bigqbism" article that you sent to me, making me think of some Pauli quotes I had inserted into my dissertation . . . roughly pages 137 and 138 of the PDF file that is now posted to arXiv (excerpt appears below). Pauli's remarks imply to me he may have been thinking of issues we now relate to quantum entanglement. Now I'm motivated to dig further into his later writings. Here's that excerpt.

> In any case, Pauli saw in the concept of synchronicity a direct link to Arthur Schopenhauer's philosophy, which he made clear in a letter to Jung of June 28, 1949:
>
> > The idea of *meaningful coincidence*–i.e., simultaneous events not causally connected–was expressed very clearly by Schopenhauer (1788 – 1860) in his essay . . . . There he postulates an "ultimate union of necessity and chance," which appears to us as a "force," "which links together all things, even those that are causally unconnected, and does it in such a way that they come together just at the right moment."

To my knowledge, Pauli never connected his exclusion principle to sychronicity. He did talk about his exclusion principle and "action-at-a-distance," however, in a course on *Wellenmechanik* at the Federal Institute of Technology (ETH) in Zurich in 1956-1957, according to notes taken by his students:

> From a superficial consideration of the exclusion principle, it might be thought that a sort of action-at-a-distance is being postulated, as a result of which even two widely separated particles are aware of one another ("sign a contract"). However, this is not so, because the exclusion principle is valid only as long as the wave packets of the two particles overlap.

Still, Pauli later called for a unification of physics and psychology, which would have entailed both his exclusion principle and the concept of synchronicity.

19-03-10 *Lunchtime Guilt* (to R. W. Spekkens)

You won't get this note until I can drop it off at the airport, but I compose it at the famous Rocco's, caddy-corner to Powell's. I just want to say I've been walking around the city feeling ashamed and guilty since my outburst the morning. Ostensibly, it was initially meant to be a kind of biting joke ... until I could see the hurt on your face. But the more I've examined the moment in my meanderings through the streets, the more I've become convinced that its real source was a rather deep bitterness in me. Looking back on it, I think I can even identify that I had an image of Travis Norsen in my mind as I lashed out at you. I could be fooling myself, but it feels very palpable at the moment. The sad fact is, like Pavlov's dogs, I carry associations in my mind, and the association you inadvertently provoked my reaction to was between those who give up on Einstein locality and those who call me solipsist. The latter has indeed been a thorn in my side for many years. But you—of all people!—in no way deserved that reaction from me. Even though we at times disagree about the future direction of physics, I *do* recognize that you think harder and more deeply about quantum mechanics than anyone else I know. And I don't say that lightly; I mean it. So, I hope you will accept my apology. It's useful for me to recognize my demons, but there is no reason for my friends to be the targets of my primal screams.

I got quite a lot out of our dinnertime conservation the other night. Some stuff of lasting thought. I want to continue that from time to time without your fearing that I'll crack.

Please don't forget to send me your spellcraft talk. Emma will be thrilled to see it.

19-03-10 *Book Report* (to Å. Ericsson)

1. Harold Morick, editor, *Challenges to Empiricism*, PB, 6.95 USD.

2. Myles Brand, editor, *The Nature of Human Action*, PB, 9.95 USD.

3. F. W. J. Schelling, *Philosophical Inquiries into the Nature of Human Freedom*, PB, 9.95 USD.

4. G. E. Moore, *Philosophical Studies*, HC, 12.95 USD.

5. John Dupré, *Humans and Other Animals*, HC, 15.95 USD.

6. Gilles Deleuze, *Difference & Repetition*, translated by Paul Patton, PB, 24.95 USD.

7. Bina Gupta, *The Empirical and the Transcendental: A Fusion of Ideas*, HC, 23.97 USD.

8. Maurice Freedman, *To Deny Our Nothingness: Contemporary Images of Man*, PB, 6.95 USD.
9. Frederick C. Beiser, *German Idealism: The Struggle Against Subjectivism, 1781-1801*, PB, 29.95 USD.
10. Barry Allen, *Truth in Philosophy*, PB, 9.95 USD.
11. Alexander Roob, *Alchemy & Mysticism: The Hermetic Cabinet*, HC, 7.99 USD.
12. Willaim James, *Essays in Radical Empiricism & A Pluralistic Universe*, HC, 15.95 USD.
13. Eugene Fontinell, *Self, God, and Immortality: A Jamesian Investigation*, HC, 19.95 USD.
14. Irving J. Good, *Good Thinking: The Foundations of Probability and Its Applications*, PB, 10.95 USD.
15. Abner Shimony, *Search for a Naturalistic World Views, Volume I: Scientific Method and Epistemology*, PB, 26.95 USD.
16. Dale H. Porter, *The Emergence of the Past: A Theory of Historical Explanation*, PB, 8.50 USD.
17. John K. Sheriff, *Charles Peirce's Guess at the Riddle: Ground for Human Significance*, PB, 10.00 USD.
18. Roger Poole, *Towards Deep Subjectivity*, PB, 3.95 USD.
19. Ishtiyaque Haji, *Incompatibilism's Allure: Physical Argument for Incompatibilism*, PB, 26.95 USD.
20. Arhur O. Lovejoy, *The Great Chain of Being: A Study of the History of an Idea*, PB, 12.50 USD.
21. Richard M. Gale, *John Dewey's Quest for Unity: The Journey of a Promethean Mystic*, HC, 32.95 USD.
22. Edwin B. Holt, Walter T. Marvin, William Pepperrell Montague, Ralph Barton Perry, Walter R. Pitkin, and Edward Gleason Spaulding, *The New Realism: Cooperative Studies in Philosophy*, HC, 65.00 USD.
23. R. W. B. Lewis, *The Jameses: A Family Narrative*, HC, 6.95 USD.

19-03-10 *Counterexample, 2* (to J. Emerson)

Sorry it's taken me so long to get back to you. I've been in Portland all week and this meeting has dulled all my senses. (Though I am presently writing you from a nice Argentinean restaurant.) To give a short reply, let me just say we should discuss this paper by Oreshkov and Brun in the context of your framework: `http://arxiv.org/abs/quant-ph/0503017`. I think it gives a formal account of what I was claiming about Aephraim's SIC-POVM technique. Particularly, it shows a way of trading off continuous interaction for ancilla.

My general philosophical beef is this. If one must recognize measurement as a "primitive" in the QM framework, then I see no reason to prefer one kind of primitive (PVM) over another kind of primitive (POVM). One can think of the PVMs as a subset of the POVMs, or one can think of the POVMs as a subset of the set of all possible two stage thingies (unitary coupling to ancilla plus PVM). Since POVMs are formally powerful, that tips the scale for me. Furthermore for a given POVM there is no unique Naimark-extension way of thinking of it: The POVM corresponds to an equivalence class. And where there is such "gauge freedom" I say, one has the wrong object—one should mod out the freedom, and find the mathematical object that remains.

In any case, in my truest philosophy, a POVM is *nothing other than* a collection of conditional probability distributions. See footnote 22 on page 13 of this (still unposted!) paper attached. [See `http://arxiv.org/abs/1003.5209`.] If one takes that point of view, the unitary + ancilla + PVM take on POVMs is rather unnatural way of thinking. That's my truest philosophy, independent of the Oreshkov-Brun stuff. I just threw it out by way of ammunition (on the suspicion that you won't find my identification of a POVM with a set of $P(D_j|H_i)$ as compelling as I do).

I'm getting primed for those beers you offered.

22-03-10 *Pong* (to R. Schack)

Sorry, the March Meeting turned out to be more taxing than I had imagined. I'm back home now and getting back on track. Today I plan to get my email box thoroughly cleaned out and get to work on two things: 1) Templeton proposal, and 2) Research statement for PI. I have no intention of letting Templeton slip through my fingers.

Quick answers to some old items:

Schackcosm 125: *Did we ever make progress on communication?*

No we didn't, or at least no great amount. And I don't think much of what we did talk about is in the correspondence. I am now much happier with my presentation of Wigner's friend. But it leaves communication out other than to speak of it as another quantum measurement. This leaves much, much to be desired. There is no doubt we have to come back to this.

Schackcosm 126: *I feel strongly that Wigner's friend, communication, counterfactuals and reflection should be coming together in one coherent story . . .*

Yes, I couldn't agree more. In most ways, we're just at the beginning of this project.

Schackcosm 127: *You still haven't given me any feedback at all on the reflection draft paper. It would help me enormously to discuss it with you.*

I understand this. I know so much that I must get on it and help you out. I did read the full draft in Portland, and I must get that conveyed to you. But my guess is, with honesty, we're going to have to knock Templeton out of the way first.

Schackcosm 128: *I have been reading, pondering, and rereading your two notes. There is lots in there which I like a lot. Most of all I like the idea of coming up with a theory that is different from quantum mechanics. I am not sure the urgleichung is sufficiently compelling to warrant such a departure, but there are other reasons to want it. . . . If the new theory derived in a world in which SICs exist only as approximations, or only in small dimensions, had features that make a block universe look unnatural, this would be marvelous.*

My guess is that a departure will not be required—my guess is that SICs actually DO exist always. That aside, what I was hoping to express is that my mind is now prepared for what should be done on the off chance that they do not. In my bones, I do feel the urgleichung is sufficiently compelling, but there is no doubt that must be articulated further: For what we already have, in a way that has never been seen before (certainly not from Gleason's theorem, or the raw injunction of a linear rule as in Hardy or Barnum, Barrett and Wilce, etc.), shows that the Born Rule *is* a rule connecting probabilities to probabilities—between counterfactual and factual. It is the "engaged observer" in crystal clear form, and one that pulls the essential factor to the very front of the equation. We need a more metaphysical story to shore it up with, but that's why the equation already feels completely right to me as the foundation of things.

Schackcosm 129: *A more important one is that some aspects of quantum mechanics make it very hard not to be driven to a many-worlds view. The church of the larger Hilbert space can be regarded as simply a mathematical convenience, but any defender of Everett can claim that it is extremely suggestive.*

You should watch this talk or read the associated `arXiv` paper: `http://pirsa.org/10020070/`. It is a bit frightening how much power "purification" can have as an axiom. (As I write this, it starts to strike me that there might possibly be a connection between it and our "posteriors equal priors" postulate. Very slippery thought, but something we might think about.)

Schackcosm 130: *That is my vision at the moment. To come up with a theory that makes the agent dependence explicit in a far stronger way than quantum mechanics does it. To derive quantum mechanics from the requirement that the theory rules out a block universe.*

I am certainly very sympathetic to deriving the existence of certainty in some way. Maybe even finding that certainty only exists in the approximate theory called quantum mechanics.

And I derive a small amount of satisfaction from the fact that you have finally come round to my way of phrasing maximality and abandoned building up maximal consistent sets point by point.

This is important! I had not realized you proposed this. Clearly my memory is not infallible. What was the context of your earlier proposal? What were the sorts of things we were thinking about at the time?

Schackcosm 131: *Are you somewhere out there? Do you want to give me a ring? What time zone are you in?*

If it's OK with you, let me try to stay away from the phone for a couple of days. I'm working from my study in the house today, and trying to avoid contact with other humans (even the best of the other humans). My social spring got stretched beyond its elastic limit at the March meeting.

22-03-10 *A Line I Shall Use in Templeton* (to R. Schack)

Below is a note I had gotten from one of the students, Ian Hincks, in the Emerson-Laflamme quantum foundations class after my first lecture. His last two sentences are a lovely testimonial to what you and I are up to:

> "I like your theory because it returns to me as much freedom as I feel that I have. Such freedom is lost or partially lost in other interpretations."

I'm going to try to find some way to use this as a testimonial in our Templeton proposal.

22-03-10 *Haeccity Through and Through* (to G. Smith)

Wiki says this of it:

> **Haecceity** (from the Latin *haecceitas*, which translates as "thisness") is a term from medieval philosophy first coined by Duns Scotus which denotes the discrete qualities, properties or characteristics of a thing which make it a particular thing. Haecceity is a person or object's "*thisness*".

Thisness would be easier to say. Anyway, it imbues my philosophy of things through and through, and looking back on it, maybe that's why I liked John's lockboxes. Attached is the new paper that I've had trouble posting with the new `arXiv.org` system. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] The haeccity business becomes quite apparent in Section VI. You might enjoy the big eyeball as well.

24-03-10 *Your Discussion with Tumulka* (to R. W. Spekkens)

Coming back to the point I wrote you about earlier, about why the psi-ontologists generally do not find your notion of noncontextuality so compelling (as you reported of Tumulka). Previous note pasted below. [See 18-03-10 note "Very Final Version" to R. W. Spekkens.] I was just cutting and pasting the footnote mentioned there for something else and discovered that I had originally started to write a follow-on footnote as well, but apparently decided not to use it in the paper. Since it's relevant, let me at least use it in this note:

> It is perhaps worthwhile contrasting this to the "hidden-variable vision" of the quantum world, say of the Bohmian pilot-wave theory mentioned previously. The implication of a hidden variable is that it is not directly experienceable, and to the extent that it does ultimately trickle into experience, it is only so that it props up the quantum mechanical equations.

24-03-10 *Notes on the Open Future* (to L. Smolin)

I had a chance to read your draft on "The Philosophy of the Open Future" again. Attached are some notes on it.

Notes on Lee Smolin's 15 February 2010 draft of
The Philosophy of the Open Future

With the APS March Meeting behind me, I've gotten a chance to reread your draft on the philosophy of the open future. It's got some big ideas in it, and I like it very much for the message it's starting to formulate.

I've got nothing very deep to say in response at the moment, but I'll react to a few points in your draft.

First a very general thing. Of the philosophers I have read, the thoughts in your essay sound to me the most like John Dewey's. I say this not to mean that "it's all been said before," but that you two might make good conversation partners. Particularly, you might find some congenial inspiration in his writings. With regard to the subject matter of your essay, I think his sweep and interests are similar to your own. Dewey's thoughts, of course, will not be informed by today's physics or today's economical issues, but he has nonetheless a well-developed system that might be a good backdrop for your thinking. Picking a nearly random book off my shelf (*Dewey's Logical Theory: New Studies and Interpretations*, edited by Burke *et al.*), I find this key point about what Dewey was thinking:

> Dewey begins from a Darwinian premise of interaction. On this view, experience is "an affair of the intercourse of a living being with its physical and social environment" and thus an affair "primarily of doing." Experience then is not a receiving and filtering sensory data from an external world. It is rather an *exchange*, a *transaction*, between an organism and the physical and social factors within its environment: "When we experience something, we act upon it, we do something with it; then we suffer or undergo the consequences. We do something to the thing and then it does something to us in return."
>
> Dewey's placement of experience in the interactions and transactions between an organism and its environment is further augmented with the recognition of a stabilization propensity characteristic of living beings. Experience is episodic, punctuated by occasions of disturbance and resolution, of imbalance and regained composure. Thus experience is not only transformational. It also has force and direction, impelled by an innate drive of the living being to maintain its own *well*-being. In short, experience is an activity in and by which an organism maintains integration with its environment.

And here's another way the point was made in an article on Sidney Hook's intellectual debt to John Dewey:

> In Dewey's naturalistic metaphysics, a contextual and empirical account of the origin and function of human knowledge recognized man's quest for adaptation to and control of his environment. This emphasis upon human adaptation and control characterized the recognition within pragmatic philosophy of its debt to Darwinism in the development of its theory of mind's place in nature and of the nature of human knowledge.
>
> The thesis that human beings were capable of transforming the natural environment and the seemingly "given" facts of nature was a feature of Deweyan naturalistic metaphysics that was relevant for the pragmatic theory of knowledge. Hook followed Dewey in stressing the connection between a naturalistic metaphysics and the identification of human knowledge as an instrument in transforming the real nature of Nature. Significantly, this feature of naturalistic metaphysics heightened the pragmatic emphasis upon the capacity of human agency to function as an activist transformer of the natural environment. To a significant degree, what was natural, whether in the sphere of the universe or the sphere of human affairs, could be considered in the idiom of pragmatism not so much as a "given," but as a possibility for a "taken." The theme of human transformation of the natural environment carried Hook from the initial interest in the metaphysics of pragmatism to the possibilities of pragmatism as a philosophy which could apply scientific method for social change.

When I read your sentences,

Lee Smolinism 4 : *The problem requires not a choice between nature and technology but a reorientation of their relationship to each other.*

Lee Smolinism 5 : *As long as we have the capability to do it, we must prevent or moderate major changes in the climate, for the same reason we must look out for and destroy asteroids that might* *collide with earth. Once we have resolved this emergency we will be committed to a continuing regulation of the climate to keep it in a range where human beings can thrive. This means melding our technologies with the natural cycles and systems that already regulate the climate.*

Lee Smolinism 6 : *The economy and the climate will not be two things, they will be aspects of a single system. Thus, to survive the climate crisis we have to conceive of and bring into existence a novel kind of system, which will be a symbiosis of the biological processes which determine the climate with our technological civilization.*

I hear an echo of the Dewey above. For, like you, Dewey saw these transactions with nature occurring at all scales, from simple organisms to large societies, nations, and economies. It is particularly the last quote of you that I was thinking of when I said that your essay contains some big ideas—the economy and the climate as two aspects of a single thing. I liked your way of phrasing the issue.

Dewey called his philosophy "instrumentalism," but by that he meant something *not at all like* what most modern philosophers of science (Harvey Brown, Richard Healey, Jeff Bub, and the like) mean when they say instrumentalism. The common, modern definition goes something like this: "instrumentalism is the view that a concept or theory should be evaluated by how effectively it explains and predicts phenomena, as opposed to how accurately it describes objective reality" (wiki), or "instrumentalism can be formulated as the thesis that scientific theories—the theories of the so-called 'pure' sciences—are nothing but computational rules (or inference rules)" (Popper). For Dewey, a scientific theory is a good instrument to the extent that it can be used to mold the world to our needs—some of that might be about prediction, but much of it is not. It is all about the actions we can reasonably take upon the world in our present stage of development. Here is the way William James put it, but this strain is certainly in Dewey too:

> [I]f you follow the pragmatic method, you cannot look on any such word as closing your quest. You must bring out of each word its practical cash-value, set it at work within the stream of your experience. It appears less as a solution, then, than as a program for more work, and more particularly as an indication of the ways in which existing realities may be *changed*.
>
> *Theories thus become instruments, not answers to enigmas, in which we can rest.* We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid.

The purpose of a good theory is not to draw an accurate image of a pre-existent static world, but to be like a hammer, saw, or screwdriver: It is a thought structure that helps us to participate in the inevitable flow and change of the world as best we can. And as we evolve/devolve/change such laws evolve/devolve/change as well (and reciprocally so).

Certainly I believe this of quantum mechanics—that it is a hammer we use in light of the world possessing a certain active, life-breathing power—for I think this is much of its very message. (I probably believe the hammer metaphor of other theories as well, like classical mechanics and general relativity, but there it is not so "in your face" as it is with quantum mechanics.) Maybe it's worthwhile putting it like this. In your essay and my own, one common theme comes out: Human agency matters. "We can on occasion make nature over." Though I myself like to emphasize that agency matters from everything so large as the ecosystem and climate, to everything so small as a quantum measurement.

If you would like to borrow my copy Dewey's *Reconstruction in Philosophy* (a good introduction to his thought), or most anything else for that matter that he's written, let me know.

Now on to more specific matters.

Lee Smolinism 7: *This hierarchy applies also to the natural/artificial divide, in that the natural is valued over the artificial because it is closer to absolute perfection, and therefor closer to timelessness.*

Typo.

Lee Smolinism 8: *But if the search for knowledge is essentially the task of remembering truths which have been ever-present, then the possible solutions to any problem already exist in fixed menus of possibility, and novelty is an illusion. A world without time is a world with fixed categories and fixed possibilities that cannot be transcended.*

I like that phrase "fixed menus of possibility" and can see myself using it. Is the phrase your invention? Or Unger's or Kauffman's or still someone else's?

Lee Smolinism 9: *To fully realize the conception of a time bound world, we need to invent a notion of truth which removes the equation of truth with timelessness and replaces it with a notion of truth that is no less objective, but which has room for surprise, invention and novelty.*

Somewhere—and I have not been able to pinpoint where this morning—William James makes the point that the pragmatic theory of truth he was involved in developing was only a secondary aim for him. It might be written somewhere in *The Meaning of Truth* or *Essays in Radical Empiricism*. Instead James's main aim was something very much like what you say here: One needs more looseness than a correspondence theory of truth allows if one is going to have a malleable, creative world. Truth has to have a different meaning in a world that is "ever not quite." If I can dig up a reference, I'll send it to you.

[Time elapsed.] Aha, I found it! It's in the conjunction of two prefaces—that for *The Will to Believe* and that for *The Meaning of Truth*. Let me quote the relevant pieces:

> Were I obliged to give a short name to the attitude in question, I should call it that of *radical empiricism*, in spite of the fact that such brief nicknames are nowhere more misleading than in philosophy. I say 'empiricism,' because it is contented to regard its most assured conclusions concerning matters of fact as hypotheses liable to modification in the course of future experience; and I say 'radical,' because it treats the doctrine of monism itself as an hypothesis, and, unlike so much of the half-way empiricism that is current under the name of positivism or agnosticism or scientific naturalism, it does not dogmatically affirm monism as something with which all experience has got to square. The difference between monism and pluralism is perhaps the most pregnant of all the differences in philosophy. *Primâ facie* the world is a pluralism; as we find it, its unity seems to be that of any collection; and our higher thinking consists chiefly of an effort to redeem it from that first crude form. Postulating more unity than the first experiences yield, we also discover more. But absolute unity, in spite of brilliant dashes in its direction, still remains undiscovered, still remains a *Grenzbegriff*. "Ever not quite" must be the rationalistic philosopher's last confession concerning it. After all that reason can do has been done, there still remains the opacity of the finite facts as merely given, with most of their peculiarities mutually unmediated and unexplained. To the very last, there are the various 'points of view' which the philosopher must distinguish in discussing the world; and what is inwardly clear from one point remains a bare externality and datum to the other. The negative, the alogical, is never wholly banished. Something—"call it fate, chance, freedom, spontaneity, the devil, what you

> will"—is still wrong and other and outside and unincluded, from *your* point of view, even though you be the greatest of philosophers. Something is always mere fact and *givenness*; and there may be in the whole universe no one point of view extant from which this would not be found to be the case. "Reason," as a gifted writer says, "is but one item in the mystery; and behind the proudest consciousness that ever reigned, reason and wonder blushed face to face. The inevitable stales, while doubt and hope are sisters. Not unfortunately the universe is wild,—game-flavored as a hawk's wing. Nature is miracle all; the same returns not save to bring the different. The slow round of the engraver's lathe gains but the breadth of a hair, but the difference is distributed back over the whole curve, never an instant true,—ever not quite."
>
> This is pluralism, somewhat rhapsodically expressed. He who takes for his hypothesis the notion that it is the permanent form of the world is what I call a radical empiricist. For him the crudity of experience remains an eternal element thereof. There is no possible point of view from which the world can appear an absolutely single fact. Real possibilities, real indeterminations, real beginnings, real ends, real evil, real crises, catastrophes, and escapes, a real God, and a real moral life, just as common-sense conceives these things, may remain in empiricism as conceptions which that philosophy gives up the attempt either to 'overcome' or to reinterpret in monistic form.

And

> Most of the pragmatist and anti-pragmatist warfare is over what the word "truth" shall be held to signify, and not over any of the facts embodied in truth-situations; for both pragmatists and anti-pragmatists believe in existent objects, just as they believe in our ideas of them. The difference is that when pragmatists speak of truth, they mean exclusively something about the ideas, namely their workableness; whereas when anti-pragmatists speak of truth they seem most often to mean something about the objects. Since the pragmatist, if he agrees that an idea is "really" true, also agrees to whatever it says about its object; and since most anti-pragmatists have already come round to agreeing that, if the object exists, the idea that it does so is workable; there would seem to be so little left to fight about that I might well be asked why instead of reprinting my share in so much verbal wrangling, I do not show my sense of "values" by burning it all up.
>
> I understand the question and I will give my answer. I am interested in another doctrine in philosophy to which I give the name of radical empiricism, and it seems to me that the establishment of the pragmatist theory of truth is a step of first-rate importance in making radical empiricism prevail.

Lee Smolinism 10 : *A new philosophy is needed which anticipates the merging of the natural and the artificial by achieving a consilience of the natural and social sciences, in which human agency has a rightful place in nature. This is not a relativism in which anything we want to be true can be. To survive the challenge of climate change it matters a great deal what is true.*

And here I have to say you led my thoughts back to F. C. S. Schiller's paper, "Axioms as Postulates." It is just a wonderful paper. Schiller emphasized that *methodologically* it is important to start by supposing the world to be plastic and malleable to the fullest extent imaginable (i.e., that we *can* have anything we want), and only in exhaustion—when the world does not yield it after our impassioned tries—to accept otherwise.

Let me quote him at length; I think you'll find it fun:

[M]y fourth and most important point is that the world is *plastic*, and may be moulded by our wishes, if only we are determined to give effect to them, and not too conceited to learn from experience, *i.e.* by trying, *by what means* we may do so.

That this plasticity exists will hardly be denied, but doubts may be raised as to how far it extends. Surely, it may be objected, it is mere sarcasm to talk of the plasticity of the world; in point of fact we can never go far in any direction without coming upon rigid limits and insuperable obstacles. The answer surely is that the extent of the world's plasticity is not known *a priori*, but must be found out by trying. Now in trying we can never start with a recognition of rigid limits and insuperable obstacles. For if we believed them such, it would be *no use* trying. Hence we must assume that we can obtain what we want, if only we try skilfully and perseveringly enough. A failure only proves that the obstacles would not yield to the method employed: it cannot extinguish the hope that by *trying again* by other methods they could finally be overcome.

Thus it is a *methodological necessity* to assume that the world is *wholly plastic*, *i.e.* to *act as though* we believed this, and will yield us what we want, if we persevere in wanting it.

To what extent our assumption is true in the fullest sense, *i.e.* to what extent it will work in practice, time and trial will show. But our faith is confirmed whenever, by acting on it, we obtain anything we want; it is checked, but not uprooted, whenever an experiment fails.

As a first attempt to explain how our struggle to mould our experience into conformity with our desires is compatible with the 'objectivity' of that experience, the above may perhaps suffice, though I do not flatter myself that it will at once implant conviction. Indeed I expect rather to be asked indignantly—'Is there not an objective nature which our experiments do not make, but only discover? Is it not absurd to talk as if our attempts could alter the facts? And is not reverent submission to this pre-existing order the proper attitude of the searcher after truth?'

The objection is so obvious that the folly of ignoring it could only be exceeded by that of exaggerating its importance. It is because of the gross way in which this is commonly done that I have thought it salutary to emphasise the opposite aspect of the truth. We have heard enough, and more than enough, about the duty of humility and submission; it is time that we were told that energy and enterprise also are indispensable, and that as soon as the submission advocated is taken to mean more than rational methods of investigation, it becomes a hindrance to the growth of knowledge. Hence it is no longer important to rehearse the old platitudes about sitting at the feet of nature and servilely accepting the kicks she finds it so much cheaper to bestow than halfpence. It is far more important to emphasise the other side of the matter, viz. that unless we ask, we get nothing. We must ask often and importunately, and be slow to take a refusal. It is only by asking that we discover whether or not an answer is attainable, and if they cannot alter the 'facts,' our demands can at least make them appear in so different a light, that they are no longer *practically* the same.

For in truth these independent 'facts,' which we have merely to acknowledge, are a mere figure of speech. The growth of experience is continually transfiguring our 'facts' for us, and it is only by an *ex post facto* fiction that we declare them to have been 'all along' what they have *come to mean for us*. To the vision of the rudimentary eye the world is *not* coloured; it becomes so only to the eye which has developed colour 'sensitiveness': just so the 'fact' of each phase of experience is relative to our knowledge, and that knowledge depends on our efforts and desires to know. Or, if we cling to the

> notion of an absolutely objective fact of which the imperfect stages of knowledge only catch distorted glimpses, we must at least admit that only a final and perfect rounding-off of knowledge would be adequate to the cognition of such fact. The facts therefore which *we* as yet encounter are not of this character: it may turn out that they are not what they seem and can be transfigured if we try. Hence the antithesis of subjective and objective is a false one: in the process of experience 'subject' and 'object' are only the poles, and the 'subject' is the 'positive' pole from which proceeds the impetus to the growth of knowledge. For the modifications in the world, which we desire, can only be brought about by our assuming them to be possible, and *therefore* trying to effect them.

Lee Smolinism 11 : *What is needed is instead a relationalism, according to which the future is restricted by, but not determined from the present, so that genuine novelty and invention, while rare, are real and possible.*

Why relationalism? I don't see a connection between (what I know of) that word and much of the rest of your essay.

Lee Smolinism 12 : *The notion that truth is eternal and lies outside of the world we experience is not only a religious idea. It is in science as well, as is exemplified by the Platonic view of mathematics.*

You remind me here of a point that Richard Rorty once made forcefully. Unfortunately, I seem to have lost my copy and I haven't been able to get hold of the full paper online (titled "Pragmatism as Anti-authoritarianism"). But here is a part of the abstract for your enjoyment:

> There is a useful analogy to be drawn between the pragmatists' criticism of the idea that truth is a matter of correspondence to the intrinsic nature of reality and the Enlightenment's criticism of the idea that morality is a matter of correspondence to the will of a Divine Being. The pragmatists' anti-representationalist account of belief is, among other things, a protest against the idea that human beings must humble themselves before something non-human, whether the Will of God or the Intrinsic Nature of Reality. Seeing anti-representationalism as a version of anti-authoritarianism permits one to appreciate an analogy which was central to John Dewey's thought: the analogy between ceasing to believe in Sin and ceasing to accept the distinction between Reality and Appearance.

Lee Smolinism 13 : *This view is not popular among philosophers due to a powerful and commonsense objection to it, which is that to the extent that mathematical objects live in a separate realm outside of time and space, there is no way for human beings to gain access to or knowledge about them.*

Typo? "gain access to or have knowledge" maybe?

Lee Smolinism 14 : *This was the view of Einstein and many 20th Century philosophers (although, one of them. the logical positivist Carnap, recounts conversations in which Einstein regretted the loss of a place for the present moment).*

Typo: period after them. Also, it is not completely clear that the word "regretted" should be used here. Here's the Carnap quote,

> Einstein said that the problem of the Now worried him seriously. He explained that the experience of the Now means something special for man, something essentially different from the past and the future, but that this important difference does not and cannot occur within physics. That this experience cannot be grasped by science seemed to him a matter of painful but inevitable resignation.

On the other hand compare that to what he wrote to Michele Besso's widow,

> In quitting this strange world he has once again preceded me by just a little. That doesn't mean anything. For we convinced physicists the distinction between past, present, and future is only an illusion, however persistent.

Just a question: Does "painful resignation" imply "regret"?

Lee Smolinism 15 : *This field is as much speculative metaphysics as science. A lot of its literature is taken up with issues that can have no bearing on experiment, such as how probabilities are to be defined in an infinite and timeless realm . . .*

Funny, this is just about the same thing I would say of the Everettians.

Lee Smolinism 16 : *There is a powerful if unconscious attraction to theories in which time plays no role. It gives theorists the impression of standing outside the world, in a timeless realm of pure truth, against which the time and contingency of the real world pale.*

I love the way Arthur Lovejoy puts a similar point in his book *The Great Chain of Being*:

> Some examples of metaphysical pathos in the stricter sense ought . . . to be given. A potent variety is the eternalistic pathos—the aesthetic pleasure which the bare abstract idea of immutability gives us. The greater philosophical poets know well how to evoke it. In English poetry it is illustrated by those familiar lines in Shelley's *Adonais* of which we have all at some time felt the magic:
>
> > The One remains, the many-change and pass,
> > Heaven's light forever shines, earth's shadows fly . . . .
>
> It is not self-evident that remaining forever unchanged should be regarded as an excellence; yet through the associations and the half-formed images which the mere conception of changelessness arouses—for one thing, the feeling of rest which its *innere Nachahmung* induces in us in our tired moods—a philosophy which tells us that at the heart of things there is a reality wherein is no variableness nor shadow that is cast by turning, is sure to find its response in our emotional natures, at all events in certain phases of individual or group experience. Shelley's lines exemplify also another sort of metaphysical pathos, often conjoined with the last—the monistic or pantheistic pathos. That it should afford so many people a peculiar satisfaction to say that All is One is, as William James once remarked, a rather puzzling thing. What is there more beautiful or more venerable about the numeral *one* than about any other number? But psychologically the force of the monistic pathos is in some degree intelligible when one considers the nature of the implicit responses which talk about oneness produces. It affords, for example, a welcome sense of freedom, arising from a triumph over, or an absolution from, the troublesome cleavages and disjunctions of things. To recognize that things which we have hitherto kept apart in our minds are somehow the same

> thing—that, of itself, is normally an agreeable experience for human beings. . . . So, again, when a monistic philosophy declares, or suggests, that one is oneself a part of the universal Oneness, a whole complex of obscure emotional responses is released. The deliquescence of the sense—the often so fatiguing sense—of separate personality, for example, which comes in various ways (as in the so-called mob-spirit), is also capable of excitation, and of really powerful excitation, too, by a mere metaphysical theorem. Mr. Santayana's sonnet beginning "I would I might forget that I am I" almost perfectly expresses the mood in which conscious individuality, as such, becomes a burden. Just such escape for our imaginations from the sense of being a limited, particular self the monistic philosophies sometimes give us.

Of course, when he goes on to opine about my own pathos (the voluntaristic one), I'm not so enamored with his sharp tongue!

Lee Smolinism 17 : *We live in a world in which it is impossible in principle to anticipate and list most of the contingencies that will arise in the future. Neither the political context, nor the inventions, nor the fashions, nor the weather nor climate are specifiable in advance.*

Nor the outcome of a single minuscule quantum measurement. All these instances, if you ask me, are cut from a single cloth. Where quantum mechanics has pedagogical value in comparison to the others, however, is with its shock value. For it is in fundamental physics that one *previously* would not have expected such a thing. Most importantly, it gives a sense that the issue is central to our world.

Lee Smolinism 18 : *Moreover, the higher level laws can and often do influence the lower levels of description. There are molecules which only exist on Earth because they are components of biological systems and hence byproducts of evolution. Suppose we wanted to know how many hemoglobin molecules are present on Earth. Hemoglobin is just a big molecule, completely describable by the laws of quantum mechanics. But there is no way the question of how many exist at a given time could be arrived at just by solving the laws of quantum mechanics. To explain why there are any hemoglobin molecules on Earth, and to estimate their number, one must reason in terms of higher order laws which apply to animals and are not completely reducible to quantum mechanics.*

In my present stage of thought, I myself would never say "hemoglobin is a big molecule **completely** describable by the laws of quantum mechanics." As I wouldn't say it of humans, nor of diamonds (see Section VI in my essay), I wouldn't say it of hemoglobin. It is the "completely" thing that contradicts my own nonreductionism. For me, quantum mechanics is *additive* to whatever is particular in an object/concept. Be that as it may, there are definitely similarities in our thinking here. Here's a little passage and footnote from my essay:

> The metaphysics of empiricism can be put like this. Everything experienced, everything experienceable has no less an ontological status than anything else.
>
> Footnote: That is, every piece of the universe had better be hard-wired for the contingency that an agent might experience it somewhere, somehow, no matter how long and drawn out the ultimate chain might be to such a potential experience. Does this mean even "elementary" physical events just after the Big Bang must make use of *concepts* that, to the reductionist mind, ought to be 15 billion years removed down the evolutionary chain? You bet it does. But a nonreductionist metaphysic need make no apology for this—such things are in the very idea. John Wheeler's great smoky dragon comes into the world biting its own tail.

> W. K. Wootters tells a lovely story of an encounter he had several years ago of with his young son Nate. Nate said, "I wish I could make this flower move with my mind." Wootters reached out and pushed the flower, saying, "You can. You do it like this." From the perspective here, this is an example of an interaction between two nonreductionist realms. Each realm influences the other as its turn comes. There is a kind of reciprocality in this, an action-reaction principle, that most reductionist visions of the world would find obscene.

Lee Smolinism 19 : *The best that theory may be able to do is describe what complexity theorist Stuart Kauffman calls the adjacent possible, that is discern some of the very next forms that may emerge. Or there may be singularities where not even this is possible.*

I've always been intrigued by this passage from Henri Poincaré's essay, "The Evolution of Laws," in his *Dernières Pensées*:

> Mr. Boutroux, in his writings on the contingency of the laws of Nature, queried, whether natural laws are not susceptible to change and if the world evolves continuously, whether the laws themselves which govern this evolution are alone exempt from all variation. ... I should like to consider a few of the aspects which the problem can assume. ...
>
> In summary, we can know nothing of the past unless we admit that the laws have not changed; if we do admit this, the question of the evolution of the laws is meaningless; if we do not admit this condition, the question is impossible of solution, just as with all questions which relate to the past. ...
>
> But, it may be asked, is it not possible that the application of the process just described may lead to a contradiction, or, if we wish, that our differential equations admit of no solution? Since the hypothesis of the immutability of the laws, posited at the beginning of our argument would lead to an absurd consequence, we would have demonstrated *per absurdum* that laws have changed, while at the same time we would be forever unable to know in what sense.
>
> Since this process is reversible, what we have just said applies to the future as well, and there would seem to be cases in which we would be able to state that before a particular date the world would have to come to an end or change its laws; if, for example, our calculations indicate that on that date one of the quantities which we have to consider is due to become infinite or to assume a value which is physically impossible. To perish or to change its laws is just about the same thing; a world which would no longer have the same laws as ours would no longer be our world but another one.

Your remark about singularities brought that to mind.

Lee Smolinism 20 : *The first person to ask this question cogently seems to have been the founder of the American pragmatist school of philosophy, Charles Sanders Pierce. He wrote "The only possible way of accounting for the laws of nature and of uniformity in general is to presume them results of evolution".*

Typo: should be Peirce. With regard to Peirce being the first, I suppose it depends upon what you mean be "cogently." Peirce wrote those lines in "The Architecture of Theories" in 1891. On the other hand Émile Boutroux published his book *The Contingency of the Laws of Nature* in 1874. And then there was Bergson, whose books started appearing in 1889. James in his essay, "On the Notion of Reality as Changing," writes:

> Volumes i, ii, and iii of the *Monist* (1890–1893) contain a number of articles by Mr. Charles S. Peirce, articles the originality of which has apparently prevented their making an immediate impression, but which, if I mistake not, will prove a gold-mine of ideas for thinkers of the coming generation. Mr. Peirce's views, tho reached so differently, are altogether congruous with Bergson's. Both philosophers believe that the appearance of novelty in things is genuine. To an observer standing outside of its generating causes, novelty can appear only as so much 'chance'; to one who stands inside it is the expression of 'free creative activity.' Peirce's 'tychism' is thus practically synonymous with Bergson's 'devenir réel.' The common objection to admitting novelties is that by jumping abruptly in, *ex nihilo*, they shatter the world's rational continuity. Peirce meets this objection by combining his tychism with an express doctrine of 'synechism' or continuity, the two doctrines merging into the higher synthesis on which he bestows the name of 'agapasticism' (*loc. cit.*, iii, 188), which means exactly the same thing as Bergson's 'évolution créatrice.' Novelty, as empirically found, does n't arrive by jumps and jolts, it leaks in insensibly, for adjacents in experience are always interfused, the smallest real datum being both a coming and a going, and even numerical distinctness being realized effectively only after a concrete interval has passed. ...
>
> I can give no further account of Mr. Peirce's ideas in this note, but I earnestly advise all students of Bergson to compare them with those of the french philosopher.

Thus, I myself, would probably credit the three as a triumvirate.

Lee Smolinism 21 : *This provides further motivation for believing in the reality of time and the openness of the future. If the selection of the laws of nature that determine what elementary particles exist and how they interact is a result of evolution analogous to natural selection, then there must be a time that this evolution plays out in. And it must be a time that exists prior to the laws, for the laws must have scope to evolve in time.*

Yes, but I wonder whether there might not *in principle* be as many "reality of times" as there are agents (more broadly, objects)—time being single valued only to the extent that groups of agents interact and "synchronize." My QBist program already captures a bit of an image like this. Wheeler had his "many-fingered time," I need a good name for mine.

Lee Smolinism 22 : *The big bang may even be an event that brought into being a novel form of organization in which particles move in space; if this is so then the laws we probe with particle accelerators like the LHC emerged then.*

I'm not sure I understand how to parse this sentence. I.e., there's something about it that confuses me; I'm not sure what it means.

Lee Smolinism 23 : *But we also have evolved the capacity to think quickly and act decisively at the snap of a twig.*

Nice; immediately gave me imagery of early man huddled in the night.

Lee Smolinism 24 : *Fundamentalist communities and open communities can be distinguished by their understanding of time and the possibilities for knowledge of the future. Fundamentalists believe that the future already exists, and are prone to believe that time is altogether an illusion. They believe in mythological stories according to which the ever changing world we perceive is an illusion that hides a true timeless reality.*

This took my mind back to the fact that Travis Norsen, beside calling me a solipsist at every turn just because I say "quantum states do not exist," also happens to be a follower of Ayn Rand.

Lee Smolinism 25 : *Open communities teach that the future is not yet real, what is real instead is time and the processes by which the future unfolds and emerges out of the present. For a fundamentalist, human agency is an illusion, for a member of an open society human agency is the necessary means of constructing a future that will not otherwise exist.*

I might as well end things with another long quote—Richard Rorty and William James this time:

> By way of conclusion, I want to offer a gloss on the passage from William James which supplied the title for this conference. James writes
>
> > What really exists is not things made but things in the making. Once made, they are dead, and an infinite number of alternative conceptual decompositions can be used in defining them. But put yourself in the making by a stroke of intuitive sympathy with the thing and, the whole range of possible decompositions coming at once into your possession, you are no longer troubled by the question of which of them is the more absolutely true. Reality falls in passing into conceptual analysis; it mounts in living its own undivided life-it buds and burgeons, changes and creates. Once adopt the movement of this life in any given instance and you know Bergson calls the *devenir réel*, by which the thing evolves and grows. Philosophy should seek this kind of living understanding of the movement of reality-not follow science in vainly patching together fragments of its dead results.
>
> There are various things wrong with this passage .... But if you read this passage as a meditation on the relation between history and philosophy, it takes on a non-metaphysical meaning, and says something important: namely that the human future will always, with a little luck and a lot of imagination, be so different from the past that it is pointless to look for a set of concepts that will cover both. Bergson was right to think that the metaphysical attempt to see things under the aspect of eternity was a failure ....
>
> Reading James in this way lets one draw the moral: do not think that making the past ideas coherent with one another will ever enable you to find a substitute for imagination. Do not think that philosophy will ever succeed in its attempt to trump poetry and the arts. Do not look to philosophers for anything different than the sort of inspiration that you get from poets, painters, musicians, and architects. For their ability to find coherence will never be more than a perspicuous archival arrangement of the imaginative products of the past. They will never provide authoritative guidance for the imagination of the present.
>
> John Rajchman describes James as preoccupied with "the problem of novelty"—the problem of how to deal with "things in the making", how to see and respond to the emergence of things for which we have no preset manner of seeing or responding. I do not think that there is a solution to this problem, and therefore, as a good verificationist, I do not see it as a problem. The thing to do with novelty is just to be grateful for it, and to create the socio-political conditions which will ensure that there will be a lot more of it. There is a political problem about how to encourage novelty without weakening communal solidarity and social order, but this is a problem to be solved

> ambulando, experimentally, and democratically. All that philosophy can do to help out with this political problem is to keep reminding us of what is likely to happen if the past is allowed to dictate terms to the future.

For a good bit of the point of your essay is in "reminding us of what is likely to happen if the past is allowed to dictate terms to the future."

Thanks for sharing your essay and giving me the opportunity to think a little. I hope it's well-received wherever you send it.

24-03-10 *Revealing* (to D. M. Appleby, R. Schack, and H. C. von Baeyer)

I've just had a little exchange with Adrian Kent on my lines,

> What are we then to do with the Born Rule for calculating quantum probabilities? Throw it away and say it never mattered? It is true that quite an effort has been made by the Everettians to rederive the rule from *decision theory*. Some sympathizers think it works [Wallace09], some don't [Kent09]. But outside the *sprachspiel* who could ever believe? No amount of sophistry can make "decision" anything other than a hollow concept in a predetermined world.

from the new paper.

My guess is that you'll really enjoy the contortions in his reply, so I'll share it with you:

> It seems to me that (a) we can sensibly discuss the preferences of agents in a multiverse — i.e. what successor states they might prefer if they had freedom to choose amongst them — and so that it isn't ridiculous to formulate a decision theory in this context. But also that (b) the project of trying to recover Copenhagen predictions from such a theory ultimately fails. Maybe we disagree on (a)?

Just look at that lovely counterfactual! "what successor states they *might* prefer if they *had freedom to choose* amongst them"!!! No, we wouldn't dare say outright that they *have* freedom; we only talk about what they would do if they *had* freedom! What a sad statement on the common philosophies of our time!

25-03-10 *Very, Very Final Version* (to the QBies & C. Ferrie)

You may be interested in Howard Barnum's posting this morning: "Quantum Knowledge, Quantum Belief, Quantum Reality: Notes of a QBist Fellow Traveler," `http://arxiv.org/abs/1003.4555`.

Also, attached is the very, very final version of my own screed. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] I'm posting the thing today finally. Since the version you've all read, the very important Footnote 22 has been added.

25-03-10 *Possible?* (to Å. Ericsson)

Åsa-ism 1: *If you should assume everything to be possible because you have no proof of the opposite, wouldn't that lead you to assume just anything "to be the case" (or whatever one should* *say without using the word "true") for which there is no proof of the opposite? But there cannot really be any such rigorous proof. And we cannot live like that. We form beliefs from our experiences, which leads us (at least me!) not to believe in* [just] *anything and also not to believe everything is possible for us.*

If we did not believe that we could prolong human life with better technologies, we as a society would not invest in medical research. But we do. And what is the limit of the prolongation? How far can we go? Can we make people last 100 years on average? 150? Why should there be any end in sight? We console our personal selves to the idea of eventual death because the alternative is not within reach now—not within our expected lifetimes. And that is as we should do. But if death is a *necessary* end, why worry about the difference between 80 and 100 years? *Methodologically* (as Schiller says), we are behaving as if eventually it can be completely overcome. All our industry is geared toward that, one step at a time. First we wipe out HIV, then we wipe out heart disease, then we wipe out cancer (all cancers!). There is no end to the process. And we should not believe that there is one.

25-03-10 *Torture and Relief* (to A. Kent)

Thanks for your note.

(1) OK, I back off on the label "sympathizer." The insult now reads:

> Or take the Everettians. Their world purports to have no observers, but then it has no probabilities either. What are we then to do with the Born Rule for calculating quantum probabilities? Throw it away and say it never mattered? It is true that quite an effort has been made by the Everettians to rederive the rule from *decision theory.* Of those who take the point seriously, some think it works `\cite{Wallace09}`, some don't `\cite{Kent09}`. But outside the *sprachspiel* who could ever believe? No amount of sophistry can make "decision" anything other than a hollow concept in a predetermined world.

You were right to call me on that, for all I had really meant was, "of those who take the point seriously." My plan is to finally post the darned thing today.

(2) Because I don't really. For me it is not that the Everettian interpretation is "bizarre" (as many people say) or carries too much "ontological baggage" (as others say), but that it is simply empty of content. It is a great effort full of sound and fury, in the end signifying nothing. When I heard David Wallace once admit (at the MWI meeting you organized here at PI) that one *could* give an Everettian interpretation of classical Liouville mechanics, that capped it all off for me. His defence was only, "but there is no great need in that case." And so I say of QM anyway. But the essential point is, one can play the MWI game with *any* probabilistic theory. And when done, it is a "more or less arbitrary appendage" to the theory.

(3a) I have to admit getting a really good laugh from this part of your sentence:

Kentism 10: *[W]e can sensibly discuss the preferences of agents in a multiverse – i.e. what successor states they might prefer if they had freedom to choose amongst them ...*

For the Everettians, each one of them, are great monists at heart; in a different century they would have been Hegelians. The real thing behind their motivation is that paper thin. They contemplate a world with no true counterfactuals from within, but then to make sense of this world they invoke a counterfactual at the outset, "what successor states they might prefer if they had freedom to choose amongst them." When I read your sentence, a little sarcastic voice in me came out fighting, "No, we wouldn't *dare* say outright that they *have* freedom; we only talk about what they would do if they *had* freedom!"

Kentism 11: *Obviously, though, I'd be intellectually unhappy with the suggestion that there isn't anything substantive to discuss.*

My psychoanalysis is that this is why my first version of the Everettian insult stung you, when I had intended you to be "on the good side of it". I am sorry about that—I don't mind insulting people at times if I think it might help shake a nut or bolt loose, but I did not want to insult you. On the other hand, I can't hide what you suspect—that I do believe there is not too much substantive to discuss in the MWI view *at this stage in our thinking.* (There once was, even for me, before I had wholly given up on psi-ontology. But now I think it is time to move on.)

One technical point related to this about my present attitude on the Born Rule. It is that it is not something to be *derived* (in Wallace's way, or Gleason's way, or any other way). Rather my present feeling is that it is the fundamental statement of what quantum mechanics is about—it is a rule that takes probabilities in, and gives new probabilities back out. (See the attached "press release" on my house; what I mean in the last sentence is made formal by the "pretty equation" at the bottom of it. Rüdiger and I call it the urgleichung.) Two further notes below on this "attitude" in case you're interested.

I'd love to come to Cambridge again (if you still want me after the note above). Thanks for the invitation. Maybe sometime this Fall.

25-03-10 *What Bayesians Expect of Each Other* (to G. Chiribella)

This is the paper I was talking about. [See M. J. Bayarri and M. H. DeGroot, "What Bayesians Expect of Each Other," J. Am. Stat. Assoc. **86**, 924–932 (1991).] It'd be nice to get a quantum version of this theorem. If you'd like to discuss and think about this, let me know. It'd be great if we could get something solid.

26-03-10 *Templeton OFI* (to R. Schack)

There is very little on paper so far, but I have been thinking much in my head. Fear not: It will come together!

Any preference on these potential titles?

- That the World Can Be Otherwise: The Essence of Quantum Theory
- That the World Could Be Otherwise: The Essence of Quantum Theory
- That the World Has the Power to Be Otherwise: The Essence of Quantum Theory
- The World's Power to Be Otherwise: The Essence of Quantum Theory
- The Essence of Quantum Theory: That the World Can Be Otherwise
- Quantum Theory's Essence: That the World Can Be Otherwise

26-03-10 *Title* (to R. Schack)

"That the World Can Be Shaped: Quantum Mechanics, Counterfactuality, Free Will"

26-03-10 *Thinking, Scheming* (to S. Weinstein & D. Fraser)

Rüdiger Schack and I are working on a Templeton Foundation proposal, and I find myself trying to think big. At the moment, I am playing with, "That the World Can Be Shaped: Quantum Mechanics, Counterfactuality, Free Will," for its title. Anyway, the reason I write you is that I am also playing with the idea of using part of the money to hire a QBism-pragmatism-Cartwrightism sympathetic philosopher as a two-year postdoc. (Beware!) But one of the things I'm bumping into is the space limitations at PI and the "second-class status" of our "associate postdocs" (those that are housed at PI but have funding coming from external sources). Which leads me to the following idea: How receptive would the UW philosophy department be to a postdoc freebie of this variety? There is the space issue already mentioned, but also I'm thinking it might be healthier for the person to be around there more than here anyway. He/she and I could have weekly meetings, lunch, etc., but otherwise it'd probably be good for the person to mingle more closely with proper philosophers.

A couple of questions if this is a possibility. What is the standard salary of a philosophy postdoc at UW? And how much overhead would your department require? (Templeton will give up to 15%. But of course the smaller the percentage I can get away with the happier I'd be.)

Just some thoughts.

By the way, if you don't know what I mean by QBism see the attached new paper and "press release" on my house. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] Particularly the connection between QBism and pragmatism / "Cartwrightism" is described in Section VI, starting page 19.

26-03-10 *QBist Double Slit* (to M. A. Graydon)

Graydonism 1: *Question: Have you written anything on a QBist interpretation of Young's experiment? In the text-book explanations we find references to free-particle wavefunctions in infinite dimensional Hilbert spaces . . . so does this mean that one cannot translate the math into SIC language?*

The Mach-Zehnder interferometer captures the essence of that, and it is a finite dimensional system. Gelo is looking at this in detail at the moment—you might get his take on that.

With regard to infinite dimensional systems more generally, I really don't know how the answer is going to go: It could be that SICs do after all have an infinite dimensional limit, but it could be that they don't. Gutwise, I kind of hope that they don't have an infinite limit. So much the worse for infinite dimensional Hilbert spaces, I say. Physics has been wrenching with the difficulties of field theory and (the absurdity of) renormalization for so long, that maybe it is time for worldview re-adjustment. Infinite dimension might be like infinite inertial mass—a handy approximation to make at times, but ultimately not something that is ever really true.

In general I advocate what Jaynes calls the "the cautious approach policy":

> **Our "Cautious Approach" Policy**
>
> The derivation of the rules of probability theory from simple desiderata of rationality and consistency in Chapter 2 applied to discrete, finite sets of propositions. Finite sets

are therefore our safe harbor, where Cox's theorems apply and nobody has ever been able to produce an inconsistency from application of the sum and product rules. Likewise, in elementary arithmetic finite sets are the safe harbor in which nobody has been able to produce an inconsistency from applying the rules of addition and multiplication.

But as soon as we try to extend probability theory to infinite sets, we are faced with the need to exercise the same kind of mathematical caution that one needs in proceeding from finite arithmetic expressions to infinite series. The "parlor game" at the beginning of Chapter 15 illustrates how easy it is to commit errors by supposing that the operations of elementary arithmetic and analysis, that are always safe on finite sets, may be carried out also on infinite sets.

In probability theory, it appears that the only safe procedure known at present is to derive our results first by strict application of the rules of probability theory on finite sets of propositions; then after the finite set result is before us, observe how it behaves as the number of propositions increases indefinitely. There are, essentially, three possibilities:

(1) It tends smoothly to a finite limit, some terms just becoming smaller and dropping out, leaving behind a simpler analytical expression.

(2) It blows up, i.e., becomes infinite in the limit.

(3) It remains bounded, but oscillates or fluctuates forever, never tending to any definite limit.

In case (1) we say that the limit is "well behaved" and accept the limit as the correct solution on the infinite set. In cases (2) and (3) the limit is ill-behaved and cannot be considered a valid solution to the problem. Then we refuse to pass to the limit at all.

This is the "Look before you leap" policy: in principle, we pass to a limit only after verifying that the limit is well-behaved. Of course, in practice this does not mean that we conduct such a test anew on every problem; most situations arise repeatedly, and rules of conduct for the standard situations can be set down once and for all. But in case of doubt, we have no choice but to carry out this test.

In cases where the limit is well-behaved, it may be possible to get the correct answer by operating directly on the infinite set, but one cannot count on it. If the limit is not well-behaved, then any attempt to solve the problem directly on the infinite set would have led to nonsense, *the cause of which cannot be seen if one looks only at the limit, and not the limiting process.* The paradoxes noted in Chapter 15 illustrate some of the horrors that have resulted from carelessness in this regard.

Recommended reading!

28-03-10 *Game Theory and Quantum Mechanics* (to D. H. Wolpert)

Wolpertism 1: *There's been a lot of stuff on applying game theory to situations where the game players have quantum-mechanical apparatus to play with. "Quantum games" it's called. IMO, it's physicists looking for something to do.*

There's also been work where people somehow try to modify the Nash equilibrium concept to be "quantum mechanical". The point of which eludes me.

Both of these are simply importing physics into game theory. Yawn.

Now there's another set of ideas, which nobody has managed to run to ground, which would be truly profound, and (I think) related to what you're talking about. This is importing game theory *into physics, rather than vice-versa. Basically, can one cast various physics scenarios in terms of particles that are "players in a game" in some sense.*

I like the sound of such an idea. A bit like exporting "agency" to the "inanimate" world.

29-03-10 *Symmetry of Quantum State Space* (to A. Fenyes)

A SIC measurement allows a one-to-one correspondence between $d \times d$ density operators (positive semi-definite matrices with trace 1) and probability distributions over d^2 points. The result is an affine transformation from one set to the other. Affine transformations preserve various convexity properties (like mixtures, extreme points, and maybe some other things), but they don't necessarily preserve geometric symmetry. For instance, we can get from an ellipsoid to a sphere by an affine transformation. And here is a nice symmetry property of the sphere that a proper ellipsoid doesn't share: The sphere (the ball really) can be thought of as a union of *regular* simplexes each of full dimension.

If SICs exist, then the space of density operators has that property as well—quantum state space can be thought of as a union of regular simplexes.[222] In either representation, I guess: I.e., represented as complex matrices or as probability distributions.

I've always thought of the SIC-induced affine mapping as the best way to "straighten out" out the space of quantum states. For instance, if for a qubit we do not use a SIC, but some other informationally complete POVM to induce a mapping from quantum states to probability distributions, we will not get a sphere, but an ellipsoid. That's a kind of trivial change, of course, but I don't know the full implications of the change in higher dimensions. Maybe there are better reasons to resist "ellipsifying" the sphere there. (Given my remarks above, if SICs exist, quantum-state space is already "straightened out" in a way. I.e., the density operator formalism already captures this nice symmetry—one doesn't have to map on to the probability simplex to see it. If on the other hand, SICs don't exist—then I think one must always "ellipsify" the Bloch ball when transforming from density operators to probability distributions via an informationally complete POVM with rank-1 elements.)

Anyway, I didn't know how sensitive $\mathrm{sym}(x, S)$ was to affine transformations when I wrote my first note to you—I hadn't really looked at the paper to any depth. But now, looking over the paper, I guess Proposition 3 caps off this question. [See A. Belloni and R. M. Freund, "On the Symmetry Function of a Convex Set," Math. Programming **111**, 57–93 (2008).]

Still though, one can ask whether better knowing the structure of the function $\mathrm{sym}(x, S)$ for the set of quantum states might give any clues on SIC existence or nonexistence, or even just reveal some nice features of the state-space that we hadn't known before.

I like your question of comparing the scaling of S to that of a simplex as dimension grows. Also, what is the symmetry value of a sphere of the same dimension? (I'm bad: When I say sphere, I most always mean ball.) I guess it's 1.

29-03-10 *Premature QBism!* (to N. D. Mermin)

To reply:

Merminition 185: *1. Of the three examples at the end of page 1, the second (spontaneous collapse) is more than an interpretive strategy, since it alters the quantitative predictions of the theory. Of course it's motivated by an implicit interpretive strategy.*

[222] See Footnote 218 for a technical, but remediable, mistake in the original statement of this.

Agreed, it is "more than," but I had meant to cover that with this: "The trouble with all these interpretations as quick fixes for Bell's hard-edged remark is that they look to be just that, *really quick fixes*. They look to be interpretive strategies hardly compelled by the particular details of the quantum formalism, giving only more or less arbitrary appendages to it." Looking back at the draft, I see I had cut out the appendage part in the small version of the paper. I hope you'll give me a pass on this one: I'd rather not complexify the issue too much in a "quick introduction."

Merminition 186 : *2. Remark on pilot-wave theories on upper left of page 2. Do you really mean "pretending" (and not, for example, "presenting")? It's a peculiar use of "pretend" and I don't understand what it means here. The term veneer is already disparaging enough. And what is it about counting angels on the head of a pin that is supposed to resemble pilot-wave theories. (When Pauli used the image in his letter to Born he was illustrating worrying about things you can't know anything about.)*

You're right. I was worried there was something wrong with that sentence, but could never quite put my finger on it. Is this better (basically what you suggest):

> If there were no equations to give the illusion of science, this would have been called counting angels on the head of a pin.

And yes, it is precisely the Pauli point. There is a fun exchange between Bill Unruh and Roderich Tumulka during one of the PIAF meeting panel discussions a couple years back. Tumulka said something like, "Bohmian mechanics allows one to calculate answers to some questions that all the other interpretations can't even formulate. For instance, the amount of time a particle is on the inside of a potential barrier." Unruh said, "That's not true! My interpretation allows it. Every particle stays in every potential barrier precisely seven seconds." Tumulka said, "But this is nonsense. We're not just making some number up; we have a scientific theory." Unruh said, "Your theory is every bit as scientific as mine."

Merminition 187 : *3. Remark about Bell on upper right of page 2. You attribute to Bell a fear that is not in the quotation you cite on page 1 but in your own broader remarks in the paragraph that follows it. I'm see no evidence that Bell feared that the conceptual weakness of the theory might lead to a breakdown in its empirical validity.*

Not a fear of a breakdown in QM's empirical validity, but that physics is way, way on the wrong track because of its seeming need to invoke the observer. I've always interpreted Bell as thinking along the lines of something Einstein said:

> It may appear as if all such considerations were just superfluous learned hairsplitting, which have nothing to do with physics proper. However, it depends precisely upon such considerations in which direction one believes one must look for the future conceptual basis of physics.

Merminition 188 : *And I'm not sure what you mean when you say that the only thing to fear is that particular fear itself, regardless of whom you attribute it to. Are you saying that we should take QM as correct and try to understand it better?*

Yes. Or more accurately that the observer is essential in formulating quantum mechanics, and we should try to understand that better. FDR: "The only thing we have to fear is fear itself."

Merminition 189 : *Finally, I don't see anything "shrill" in Bell's rhetoric, and I don't see why your simple resolution of his worry about the collapse of the wave-function of the universe warrants calling his concern "the least of his worries."*

In that spot, I had originally written:

> The real substance of Bell's fear is just that—fear itself. To succumb to it is to block the way to really understanding the theory. Moreover, the harsher shrills of Bell's rhetoric are the least of the worries:

Upon which, Hans Christian von Baeyer who read the draft wrote:

> I don't think shrill is a noun. For "hasher shrills" I would put "shriller notes."

I just basically followed through with his suggestion. But you're right, "shrill" is too strong. How about simply,

> the harsher notes of Bell's rhetoric are the least of the worries:

Merminition 190 : *This leads the reader to expect an even bigger problem — not the solution to the problem.*

Because that is basically where Asher would have left it ... never trying to make his view of quantum mechanics completely consistent.

Merminition 191 : *4. Still upper right, page 2. I'm not sure how you get from antibodies in some of the elderly to the antibody itself being elderly. And I'm not sure you want to call it ineffective, when it's the direction you want to move in.*

Right again: Strange for me to call the antibody itself elderly. How about simply,

> But this much of the solution is only a somewhat ineffective antibody. Its presence is mostly a call for more research. Luckily the days for this are ripe, and it has much to do with the development of the field of quantum information

Merminition 192 : *Nothing else for now.*

The bigger paper is a better paper, but of course inherits many of the bad word choices you mention above.

The only trouble with your being right with your remarks, is that now I'll have to repost the damned thing! And frankly I'm frightened of that.

I hope you'll still read the big paper, even though you've read the little one—it contains a load of mini-epiphanies for me. For instance, Footnote 44, where I turn "the quantum-to-classical transition" around. I'd like to hear whether you think the paper makes any progress toward something else you said:

Merminition 193 : *I suppose it's ineffective in that it hasn't convinced everybody, and is not yet sharply enough developed.*

30-03-10 *Quantum Bayesianism* (to J. Wright)

This is a very late reply; I'm sorry about that. Let me just respond to two points.

Wrightism 1: *My concern is that on one hand the Bayesian view takes a very forward stance, saying so much as "quantum states are not real" and then you said in your lecture that you'd be willing to accept other interpretations on pragmatic grounds — in the sense of electromagnetic fields.*

and

Wrightism 2: *It was an example used in jest but then moments later you talked about the problem of quantum channels and how you wouldn't stand against an interpretation if thinking in that manner provided a solution to such a problem.*

The root of these blurbs is not an inconsistency in my thinking or a religious reliance on pragmatism, but a politician's way of negotiating—a way of demonstrating the possibility of compromise. I give a criterion for what *would* move me toward their views. And I'm honest about that. But it is a bit hollow nonetheless: For I make this "bargain" in very strong confidence that they won't be able to deliver the goods. 50 years of history already shows a world of difference between electromagnetic potentials and the Bohmian trajectories. Potentials, though unobservable except through the forces "they give rise to," are used every day in the aid of hundreds of problems in electromagnetism classes. Bohmian trajectories, even if assumed unobservable except through the measurement clicks "they give rise to," are *nearly never* used. The only exceptions I know of are in the demo problems invented to make them look like a healthy option (like the question of how long in a potential barrier). No actual practical problem is ever helped by their aid.

I hope that makes my strategy a little more understandable, even if a bit slimy.

30-03-10 *That Old Shoe* (to S. Lloyd)

I nearly forgot: I believe I thanked you for the first time in a paper. Have a look at the end of `http://arxiv.org/abs/1003.5209`; the wording might amuse you. The paper is my latest attempt to verbalize a worldview that's been growing in me. Apparently that '92 lecture of yours left a lasting impression on me. (Though in the text of Section VI, I transformed your shoe into a diamond to fit the rest of the story. It deserved to be a diamond anyway!)

30-03-10 *I Knew He'd Just Call Me Solipsist Again, and he did* (to N. D. Mermin)

I made a probability one assignment, and the world complied to what I believed with all my heart. Does that synchronicity call out for further explanation? I made the probability assignment because all my experience thus far had pointed me in just that direction. I compacted all that I felt and saw around me, all my previous dealings with people and *particular* people, my choice of words while writing, and everything else in my memory into a singular supremely strong probability assignment. And the world complied: The action I took in posting my paper to `arXiv.org` led another part of the world to a reaction that I was quite sure I could see in advance.

I suppose it'd be no solace to Travis to know that I really do believe in his autonomy—that he is more than the sum total of my provocations. See below.

With a smile of satisfaction,

Original note from Travis Norsen, "Bell, locality, etc," dated 30 March 2010

I've just read the short QBism paper you put on arxiv the other day, and skimmed the longer one, and thought I'd write and just say hi and register a couple of very quick comments. First, I appreciated the compliment in the footnote of the longer paper, even if it was, well, somewhat qualified! =) More seriously, I think there is a kind of deeper disagreement between us (or between you and Bell) on this stuff than you acknowledge. Let me try to explain what I think the issue is.

I think that, from the very beginning, you are just on the premise of understanding probability statements as referring to subjective degrees of belief (or something in that neighborhood). And, on that basis, your reaction to (for example) the passage from one of my papers that you quote in that footnote, is completely understandable. If you understand probability statements to be, in principle, about degrees of belief, and then you see $P(a) = 1$, then you are absolutely right: that just means that somebody believes "a" with all their heart. It doesn't necessarily mean or warrant some further claim about there being some real element of reality, "a", out there independent of human intervention, awareness, etc.

So on that part we agree. But where we disagree is about whether that's what the probabilities in this particular context actually mean. As a perfectly uncontroversial factual/historical matter, that (i.e., subjective degrees of belief) is *not* what Bell meant by the probabilities he writes down in his various papers. He is just coming from a very different place. He's taking for granted what I've elsewhere called "metaphysical realism" (i.e., that there is some kind of physical world out there independent of us, which it is the goal of candidate physical theories to describe). Then the specific concern which brings probabilities into the discussion is the desire to avoid *assuming* that determinism is true. That is, he very carefully and deliberately sets things up so that we are open, in principle and from the very beginning (and here I'm mostly talking about his formulation of "local causality" from, e.g., "la nouvelle cuisine"), to candidate physical theories (i.e., candidate descriptions of external physical reality) which involve irreducible stochasticity. So he gives, for example, a mathematical definition of "local causality" that is a statement about certain probabilities – and then literally from there (plus the additional assumption of "no conspiracies", aka "freedom") you can run the EPR argument for the existence of certain things, which then run afoul of experiment via Bell's inequality.

Now the key point here is just that the probabilities that are in the mix here – that is, the ones in Bell's definition of "local causality", because that's the premise from which everything else follows – are *not* subjective degrees of belief. They are rather the "irreducible dynamical probabilities" of some candidate theory. In particular, they are the probabilities assigned by the theory to certain physical happenings, not on the basis of anyone's subjective and perhaps-incomplete knowledge of various goings-on, but rather on the basis of a complete description of physical goings-on in some relevant spacetime region. (And note here that both the "complete" and "physical goings-on" parts of the previous sentence mean: according to, i.e., relative to, some particular candidate physical theory. So there simply is no place here where anything relating to knowledge or subjective belief or *anything like that* enters *at all*.) And so, given *this* understanding of the probability statements that are in the mix here, you can surely see why it would be quite inappropriate to say "Just because $P(a) = 1$ doesn't mean there's some fact of reality out there corresponding to 'a'." Yes, it does mean that, because (with this alternative understanding of probabilities in place) "$P(a) = 1$" literally just means that, according to some particular candidate physical theory, the physical fact corresponding to "a" definitely happens. And that means: really happens, out there in reality (according to this theory). And that is all that is needed for the whole argument to go through.

Now, don't get me wrong. I'm not saying "Well, Bell meant something different by probabilities, and so you just have to accept his view." I appreciate that you could – and I'm sure do – disagree with Bell. That is, I suspect you think that Bell's whole way of understanding probabilities is just wrong, and that it just doesn't make any sense to interpret probability statements as propensities (if that is the right terminology for Bell's view) rather than subjective degrees of belief. Fine. But then you should I think more openly acknowledge that you have this more basic disagreement – instead, I mean, of writing as if you and Bell and everybody obviously just agree that probabilities have to mean subjective degrees of belief, and then pointing to the end of some argument (like the one you quote from me) and saying "obviously this conclusion doesn't follow". Because doing that makes your argument into a kind of petty straw man sort of thing, right?

Let me also say that – *if* you really accept what I called "metaphysical realism" above, as I believe you *claim* to do – I don't see how you can possibly argue that it is meaningless/nonsense to talk about probabilities in Bell's "propensity" sense. If you think there's a physical world out there, and you think that in principle its fundamental laws might be stochastic rather than deterministic, then you just have to accept the possibility of candidate physical theories like the ones Bell contemplates. And so I have the very strong suspicion – or perhaps I should say this strengthens my very strong suspicion – that (your own protestations to the contrary notwithstanding) you actually reject "metaphysical realism". Because, frankly, it is only if you reject that, that I think it then makes any kind of logical sense to insist that *all* probability statements *must* just refer to subjective degrees of belief. And that, of course, would just make you a **solipsist**.[223] Which I know you vehemently deny. But in your heart of hearts, come on – you must understand why you keep hearing this accusation.

Let me also just say that I haven't forgotten that you owe "me" (really, the world) some kind of more explicit and careful QBist analysis of (in particular) Bell's formulation of "local causality" from "la nouvelle cuisine". This is what you thought you were going to talk about at PIAF, right, but then you ended up going in a different direction? Anyway, I'm being tongue in cheek in saying "you owe me" – the point really is just that if you want to have a serious and possibly-productive discussion about Bell, locality, QBism, etc., I think this is going to have to be a central part of the discussion.

OK, enough for now.

30-03-10 *I Knew He'd Just Call Me Solipsist Again, and he did, 2* (to M. Schlosshauer and R. Schack)

You two might enjoy this one as well, given your own previous run-ins with Travis Norsen. [See 30-03-10 note to David Mermin titled "I Knew He'd Just Call Me Solipsist Again, and he did."] I've got a good racket going: I post a paper; I get called a solipsist. If I could only figure out how to bottle this stuff and sell it for profit!

[223] My emphasis!

30-03-10 *Soliciting Your Thoughts on Intro to Our Steering Paper* (to H. Barnum)

Barnumism 18: *I meant to ask you this earlier… but when writing the intro to our steering paper, I said "The Bohrian notion of complementarity, for example, can be understood in terms of one type of knowledge about a system precluding another." I felt a bit uncomfortable about this… I thought one or both of you might object based on the idea that what one acquires in a measurement is not really "knowledge about a system" but "knowledge about the outcome of my just-completed interaction with a system". There is a similar issue with the next sentence, about information-disturbance relations.*

I wonder if that's right… and if either of you can think of a better way of pithily summarizing complementarity that still relates it to information/information processing in the way we want to in our intro.

Let me think on this for a bit. I'm not sure at all about how I would approach trying to say something about "complementarity". As for "steering" being such a prominent component, its real essence (from a QBie perspective) must be about the possibilities for conditioning. I keep thinking there must be a deep connection between it and the axiom that Rüdiger and I toy with: That in our urgleichung set-up, any posterior for the sky could have been taken as a prior for it. It's this that sets up a duality between states and measurements in our scheme. But I can't quite put my finger on how this and "steering" (god I hate that name) should amount to the same thing.

30-03-10 *Qbism Questions* (to C. Ududec)

Wow! I just read this all again. I feel I can't even begin to answer you. Would you hold it against me for a still further while? Question 1, for instance, is a research project in itself. Maybe we should just "talk" more. How about we get together sometime next week for an oral version of this?

Cozmin's Preply

> [Concerning a draft I handed out at the Laflamme-Emerson quantum foundations course and became "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.]
>
> Sorry for being so late with this, but I didn't get a chance to look at your essay until this Friday. I have a few long winded (and probably still confused) questions and comments. Id like to think about the following more, so maybe this should be a kind of first draft of what I'd like to ask and comment on.
>
> 1) In section three, you argue that since probability does not exist, quantum states do not exist. Presumably this also means that CP maps do not exist. I'm inferring this since if teleportation (which I think you used in an older paper) can be used to argue that quantum states do not exist, then gate teleportation can be used to argue that CP maps do not exist. So what about tensor product decompositions of a Hilbert space, do they exist? There is a version of the Choi-Jamiolkowsky isomorphism (introduced by Matt Leifer in 2006) which shows that specifying a bipartite quantum state ρ_{AB}, and two POVMs, M_A, N_B (which commute), is equivalent to specifying an initial state on A (given by an M_A preparation), and a CPTP map from A to B. So if one agent says that he is working with a bi-partite system and an entangled state, another agent can say he

is working with one system which is mapped to another by some CP dynamics. This is analogous to how specifying a joint probability for two random variables $P(X,Y)$ is equivalent to specifying a pair of $(P(X), P(Y|X))$ where $P(X)$ is now understood as a distribution for X at some initial time, and $P(Y|X)$ is a channel, taking $P(X)$ to $P(Y)$ at a later time.

Also, would it be worth your discussing at least that CPTP maps do not exist in class? Is this implicit in your Wigner's friend argument?

2) Could you comment a little on someone like Leslie Balletine's views on the issues from section III, and his best argument for his views and how you understand them? This may be a strange request, but I think it's interesting to see how people with opposing views evaluate and argue against each others' best arguments. I think of it as a kind of second order evaluation of two positions, with the first order being just looking at both and evaluating them personally.

3) On page 7 you discuss the issue of "Whose information? Information about what?", as well as Pauli's statement that "the results of [an observation] are objectively available for anyone's inspection." Would you still say that the fact of an observation/intervention are objectively available for anyone's inspection? I'm not sure if this is right, but I had the thought that maybe the strong reactions you get to your answer to the information about what question is partly based on confusion over the difference between a statement about the consequences (for me) of my actions on a system, and the consequences of my actions on the system, on someone else's beliefs, or alternatively, the difference between the result of an observation/intervention, and the fact of that intervention. Talking about 'the fact of' an observation already seems to be presuming the answer here, but I'm not sure how to phrase this better right now.

4) In the convex sets, or general probabilistic theories framework which Luca, Howard, Lucien, myself, and others are constantly going on about, we often talk about equivalence classes of preparation devices, and often say (maybe imprecisely) that a 'state' is something like a concise representation of — everything that is statistically relevant about — this equivalence class, i.e., it's a collection of probabilities for the outcomes of future measurements, conditional on the system being prepared by a device from this equivalence class of procedures. From this point of view, statements about unknown quantum states become statements about unknown quantum preparation devices or more generally unknown quantum interactions between the system of interest and another piece of the world. So an agent using some quantum state, is equivalent to (?) it making a statement about the past history of the system of interest. Could you comment on this, and how it fits with your view of quantum states, the quantum de Finetti theorem, and the discussion of the 'right' quantum state?

5) Regarding your comments about the elementary notion of what it means to be two objects rather than one, you say that if one gives up the autonomy of one system from the other, it surely amounts to saying that there were never two systems there after all, and ask why the quantum formalism would engender us to formulate our description in terms of a tensor product of two four dimensional Hilbert spaces, rather than simply a raw 16 dimensional Hilbert space. The conceptual distinction that the tensor product brings in is purely at the operational level, while the question of the autonomy of systems seems to have a much more ontological flavor. The tensor product is a structure we use within quantum theory (which you've argued is a manual that any agent can use to make wiser decisions) given certain other assumptions about some physical situation and what statistics we expect to see (i.e. No-Signaling statistics). My

question (1) above also seems very relevant here, but I'm not sure how to phrase things properly in relation to that ....

For example, I can imagine a world where at some basic ontological level, there are objects in the world, which after interacting with each other in a precise way no longer claim an existence independent from each other. However at the operational level, the level with agents and preparation and measurement boxes, it appears as if — the statistics of various measurements show that — they do have an existence independent of each other. So the agents in this world, would use something like a tensor product decomposition for their description of the statistics of their experiments.

6) A warning about the following: this is partly my attempt to understand the various issues surrounding things like scientific explanation, causation, Bell's theorem, and last but not least, Bell's theorem Qbism style.

I don't think it would be controversial to say that probability theory conceived of as a calculus of consistency in the face of uncertainty (or language for dealing with uncertainty coherently?) does not have the structure or syntax to express the concept of causation. For example, the syntax of probability theory does not allow us to express the statement that "symptoms do not cause disease". There is nothing in the joint distribution of symptoms and diseases to tell us that curing the former would or would not cure the latter.

Why am I bringing this up? Well, I would argue that research in most areas of science such as physics, and biology, as well as in engineering, law, economics, and immunology, is not statistical but is causal in nature. Many questions in these fields are not just about discovering correlations between different variables, but are questions about the process by which the data was generated. They are questions of an explanation of the data, or of what caused or generated the data. We can look at the world, and the things in it as black boxes whose internal workings we can't directly observe. What we want to do is open the black boxes and expose their inner mechanisms.

Why should we care about this notion of explanation through mechanisms? Many laws or statements in physics do not express causal relations, but just regularities. Some examples are A) the daily regularity of the tides, and their correlation with the positions of the sun and moon in the sky, B) the ideal gas law, which relates pressure, volume, and temperature for a given sample of gas, and C) Kepler's laws of planetary motion which describe relationships between various parameters of the orbits of the planets.

There are also many laws or theories which do express causal relations. For example, the increase in pressure of a gas when we change the volume with a piston is explained causally by postulating particles which collide with the walls of the container more frequently when the volume is decreased. So we have a noncausal regularity which we explain on the basis of some postulated underlying causal mechanism. Similarly, Newton's laws of motion and gravitation explain such regularities as Kepler's laws, as well as the tides. The theories (not just in physics) which postulate some causal processes and explain some observed regularities tend to be more precise or powerful (and let us sleep better at night). They also tend to come with more baggage, like postulated particles or fields, and their dynamics.

Now, it seems to me that the way Bell's theorem is usually presented is as a statement about causality, or about the structure (the functional relationships between various postulated variables) of physical theories which explain certain correlations in certain well specified experiments. I say explain, rather than account for, since in a sense quantum theory already accounts for the correlations, i.e., given a belief about how a bipartite system was prepared, and some beliefs about the structure of two further interactions with the systems, the correlations between the outcomes of the interactions are predicted by the theory. Quantum Theory seems more like the examples A, B, C than say General Relativity or Statistical Mechanics. So the way I see the ontological model's program, and more specifically, Bell's theorem, is as an attempt to find a mechanism for the observed correlations, i.e., to find a General Relativity for the Kepler's laws of Quantum Theory.

Given the above, I'm worried that analyzing Bell's theorem purely from a "probability theory conceived of as a calculus of consistency in the face of uncertainty" doesn't do justice to it. So I'm curious what you think of the above considerations of explanation and causation.

You also argue that a probability-one assignment lays no necessary claim on what the world is. Most scientific theories have many probability-one assignments, and they do seem to make claims about the way the world is. I'm wondering how you would rephrase those theories (like classical Electromagnetism, modern genetics, and chemistry experiments where catalysis is important) and the claims they make, to be more consistent with the "probability theory conceived of as a calculus of consistency in the face of uncertainty" point of view. Also, how would you look at a statement like $Pr(X = i|B, T)$, i.e., "Given background knowledge B, and postulated theory T (with all the extra variables, and functional relationships between variables which it postulates), the value of observable X will be i, with probability one"? It seems to me like the quote in your footnote 27 has the structure of a statement like the above, rather than a statement like "this probability one assignment for variable X (given background knowledge B), necessarily implies the world is such and such …".

Do you think that Bell's theorem (or maybe some slightly weakened version of it) could be formulated without something explicitly or implicitly equivalent to the EPR criterion of reality? In other words, is taking something like that criterion a necessary condition for Bell like no-go theorems?

Finally, do you mind sending me an electronic copy of the Qbism essay?

31-03-10 *An Important Message from the QBomancer* (to the QBies)

(OK, I need a better name for myself or my role in this group. "Cubomancy" is defined as "divination by throwing dice." … Given that that's what I've been doing all my life, it seemed a little appropriate. QBomancy—divination by performing quantum measurements.)

Anyway on to more serious matters: […]

31-03-10 *Bell, Locality, Etc.* (to T. Norsen)

Thank you for your note. I have studied it carefully, and plan to study it still more carefully on second and third passes. But your remark, "I've just read the short QBism paper … and skimmed the longer one," coupled with your second-to-last paragraph where the S-word is mentioned, made me smile and reminded me of an encounter I once had with Leslie Ballentine at a meeting in Sweden. Leslie had sent me his previous conference proceedings a bit before my travel to the new meeting, and we met in the coffee line the first morning. I had skimmed his paper, and to outward appearances, it looked to have no innovations over his previous discussions; so I left it at that. But as you can guess, at coffee the conversation very quickly came to the issue of quantum probability.

Leslie said, "Did you read my new paper?" I said, "No, actually." He crisply turned 90 degrees and said as he walked away, "Then we have nothing to discuss."

Much of Section VI was written with your inevitable challenge at the back of my mind. Your image arose fleetingly as I composed Section V, but it was the composition of Section VI that more than once brought me back to thinking of you. (Maybe it was the big eye in Figure 5 that did it.)

When you have read the whole paper, like I have read your whole note, we can discuss.

31-03-10 *Erwin Schrolipsism* (to N. D. Mermin)

Merminition 194: *As one of the earliest people to accuse you of solipsism (do a search for the word in our correspondence, if you're well enough organized to do that — it was a very long time ago) and as one who understands how wrong-headed that was of me, I can only sympathize.*

Well you called Erwin Schrödinger a solipsist long before you called me one! That note dates from 25 January 2000:

> P.P.S. Have you seen a little book by Schrödinger called "My View of the World"? It contains two extended essays on the nature of objective reality, one in 1925 (before his Equation!) and one in 1960. They're surprisingly similar. They turn upside down the notion that objective reality is a valid inference from the fact of intersubjective agreement. He argues instead (I think) that intersubjective agreement is a manifestation of the unity of all consciousness, which is also what creates [the illusion of] objective reality. A kind of global mass-solipsism. But very beautifully put. Makes me think I should read Spinoza, which I never have done. So the IIQM is being tugged in one direction by QIP and quite another by goddamed mysticism. It's really delicious that Schrödinger is the hero of those who believe Bohr abandoned hard-headed rationality.

31-03-10 *I Knew He'd Just Call Me Solipsist Again, and he did, 2* (to N. D. Mermin)

Merminition 195: *On the other hand I'm sympathetic to Travis's complaint that you owe the world the critique you promised but never delivered on Bell's analysis of what counts as an explanation of correlations in distant places, that I commended to you in his essay on the new cooking (and in Bertlmann's Socks). I remember pressing Rüdiger on it when I visited him in London several years ago, and the best he could come up with was to shrug his shoulders and say something like, "Why should correlations have to have explanations?" I still regard the absence of a better rejoinder than that as the weakest point in our battle against the purveyors of nonlocality.*

The key issue is this: WHAT QUANTUM MECHANICS, AS A SINGLE-USER THEORY, IS TALKING ABOUT ARE NOT IN *DISTANT* PLACES. They ARE wherever the agent is whenever he performs the action (you'll see what I mean when you read Sections III and V). Of course, I am using William James's notion of 'the self':

> "In its widest possible sense, however, a man's self is the sum total of all that he can call his, not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works, his lands and horses, and yacht and bank account."

But I'd never get you to write nice things about me in recommendation letters if I told the world the real source of all my ideas!

31-03-10 *Bell, Locality, Etc., 2* (to T. Norsen)

Norsenism 1: *Your point is fair and well taken. For what it's worth, I actually have skimmed the whole paper – that is to say, I was already aware that there was significant explicit discussion of the S-word in these later sections. But you're absolutely right that there's no point discussing it until I've read those sections fully and carefully. I'll be trying to get around to that in the next few days, and so I'll just let you know what I think afterwards.*

That said, though, and to try to explain further why I wrote what I did yesterday even before having read the whole paper carefully, let me say this: the subjective probability I assign to my coming to believe that your sections 5 and 6 address all of my concerns, is very low.

Don't think that I myself didn't already assign a subjective probability of nearly one to this proposition long before reading it this morning!

Travis's Reply

> I figured you did. More precisely, I assigned a subjective probability near one to the proposition that you did.
>
> By the way, despite really loathing your solipsism and even more the fact that you're in denial about it, I love and respect your openness and sense of humor.
>
> OK, maybe some serious discussion in a couple days …

31-03-10 *Truth and Happening* (to N. M. Boyd)

I'm looking forward to your seminar on "truth" that was just announced. Just to throw in a vote: I hope you will have the time to say a few words (pro or con) on the notion of truth in American pragmatism (particularly William James's and John Dewey's) and that this important notion from a century past will not get lost in the shuffle. Here's one of my favorite sentences of all time by William James:

> The truth of an idea is not a stagnant property inherent in it. Truth *happens* to an idea. It *becomes* true, is *made* true by events. Its verity *is* in fact an event, a process: the process namely of its verifying itself, its veri-*fication*. Its validity is the process of its valid-*ation*.

Attached is a page of something I wrote to Lee Smolin a few days ago in response to reading one of his drafts. [See 24-03-10 note "Notes on the Open Future" to L. Smolin.] It says a bit of why this notion of truth is important to me. The relevant discussion is between Smolinisms 9 and 10. It says that this notion of truth is important for making possible a certain kind of ontology. The reason that in turn is important to me is that it happens to be the kind ontology I myself am aiming for. If you have any interest in my motivations, you might skim the last three sections of my recent essay: [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.]

As I say, looking forward to hearing some discussion on the important issue of "truth" at PI.

31-03-10 *Working Glossary* (to the QBies, R. Schack & N. D. Mermin)

I thought it'd be useful to update our group glossary. Please bring any omissions of words we have used before to my attention. Creativity is welcome as well; though I reserve final say on quality control. Particularly, I'd like a good word for myself or my role in this group. Matthew suggested QBoss or QBishop, but that feels a bit too aggrandizing for me to use as a description of myself—I need something a little more Chris-safe. (Of course, you can call me anything you want in private! Even QBastar. . . .)

Also it'd be a nice to find a grammatically correct nickname for Joan Vaccaro's painting *Broken Block* that I think illustrates so well the inevitable cracks in the block universe conception of things.

QBism – the quantum foundational program of quantum Bayesianism; see `http://arxiv.org/abs/1003.5209`.

QBist – a practitioner of QBism.

The QBies – C. A. Fuchs, along with his group of three grad students (Hoan Dang, Matthew Graydon, Gelo Tabia), PI associate postdoc Asa Ericsson, and PI visiting researcher Marcus Appleby.

QBism House – 49 George Street, Waterloo, Ontario; see attached "press release".

The QBibbo Angle – 60 degrees; see Assumption 7 and Equation 24 of `http://arxiv.org/abs/0912.4252`.

The QBicle – the office of the student QBies, PI room 415.

QBic Equation – the most important equation underlying the shape of quantum state space; see Equation 19 of `http://arxiv.org/abs/1001.0004` or Gelo's beautiful rendition of it in $d = 3$:

$$\sum_i p(i)^3 \;-\; 3 \sum_{(ijk)\in Q} p(i)p(j)p(k) \;=\; 0$$

where Q consists of all lines in the affine plane,

$$\begin{array}{ccc} 1 & 2 & 3 \\ 4 & 5 & 6 \\ 7 & 8 & 9 \end{array} \qquad \text{I.e.,} \qquad Q \;=\; \begin{array}{ccc} (123) & (456) & (789) \\ (147) & (258) & (369) \\ (159) & (267) & (348) \\ (168) & (249) & (357) \end{array}$$

QBissel – one who is intrigued by QBism but not yet convinced (compare German word "bissel").

QBicity – the quality or state of being properly oriented with respect to quantum interpretational issues (compare "cubicity" and "state of nirvana").

QBomancy – divination through quantum measurements (compare "cubomancy").

QBomancer – one who divines with the help of quantum measurement (compare "Raussendorf-Briegel model of quantum computation")

QBalla – ??

01-04-10 *Too Short an Answer* (to T. Norsen)

This is no answer to your nice note — I won't be able to do that for a few days. (I am not so quick as you.) But I want to try to make one point very tersely, in case it might open up some understanding. It is related to your Ballentine bullet.

With regard to the issue of quantum states (and probabilities) being "epistemic" (broadly construed), there are two points that must be addressed:

1. Are they? I.e., are they "epistemic" (broadly construed as "in your head rather than out there")? My stance, and Ballentine's (possibly, since he also will say things like "a hydrogen atom ground state is no one's degree of belief") and Einstein's and Peres's and Peierls's and Spekkens's (all to varying degrees of consistency), is that they are.

2. But epistemic about what? There is more than one possibility here, and that seems to be one of the things getting in the way of your seeing where I am coming from. The epistemic states could represent uncertainties about hidden/ontic variables completely external to the agent—that is the common story of Einstein's take on it, and it seems to me what Ballentine must be talking about implicitly. (Though as I say you never know about Ballentine.) It is definitely Rob Spekkens's desired interpretation of the epistemicity of quantum states. But it is not mine. My whole paper is about exploring a different view: That they are an agent's epistemic states for his own potential experiences *under the premise* that those experiences will arise as a consequence of his actions on ("interactions with" is too loaded of a term for me to want to use it here) the world external to him.

The imprint of the character of the external world (and the world as a whole, including the world before man)—this is an idea that approaches your definition of "metaphysical realism"—does find its way into the formalism (in my view), but when it does it does so as an additional normative rule for the agent's gambling attitudes on those potential experiences. Where "normative rule" does not mean that one must adopt this or this or this *particular* probability assignment, but that one should try to make all of one's gambling attitudes (for all kinds of things) mesh together according to a certain scheme. In the case of quantum phenomena (when made explicit with SIC language), the Born Rule is seen as a normative rule for meshing together beliefs between "a one-step factual measurement" and "a two-step counterfactual one."

I'll leave it at that for now, and just ask that you mentally readdress your questions, issues, and reading of the paper with distinction number 2 at the forefront of your mind. It is the predominant thing that sets QBism apart from the "epistemic-state, ontic-variable program." And it is underneath everything I say about the locality issue: For the quantum state, on this view, is only calibrated to what the agent believes he himself will experience (which in common language one might say as "happens in his vicinity").

I'll come back to your many other issues at the end of Easter.

I'll cc this note to Rob Spekkens since I impute a definite statement onto him, and he may disagree. (I'd cc it to Einstein, Peierls, and Peres too, but they're dead.)

01-04-10 *Strange Anton Dream* (to A. Zeilinger & H. C. von Baeyer)

Since H. C. von Baeyer is convincing me more of the importance of dreams, I'll record this one from the night before last. You (Anton) were visiting Waterloo, and I had had some house work done a week or so before your visit. There was a fresh concrete sidewalk laid and some concrete steps leading up to my house. The concrete was dry and hard—it was already a week old. When you arrived at my house, there was some melting snow on the ground; the new sidewalk was a bit wet. We started discussing matters quantum mechanical on my porch, or maybe at my chalkboard at the wall. I told you that there is some pattern you must understand—it was a very important thing. The pattern was apparently on a piece of paper or a leaf, or "both" if you can figure out what that means. (I have conflicting images of both.) But the wind had blown it away. I walked into my yard looking for it in the snow, but finally found it wet and crumbling on the new sidewalk.

I knew that it could not be recovered without destroying it if I tried to peel it from the sidewalk. I became very sad at the loss, and I tried to peel it anyway. It did indeed tear into a hundred pieces, or was a bit like oatmeal as I tried to pull it up. But to my astonishment the pattern had been transferred into the concrete—the concrete now had etchings and lines and bumps. And it was a bit surreal even; I seemed to be able to see it more clearly now than I could before. I said, "Anton, Anton, you must see this!" You walked over and we studied it very carefully, more excited about the transference than about the original pattern that I had thought was so important. Then I looked up and down the sidewalk and steps and found all sorts of other things recorded in it. For instance, on one of the steps, there was a very deep footprint (I think yours) showing every detail of the underside of your shoe. I thought, "But this is finished concrete; how can this be?" And what struck me again, was the surreality of it: The edges were extremely crisp, almost as if the shoe had been pressed into a kind of plastic. I thought, this is so much like the *Magic Eye* books, where when the 3D image finally becomes visible it is always strangely crisper and clearer than it should be.

And that is all I remember. (See `http://en.wikipedia.org/wiki/Magic_Eye` if you don't know what I mean by the *Magic Eye* books.) Of course, I have no real idea what it means (if anything), but I wonder if it is not connected to an excited note I wrote to my colleagues a couple weeks about about the SIC problem. I'll append part of that below for completeness.

On a not unrelated matter: Did you ever make contact with Karl von Meyenn about the funding issue? I continue to worry that Pauli's work on dreams and alchemy will be lost forever to the broader audience if von Meyenn does not complete his project.

01-04-09 *Beyond QT?* (to M. A. Graydon)

Graydonism 2: *I've been thinking harder these days about more conceptual issues (must be the hot weather), and I wonder if you might agree with the following:*

The reality underlying quantum theory consists of systems, and those systems are defined exclusively by a single, objective, ontic property denoted by D. Of course, the QBist need not posit that the whole of physical reality may be described in these terms, for the QBist is primarily concerned with an interpretation of the quantum theory. In QBism, there are no mathematical symbols in the quantum formalism that point to any other ontic property other than D. In particular, the quantum state represents one subjective agent's degrees of belief regarding the outcomes of future measurement interactions with an objectively existent external physical system. An 'agent' is one in the world who may identify systems, contemplate their dynamics, and participate in measurement interactions. A 'measurement interaction' involves an agent and a system. Operationally speaking, the agent passes the system through some measuring device and learns that one of n outcomes has occurred. But then it seems that, at least for the QBist — measurement interactions cannot change the (quantum) ontic nature of the system, for the system's only property is D. Would you agree? Must we remove the '(quantum)' and look for a deeper theory?

Pretty darned good summary. I was with you all the way until the last two sentences. I think my best reply is to suggest re-reading Section VI in my recent long paper "QBism, the Perimeter ..." on `arXiv.org` now. I think you'll see in that how your last two sentences contradict what I'm thinking. I'm prepared to think that quantum measurements (like any human action) change the world in a very deep way. I'll send you some supplementary material in a minute, but do reread Section 6 (and 7 and 8 as well).

01-04-10 *Explanatory Note* (to M. A. Graydon)

"More to Read x" where $x = 1, 2, 3, 4$. They're all about how "measurement" changes the world. Happy Easter!

I was just reading your note again before filing it. [See 01-04-09 note "Beyond QT?" to M. A. Graydon.] Part of me wants to say "broader" is a better term. But I do get where you're coming from in the last question. Absolutely, there must be a "next step" in physics. My own favorite dream is that whatever "quantum measurement" is, the world is made of it—it was a dream instilled in me by John Wheeler long ago. QBism, as it presently exists, has for me always been preparatory work for getting at that deeper question. (There, I used "deeper" myself.) John's sense was that quantum measurement has more to do with all kinds of things than many imagine (for instance, "the big bang").

So, getting back to D. I view it as a quantitative measure of "one aspect" of everything. But what is that aspect? That's the broad question we're gearing up to with QBism. The aspect I think has to do with is "whatever quantum measurement is."

02-04-10 *More to Read 5* (to M. A. Graydon)

[See 01-04-10 note "Strange Anton Dream" to A. Zeilinger and H. C. von Baeyer.] Here is my interpretation at the moment. That the urgleichung is the "imprint" in the concrete (world), and that QM and SICs were the original "pattern" that I was trying to tell Anton about. The imprint is surreally crisper than the original.

02-04-10 *The Quantum Bayesian Glossary – Some Updates* (to N. D. Mermin and R. Schack)

Boys, it's just like the old days! Remember when we put so much effort into constructing:

Mechanica Quantica Lex Cogitationis Est

Makes me feel young again!

02-04-10 *The Friday Philosopher* (to Å. Ericsson)

I'm sorry: It's turned out to be such a pleasant day outside, that I think I'll just stay home. In fact, just a little while ago, I wrote Max Schlosshauer the lines below.

It's funny, earlier today I was thinking I would ask you if would be interested in going to see *Clash of the Titans* with me at the 1:30. (I figured you wouldn't be so interested—none of my girls were—but I thought it'd be worth a shot.) Then I went outside for breakfast, and it was just so pleasant, it rearranged my whole mentality.

If you want a reading suggestion, for something to print out and take under a tree, here's what I'm dipping into from time to time today (I've already told you about it before):

http://en.wikisource.org/wiki/Personal_Idealism/Axioms_as_Postulates.

02-04-10 *Congratulations!* (to J. Emerson)

Emersonia 4: *Thought you might be happy to hear, Quantum Bayesianism won the vote in class today for "favorite interpretation"! Well done! PS: Many worlds got zero votes... So the tides may be turning!*

That is great news! What were the other interpretations on the table? And what was the margin? Definitely let's get some beer Wednesday.

02-04-10 *Congratulations!, 2* (to the QBies)

Joseph Emerson just wrote me this:

Emersonia 5: *Thought you might be happy to hear, Quantum Bayesianism won the vote in class today for "favorite interpretation"! Well done! PS: Many worlds got zero votes... So the tides may be turning!*

Those of you in the class, tell me more about this! Was there much of a margin or was it slim? What were the other interpretations on the table?

Hoan's Reply

QBism got 8 most-favorite votes and 3 least-favorite votes, resulting in +5 (we could have got +6 had Gelo had not to leave for Europe).

If I remember right, ensemble and operationalism both got +4. Many-worlds got -9, Bohmian mechanics got -3.

There were totally 8 or 9 interpretations on the table, but some of those we never studied carefully so most of the people didn't vote on those.

02-04-10 *Congratulations!, 3* (to the QBies)

Who voted "least favourite" for QBism. Do you know the names of anyone? I won't throw eggs at their houses: I'm just curious. It'd be interesting to learn their reasons.

Hoan's Reply

I think they were all undergrads, but I didn't recognize them. One thing I remember is that when they raised their hands to vote QBism as the least favorite, one other undergrad (who voted most favorite for QBism) shouted out "come on, it's not solipsism!"

03-04-10 *The Quantum Bayesian Glossary – Some Updates, 2* (to M. Schlosshauer)

Schlosshauerism 22: *Q'ran (noun) – the Holiest of Holy Scriptures telling the story of QBism from the ground up. (This is of course nothing but a thinly veiled attempt at letting the idea of a book on QBism resurface. Honestly, I've been thinking about writing something, sometime. But it remains a ridiculously vague plan for now.)*

Now, that's an idea I like! You do the writing, and I'll do the praising! I say, find a way to make it a less vague plan! One of my own troubles is that I don't know where the ground is yet. Also, I don't have the broad perspective that you do: I've been a QBist-in-the-making since the beginning, with all kinds of petty prejudices against other directions since the beginning. With honesty, I know that I've never given most of the other foundational programs a fair shake. What the program really needs is someone who can give it a reasonably careful comparison to the other extant programs. Someone too who can more detachedly discuss QBism's weaknesses.

And I'm getting good at "praising." I wrote something for the back of Schumacher and Westmoreland's new book. And I just wrote this a few days ago for the Nielson and Chuang's 10th anniversary edition:

> Nearly every child who has read Harry Potter believes that if you just say the right thing or do the right thing, you can coerce matter to do something fantastic. But what adult would believe it? Until quantum computation and quantum information came along in the early 1990s, nearly none. The quantum computer is the Philosopher's Stone of our century, and Nielsen and Chuang is our basic book of incantations. Ten years have passed since its publication, and it is as basic to the field as it ever was. Matter will do wonderful things if asked to, but we must first understand its language. No book written since (there was no before) does the job of teaching the language of quantum theory's possibilities like Nielsen and Chuang's.

Don't know whether the editor will let me keep it in this form (or make me make it more technical), but anyway, just imagine what'd I'd write for you!

As far as the glossary term Q'ran goes, though, I know you've recognized that it breaks the B rule. QBie's already taken (that's what I call my group members.) And QBest was sweet, but David Mermin already beat you out:

> QBlue – a fanatical, unshakeable QBist; codename for C. A. Fuchs.

I'll think about QBitter. How about this variation:

> QBitter – despair experienced by a QBist every time he is called a solipsist (again!); not to be confused with QBiteer (see next term).

03-04-10 *QBar!* (to M. Schlosshauer)

Schlosshauerism 23: *Here's another one:*

> QBar (or QBa/QBah) – the cocktail lounge at QBism house, serving exclusively mix drinks from Havana and the rest of the forbidden island.

Oh, bad, bad, bad. I stop now.

OK, that was bad! But it did inspire me to think that we need a LaTeX symbol `\qbar`, analogous to `\hbar`. It is QBism's version of Bohr's "finite quantum of action." I'm thinking here of the value `\qbar=2` for Eq. 12 (and discussion following) in the new paper. Appleby when he writes a lower-case `q` vector on the chalk board, always puts a line through the tail. That's the sort of thing I'm imagining. The value `\qbar=2` corresponds to 60 degrees for the QBibbo angle.

Thanks! I wanted to respond to your science of philosophy note, but I finally got into my hotel room and am getting a bit sleepy. It might wait until after a nap, i.e., you'll have some morning reading. (The family is in Buffalo today and Niagara tomorrow, and while the girls have been out shopping, I've been working in the hotel lobby. We're having the Easter egg hunt in the hotel room tomorrow morning.)

03-04-10 *Philosophys* (to M. Schlosshauer)

Schlosshauerism 24: *Namely, have* you *ever considered crossing over to a philosophy department?*

Too many times. But the philosophers never wanted me.

John Preskill and I once worked to get Caltech to make a joint physics/philosophy position for me through a very broad "information department" initiative. Attached are my proposals for that. [See 12-02-04 note "The House Philosopher" to J. Preskill and H. Mabuchi.]

Now a nap. More later on how at times, I have actually gotten something from the philosophers. I exaggerated a bit in that sentence you quoted. (And I should be careful before samizdatizing it.)

03-04-10 *The Science of Philosophy – 2* (to M. Schlosshauer)

Now to this:

Schlosshauerism 25: *It was a nice coincidence that you again appended your note "Conceptual Barrier!" to your email. After reading it the last time, I was struck by these words of yours: "I have many times said that I have never once gotten anything of any value from a philosopher of science ..."*

I'm glad you did mention Arthur Fine's name, because it reminded me that there are many ways to get "something of value" from people. And from Arthur, I got quite a lot from his historical work on Einstein and Schrödinger. A lot of *The Shaky Game* was quite good for me.

Also his "Do Correlations Need to Be Explained?" (or a similar title) gave lots of food for thought for me, particularly with his discussion of "the hidden hand." Did I ever recommend Bruno de Finetti's "Probabilismo" to you? He has a lovely discussion on this: Suppose you make a probability-nearly-one assignment and then upon looking at the event you find that it did indeed happen. Do you then have the right to be surprised? Do you then have the right to declare that the fact of the outcome actually occurring was a great mystery, needing more explanation than it already had? Well, "correlation" is just a two-variable version of that, and Arthur came so close to the point. If he hadn't gotten confused with frequentism, he would have had it. So, looking back, I should recognize that he likely planted a seed in my mind that would help me better appreciate de Finetti when that later time came.

Also, I remember reading Richard Rorty say that Arthur Fine was his favorite philosopher of science. And that means something to me. I have gotten so much from reading Richard Rorty. If there are some confluences in their thoughts, then more familiarity with Fine might do me some good.

Another example is Bas van Fraassen. There's been very little I've gotten from him, and for instance, when I sat in his seminar with Halvorson on the philosophy of quantum information, I thought both of them so amazingly confused and far from clear thinking that I wondered how they could be bestowed with the credential of "philosopher" in the first place. *But* Bas's "reflection principle" within probability theory is a *golden nugget*. It has turned out to be the key to a QBist's understanding of why everyone *thinks* (i.e., gets confused) that decoherence *precedes* the registration of a measurement outcome (and thus seeks out detailed measurement models to make that turn out to be the case). Decoherence as a *physical process* does not precede registration: It is only the agent "reflecting" in van Fraassen's sense that causes him to gamble *as if* it had been so. And if *Ruediger* and I ever write this up, we will owe Bas an immense intellectual debt.

Finally, there is no doubt that Jeff Bub, Bill Demopoulos, Richard Healey, Wayne Myrvold, Huw Price, Allen Stairs, and Chris Timpson have provided a kind of tension in my conversations with them that I have found extremely useful for organizing my thoughts.

So now I qualify myself, I should never say "never once gotten anything of any value from a philosopher of science."

06-04-10 *In Reverse* (to M. Schlosshauer & H. C. von Baeyer)

I'll send this to Hans Christian too.

We're back home today, and I've really got to kick in the work the next nine days. But I want to first record an experience that quite knocked me for a loop yesterday. It has to do with your remark [concerning my 01-04-10 note to A. Zeilinger and H. C. von Baeyer titled, "Strange Anton Dream"]:

Schlosshauerism 26: *That's an impressively surreal dream you've had there! I'm also flabbergasted at the amount of detail you could remember.*

Sometimes I get a little too close to dreams. And vice versa. I'll try to make the story short and simple; I suspect nothing I write will convey the impact those few minutes had on me.

It was yesterday, early afternoon and we had been walking around the cheesy part of Niagara for a couple of hours—that is, the amusement-park looking area, with wax museums, haunted houses, a Harley-Davidson memorabilia shop, etc. We started by taking a ride on the big Ferris wheel, letting the girls buy a couple of postcards, and ended with a walk through a park closer to the Falls. When it was time to go, we trudged back up the hill toward where our mini-van was parked. I remember well holding Emma's hand for about the last block of the walk, and just as we neared the car I gave it a firm squeeze before letting go. Kiki said with a bit of triumph, "All right, we're back." The kids went straight to the car door on the sidewalk side, and I went to the street side of the car to get into the driver's seat. Nothing out of the ordinary you would say.

There was a bit of traffic, so I waited for a second to open the door. I did note to myself, "Funny that Kiki didn't unlock the door with the clicker. I guess she had left it in the car." I inserted the key, used the button to unlock everyone else's doors, and looked up. There was no one with me! No one behind the car, no one near it, and all of this happened within the small flash of a second. Everyone was gone. I walked back onto the sidewalk, and looked into four nearby store fronts—no sight of them. I can't describe the feeling of how disorienting this was. I even came to the car to look in and see if I hadn't missed them somehow. It was an absolutely strange experience. I thought to myself, "Have I been living in a dream for months? Was this happy family life and all of today just a dream? Could it be that Kiki left me long ago, and I've been living on my own and out of my head?" I was in shock. I looked down at my clothes and thought, "They look clean, not like the clothes of a transient." I looked in all the stores again, this time more thoroughly, and nothing. My mind kept coming back to the image of the firm squeeze I had given Emma just before we parted—I remembered it vividly. I had this really palpable fear that I had been living a dream and just become conscious. I started to look for some evidence of the reality of my life. I went into the shade under an awning and pulled out my digital camera. To my great relief, there were images of the morning. Still I waited a lifetime—or so it felt—and they never came. The fear kept nagging me.

Finally I went back into a store and started to pull out my digital camera as I asked a lady at the front, "Have you seen a woman in a blue coat with two young girls in the last few minutes?" She said, "No, but you might check downstairs; sometimes I don't pay attention to everyone coming and going." Sure enough, just as I turned to the stairs, Kiki and the kids emerged from the lower floor. I nearly cried with relief, and it took me much of the time driving home to completely recover.

With hindsight, what I find most interesting about this is the question of why my mind was so susceptible to the theory that I might have just become conscious from a dream. There is a little bit of a cheap answer in that we had just watched the movie *50 First Dates* with Drew Barrymore and Adam Sandler two nights before. It is the story of a man who romances a woman that had suffered a head injury: Each night when she falls asleep, her short term memory disappears. Each day he must romance her again, and eventually, with the help of a growing video montage, becomes her memory. They marry, and in the last scene of the movie, he introduces her to their 10 year-old daughter. It was a cute movie and must have been in the recesses of my mind. Also there is another Hollywood precedent in my mind, a movie I saw 21 years ago: *Dad* starring Jack Lemmon, Ted Danson, and Olympia Dukakis. Lemmon had two completely separate lives that he had been sustaining for years—one in a dream, and one in the real world, and only when he became quite ill did he realize that there were these two separate lives and he was one person. The memory of that movie flashed through my mind yesterday as well.

But neither of these thoughts feel like they go deep enough to explain my susceptibility. It's absolutely freaky that I "freaked out" to the extent described above. So, I record the story here with you two because you have been involved in my dream thoughts lately (in all kinds of ways), and I continue to think about what this might mean in the wider context.

06-04-10 *QBism Glossary* (to the QBies and Other Friends)

The long awaited QBism Glossary is near completion. If you have any suggestions for improvement, or any terms so good that they'd knock some of the existing ones off the page, now's your time to speak up: I will finalize and post on my door soon.

I'm not all that happy with the look of the present QBar. Better might be to have a q with an extended tail and a bar through it, but I couldn't figure out how to do that with LaTeX. If anyone has any ideas on how to improve the symbol, please say so.

Luca Mana's Reply

There could also be QBismillah, from bismillah … Good for quantum fundamentalists.

06-04-10 *QBism Glossary* (to M. Schlosshauer)

Schlosshauerism 27: *Love it!*

And thanks for the credit – which I don't even deserve, since it looks like none of my suggestions made the cut anyway. (Which is absolutely fine I hasten to add: I wasn't too electrified myself about my submissions.)

That is not true: You directly inspired QBar. One of the most important concepts in the whole list. Attached is Åsa's variant: When blown up like this, it's beautiful:

$$\underline{q}$$

Unfortunately it doesn't work so well in small font.

07-04-10 *Paper and Glossary* (to D. B. L. Baker)

I never did send you a final version of the paper. You might enjoy the obscenities in the closing paragraphs of Section VIII. [See "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.] (It starts with "mount" and goes downhill from there.) More than that though, you might enjoy the further glossary of terms, fresh off the keyboard this morning.

07-04-10 *SICs, Counterfactuals, and Other Stuff* (to O. Cohen)

Cohenism 1: *Secondly, I wanted to mention that I have been following your recent work on Quantum Bayesianism with interest. I think that the SIC structure is very elegant and compelling. I am still digesting the longer of your two recent postings on* `quant-ph`*, and may get back to you in due course with some comments, if that's OK with you. Your formulation in terms of counterfactual vs actual measurements is of particular interest to me right now as . . .*

Thirdly, I have been drafting up some recent musings of mine, which were triggered after re-visiting some of Bell's papers. I have been mainly concerned with looking at Bell/CHSH from a different perspective, which focuses on the actual vs counterfactual relationship when viewed from a local perspective. I think there may be a connection with your formulation, though I haven't had time to explore this yet. I am attaching a draft of my paper, which I am thinking of posting on `quant-ph`*, though I am a bit tentative as it's possible that what I have done is trivial and/or nonsense. If you are able to give any comments or feedback, that would be great and I would really value your opinion. But if you are too busy to spend time looking at the work of amateurs like me, that's also fine and I fully understand!*

Well, I certainly like this line in your paper, "By exploring possibilities such as this, as a means to unravel Bell's theorem, we are moving the focus away from locality and on to counterfactuals." I've only skimmed your paper just a little, but that philosophical or programmatic position seems to be on the right track to me.

Hopefully I'll get a chance to look at your paper in seriousness in a couple of weeks, after I get some grant proposal things cleared off my desk. Also, I hope you don't mind, but I'm going to encourage Howard Barnum, our seminar chair, to invite you to give us a talk on the subject.

07-04-10 *Templeton* (to H. Price)

Pricey Quote 3: *Dean and I are putting together a proposal for the Templeton Foundation, to try to get money for a series of three conferences (and a postdoc). One of them is intended to be a "motivations for thinking that physics needs a flow of time" meeting. You'll see the general line, in the attached printout of our draft from their online application page. Can we list your name, as one of our intended participants/collaborators?*

Absolutely. Thanks: It's nice to be called, even if implicitly, one of "the world's leading philosophers and physicists of time." You might consider Lee Smolin as well, since he now writes things like,

> This provides further motivation for believing in the reality of time and the openness of the future. If the selection of the laws of nature that determine what elementary particles exist and how they interact is a result of evolution analogous to natural selection, then there must be a time that this evolution plays out in. And it must be a time that exists prior to the laws, for the laws must have scope to evolve in time.

It would seem the meeting you wish me to play a role in has a little bit to do with this gem of a book I found in Portland: *Incompatibilism's Allure*, `http://metapsychology.mentalhelp.net/poc/view_doc.php?type=book&id=4752`. Just what is this allure of Chris and others to something *you* cannot even formulate a meaning for? ☺

Anyway, maybe we'll get a chance to play north and south pole within the Templeton Foundation. Rüdiger Schack are at the moment writing a proposal, "That the World Can Be Shaped: Quantum Mechanics, Counterfactuality, and Free Will" and you know me well enough to guess where that's going!

07-04-10 *Yet Another Workshop* (to Č. Brukner)

Unfortunately I think it is going to be almost impossible for me to attend this, as I must be at QCMC in Brisbane, and its dates are July 19-23.

This is really sad, because you know your meeting's topic is much closer to my heart! To your title question, you would do me honor to read one paragraph from my paper in one of the discussions.

"What exists in the quantum world?" Tell them, "Everything!" and read:

> Physics—in the right mindset—is not about identifying the bricks with which nature is made, but about identifying what is *common to* the largest range of phenomena it can get its hands on. The idea is not difficult once one gets used to thinking in these terms. Carbon? The old answer would go that it is *nothing but* a building block that combines with other elements according to the following rules, blah, blah, blah. The new answer is that carbon is a *characteristic* common to diamonds, pencil leads, deoxyribonucleic acid, burnt pancakes, the space between stars, the emissions of Ford pick-up trucks, and so on—the list is as unending as the world is itself. For, carbon is also a characteristic common to this diamond and this diamond and this diamond and this. But a flawless diamond and a purified zirconium crystal, no matter how carefully crafted, have no such characteristic in common: Carbon is not a *universal* characteristic of all phenomena. The aim of physics is to find characteristics that apply to as much of the world in its varied fullness as possible. However, those common characteristics are hardly what the world is made of—the world instead is made of this and this and this. The world is constructed of every particular there is and every way of carving up every particular there is.

If you were to say that, it'd be like my being at the meeting anyway! For, in spilling those I would already exhaust all my thoughts up to this date! (Though I miss a chance to learn from all of you.)

07-04-10 *Is the Compromise Worth It?* (to N. D. Mermin)

Waking up in the middle of the night, I'm torn worse than earlier. Åsa wrote me this,

> Mermin's comment makes sense for someone with a broad knowledge. But I believe your response is correct. When it comes to understanding these words I don't think I am much worse than many others at PI. The glossary looks good now. With footnotes it will likely look cluttered. To make the 'Compare "____"' not stick out as much, I suggest you use 'cf. ____', perhaps in parentheses or with ; before instead of starting a

new sentence (but this is perhaps incorrect, I don't know rules for semi-colon). The "
can be taken away if the words are italicized instead.

My gut feeling is that she's more right for the common folk and that your opinion, though more pure (and godlike for it, I emphasize), will lose me too much of an audience.

Your QBit was different. The QB here is to emphasize "Quantum Bayesian" and the two letters come as a package. Moreover, you were fighting the long usage of qubit. In contrast, before QBism there was no QBism!

Of course, you are right about "consistency." If I had not been driven by insane consistency, I would not have arrived at QBism as it presently stands. I would rather have ended at a sort of ugly halfway house, like Peres and Ballentine did, each in their own way. Thus, if I were to revert, I should give QBlue a "compare" as well.

Well, I will try to go back to sleep in spite of these deep troubles, and hope that a solution comes in the morning.

07-04-10 *Up Front* (to Å. Ericsson, N. D. Mermin, and R. Schack)

My three most trusted consultants,

I think the attached is my own favorite version: I decided to put the cognate words right up front. In my mind that somehow diffuses David's criticism . . . but I don't know, he might find it even more annoying now. I'll listen to all your opinions carefully. Clearly this is something I want to get right!

Glossary of Quantum Bayesianism

(with contributions from the QBies, Kiki Fuchs, N. David Mermin, Rüdiger Schack, and Maximilian Schlosshauer)

QBism – the quantum foundational program of quantum Bayesianism; see "QBism, the Perimeter of Quantum Bayesianism," `arXiv.org:1003.5209v1`.

QBist – adjectival form of QBism and a practitioner thereof.

QBlue – cf. *true blue*; a fanatical, unshakeable QBist; codename for C. A. Fuchs.

QBies – everyone supported by ONR grant N00014-09-1-0247: QBlue, along with graduate students Hoan Dang, Matthew Graydon, and Gelo Tabia, PI associate postdoc Åsa Ericsson, and PI visiting researcher Marcus Appleby.

QBism House – 49 George Street, Waterloo, Ontario, "where the ghosts of pragmatist philosophers past listen to how their ideas of an unfinished, malleable world take a more exact form with the help of quantum theory."

QBicle – cf. *cubicle*; the office of the student QBies, PI room 415.

QBuki – cf. *kabuki*; the theater of the QBies' weekly group meeting outside PI office 355, Fridays 1:00–3:00 PM.

QBicity – cf. *cubicity*; the state of being properly oriented on quantum interpretational issues. "Along with nirvana, one should strive for QBicity."

QBar – the QBist's *elementary quantum of action*, $\underline{q} = 2$. When $q = \underline{q}$, the generalized urgleichung

$$Q(D_j) = \left(\frac{1}{2}qd + 1\right) \sum_i P(H_i) P(D_j|H_i) - \frac{1}{2}q$$

becomes the usual Born Rule if SICs exist.

QBibbo Angle – cf. *Cabibbo angle*; maximum angle $\theta_{\rm QB}$ between quantum states in a QBist representation. See Assump. 7 and Eq. (24) of "A Quantum-Bayesian Route to Quantum-State Space," `arXiv.org:0912.4252v1`,

$$\cos\theta_{\rm QB} = \frac{q}{q+2} \ .$$

When $q = \underline{q}$, $\theta_{\rm QB} = 60$ degrees.

QBissel – cf. Bavarian *bissel* and *N. D. Mermin*; one who is intrigued by QBism but not yet convinced.

QBisl – cf. Yiddish *bisl*; one who is intrigued by QBism but is too busy studying the Talmud to give it the time it requires.

QBitz – cf. *kibitz*; a common social interaction between a QBist and a QBissel—QBist as the QBitzer of course.

QBic Equation – most important equation underlying the shape of quantum state space; see Eq. (19) in "The Lie Algebraic Significance of Symmetric Informationally Complete Measurements," `arXiv.org:1001.0004v1`,

$$\frac{d}{2} \sum_{ijk} R_{ijk}\, p(i) p(j) p(k) = \frac{d+7}{(d+1)^3} \ .$$

QBomancy – cf. *cubomancy*; divination through quantum measurements, as with the Raussendorf-Briegel model of quantum computation.

QBosh – cf. *kibosh*; rare moment when a QBist, by force of argument, gets the holder of an alternative quantum interpretation to flinch. *Inside the Perimeter* headline: "QBist Puts the QBosh on Visiting Everettian!"

QBlah – cf. Islamic *kiblah*; the direction QBists face when contemplating the beauty of SICs—inward!

QBalah – cf. *kabalah*; mystical practice of demanding SICs to exist in all dimensions, or, if mathematics won't yield, that quantum mechanics be scrapped in favor of "urgleichung mechanics." See "Quantum-Bayesian Coherence," `arXiv:0906.2187v1`.

07-04-10 *Finished Product* (to J. Rau)

Rau-ism 2: *Many thanks! I really enjoy the wit and clarity with which you expose your scientific and philosophical point of view. I was wondering, actually (and asked this question to Rüdiger who didn't know the answer) whether your acronym QBism was intentionally close to cubism, and whether you would like to become the Picasso of quantum mechanics?* ☺

Thanks for the papers. On your questions, I have quite wondered how I might use an association with cubism to further my agenda, but so far I have fallen short. On the other hand, you might enjoy a few of the other words in the QBism Glossary. See attached.

07-04-10 *Mid Conversation Frustration* (to T. Norsen)

Norsenism 2: *Anyway, running with the photocopy metaphor, I see our dispute this way. We both look at something and agree: this is definitely a photocopy. I say "there's got to be an original somewhere, let's start working on finding it". You say, "ah, but maybe this photocopy is only a copy of another photocopy, in which case we don't have to talk any longer about originals and all the genuinely foundational issues (though not some interesting technical ones) evaporate."*

I know that you like to think that you are a careful reader, but all the evidence I have ever gathered from interacting with you shows that the thing you read best is your own mind.

If we had agreed, why would I have written these (unnoticed?) lines in the paper?

> A fairer-minded assessment is that the accusation springs from our opponents "hearing" much of what we do say, but interpreting it in terms drawn from a particular conception of what physical theories *always ought to be*: Attempts to directly represent (map, picture, copy, correspond to, correlate with) the *universe* ...
>
> Quantum Bayesianism sidesteps the poisoned dart, as the previous sections have tried to convey, by asserting that quantum theory is just not a physical theory in the sense the accusers want it to be. Rather it is an addition to personal, Bayesian, normative probability theory. Its normative rules for connecting probabilities (personal judgments) were developed in light of the *character of the world*, but there is no sense in which the quantum state itself represents (pictures, copies, corresponds to, correlates with) a part or a whole of the external world, much less a world that *just is*. In fact the very character of the theory seems to point to the inadequacy of the representationalist program when attempted on the particular world we live in.

Part of me sometimes wants to say, "Oh, why don't you stop talking so much, and just go out and find an equation that someone will believe?"

But I hope my frustration will eventually wane, and I'll get back to "fairer-minded assessment" mode. Maybe indeed much does depend on what you mean by the phrase "physical world out there independent of us." As I read you, if I don't mean by the phrase *exactly* what you mean, then the view *must* collapse into a belief in no external world at all. It's probably a useful strategy for starting a church, but it's a waste of your time (trying to straighten me out) if your aim really is to find a bigger, better equation that will pass the tests of other physicists' notice. (If you're not worried about it passing the test of other practitioners finding it useful in some way, then you're the solipsist, not me.)

What is the thing you truly want to accomplish in life? And are your energies really turned to it?

07-04-10 *And After a Deep Breath* (to T. Norsen)

More nicely,

That the external world is at times resistant to my actions upon it, and participating in the consequences of my actions upon it, is enough of a notion of "independence" for me. It is a view that gives both conceptual players (agent and world) an autonomy that doesn't take away from the other's. Indeed, I suspect that this is not what you mean by "physical world out there independent of us." Rather what I can tell of your view is that the world underneath us acts upon us insofar as we are part of its great cog, but that there is no meaningful reciprocity (for we are parts, and it is the whole). This is the kind of thing I call "monism" in the paper, in distinction to "pluralism." Solipsism, as far as I can tell, is a monism, just a monism at the other end of speaking: for, the agent is now the whole, rather than a part.

If you want some philosophical literature to rip into, I would suggest this paper by F. C. S. Schiller: `http://en.wikisource.org/wiki/Personal_Idealism/Axioms_as_Postulates`

Calmer wishes,

07-04-10 *Second Deep Breath* (to T. Norsen)

This is an example of what I find so frustrating in trying to discuss things with you. My paper says this:

> The metaphysics of empiricism can be put like this. Everything experienced, everything experienceable has no less an ontological status than anything else. You tell me of your experience, and I will say it is real, even a distinguished part of reality. A child awakens in the middle of the night frightened that there is a monster under her bed, one soon to reach up and steal her arm—that *we-would-call-imaginary* experience has no less a hold on onticity than a Higgs-boson detection event would if it were to occur at the fully operational LHC. They are of equal status from this point of view—they are equal elements in the filling out and making of reality. This is because the world of the empiricist is not a sparse world like the world of Democritus (*nothing but* atom and void) or Einstein (*nothing but* unchanging spacetime manifold equipped with this or that field), but a world overflowingly full of variety—a world whose details are beyond anything grammatical (rule-bound) expression can articulate.

And then it goes on, I think rather clearly and carefully, to say that "dimension" is a numerical *quality* common to all pieces of the physical world, first prefacing it with

> Physics—in the right mindset—is not about identifying the bricks with which nature is made, but about identifying what is *common to* the largest range of phenomena it can get its hands on. The idea is not difficult once one gets used to thinking in these terms. Carbon? The old answer would go that it is *nothing but* a building block that combines with other elements according to the following rules, blah, blah, blah. The new answer is that carbon is a *characteristic* common to diamonds, pencil leads, deoxyribonucleic acid, burnt pancakes, the space between stars, the emissions of Ford pick-up trucks, and so on—the list is as unending as the world is itself. For, carbon is also a characteristic common to this diamond and this diamond and this diamond and this. But a flawless diamond and a purified zirconium crystal, no matter how carefully crafted, have no such characteristic in common: Carbon is not a *universal* characteristic of all phenomena. The aim of physics is to find characteristics that apply to as much of the world in its

> varied fullness as possible. However, those common characteristics are hardly what the world is made of—the world instead is made of this and this and this. The world is constructed of every particular there is and every way of carving up every particular there is.

And yet you say,

Norsenism 3: *you* claim *to believe in reality (and say some things always at the end about how maybe at the end of the day we'll learn about the dimension of the hilbert space of some system, which you take to be as far as I can tell the one-and-only external fact that exists or that we might get access to in the near future, etc.), . . .*

Where in THIS paper did it say that dimension is the one-and-only external fact? Two objects have *this* quality; five objects have *another* one. But dimension is something that they all seemingly have—it is a quality, and I would say the only one that quantum theory identifies. But I was not born with this answer in my head; I have been working the idea out slowly, and this paper represents the present status of formulation. What you write above may refer to an inarticulate way in my previous expression from the 2002 paper, but I thought the issue at hand was what is in this paper.

07-04-10 *Me, Me, Me Again!* (to N. D. Mermin)

You always delight me!

Merminition 196: *14R. I was surprised to read that "quantum measurement outcomes" are personal. I had thought the probabilities of those outcomes were personal. I will peruse the earlier sections to see if you really meant this. It sounds (ahem) like solipsism.*

It goes back *at least* to the attached note to YOU (and Rüdiger) in 2003 [titled "Me, Me, Me" and dated 12-08-03], and the idea has been in every one of my papers since then!

Here is the way I put it to Norsen in yet the latest attempt to get at the two-level personalism of QBism. [See 01-04-10 note "Too Short an Answer" to T. Norsen.] (He in his oh-so-careful reading didn't even notice the personal bit, and couldn't figure out the distinction between Ballentine and me. At least you noticed it!) *Even* Einstein recognized that the probability statements of QM might refer to "personal outcomes"—that was the reason for that very long quote!

07-04-10 *LNC Section 6.8* (to N. D. Mermin)

Merminition 197: *There are no probabilities of 1 in the argument in LNC* [J. S. Bell's article "La Nouvelle Cuisine"]. *(Indeed arguments of that type were originally developed explicitly to avoid using probability-one assignments, which were held to be unphysical idealizations of the probabilities one could assign to the outcomes of actual experiments.) No agent is certain about anything.*

> That ordinary quantum mechanics is not locally causal was pointed out by Einstein, Podolsky and Rosen, in 1935. Their argument was simplified by Bohm in 1951. . . . Each of the counters considered separately has on each repetition of the experiment a 50% chance of saying "yes". But when one counter says "yes" so also always does the other, and when one counter says "no" the other also says "no", according to quantum mechanics. The theory requires a perfect correlation of "yeses" or "nos" on the two sides. So specification of the result on one side permits a 100% confident prediction

> of the previously totally uncertain result on the other side. Now in ordinary quantum mechanics there just is nothing but the wavefunction for calculating probabilities. There is then no question of making the result on one side redundant on the other by more fully specifying events in some space-time region 3. We have a violation of local causality.
>
> Most physicists were (and are) rather unimpressed by this. That is because most physicists do not really accept, deep down, that the wavefunction is the whole story. They tend to think that the analogy of the glove left at home is a good one. If I find that I have brought only one glove, and that it is right-handed, then I predict confidently that the one still at home will be seen to be left handed. But suppose we had been told, on good authority, that gloves are neither right- or left-handed when not looked at. Then that, by looking at one, we could predetermine the result of looking at the other, at some remote place, would be remarkable. Finding that this is so in practice, we would very soon invent the idea that gloves are already one thing or the other even when not looked at.

QBISM BLOCKS THAT MOVE RIGHT THERE. QBists would not go with EPR and Bell and declare "ordinary quantum mechanics is not locally causal" just because of that. And it is not by a quick assumption of action-at-a-distance, but because for the QBist, probability-1 carries no force in truth assignments.

Yet, the QBist, might have gone *partially* with EPR and said,

> They decided that the wavefunction, making no distinction whatever between one possibility and another, could not be the whole story.

I.e., the QBist might have said, "Yet it gets me into no trouble to provisionally accept that my probability-1 assignment does imply that I actually believe in a pre-existent truth value." And that is where Bell's reasoning shows him a contradiction. So long as the QBist holds on to locality (and he can because of the blocked move above), he *cannot* entertain that his "probability-1 assignment for a measurement outcome implies that he actually believes in a pre-existent truth value for it."

The issues are exactly the same in the algebraic version of it all that I gave in the paper.

Bell and Norsen (and most everyone else but the three QBists) rely on the same crutch when they say that raw, ordinary quantum theory is "not locally causal".

Geesh!

07-04-10 *Me, Me, Me Again!, 2* (to N. D. Mermin)

Merminition 198: *What, for example, does it mean to make a bet that the reading will be 0 when the Dutch bookie I'm placing the bet with, watching the same measurement gate, may disagree? Do you need this additional layer of looseness to get rid of "the measurement problem"?*

Rüdiger is always careful to point out that Dutch-book coherence really is always an internal requirement (i.e., not in last analysis a two-person adversarial thing). It is really only that probabilities be consistent with logic. I'll cc him on this in case he wants to send you something he's written on this take of it.

07-04-10 *Births and Preclusivity* (to R. D. Sorkin)

By the way, I much appreciated your talk yesterday; I got a lot out of it and it helped clarify things to me. (You're on my mind because of a current email debate I'm having with Travis Norsen.)

There is certainly a broad way in which our two research programs overlap: it is that quantum mechanics is ultimately about events being born, and that what we presently call "measurement" should be considered instances of such, though that is not the exclusive provenance of such events. Or at least this is what I thought I understood from you.

Where the programs seem to part is in 1) the manner in which that broad statement above is implemented, and 2) the exact usage/meaning of probability within QM.

For more personal (or petty) reasons I was quite happy to see your *axiom* of preclusivity laid out so explicitly and so honestly. For, by recognizing it as an axiom, you show that you implicate it (at least in the case of your "quantum measures") as *no* logical necessity. You take preclusivity as an axiom connecting the measures to truth values. Nothing wrong with that, but it is an axiom—in principle, one can take it or leave it. Of course, you tried to motivate this by an analogy to old-time probability theory—but my guess is that you would admit that preclusivity is really an axiom there too. I don't know; am I wrong? Anyway, it was useful for me to see these things. For in my program of QBism, one of the key points is that preclusivity is not a logical necessity in personalist Bayesian probability theory either (though I haven't yet used the word "preclusivity"). In fact, the whole approach is based on trying to get some interpretative traction by going the opposite way from you on this issue! For instance, it is the point of Footnote 29, page 18, of my recent posting, `http://arxiv.org/abs/1003.5209`, where I take Norsen and Bell's nonlocality on.

I've just printed your new paper out and will be studying it.

08-04-10 *Me, Me, Me Again!, 3* (to N. D. Mermin)

Merminition 199: *Do you need this additional layer of looseness to get rid of "the measurement problem"?*

In a word, yes. That's what the section on Wigner's friend is about. If quantum theory is a "user's manual" for one's personal experiences, then there is no measurement problem. Each of us uses it, each of us get the consequences of our actions upon the world, but we never let our own uses of the manual be dictatorial over others' experiences.

08-04-10 *Me, Me, Me Again!, 4* (to N. D. Mermin)

Merminition 200: *What, for example, does it mean to make a bet that the reading will be 0 when the Dutch bookie I'm placing the bet with, watching the same measurement gate, may disagree?*

And it's not that he would disagree, but that "to see what I have seen" he would have to interact with me. An old and metaphorical idea: No two poets see the same world. But seriously. It is the point of Footnote 35 and the text to which it is attached.

Sorry I didn't catch this yesterday.

08-04-10 *Your Weltanschauung* (to N. D. Mermin)

Merminition 201: *So I don't think you should have been surprised at my reaction to 14L in the context of your paper, though I really have to take more time to inspect the document before deciding that I should have been reading you more carefully. But my present view is that my confusion on this point is at least as much your fault as mine.*

All fair enough. But I think I was referring to a conversation that I had had with you when I talked about shooting myself in the foot in the passage below (I was never completely sure, but someone definitely said it):

> The answer to the first question surely comes as no surprise by now, but why on earth the answer for the second? "It's like watching a Quantum Bayesian shoot himself in the foot," a friend once said. Why something so egocentric, anthropocentric, psychology-laden, myopic, and positivistic (we've heard any number of expletives) as *the consequences (for me) of my actions upon the system*? Why not simply say something neutral like "the outcomes of measurements"? Or, fall in line with Wolfgang Pauli and say:
>
>> The objectivity of physics is ... fully ensured in quantum mechanics in the following sense. Although in principle, according to the theory, it is in general only the statistics of series of experiments that is determined by laws, the observer is unable, even in the unpredictable single case, to influence the result of his observation—as for example the response of a counter at a particular instant of time. Further, personal qualities of the observer do not come into the theory in any way—the observation can be made by objective registering apparatus, the results of which are objectively available for anyone's inspection.
>
> To the uninitiated, our answer for *Information about what?* surely appears to be a cowardly, unnecessary retreat from realism. But it is the opposite. The answer we give is the very injunction that keeps the potentially conflicting statements of Wigner and his friend in check,[224] at the same time as giving each agent a hook to the external world in spite of QBism's egocentric quantum states.

I hope you didn't take my exclamation marks yesterday too seriously. I resonated deeply to what you said with, "I have trouble enough keeping a grip on my own Weltanschauung, so it's not surprising that I can't hang on to yours." It demands too much of a QBissel. And Carl worked like hell to suppress drawing direct attention to it—to the extent that he could do so without speaking an untruth—in the paper on "quantum certainty" that you refereed for us ... which could have better prepared the ground for you. That is why that paper marked our last-ever joint conceptual-writing project together. (I leave open the possibility that we could write something technical together again. But something where words are particularly important, no.)

I'm glad your daughter is coming by for a visit.

08-04-10 *Your Weltanschauung = My Weltanschauung–Epsilon?* (to N. D. Mermin)

You mean 14R (should have told you that earlier). "Outcome" seems to be too loaded of a term, for you and probably for others as well. Would "experience" in the caption to Figure 1 have alleviated some of your pain? I use it in the figure itself, but I had not in the caption: "consequence = experience" the figure says. Similarly for Figure 5. Read the caption to Fig 5 with "experience" in place of "outcome."

[224] Pauli's statement certainly wouldn't have done that. Results objectively available for anyone's inspection? This is the whole issue with "Wigner's friend" in the first place. If both agents could just "look" at the counter simultaneously with negligible effect *in principle*, we would not be having this discussion.

As a long exchange with you once taught me to stop using the word "knowledge" for quantum states (that's where "belief" came from), maybe you are teaching me that I should be more honest and forthright with the word "experience." For it is definitely where I am heading—that the world before man, after man, in between and everywhere, is a patchwork of "pure experience" (as James called it). See 27L and 27R for this said rather explicitly.

In some ways, this strikes me as a lot in the spirit of your old "correlation without correlata" — that the world becomes, like Noah filled the arc, bipartite by bipartite (instead of the more unary "event by event" for instance). But to get at the ultimate idea, one must jettison the notion of quantum state in its formulation. That is what always clouded me away from your earlier program—it made explicit use of quantum states. But if one strips away the earlier technical formulation, there might be some of your spirit in this present stage of my thinking. Though still another difference might be my emphasis on "becomes." It is the same thing I meant when I told Rob Spekkens that his hoped for view was not "sexual enough" in the attached pages [see note to Rob Spekkens titled "The Helping Hand" and dated 24-09-03.]. With him, the idea of true becoming, is a non sequitur (he embraces the block universe conception of things). With you, I haven't been so sure, but I always felt your "correlation without correlata" at least in those days leaned in that direction.

Does this sound credible to you? Are we making progress?

08-04-10 *Too Silly?* (to N. D. Mermin)

Merminition 202: *Yes, they're too silly. CVs should be concise. People spend a lot of time reading them. You don't want to waste their time with ephemera, however entertaining they might be. But breaking up the writings into categories is a good idea. It's surprising how many people don't.*

This is a good word to know actually. "Ephemeron" and "Ephemera". The world "becomes" ephemeron by ephemeron. Doesn't capture the bipartite by bipartite part of it though.

Thanks for the advice.

08-04-10 *Starting It Over* (to N. D. Mermin)

Your last hatching system was confusing too me; line breaks got deleted or something. So, I'll just respond to your responses directly.

Merminition 203: *So even you, whose writing I admire, use the redundant "of"! Then the battle is lost.*

Not really: I had thought there was something strange in what I wrote—I recognized that, but couldn't figure out what it was.

Merminition 204: *Are you willing to say up front that different agents can experience different readings of a single measurement gate at a single moment.*

To paraphrase William James,

> In its widest possible sense, however, an agent is the sum total of all that he can call his, not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works, his lands and horses, and yacht and bank account.

See footnote 35 on 20R. More seriously, if "two" agents are part of a single common experience, then they are "one" with respect to the experience. Quantum theory as a "single agent's user's manual" only has two slots: one for the agent and one for the system. The theory as it stands, gives no tool for even posing what it means for "two different agents to read a single measurement gate at a single time." In a way, that's been the conundrum since the very beginning of the theory—QBism is just an attempt to embrace that as positively as possible.

Merminition 205: *If so, then you ought to spell out explicitly why QBism requires this.*

I thought I had done that with the Wigner's friend discussion on 5R through 8L. What's lacking there?

Merminition 206: *Yes indeed. And also in the spirit of that Physics Today column on reifying abstractions — and the talk I gave at PI last October.*

I definitely want to watch that again after Templeton. I remember liking it, and also the column, because you did use the word "experience."

Merminition 207: *but now I lose you. The correlata are the unary experiences, which belong to me, but not to physics. What's bipartite here? Is the pair experience-outcome?*

Maybe so (i.e., you lose me), because I'm wanting to put the experiences out into the world as its atoms. That is to say, to put it into physics. See the Dupré quote on 19L and the thing on Bohr in Footnote 34 on 20L. Bipartite refers to the way we usually think of what is happening in quantum measurement—one part is the agent, one part is the system. A clearer view might get rid of the cut. William James would say, the cut is *additive* to the pure experience; it is something placed on top of it. I'm still working out how far I can go with that. But all of this is far down the road for me. The first step is to understand what the QBist's "elementary quantum of action" QBar is telling us in the usual (bipartite) picture. At the very least, when I put an experience into spacetime, it becomes bipartite.

08-04-10 *UNC Visit* (to Y. J. Ng)

I'm just about to remove some silly lines that I had provisionally put in my CV, but I thought you might enjoy seeing them first. They're the two entries just above the "Awarded and Endowed Lectures" section on page 2:

> A true-to-form conversation with Terry Rudolph on the meaning of quantum mechanics is immortalized in Louisa Guilder's history of quantum entanglement, *The Age of Entanglement: When Quantum Physics was Reborn*, (Alfred A. Knopf, New York, 2008), pp. 332–336.
>
> Still Sillier Things: Academic Lineage — F. S. Exner, F. Hasenöhrl, K. Herzfeld, J. A. Wheeler, K. S. Thorne, C. M. Caves, C. A. Fuchs. Erdös number — 3 (but who doesn't have one). Einstein number — 3 (Einstein–Rosen–Peres–Fuchs). Wolfgang Pauli number — 4 (Pauli–Einstein–Rosen–Peres–Fuchs).

Looking at it, they're too silly. But maybe they'll give you some material for my intro! Exner was Schrödinger's advisor, and actually is someone who embraced indeterminism even before quantum mechanics. And Herzfeld (according Mehra and Rechenberg's history) was the first person to coin the term "quantum mechanics."

08-04-10 *What I Mean* (to P. G. L. Mana)

Yes, I like the Truesdell quote very much.

Luca's Preply

Physics is a kind of art, like painting (C. A. Truesdell):

> A theory is a mathematical model for an aspect of nature. One good theory extracts and exaggerates some facets of the truth. Another good theory may idealize other facets. A theory cannot duplicate nature, for if it did so in all respects, it would be isomorphic to nature itself and hence useless, a mere repetition of all the complexity which nature presents to us, that very complexity we frame theories to penetrate and set aside.

and

> If a theory were not simpler than the phenomena it was designed to model, it would serve no purpose. Like a portrait, it can represent only a part of the subject it pictures. This part it exaggerates, if only because it leaves out the rest. Its simplicity is its virtue, provided the aspect it portrays be that which we wish to study. If, on the other hand, our concern is an aspect of nature which a particular theory leaves out of account, then that theory is for us not wrong but simply irrelevant. For example, if we would analyse the stagnation of traffic in the streets, to take into account the behavior of the elementary particles that make up the engine, the body, the tires, and the driver of each automobile, however "fundamental" the physicists like to call those particles, would be useless even if it were not insuperably difficult. The quantum theory of individual particles is not wrong in studies of the deformation of large samples of air; it is simply a model for something else, something irrelevant to matter in gross.

09-04-10 *Starting It Over, 2* (to N. D. Mermin)

Merminition 208: *But now you're telling me that the two agents can't both assign probabilities to the readings of one and the same measurement gate when it interacts with that system?*

That's right: The agent IS the measurement gate. The prosthetic hands are crucial to Figure 1.

Merminition 209: *That not only are the probabilities personal to the agent but so is the event to which the agent assigns those probabilities?*

Well, it is shared between him and the system. So, in a sense it's personal to the system as well.

Merminition 210: *If they can then we're back to my original question — can one experience 0 while the other experiences 1?*

Right, I'm trying to nullify your original question.

Merminition 211: *If they can't, what does it mean for them both to place bets on that reading?*

I don't think it means anything within the quantum formalism. If they want to discuss both seeing something from the same system, they have to "talk" to each other and that is another quantum measurement—one shared between the two of them: From one point of view, agent and system, and from another point of view, system and agent.

I don't want to infuriate you, but this is the way I'm seeing it. And I recognize that these questions are good for me.

09-04-10 *Moving Along* (to N. D. Mermin)

Merminition 212: *since for Bohr (and for me, and I had thought for you) communication between agents was at an entirely different level.*

No, for me, every action an agent takes on the external world (which can be putting an SG device in front of an electron or talking to another agent) falls within the framework of a "quantum measurement". Please see Footnote 46, the paragraph to which it is attached, and the one before that on page 24. Whacking a baseball is a quantum measurement from this point of view.

Here's where I hope you'll say that "strikingly original" phrase again. It is that quantum mechanics is "additive" to the classical world.

Give that little bit of reading a shot, and I'll try to fill the further head-scratching it might engender.

Glad to hear you're not infuriated (and I hope not appalled either).

09-04-10 *And Coming Back a Bit* (to N. D. Mermin)

Merminition 213: *But now you're telling me that the two agents can't both assign probabilities to the readings of one and the same measurement gate when it interacts with that system?*

That not only are the probabilities personal to the agent but so is the event to which the agent assigns those probabilities?

Thinking upon this, you may not realize, but it is why I have always been fascinated with Pauli rather than Bohr. I never felt Bohr's position was consistent, and I tried very hard to believe that it was (and read nearly every philosophical thing he wrote). It is in exactly whether the measurement device is a prosthetic hand, rather than a piece of equipment in the laboratory separate from the agent, that the two men disagreed. This is why the old book is *Notes on a Paulian Idea*—this very issue is the deep part of THE Paulian Idea. (OK, so Pauli himself is not completely consistent, and I take him to task in the present paper, but he came closer to consistency than Bohr, I always felt.) In a way, it means you never quite took me seriously on this "detail." (Quotes because who would take this seriously as "just a detail.")

Actually, I'm sorry I didn't realize that we weren't communicating on this for all this time! Wow, I'm feeling idiosyncratic at the moment! It makes me wonder how many people who think they follow me (to some extent at least, like Max) don't realize how radical I (and Pauli) get.

I deeply appreciate having you as a reader!

09-04-10 *QB Decoherence* (to M. Schlosshauer)

I talked with Rüdiger, and he was OK with my showing you the draft as it stands. As I wrote to him, "Max's feedback may well spur us on to finish the thing and make it an easier paper to write in the long run."

Let me try to sketch a bit of the idea. When von Neumann was not using "Type 1 process / Type 2 process" language for collapse and unitaries, but rather discussing "measurement models" he described it as the two-step process you described nicely in your paper with Camilleri. First there is entanglement between the system and device (and with the addition of Zeh and Zurek, entanglement with the environment), and then—mysteriously—selection. (Of course the Everettians try to drop that last step, but they're not my target here.) You can probably guess that I would say, "von Neumann's setting the issue of measurement in these terms was the great original sin of the quantum foundational debate," and I would. The question is, what would a QBist put in its place and what connection, if any, is there to the previous notion of decoherence occurring before the magical selection step?

For a QBist, the only *physical* process in a quantum measurement is the selection step—i.e., the part the decoherence program tries to ignore—the whack, the action/reaction, the data that leads to a new state of belief about the system. Decoherence? There's no room for it, no role for it in this picture ... or is there? See attached photo (of my chalkboard before it was painted). The ghostly arrows represent the vN-Z-Z way of looking at measurement: We go from an initial quantum state and a specification of a "measurement interaction" (using something closer to my own terms, I have it written the specification as a set of completely positive maps Φ_i) to a decohered quantum state to a selected final quantum state (one of the ρ_i with probability $p(i)$). The arrows are ghostly because for us, that's all imaginary. What happens in reality is the dark upper arrow labeled "whack." It would appear that the decohered state ρ_w is never the state of belief of anybody. And in a way, that is true. For though the usual story is that the outcome of the measurement is "out there" simply unknown to the agent, and so he should adopt this as his new state of belief, we QBists reject that: There is no outcome for the agent except in his own experience. Like the thing John Wheeler used to say about the umpire, "They ain't nothin' till I calls 'em."

Nonetheless, one can imagine a situation in which an agent would adopt ρ_w for his gambling attitudes. It has to do with van Fraassen's reflection principle. Suppose I know that I'm going to whack the system *tomorrow morning* and that consequent to it I will adopt one of the ρ_i for my future beliefs. But you, Dutch bookie, offer me a lottery ticket *today* for a second whack that I'll perform *tomorrow afternoon*. What should I regard as a fair price for the ticket? The reflection principle requires I adopt ρ_w, or I can be Dutch booked.

And that from the QBist side of the world is the story of decoherence. Decoherence doesn't come conceptually before a "selection," but rather is predicated on the possibilities for the next quantum state. Decoherence comes conceptually *after* the recognition of the future possibilities (i.e., what used to be called "selection"). This is why I've been going around for a year saying that Zurek has it exactly backwards.

10-04-10 *Introduction, Pure Experience, Quantum Bayesianism* (to D. C. Lamberth)

First off, let me say that I am a great fan of your book on James and the metaphysics of pure experience, as well as your contribution to the *Cambridge Companion to William James*. Both writings influenced me deeply, and I think it is time I met you (electronically at least).

I am a physicist at the Perimeter Institute for Theoretical Physics in Waterloo, Canada with a deep interest in pragmatism and radical empiricism. You can read about Perimeter here if you care: `http://www.perimeterinstitute.ca/`. As well, I attach my CV, so that you will have some evidence that I'm not a lunatic coming out of the woodwork.

I also attach a recent paper on the research program into the foundations of quantum theory that I head (Quantum Bayesianism, or QBism), and a "press release" on my research group and the pragmatism library I have built up for everyone. I don't expect you to be aware much of quantum theory, but my guess is that the last three sections of the paper should still be quite readable to you nonetheless (because of their broader nature). Most importantly, they give some hint of why I am so interested in James's notion of pure experience, and why I cite you. My feeling is that this phenomenon in my field called "the quantum measurement problem" is as close to an expression of pure experience as we have ever encountered in physics. Moreover, that thinking in these terms will ultimately delete the word "problem" from the phrase "measurement problem" and open up great new vistas of physical exploration.

That is the general reason I contact you—to ultimately set up a dialogue on this subject. I think much progress can be made, but it will require a dialogue between physicists and the right kind of philosophers (pragmatists).

The more specific reason that I contact you now, rather than say three months from now, is that I am working on a pre-proposal for the Templeton Foundation for their call on research in quantum theory (due Thursday, April 15). The title of the proposal (if it gets invited) will be, "That the World Can Be Shaped: Quantum Bayesianism, Counterfactuals, Free Will," and the research it describes will have a large component in making the things I mentioned above more concrete. Below is the "Executive Summary" I have written for it, and it may give you some hint of what I am aiming for. With my reputation, I believe the pre-proposal has a strong chance of being invited as a proposal and then ultimately fly as an honest-to-god grant.

You will note that I mention "two international workshops" in the summary. I am thinking of something fairly intimate, with approximately 15 people for each. One will be targeted for the Alps near Zurich in the land of Wolfgang Pauli and his particular contributions on this subject (along with his interactions with Carl Jung). But for the other workshop, I would like to devote it to "Quantum Theory and Pure Experience" and make it a dialogue between physicists, philosophers, and theologians. Since you are my favorite contemporary thinker on pure experience, it seems natural to apprise you of this, and test whether you might have any interest in being involved. We have excellent conference facilities at Perimeter Institute, but I would likely try to hold it in a more inspiring setting.

Involvement only means spiritual involvement, organizational involvement, intellectual involvement. I would provide all of the funds for this workshop if it materializes.

A student wrote me recently (and it is certainly a line I will use in the proposal), "I like your theory because it returns to me as much freedom as I feel that I have. Such freedom is lost or partially lost in other interpretations [of quantum mechanics]." I hope the same point will help cause you to take notice of what I am writing you here.

> <u>Executive Summary</u>
>
> Speaking on scientific theories, the philosopher William James once said, "If you follow the pragmatic method, you cannot look on [a completed theory] as closing your quest. You must set it at work within the stream of your experience. It appears less as a solution than as a program for more work, and more particularly as an indication of the ways in which existing realities may be changed. Theories thus become instruments, not answers to enigmas. We don't lie back upon them, we move forward, and, on occasion, make nature over again by their aid." On this conception, a theory is not a statement about what the world is, but is a tool, like a hammer, to aid in making the world what we want it to be. The world may resist, but to some extent it is plastic.
>
> The research program of Quantum Bayesianism (or QBism) is a take on the quantum interpretation problem that reveals with mathematical precision, not poetry, that such

plasticity is quantum theory's greatest lesson. With every quantum measurement set by an experimenter's free will, the world is shaped just a little; and so of every action of every agent everywhere.

To further develop QBism and explore its broad implications, we request funds for two postdocs, two international workshops, and regular travel for collaborators.

11-04-10 *QBism Comes to Life!* (to C. Ferrie)

Ferrie-ism 9: *I wanted to play around with the website below and the first dialogue that came to mind was the one in your paper:* `http://www.xtranormal.com/watch/6389801/`*. What do you think?*

QBism already was alive! It's the most living interpretation of quantum mechanics there ever was!

But I'm very proud that that was the first dialogue that came to your mind! You flatter me. What do you think about this one from 2000?

> In the first frame God starts to speak to Adam at a time just before Genesis, "Adam, I am going to build you a world. Do you have any suggestions?"
>
> **Adam**: Mostly I don't want to be alone. I want to have friends ... and enemies to spice things up ... and generally just plenty of people to talk to.
>
> **God**: Done. I'll give you a world populated with loads of other people. But you ask for a bit of an engineering feat when you ask to be able to talk to them. If you want to communicate, the world can't be too rigid; it has to be a sort of malleable thing. It has to have enough looseness so that you can write the messages of your choice into its properties. It will make the world a little more unpredictable than it might have been for me—I may not be able to warn you about impending dangers like droughts and hurricanes anymore—but I can do that if you want.
>
> **Adam**: Also God, I would like there to be at least one special someone—someone I can share all my innermost thoughts with, the ones I'd like to keep secret from the rest of the world.
>
> **God**: Now you ask for a tall order! You want to be able to communicate with one person, and make sure that no one else is listening? How could I possibly do that without having you two bifurcate into a world of your own, one with no contact whatsoever with the original? How about we cut a compromise? Since I'm already making the world malleable so that you can write your messages into it, I'll also make it sensitive to unwanted eavesdropping. I'll give you a means for checking whether someone is listening in on your conversations: whenever information is gathered from your communication carriers, there'll be a reciprocal loss in what you could have said about them otherwise. There'll be a disturbance. Good enough? You should be able to do something clever enough with that to get by.
>
> **Adam**: Good enough!
>
> **God**: Then now I'll put you in a deep sleep, and when you awake you'll have your world.

> **Adam**: Wait, wait! I overlooked something! I don't want an unmanageable world, one that I'll never be able to get a scientific theory of. If whenever I gather information about some piece of the world, my colleagues lose some of their information about it, how will we ever come to agreement about what we see? Maybe we'll never be able to see eye to eye on anything. What is science if it's not seeing eye to eye after a sufficient amount of effort? Have I doomed myself to a world that is little more than chaos as far as my description of it goes?
>
> **God**: No, actually you haven't. I can do this for you: I'll turn the information-disturbance tradeoff knob just to the point where you'll still be able to do science. What could be better? You have both privacy and science.
>
> So Adam fell into a deep sleep, and God set about making a world consistent with his desires. And, poof(!), there was QUANTUM MECHANICS.

Or this version of it from 2010:

> Strictly speaking, meliorism is the doctrine "that humans can, through their interference with processes that would otherwise be natural, produce an outcome which is an improvement over the aforementioned natural one." But we would be reluctant to take a stand on what "improvement" really means. So said, all we mean in the present essay by meliorism is that the world before the agent is malleable to some extent—that his actions really can change it. Adam said to God, "I want the ability to write messages onto the world." God replied, "You ask much of me. If you want to write upon the world, it cannot be so rigid a thing as I had originally intended. The world would have to have some malleability, with enough looseness for you to write upon its properties. It will make your world more unpredictable than it would have been—I may not be able to warn you about impending dangers like droughts and hurricanes as effectively as I could have—but I can make it such if you want." And with that Adam brought all host of uncertainties to his life, but he gained a world where his deeds and actions mattered.

11-04-10 *Templeton, Schlosshauer* (to A. M. Steinberg)

Steinbergism 2: *Obviously, we'd be very happy to have any of your turncoats around. Does sound like philosophy or IHPST are the natural homes for him, but the more he interacted with us in Physics, the happier we'd be. I believe I could pretty easily secure him a (shared) postdoctoral office if all else fell through, but that might not be his first choice.*

Thanks on Max. With regard to Templeton, not to worry: *Everyone* I know is applying for this. There's no real competition between us unless you too are calling to make connections between quantum theory, William James's notion of "pure experience," and Wolfgang Pauli's interest in alchemy and Jungian psychology. You aren't ... are you? Though I could imagine an experiment or two that'd tidy up some of those connections ...

12-04-10 *Truth and Happening, 2* (to N. M. Boyd)

Thanks, I enjoyed that!

I'm glad you mentioned Richard Rorty. I think there's just a wealth of good stuff in his writings (though I don't follow him on many things). I have a long quote of him, that at least indicates what I find most useful to cherry-pick from him, in this pseudo paper: "Delirium Quantum," `http://arxiv.org/abs/0906.1968`.

12-04-10 *QB Glossary* (to the QBies)

By the way, here's the finalized version of the QB glossary.

QBlue signing off!

12-04-10 *Notes on a First and Second Reading* (to D. M. Appleby and H. Barnum)

Last night, I read Marcus's note "quantum belief and quantum reality" for the first time and Howard's paper for the second. Call me pigheaded, but my reaction was like that of a Japanese friend who wrote recently, "I do not understand your long sentence."

In the part that I take issue with (i.e., *not* the part about meatiness, where I do believe there is much work to be done, see Footnote 46 in my recent posting), I only felt that I saw two laments—one of five thousand words, and one of ten thousand—that boiled down to saying a single sentence, "In some cases, there *should* be a notion of a *right* probability assignment independent of the agent's personal mesh of beliefs." Why? "Well there should be." Why? "Well there should be!"

I continue to think such a notion is only trouble, a cloud that keeps us from seeing over the hill. Why would I adopt it when I can do everything your gut tells you that you need to do without it? 1) I walk into a lab, make a judgment about this or that piece of equipment; the experimentalist tells me that I'm "wrong." Fair enough: But, he does that based on his personal mesh of beliefs. 2) I use *my* mesh of beliefs to extract a probability-1 statement about some potential experience. I wait for the experience, and it does not happen. I say to myself, "That was stupid; I was wrong!" Fair enough: But "wrong" comes *after* the fact, and only then can I see that I should readjust my mesh of beliefs.

Particularly, the "wrong" does not come before the actual event. And, for me, that point is crucial for breaking the block. The block universe. What I see in your laments is a feeling that the probabilities should be right or wrong beforehand, and right or wrong based on something impersonal.

I accuse that idea of being a cloud. When it comes to quantum mechanics particularly, it is a cloud that keeps us from having a solution to Wigner's friend already in hand. And it leaves us with an action-at-a-distance that I find unacceptable (block issue again). Howard admits as much with his last sentence:

> We seem on solid ground if we wish to maintain that the fact about how Alice should bet is not a fact about how things are at Bob's site. It's not reasonable to say that quantum physics recommends that Bob should immediately change his betting behavior to the one it recommends for Alice. (Though I suppose it does claim that he'd be better off if he did.)

And I tried to make this point at length on page 5, right column, and footnote 7, of my last big paper. Furthermore, the issue has nothing to do with no-signalling or incompatibility with Lorentz invariance for me: It is all about the issue of the block. See page 18L.

Finally I'll say Marcus sizes me up wrong in the part of the discussion about my old phrase "(and only a little more)". I agree with William James that "the trail of the human serpent is over everything." Every statement of the form "I hypothesize the world to be made (in part) of stuff X" without doubt exhibits the trail of the human serpent. But still such statements have a function, and that function can be better or worse served by different choices of X. My *only* claim is that if one invokes a statement that boils down to "I hypothesize that the world is made (in part) of probability" (as you two seem to me to be doing) one does oneself no favor.

With regard to the trail of the human serpent, I believe that I agree with this paper of F. C. S. Schiller to a great extent: `http://en.wikisource.org/wiki/Personal_Idealism/Axioms_as_Postulates`. Reading it might help diffuse the repeated accusal of my believing otherwise.

13-04-10 *After Hans, 1* (to H. C. von Baeyer)

Opening section now reads:

> Of scientific theories, the philosopher William James once said, "You cannot look on [a completed theory] as closing your quest. You must set it at work within the stream of your experience. It appears less as a solution than as a program for more work, and more particularly as an indication of the ways in which existing realities may be changed. Theories thus become instruments, not answers to enigmas. We move forward, and, on occasion, make nature over again by their aid." On this conception, a theory is not a statement about what the world is, but a tool, like a hammer, to aid in making the world what we want it to be. The world may resist, but to some extent it is malleable.
>
> The research program of Quantum Bayesianism (QBism) is an approach to quantum interpretation that reveals, with mathematical precision not poetry, that quantum theory's greatest lesson is the world's plasticity. With every quantum measurement set by an experimenter's free will, the world is shaped just a little; and so of every action of every agent everywhere.
>
> To develop QBism's mathematics, amalgamate its philosophical implications, put it before a wider jury, and write a book on the subject, we request funds for two postdocs, two international workshops, and regular visits from collaborators.

13-04-10 *A Worry* (to H. C. von Baeyer)

von Baeyerism 61: *The opening quote by James is wonderful, BUT the key phrase, for which you selected it, is in brackets, giving the impression that YOU supplied it to make it fit your purpose. Maybe James was talking about something different from a theory! Is there a way to use more of his own phrasing here, fixing it up with brackets only minimally?*

I guess strictly speaking, he was talking about "metaphysics" just before those sentences, but a couple of paragraphs down, he goes on to say:

> One of the most successfully cultivated branches of philosophy in our time is what is called inductive logic, the study of the conditions under which our sciences have evolved. Writers on this subject have begun to show a singular unanimity as to what the laws of nature and elements of fact mean, when formulated by mathematicians, physicists and chemists. When the first mathematical, logical, and natural uniformities, the first laws, were discovered, men were so carried away by the clearness, beauty and simplification that resulted, that they believed themselves to have deciphered authentically the eternal

thoughts of the Almighty. His mind also thundered and reverberated in syllogisms. He also thought in conic sections, squares and roots and ratios, and geometrized like Euclid. He made Kepler's laws for the planets to follow; he made velocity increase proportionally to the time in falling bodies; he made the law of the sines for light to obey when refracted; he established the classes, orders, families and genera of plants and animals, and fixed the distances between them. He thought the archetypes of all things, and devised their variations; and when we rediscover any one of these his wondrous institutions, we seize his mind in its very literal intention.

> But as the sciences have developed farther, the notion has gained ground that most, perhaps all, of our laws are only approximations. The laws themselves, moreover, have grown so numerous that there is no counting them; and so many rival formulations are proposed in all the branches of science that investigators have become accustomed to the notion that no theory is absolutely a transcript of reality, but that any one of them may from some point of view be useful. Their great use is to summarize old facts and to lead to new ones. They are only a manmade language, a conceptual shorthand, as some one calls them, in which we write our reports of nature; and languages, as is well known, tolerate much choice of expression and many dialects.

> Thus human arbitrariness has driven divine necessity from scientific logic. If I mention the names of Sigwart, Mach, Ostwald, Pearson, Milhaud, Poincaré, Duhem, Ruyssen, those of you who are students will easily identify the tendency I speak of, and will think of additional names.

That he could have been talking strictly of "scientific theories" in the passage from my Executive Summary follows from this line in the paragraphs above:

> Their great use is to summarize old facts and to lead to new ones.

Thanks for worrying. But I'm not quite sure what to do.

13-04-10 *Possibly Final Version of Section 2* (to H. C. von Baeyer)

I turned you into a writer and added one more Jamesian phrase at the very end. 0 characters remaining:

> QBism is an interpretation and technical development of quantum theory due predominantly to C. M. Caves, C. A. Fuchs and R. Schack, with early contributions from N. D. Mermin and A. Peres and historical roots in an eclectic mix of the thoughts from W. Pauli, J. A. Wheeler, and B. de Finetti.

> Three characteristics set QBism apart from other existing interpretations of quantum mechanics. First is its crucial reliance on the mathematical tools of quantum information theory to reshape the look and feel of quantum theory's formal structure. Second is its stance that two levels of radical "personalism" are required to break the interpretational conundrums plaguing the theory. Third is its recognition that with the solution of the theory's conundrums, quantum theory does not reach an end, but is the start of a great journey.

> The two levels of personalism refer to how the "probabilities" and "measurement events" of quantum theory are to be interpreted. With regard to quantum probabilities, QBism asserts that they be interpreted as personal, Bayesian degrees of belief. This is the idea that probability is not something out in the world that can be right or wrong,

but a personal accounting of what one expects. The implications of this are deep, for one can see with the help of quantum information theory that it means that quantum states, too, are not things out in the world. Quantum states rather represent personal accounting, and two agents speaking of the same quantum system may have distinct state assignments for it. The second level of personalism appears with the meaning of a quantum-measurement outcome. On this QBism holds with Pauli that a measurement apparatus must be understood as an extension of the agent herself, not something foreign and separate. A quantum measurement device is like a prosthetic hand, and the outcome of a measurement is an unpredictable, undetermined "experience" shared between the agent and external system. Quantum theory, thus, is no mirror image of what the world is, but rather a "user's manual" that any agent can adopt for better navigation in a world suffused with creation: The agent uses it for her little part and participation in this creation.

The project we propose is to further develop this point of view, both mathematically and philosophically, by way of our own efforts as well as getting a wider community involved in its consideration. On the mathematical side, much work remains to develop a formalism that most crisply expresses QBism's worldview. Fuchs already has a group of six at Perimeter Institute tackling this head on–a key issue is understanding the structure of symmetric informationally complete (SIC) positive-operator measures. For with them, quantum theory takes the cleanest possible expression in terms of probabilities. A key collaborator D. M. Appleby lives in England–travel and accommodation expenses would allow him to become more intimately involved with the group. Co-investigator Schack would advise a postdoc in developing the notions of "quantum randomness" and "private information" from a purely Bayesian point of view. Its mathematics, we believe, particularly will take us to the next stage of understanding the "interiority" (Pauli) of quantum phenomena. Fuchs would advise proposed postdoc M. Schlosshauer in writing a full-length book on QBism, much like his well-recognized book on decoherence. On the philosophical side, we would organize two 10-day workshops. The first, with the help of Pauli scholar H. Atmanspacher and writer H. C von Baeyer, would be devoted to meshing Pauli's prosthetic-hand notion of measurement with modern developments. The second, in conjunction with the William James Society, would explore J. A. Wheeler's idea that "the big bang is here all around us" with each quantum measurement, which in the hands of QBism starts to look like a modern and promising version of James's ontology of "pure experience" where creation comes "in spots and patches."

14-04-10 *Final Cut?* (to H. C. von Baeyer)

Thanks a million! (But quit telling me to break a leg! The imagery is too vivid.) On point number 1, you're right: It's funny how many times I've had a little battle with my conscience before showing you something, and then when I do, you come out as "the good" voice that had already been in me but not listened to. Anyway, it's stripped away! On 2, I modified it to this:

> QBism is an example of the unexpected places to which striving for consistency can lead. When Quantum Bayesianism started 15 years ago, all it attempted initially was to view quantum states as knowledge to dispel some foundational conundrums. But the urge for consistency kept pushing it further, all the way to the two-level personalism we have today. The full implications of this are starting to sink in: From an original reductionist disposition, we arrived at a NONreductionist view of one of the most

fundamental theories of physics! See http://arXiv.org/abs/1003.5209. We now see the many efforts to derive the "quantum-to-classical transition" proceeding backward. Instead, quantum theory should be seen as "additive" to common experience–the union of the two make something greater. This turns the traditional debate on its head. The proposed JTF project is part of a greater goal to develop the view until other physicists feel it in their bones and use it themselves to reshape physics.

Now, if I can get Rüdiger's CV turned into .doc format without too much distortion, I'll be done!

14-04-10 *Willing Free Will* (to D. Rideout)

If I understood you correctly on our walk to the bus stop, you think "free will" is a presupposition for science. In any case, I certainly do! See for instance the left column, page 18, of http://arXiv.org/abs/1003.5209.

In case you're interested, attached is the thing I proposed to the Templeton Foundation today (actually it was a web form, but I weakly TEXified it—that's what I'm sending to you). Honestly, I was rather impressed with John Templeton's Donor Intent; see:

http://www.templeton.org/what_we_fund/donor_intent/philanthropic_vision/.

Let's talk about this again some time. It's my favorite subject on earth.

16-04-10 *Dangerous Jaynesian Tendencies* (to C. Ferrie)

I sent Rüdiger your term paper yesterday, and he replied thusly:

Schackcosm 132: *I just read his term paper. There is some very nice stuff in it. He seems to have dangerous Jaynesian tendencies, though.*

Beware those Jaynesian tendencies!!

16-04-10 *QBies Activate!* (to the QBies and R. Schack)

Today, 1:00 group meeting. It sounds like the big discussion will be on linear dependencies. And never forget these lines from Henry V'th:

This day is call'd the feast of Crispian.
He that outlives this day, and comes safe home,
Will stand a tip-toe when this day is nam'd,
And rouse him at the name of Crispian.
He that shall live this day, and see old age,
Will yearly on the vigil feast his neighbours,
And say 'To-morrow is Saint Crispian.'
Then will he strip his sleeve and show his scars,
And say 'These wounds I had on Crispian's day.'
Old men forget; yet all shall be forgot,
But he'll remember, with advantages,
What feats he did that day. Then shall our names,

Familiar in his mouth as household words—
Harry the King, Bedford and Exeter,
Warwick and Talbot, Salisbury and Gloucester—
Be in their flowing cups freshly rememb'red.
This story shall the good man teach his son;
And Crispin Crispian shall ne'er go by,
From this day to the ending of the world,
But we in it shall be remembered—
We few, we happy few, we band of brothers;
For he to-day that sheds his blood with me
Shall be my brother; be he ne'er so vile,
This day shall gentle his condition;
And gentlemen in England now-a-bed
Shall think themselves accurs'd they were not here,
And hold their manhoods cheap whiles any speaks
That fought with us upon Saint Crispin's day.

We will break this SIC problem one day! With Rüdiger in the list, we are all seven here!

17-04-10 *QBism's Foster Home* (to G. L. Comer)

I was just about to listen to your new recording, but I think I've got a "better yet." I'll listen to it tomorrow evening in The Carolina Inn. I'll be in Chapel Hill for the first time in 10 years, to give the physics colloquium and a seminar Monday and Tuesday. I'll listen to QBism from the old foster home (where the ideas were first fostered in me from reading a paper by Braunstein and Caves).

17-04-10 *World Elements* (to M. Schlosshauer)

For a couple of days I had been planning to write you on the "pure experience" idea, but your last note, replying to "Reasons Why" stole the thunder. You're on the right track, and what you wrote is about all I was going to write you anyway.

But this is all so sketchy at the moment. You wrote:

Schlosshauerism 28: *And I suppose this is where your link to James' "pure experience" comes in—provided I understand correctly what may be meant by this term.*

I think the way to treat James here is only as giving the hint of a hint. And the major part of the synthesis will have to be done by us ourselves, informed by the actual structure of quantum mechanics. This idea of "pure experience" (broadly construed) being the stuff of the world runs wide and deep in the particular lot of people I have taken the time to study over the years. It can be found in the writings of James, Pauli, and Wheeler. A good bit of the hidden Pauli, for instance in his personal letters to Fierz, Jung, and von Franz, was devoted to understanding this "neutral stuff" that quantum theory is the first indication of and dreaming of a "neutral language" to describe it all in. Also, there's some of Bohr even in this regard—revisit the Folse quote I used in the last paper.

But still, much of this only starts to click for me now: It took me nearly 26 years to get this far—i.e., the part where I start to get the hint of the hint and to see that it actually is in context (fitting for quantum mechanics). The clock started when I met John Wheeler and first read things like the quote you quote.

Anyway, I feel like I need to make a big push on reading everyone who has contributed to the neutral-stuff idea. And the group is bigger than I had previously imagined. For instance, have a look at the preface and introduction to Erik Banks' book *Ernst Mach's World Elements* on Google Books. Also, I've recently learned that Bertrand Russell embraced good bits of the Jamesian idea in his two books *Analysis of Mind* and *Analysis of Matter*. And too Ralph Barton Perry's "New Realism." Also Shadworth Hodgson. I feel like I need to get really coordinated on this, and go at it systematically for a bit, before I try to combine it with all this talk of dimension as a kind of valence.

That's where it stands at the moment. Tomorrow morning I fly to Chapel Hill, North Carolina to give the UNC colloquium and a seminar. Greetings to Kari and Eli as well.

Max's Preply

> Well, it had already dawned on me over the past few days that my query could have something to do with the Jamesian conception of "pure experience." There are certainly some hints of this in Section VIII of your paper; I did pick up on those earlier, but I feel the gears are only starting to mesh now. For example, there's the Wheeler quote that opens your section:
>
> > Is the entirety of existence, rather than being built on particles or fields of force or multidimensional geometry, built upon billions upon billions of elementary quantum phenomena, those elementary acts of 'observer- participancy'. . . ?
>
> So I suppose that part of the answer to my query would be to refer to exactly this speculation of Wheeler's: that, in fact, there are no other "physical processes" besides creation-by-quantum-measurement, and that the primary "ontology" are these acts of creation. Then there would be no "particles scattering other particles." It would be merely a figurative way of saying that two parts of the world have entered into a relationship by which they give rise to something new, namely, the "experience" that's shared between the two systems.
>
> And so on.
>
> The obvious next question would of course be: what precisely constitutes these "experiences"? How do they affect the future evolution of the world?
>
> And I suppose this is where your link to James' "pure experience" comes in—provided I understand correctly what may be meant by this term. For instance, the *Stanford Encyclopedia of Philosophy* says:
>
> > James's fundamental idea is that mind and matter are both aspects of, or structures formed from, a more fundamental stuff—pure experience—that (despite being called "experience") is neither mental nor physical. Pure experience, James explains, is "the immediate flux of life which furnishes the material to our later reflection with its conceptual categories . . . a that which is not yet any definite what, tho' ready to be all sorts of whats . . . " (ERE, 46). That "whats" pure experience may be are minds and bodies, people and material objects, but this depends not on a fundamental ontological difference among these "pure experiences," but on the relations into which they enter. Certain sequences of pure experiences constitute physical objects, and others constitute persons; but one pure experience (say the perception of a

> chair) may be part both of the sequence constituting the chair and of the sequence constituting a person. Indeed, one pure experience might be part of two distinct minds, as James explains in a chapter entitled "How Two Minds Can Know One Thing."

So, in this way of thinking, one would really close the circle back to the Wheeler quote! (I like it!)

19-04-10 *QBism* (to G. L. Comer)

Boy did that take me back to some youthful experiences in old John Hamilton's dance hall in Cuero TX, when a band would be just freestyle jamming before or after their paid performance and nearly everyone was gone (but old David and me). I have a particular memory in mind, though I can't express it adequately.

No doubt you caught an aspect of QBism—a world with creation all around, near and far. There was something about your licks that had an almost spatial feel, as if I could feel things all around me. That was really cool; thanks!

Things around here are significantly similar to the way they were 20 years ago (though all traces of Jim York are gone). I went into The Cave last night and drank a PBR, and shopped at The Bookshop tonight. Tonight's stash below.

Even Eugen Merzbacher was the same! He's 89 now, and was without doubt my most attentive listener! I have lunch with him tomorrow.

1. Murray N. Rothbard, *Ludwig von Mises: Scholar, Creator, Hero*, PB, 6.50 USD.
2. Edward H. Madden, *Chauncey Wright and the Foundations of Pragmatism*, HC, 8.50 USD.
3. George Herbert Mead, *Movements of Thought in the Nineteenth Century: Works of George Herbert Mead, Vol. 2*, edited with introduction by Merritt H. Moore, PB, 6.50 USD.
4. Robert Avens, *Imagination is Reality: Western Nirvana in Jung, Hillman, Barfield and Cassirer*, PB, 7.99 USD.
5. James Cambell, *Understanding John Dewey: Nature and Cooperative Intelligence*, PB, 8.50 USD.
6. Donald D. Palmer, *Structuralism and Poststructuralism for Beginners*, PB , 5.99 USD.
7. Jim Powell, *Derrida for Beginners*, PB, 5.99 USD.
8. David Baggett and Shawn E. Klein, editors, *Harry Potter and Philosophy: If Aristotle Ran Hogwarts*, PB, 8.99 USD.
9. Maurice Friedman, editor, *The Worlds of Existentialism: A Critical Reader*, HC, 6.99 USD.
10. Philip P. Wiener, *Evolution and the Founders of Pragmatism*, with foreword by John Dewey, PB, 3.99 USD (slated as gift for Lee Smolin).

19-04-10 *Pauli, Fechner, Schelling* (to H. C. von Baeyer)

Good morning. I'm a bit closer to your neck of the woods this morning—I'm in Chapel Hill, NC. (Though there was no gravy for the biscuits this morning: Maybe there are a bit too many souths for my taste.)

I'm reading a very nice book by Michael Heidelberger on Gustav Fechner, *Nature from Within*. Fechner was the father of psychophysics and a philosopher who argued that nature is permeated with souls. I'm halfway thinking I might write a review on this book for you if you're interested in it.

For the moment though, I'm just curious: We know that Pauli read Schopenhauer and Mach, but is there any evidence that he read Schelling, Oken, or Fechner? (Mach was greatly influenced by Fechner.) Anyway, all three of these guys thought nature was alive and in continuous creation, which you can guess intrigues me.

Hans's Reply

> Welcome to the Confederacy. Fechner is the very first reference in Karl von Meyenn's contribution to the Almanspacher/Primas book. (The chapter is entitled "Wolfgang Pauli's philosophical ideas viewed from the perspective of his correspondence.") So, to answer your questions I will hunt for clues in the yellow bible.
>
> The idea of atoms with souls has a history reaching back to Lucretius. Bernard Pullman, in *The Atom in the History of Human Thought*, cites the lovely conception of Denis Diderot (1713-1784) of animate atoms. Diderot hopes that he will be buried next to his beloved Sophie, because his atoms and hers may still retain vestiges of feeling, and may try to mingle with hers. Atoms in love!

19-04-10 *Fuchs_Penta-Chart_2010(2).ppt* (to R. F. Wachter)

Sorry for the delay. It looks pretty good. Here are my answers to your queries. [. . .]

3. *Every* quantum measurement is a little act of creation. Disturbance is too weak of a concept; it goes further than that. In a wispier philosophical mood, I wrote recently: "The research program of Quantum Bayesianism (or QBism) is an approach to quantum interpretation that reveals, with mathematical precision not poetry, that quantum theory's greatest lesson is the world's plasticity. With every quantum measurement set by an experimenter's free will, the world is shaped just a little; and so with every action of every agent everywhere." Do you dare say that in front of your admirals? You might enjoy the attached "Glossary of QBism," particularly with respect to SICs, "QBalah." [See 07-04-10 note "Up Front" to Å. Ericsson, N. D. Mermin, and R. Schack.]

20-04-10 *Fuchs_Penta-Chart_2010(2).ppt, 2* (to R. F. Wachter)

Wachterism 2: *Enjoy this well-known alternative dictionary,*

`http://www.gutenberg.org/etext/972`.

You compare my QBism glossary to the Devil's Dictionary?

20-04-10 *Adam and God Again, Again* (to Č. Brukner)

I just booked my flights to Jeff Bub's meeting. So I'll actually be there. For a bit I wavered because I had waited so long in booking my hotel that the price had jumped up to $272/night. But Spekkens is letting me room with him; so that took the sting out of it. Now I get to witness the fireworks between you and Tumulka first-hand!

On this trip, I've been reading a nice book on the life and thought of the physicist/philosopher Gustav Fechner. (I'm in North Carolina at the moment.) I'm finding many things thought-provoking in it. Here is a little passage on the thought of theologian Christian Hermann Weisse:

> All that is real originates in acts of freedom, originates in "voluntary action in general," either acts of God or acts of his creation. These acts are voluntary because they totally lack subjection to necessity. In creating the world, God willingly limits his own power and thereby continuously establishes the spontaneity and freedom of the beings he created. Spontaneity and freedom of action occur within time and thus make God a historical being, unfolding himself over time. These two traits also restrict his capacity to entirely foresee the future.

It strikes me as hauntingly similar to my Adam/God story. I didn't realize that I'd be branching into reading theology soon!

20-04-10 *My Itinerary for DC* (to R. W. Spekkens)

I've been talking with Eugen Merzbacher today: Wonderful firsthand stories of Einstein, Oppie, Lothar Nordheim (now there's a name for you to search your memory), Wheeler, Rosen, Podolsky, Glauber, Bryce DeWitt, Everett, Wigner, Dirac. Very nice.

21-04-10 *Very Nice* (to Y. J. Ng)

I just finished reading the von Baeyer article. Your idea with Henk was very nice. I am just about to board the plane. I bought six more books last night. Everything fit in one suitcase. Thanks again for all the hospitality—I think that is the best I have ever been treated just for giving a couple of seminars!

(And lobby your department chair to instate a modern quantum information course; it is sad when one of the *few* people at the colloquium to already be aware of generalized quantum measurements was an 89 year old!)

21-04-10 *World Elements, 2* (to M. Schlosshauer)

Now you flatter me too much. Lost wanderers shouldn't get such easy rides; it's not healthy. I just think there is something solid to work on here.

How is your Everettian analysis going? Are you still working on that somewhere on the side? I do hope you'll finish that. This morning I'm going to read Zeh's article, "Feynman's Quantum Theory." Before the Los Alamos fire, I had a copy of the 1957 conference proceedings he discusses, and I was aware of the dialogue he cites (nearly 20 years ago!). I happened upon it at the University of North Carolina in a room that was opened briefly so that an old, mostly out-of-commission copier could be used while the main one was broken. There was an old file cabinet in the room labeled "preprints," so I rummaged through it. Lots of stuff from when Bryce DeWitt was still there, and at the very back was a folder with two copies of this proceedings. One was labeled, “Last Copy. Do Not Take.” But the other one said nothing, and I took it. (I later told Jim York about this, and he took the last copy, ignoring the warning label.)

I had some nice discussions with Eugen Merzbacher, who was the first to *really* teach me quantum mechanics. He’s 89 now, and was actually the most attentive person in my colloquium. He moves slowly, but his mind is as sharp as it was when I first met him, without exaggeration. It was nice to hear so many first-hand stories of Einstein, Oppenheimer, Lothar Nordheim (now there’s a name for you to search for – it was at his house that Wheeler’s much advertised version of twenty questions really happened), Wheeler, Teller, Rosen, Podolsky, Glauber, DeWitt, Everett, Wigner, Dirac, Schlosshauer!, and others. Merzbacher gave me a first edition of von Neumann’s book that he had had bound long ago. It’s in beautiful shape.

21-04-10 *Sci Am* (to H. C. von Baeyer)

We never did write that Pauli article. (My guess at this stage is that, unless I sketch a first draft, it’s not going to happen. But I might be wrong?) Anyway, beside it or on top of it or instead of it, here’s an idea: See correspondence with George Musser from *Scientific American* below, where he writes:

> I’m not sure this paper lends itself to straightforward abridgment for a Sci Am article, though, because the style is that of a manifesto – an effort to rally one’s colleagues, address the misgivings they have, and set them on the right path. A general reader (and I include myself in that category) is coming to this from a very different point of view. We’d need a more straightforward account: here are the mysteries of quantum mechanics; here are the usual responses; here is why those responses fail; here is a better way. I suspect that such a reader will actually be more receptive to the notion that quantum states represent a form of gambling odds than a physicist or philosopher steeped in the subject is; we have less baggage to shed. *But* we *do* share the desire to know ultimately what the information is about and, even if this question cannot be answered, will want to know how the Bayesian approach advances that goal.

Trouble is, I don’t think I know how to write something that is *not* a manifesto—it is a character flaw. That’s where your writing discipline could come in. What would you think about us throwing in together on something like this? Does the idea evoke any emotions in you?

21-04-10 *Book Review Notes* (to H. C. von Baeyer)

Notes from a mid-flight reading below:

At the nine-volume remark, should mention or cite von Meyenn . . . at the very least so that readers who do not know the word “Briefwechsel” will know what to search on.

I’ve never liked it when people say Pauli and Jung “co-authored” a book. It gives the sense that they wrote the two papers together. It is a book of two papers by two distinct authors. They didn’t even write an introduction together, did they?

Planck length: Miller didn’t say that in the context of quantum gravity? Was there a discussion of gravity about?

Your page 100 remark, about Pauli getting to the Born Rule before Born. I have a faint (actually not so very faint) memory that Miller may be close to right on this. Trouble is, at the moment, I can’t remember my source. It was a scholarly article by Fine or someone else (probably not

Fine). Anyway, the point they made was that Born received his Nobel prize so very late because everyone in Copenhagen had already understood probabilities for measurement results as squared amplitudes. It was a vestige of the Bohr-Kramers-Slater theory, and even some Einsteinian work earlier than that. So Born's Nobel-prize referees (surely Bohr was in that lot) were not so very keen to give him a Nobel prize on that in any great hurry. I have a rather strong impression of this. I believe there is a letter from Einstein to Born in their letter collection, where Einstein laments the lateness with which the prize came. But Einstein was not part of the Copenhagen crowd. It'd be interesting to look up what Pauli wrote to Born about the award, and what Pauli wrote to his other friends at about that time.

23-04-10 *WRT Your Talk on Quantum-Bayesianism* (to I. N. Hincks)

I never did get back to you on your queries. I hope you've found some of the answers to your questions in a re-reading of the paper. But I did want to tell you how beautiful I found the closing sentences in your note. In fact, I have made use of them, and I hope you don't mind. Please see the attached proposal to the Templeton Foundation; I quote you in Section 7. If I ever use them again in a more public document, I will certainly cite you by name. Thank you for inspiring me.

Ian's Preply

> Your talk today turned some things up side down for me.
>
> I find the underlying principle of the thing rock solid, at least for now. I have questions of clarification below. (If you have already written down the answers somewhere, please refer me there.)
>
> 1. On the first page of the essay you handed out was a quote from Bell, about the qualifications involved in being an observer (does it need a PhD, etc.). Does your theory have a set of such qualifications? I imagine your response might be (it's fun to put words into other people's mouths) that such a list is not necessary, it is after all, a single-user theory, and that, perhaps, the trick is to use empirical observation to infer that (most) other humans are qualified to be agents. And perhaps an ordinary rock is just as capable an observer, but having no useful way of communicating with a rock, we choose to draw no conclusion. But let's remove the focus from other (potential) observers, or agents, onto ourselves. What I would like to get at is this: your overhead slide with the stick-man with a moustache and fan hands was drawn by you as, well, a stick figure. Was the reason for this iconic, or literal? I can perform measurements with my dial hands. If I get into an accident I can perform (albeit, more slowly) observations with just my left hand. But with no hands, what am I? Well, I get robotic arms implanted to continue with my measurements. My point is that your theory seems, at least to me, that your diagram must have been iconic, simply because it is hard to "nail the soul to the body" in a consistent way. (I had some of Walt Whitman's words, from *I Sing the Body Electric*, running through my head during your talk today: "And if the body were not the soul, what is the soul?")
> 2. This is how I understood one of your points: it is unfair to necessitate the deconstruction of the shoe into its constituents (being "elementary particles"), and call this deconstruction the shoe. Rather, a shoe is a shoe, and an elementary particle is an elementary particle, and when we are lucky, physics will give us relationships

> between the two. This has a tinge of Platonism to me. It seems like "shoeness" must exist in a realm, and it must be the physical realm since this is where our fan hands measure it. Would you agree? And if you substitute "Love" for "shoe", where is Love's realm? The physical realm? These are earnest questions I have, they are not posed as a criticism.
>
> I like your theory because it returns to me as much freedom as I feel that I have. Such freedom is lost or partially lost in other interpretations.

24-04-10 *Implications* (to N. D. Mermin)

How's Berlin? I just wrote the note below to Huw Price, and because of it, I reread the thing I had sent off to Templeton last week. It dawned on me that maybe you should know how I have implicated you in this business! You'll find your name mentioned at the top of Section 3. Thanks for all the good tension over the years!

24-04-10 *Weyl's Book and John Wheeler* (to the QBies)

- 1928. *Gruppentheorie und Quantenmechanik*. transl. by H. P. Robertson, *The Theory of Groups and Quantum Mechanics*, 1931, rept. 1950 Dover.

Wheeler, on the other hand, was born in 1911 and got his PhD in 1933 (having skipped a bachelor's degree). He wrote:

> I first knew Weyl before I first knew him. Picture a youth of nineteen seated in a Vermont hillside pasture, at his family's summer place, with grazing cows around, studying Weyl's great book, *Theory of Groups and Quantum Mechanics*, sentence by sentence, in the original German edition, day after day, week after week. That was one student's introduction to quantum theory. And what an introduction it was! His style is that of a smiling figure on horseback, cutting a clean way through, on a beautiful path, with a swift bright sword. Some years ago I was asked, like others, I am sure, to present to the Library of the American Philosophical Society the four books that had most influenced me. *Theory of Groups and Quantum Mechanics* was not last on my list. That book has, each time I read it, some great new message.

I never realized that Weyl's book was written before von Neumann's. Indeed, it was even written before Dirac's! (von Neumann's was 1932, and Dirac's was 1930.)

And if I'm not mistaken, our beloved Weyl-Heisenberg group goes back to that book of Weyl's. Does anyone know where to find it in there? Despite Andrew Gleason's spirit residing in my copy, he hasn't given me enough hints to find the right section on perusal.

24-04-10 *Saturday Morning Alchemy* (to the QBies)

It's one of those mornings when I find myself thinking that what we're doing is *really*, *really* important. Because it goes to the very heart of what matter is. Lately, I've become fixated on the imagery of "the philosopher's stone" as the root of the right way to think of quantum systems. My daughter Emma was reading to me from a book *The Sorcerer's Companion* about the real-life sources of the things in Harry Potter, and it put me in an extraordinarily sentimental/contemplative mood. "Medieval alchemists like Nicholas Flamel now spoke of producing a new substance—an extraordinarily potent catalyst that when added to common metals would trigger their transmutation ..."

I usually only share this earthy base of my thinking with Marcus, but I'm feeling rather open mouthed this morning. I say "earthy" because of this thing Marcus wrote a couple weeks back:

> One of the difficulties with this programme is that a building has to rest on something. Actual modernistic buildings rest on the same wormy, fungus and bacteria- impregnated earth that the medievals used to build their mud-huts on. With the modernistic structures we build in our minds it is even worse: they don't simply rest on the same soil, they actually rest on the mud hut itself, entire and unreconstructed. Despise the mud-hut at the bottom of the whole thing, as contemporary physicists almost all do, and you are despising the foundations on which everything else depends.

The earthy is definitely welling up in me lately. For instance, here's a blurb I wrote for the back of the new edition of Nielsen and Chuang's book:

> Nearly every child who has read Harry Potter believes that if you just say the right thing or do the right thing, you can coerce matter to do something fantastic. But what adult would believe it? Until quantum computation and quantum information came along in the early 1990s, nearly none. The quantum computer is the Philosopher's Stone of our century, and Nielsen and Chuang is our basic book of incantations. Ten years have passed since its publication, and it is as basic to the field as it ever was. Matter will do wonderful things if asked to, but we must first understand its language. No book written since (there was no before) does the job of teaching the language of quantum theory's possibilities like Nielsen and Chuang's.

I guess I just want to say something nearly the same to you QBies: Believe in the philosopher's stone!

Attached is an extract of the pre-proposal I sent off to the Templeton Foundation last week.

Top of the mornin' to you,

24-04-10 *Weyl's Book and John Wheeler, 2* (to the QBies)

The section in Weyl's book seems to be Section IV.D.14, starting on page 272. I'm not quite sure what it is saying, but it sounds awfully deep. It seems he views the generalized Pauli operators as the very defining feature of a quantum system, and bases everything on the consideration of them. I think it means SICs really do form the right "phase space" for QM.

25-04-10 *The Importance of Gadflycity* (to N. D. Mermin)

Glad Berlin is living up to your dreams. That's one German city I've never been to.

Merminition 214: *I'm honored to be mentioned with Asher as a kind of godfather of Qbism, but I'm not sure what I did to deserve the accolade. I think of myself as more of a gadfly. At best.*

I thought long and hard about putting you in that line, but decided that if I were to say something about Asher, I must also say something about you. Do not underestimate the contributions of a good gadfly—that was exactly your role; you hit the nail on the head. And I'm proud to say it; if it weren't for the tension you (more than anyone else) have given me, QBism would be just another lazy, half-way interpretation of quantum theory. Indeed another line in the proposal was an implicit statement about your gadflycity: "QBism is an example of the unexpected places to which striving for consistency can lead. When Quantum Bayesianism started 15 years ago, all it attempted initially was to view quantum states as knowledge to dispel some foundational conundrums. But the urge for consistency kept pushing it further, all the way to the two-level personalism we have today." The "urge for consistency" was really just "saving face" in light of your queries.

Merminition 215 : *Does Templeton require a religious twist? Is it necessary that Qbism end up pointing the finger at God?*

They do require an applicant to declare the relation between his proposal and "Sir John's Donor Intent." See Section 13 in the thing I sent you. But I enjoyed reading his donor intent, and found it quite liberal and humble. My reading is that it says, in essence, never throw away a possible source of enlightenment—no matter the source. My reading of it didn't sound a lot different (though less thorough and eloquent) from my readings of William James. Anyway, everything I said in this proposal is genuine me, and if that is religious, it is that I'm not going to let the world completely tell me what to do: The world must listen to man (in the end), as much as man must listen to the world. If someone construes that saying agency is real, has something to do with pointing a finger at God, then QBism is already pointing there. That's the thing that spooks Norsen and Spekkens (and Caves?) and the like—they think that if science doesn't reduce mankind's actions to a kind of nothingness, then all hope is lost. But of course, I just think QBism points to agency, NONreductionism, and plasticity: It points to the idea that the world is malleable. I wouldn't call that God.

Merminition 216 : *How did you hook up with Max Schlosshauer? I met him for the first time in Copenhagen last year, and liked him a lot.*

That's a story worth recording on a slow Sunday. My first contact with him was pretty inconsequential; I had just asked him to comment on a Kofler/Brukner paper that I think does the quantum-to-classical transition better than anything the decoherence program had ever dreamed up. Max's reply was predictable (defensive of the status quo) and not enlightening.

It was really my second contact with him that got things going in a positive direction. He wrote me in his capacity as an editor, asking if I would consider writing a book on QBism. In that note, he wrote furthermore:

> First, I'd like you to know that for quite some time I have really enjoyed following the development of the quantum Bayesian program and the intellectual discourse surrounding it. I have found your writings both illuminating and entertaining, and I feel that they have instilled in me an ever-increasing appreciation of the Bayesian viewpoint and of the motivations and philosophical attitudes underlying the approach. You see, I have been doing a lot of work in the decoherence field, and as you know several of the original forces in this area are openly or semi-openly Everettian, sometimes to the point of where one may get the impression that an Everett view is somehow *implied* by the insights brought about by decoherence. Even I had not been immune to this influence for quite some time, though I finally feel some recovery coming on; the fog is lifting.

I thought, "Yeah right, good salesman; you just want me to write a book for this series." And I pretty much wrote it off as a sales tactic.

But then I saw him at the Växjö meeting last year, and gave him a copy of the QBcoherence paper that you refereed. I thought I was just being devilish. (Zurek was there too, and Max was hanging out with him mostly.) To my surprise, throughout the week, I spied Max actually reading the paper during the talks. On my return home, I stopped in London for a night with Rüdiger and reported all this to him, saying how it surprised me and that maybe "he's not a fake" after all. Then, while in Rüdiger's living room, I looked up Max's webpage and found to even more surprise this line: "More recently, I have become interested in information-theoretic axiomatizations of quantum mechanics, quantum Bayesianism, and generalized quantum-like theories." I said to Rüdiger, "Fakes don't put lines like that in their webpages."

Thus we were off to a new start. Sometime later, Max wrote asking for what of William James I recommend he read. Lots of correspondence ensued. I think maybe what really capped off this transition in him was when I said that my vision of the world was not so different than what happens in jazz improv. That seemed to go to his bone. (You know that he's a jazz musician, don't you?) Since then, he's been a good force behind QBism, helping keep me honest, pushing me to expand points and write more clearly, etc. Then at some point he brought up the idea that he write a book on QBism, and I ran with it.

That's the story roughly.

Merminition 217: *I just turned 75.*

Congratulations. You are the most un-reified 75-year-old I have ever met. I'll be happy when you're ready to take up that gadfly role again! The program needs you.

All the best from drizzly Waterloo.

27-04-10 *Midnight Reading* (to H. C. von Baeyer)

Attached are a couple of articles you might be interested in. Particularly the Bernstein article. I like the way you have your review written now, and I wouldn't want you to change it, but midnight curiosity still drives me to find some evidence for what I claimed of Pauli vs Born. (I am not there yet, but I might be closing in ... or giving up ... we shall see what another hour gives.)

Anyway, reading these articles was quite nice for all sorts of reasons for me. Most particularly, I have recently taken an interest in the historical development of our beloved "displacement operators" or "generalized Pauli operators," by which we generate SICs. I had the most pleasant shock the other day: Weyl in his 1928 book declares, roughly, "A quantum system is one for which there are two quantities A and B that satisfy, $AB = e^{i\phi}BA$." I.e., He *defines* a quantum system by the existence of our generalized Pauli operators! (And consequently with hindsight some time from now, if all works out for us, by the existence of SICs.) See Section IV.D.14, starting on page 272, Dover edition, and notes below. I don't have the book with me at the moment, but I *believe* he takes this as the essence of Heisenberg's postulate (he calls it) when it comes to finite dimensions. Thus it was a real surprise to learn that this commutation relation (in "Heisenberg form") is actually Born and Jordan's and inscribed on Born's gravestone!

Of course, it has to be that after all this work of ours we will end up back at quantum mechanics, but it is interesting to see how some of the earliest math in one of the earliest formulations of the theory (Weyl's) came so close to what we are working with today.

By the way, the remark below about Gleason is referring to the fact that, thanks to a chance encounter of one of my students in Powell's Bookstore in Portland (and his reticence to buy it), I now own Andrew Gleason's copy of Weyl's book. He told me the story of the stamp in the book, and I got on the web and bought it right away!

27-04-10 *Questions for Aaron's Consideration* (to A. Fenyes)

Here's a document I hastily threw together this morning. Could we meet to discuss some tomorrow? I will be out of town Thursday and Friday.

> Here are some things I'd be quite interested in knowing the answer to. If anything strikes you, let's work on it. All three questions are geared toward pieces of rederiving quantum-state space from urungleichung considerations. If these question don't lead to directions for progress, and something else strikes you, feel free to modify at will. [. . .]
>
> **3. Building Up Quantum-State Space One Simplex at a Time?**
>
> Let me call a set $\mathcal{S}$ of probability distributions *consistent* if no two points violate
>
> $$\frac{1}{d(d+1)} \leq \sum_r p_r q_r \leq \frac{2}{d(d+1)} \; . \tag{99}$$
>
> (All probability distributions p_r assumed to be over d^2 points.) Let me call such a set *maximal* if no further probability distribution can be added to $\mathcal{S}$ without violating (99), but otherwise the set must be as full as can be. One can prove that all maximal consistent sets must be convex and closed. (See paper with Appleby and Ericsson.) Moreover, quantum-state space is an example of a maximal consistent set.
>
> Tell me as much as you can about maximal consistent sets with the following extra bit of structure: Every extreme point $\vec{p} \in \mathcal{S}$ must be embeddable in some set of d^2 extreme points $\vec{p}_\alpha$ that form a regular simplex.[225] I.e., for every extreme $\vec{p}$ there must exist some set $\vec{p}_\alpha$ of extreme points with $(\vec{c} - \vec{p}_\alpha) \cdot (\vec{c} - \vec{p}_\beta) =$ constant (with $\alpha \neq \beta$ and $\vec{c}$ being the center point of the probability simplex), and $\vec{p} = \vec{p}_\alpha$ for one of the values. That is to say, what happens if we build up a maximal consistent set $\mathcal{S}$ one regular simplex at time? What further properties does it share with quantum-state space? Might it just be isomorphic to quantum-state space?!?

28-04-10 *Stueckelberg??* (to L. Hardy)

Hardyism 6: *Do you know what Stueckelberg did in his paper on real Hilbert space quantum theory? I haven't read it and it doesn't seem to be available on line. In particular, did he come up with the notion of local tomography? Bill and I are hoping to finish our paper in the next few days and it would be good to cite Stueckelberg properly.*

No, Stueckelberg didn't have that notion. He just showed that one could recover "single system" quantum mechanics (or "full system" rather) using a real Hilbert space, plus superselection rule. Particularly demanding that all observables commute with a certain operator J such that $J^2 = -I$.

I think the first person to use a local tomography axiom independently of Bill was Araki. It should be at the end of this paper if recall correctly: H. Araki, "On a Characterization of the State Space of Quantum Mechanics," Comm. Math. Phys. **75**, 1 (1980). He noted the usual problem in fields other than the complex.

Stueckelberg, by the way, went quite insane at some point. I read a story a while back about how he showed up at a conference once thinking (and acting) that he was a horse. He was eventually institutionalized. Maybe it means you shouldn't think about real Hilbert spaces too much!

[225] This appears to be a more careful statement than what I was guilty of so many other times. See Footnote 218, as well as Footnotes 216, 219, 220, 221, and 222.

29-04-10 *Spectra of* $G = \sum_{r=1}^{d} \Pi_r$ (to M. A. Graydon)

Thanks Matthew! Interesting, especially $d = 5$. I've been reading on your Emersonian essay again, with my after-dinner beer. (Demopoulos's essay had more urgency, since I have to meet with him tomorrow. So he got the dinner reading.) I've just gotten to the Wigner's friend part, which I haven't started yet. That'll have to wait for another quiet spell. Eventually, I'll write you up some notes on it. You do a really good job, but I'll nitpick some.

I'm steeling myself for Spekkens' arrival. We're rooming together for this conference; he should arrive in a half hour. Imagine the shear in the spacetime continuum of this room tonight: The left-hand side a block universe, the right-hand side an anti-block! Something strange is bound to happen.

04-05-10 *Gout and Regout* (to W. G. Demopoulos)

It's slowly getting better. I soaked it in Epsom salts water a couple times yesterday. Sunday was absolute misery getting through the airports—from Washington Reagan, I had a connection in Chicago O'Hare. Getting from H12 to G8 was a triumph of human will! But by yesterday afternoon, I was able to stand on it well enough to pull dandelions (of which there has been an explosion here that I can't afford to turn my back on).

I'm glad to hear you're back home safely, and enjoyed my time with you this weekend. I had said that I would be in London for the Saturday talks of Harperfest, but I'm starting to have a few misgivings: There is so much that needs to be done in this yard, and I hate to see another weekend evaporate. (I've been gone the last two for conferences.)

Yesterday, I found Bertrand Russell's *Collected Papers*, Vol 7, which I had ordered ages ago. So, I stopped reading my book on Fechner for the evening to indulge in a little Russellania. The reason I bought this volume is because it contains the only book-sized manuscript that he never published, and it records his transition from being against to being for William James's (and Mach's) "neutral monism" (as Russell called it).

The thing in Schrödinger's *Nature and the Greeks* I was telling you about is what he calls the "principle of objectivation":

> By this [i.e., by "the principle of objectivation"] I mean what is also frequently called the 'hypothesis of the real world' around us. I maintain that it amounts to a certain simplification, which we adopt in order to master the infinitely intricate problem of nature. Without being aware of it and without being rigorously systematic about it, we exclude the Subject of Cognizance from the domain of nature that we endeavor to understand. We step with our own person back into the part of an onlooker who does not belong to the world, which by this very procedure becomes an objective world.

He also goes on about it in a lengthier way in *What is Life?* As I recall, it is all really good reading. Though of course, with my present pragmatism and radical empiricism, I don't much subscribe to the principle, at least when it comes to quantum issues: QBism's view of quantum theory as a normative single-agent "user's manual" leaves the theory necessarily in great part about the agent himself.

I will dig up the Forman article for you, about Schrödinger's and Weyl's and Exner's and others early leanings toward indeterminism, once the library restores my privileges to retrieve things online. (I discovered that I was overdue with some books.)

04-05-10 *Gout and Regout, 2* (to W. G. Demopoulos)

Demopoulosism 39: *Russell's* Theory of Knowledge *ms is somewhat misleadingly titled since it's really more about his theory of propositions and propositional understanding than anything else. I believe the best development of the views of his that you're interested in is in* The Analysis of Matter *(oddly, better than* The Analysis of Mind *on these topics, a book that I regard as something of an abortion that he should have discreetly buried). But there are parts of* Theory of Knowledge *that were published (in* The Monist*, under the title "The Principle of Acquaintance" that deal explicitly with James and Mach and which are likely to interest you, if I'm remembering correctly.*

Indeed, that is the very part of the book that interests me. Also I now have both *Analysis of Mind* and *Analysis of Matter* on my bookshelf. All of this is part of my summer reading program. (And I will keep your criticism of the former in mind as I read it.)

05-05-10 *1003.5209* (to C. H. Bennett)

When you think of me, why is it always prompted by a bad philosophical paper? What do you think about when you see the good philosophical papers? I suspect you never write Scott Aaronson, for instance, saying, "Scott, What do you think of 'QBism, the Perimeter of Quantum Bayesianism' by the physicist Fuchs?" ☺

Scott was at Jeff Bub's annual meeting last week and gave an excellent talk on the "mud fight" (he called it) between himself and Watrous, on the one side of the ring, and you, Debbie, John, and Graeme on the other side. Best talk of the whole meeting, for sure. I had some nice discussions with Scott before and after. He hadn't realized that various assumptions of linearity are pushed upon you nearly automatically by an epistemic interpretation of quantum states. For instance, if you could clone with quantum theory, you would *never* call a quantum state a state of knowledge. Which is the same as saying: If you think there's enough evidence to call a quantum state a state of knowledge, you wouldn't expect that you could clone (even if you didn't yet have a proof in hand). The "linearity trap" is a very happy home for the quantum-state epistemicist.

Attached are two pictures of Spekkens and me at that meeting, sent to me by Michael Seevinck. (The old man with his mouth agape is Charles Misner.)

Yeah—I know you could guess—I don't think much of the Lyre paper: It's the standard 80-year-old fare; no creativity. Quantum measurement problem, humbug. "Quantum theory is in conflict with [Lyre's] CMP," humbug again. My own feeling is that "measurement outcomes" and the world of common experience are just given (i.e., undeniable), but that quantum theory is "additive" to that. It is means of achieving uncommon experience in addition to the usual common stuff. I'd be curious to hear your reaction of pages 22 (right-hand column) through 25 (left column, end of first paragraph) of `http://arxiv.org/abs/1003/1003.5209`.

Well, it's a work in progress: There's no pretense that it's a final story. I am absolutely enamored, however, with Eq. (8) on page 12. If these SICs always exist they give a very pretty way of thinking of the Born Rule for calculating probabilities. I joked with Scott (about my friend Hardy and others), "If your axioms for quantum theory only took you a few months to construct, then it's too cheap a solution."

Things are going well here. My little group is discovering all kinds of things about these SIC measurements. I.e., even if we can't see how far down the beach they go, each one seems to be pretty in its own right, and every now and then one comes across two that have something in common. For instance, we just discovered something very nice in dimensions divisible by 3!

06-05-10 *The Urgleichung as a Normative Rule* (to R. Schack)

Your note from yesterday confronts the central issue, and makes it clear that we have much to discuss and work out in Zurich (what better place than where the whole thing was born!) and Växjö. My feeling is that when we adequately answer the issues you bring up, we will at the same time understand the precise reason for *this* modification to the LTP rather than some other—i.e., why the urgleichung takes the precise form it does. I may comment a little more later after I get some things cleared off my desk.

Rüdiger's Preply

For the last few weeks, I have spent a lot of time thinking about the status of the Urgleichung. Regarding it as an addition was a stroke of genius. What I am struggling with is where exactly between the normative and the empirical it is located.

It is clearly not normative in the Dutch book coherence sense, i.e., it is not a completely internal rule. It says something about the world. It gives an agent who uses it an advantage. A piece of the world has a new property, dimension. Suppose I measure a system with dimension d. Then I can analyze the measurement in terms of a particular counterfactual measurement, one that is appropriate for dimension d. The Urgleichung tells me how my betting odds for the counterfactual measurement should relate to those for the actual measurement.

What happens to me if I use different betting odds, i.e., odds that violate the urgleichung? One possible answer, which is problematical and not really what I want, is that if I violate the urgleichung, I will not be well calibrated. One way in which this might work is sketched below. Do you have any thoughts on this? This is tied up with the very definition of dimension, of course. If the definition of dimension is that it enables an agent to set probabilities according to the urgleichung, then one simply could not have a system of dimension d and not use the urgleichung. But that is begging the question.

One way to bring in calibration is through a game in which the agent announces probabilities for the ground both for the case that the measurement in the sky is counterfactual and the case that the measurement in the sky is factualized. An opponent then chooses if the measurement in the sky is performed. The statement would be that if this game is repeated many times, this creates two subsets of outcomes. The statement would be that the agent cannot be well calibrated for both subsets unless he uses the urgleichung.

Several things are wrong with this construction. If a measurement is an extension of the agent, how could an opponent choose which measurement is performed? Also, for this construction to work one would have to ascertain that the measurement in the sky is an actual SIC and not something else, which elevates "being a SIC" to a property of a piece of the world. Bad.

06-05-10 *Växjö Abstract* (to M. Schlosshauer)

Attached is my draft of an abstract for your session. If it fits the bill of what you wanted, let me know and I'll hit "submit" on the registration form. If you don't like it, or you catch any typos, let me know.

Any further word one way or the other on whether WHZ will be there? Morally, I really ought to confront him real-time with this.

Quantum-Bayesian Decoherence
Christopher A. Fuchs and Rüdiger Schack

The usual story of quantum measurement since von Neumann has been that it occurs in two steps: First the quantum system becomes entangled with the measuring device; then, mysteriously, there is a selection of one of the entangled state's components in order to single out an actual measurement result. But the entangled wave function alone (with its freedom to be expressed in any basis) cannot say how it should be decomposed so that the selection can even be effected. The theory of quantum decoherence, developed by Zeh, Zurek, and others, attempts to shim up this deficiency in the von Neumann story by supplementing it with a further story of interaction between the device and an environment: The idea is that the specific form of the interaction with the environment specifies how the system+device state ought to be decomposed. But then what of the mysterious selection? Decoherence theorists usually leave that question aside, implicitly endorsing one variety or another of an Everettian interpretation of quantum mechanics.

In contrast, the Quantum Bayesian view of quantum theory developed by Caves, Fuchs, Schack, Appleby, Barnum, and others, leaves most of the von Neumann chain aside: Instead of unitary evolution, it takes the idea of common experience as first and foremost. In this view, quantum "measurement" is nothing other than an agent acting upon the world and experiencing the consequences of his actions—the very same thing he was doing before quantum theory was even thought of. A quantum measurement is most generally an agent's "whack" upon the external world and its unpredictable consequence for him. To say it still differently, the Quantum Bayesian story is the preface and conclusion of the von Neumann one, without the dramatic artifice of unitary evolution, entanglement, and decoherence in between: selection is the consequence of a whack and nothing more.

Thus, it would seem there is no foundational place for decoherence in the Quantum Bayesian program. And that is true. Nonetheless, one can identify a two-time gambling situation (gamble now on the outcome of a measurement in the future, given that an intermediate measurement will be performed in the nearer future) for which consistent gambling behavior mimics a belief in decoherence. The consistent gamble makes use of a quantum version of van Fraassen's reflection principle, and explains to some extent the seduction (but misleadingness) of the decoherence program. This work builds on [1], [2], and [3], where the Born Rule is viewed as an empirical addition to probability theory and quantum theory is seen as "additive" to common experience, rather than "underlying it" in a reductionistic sense.

1. C. A. Fuchs and R. Schack, "Quantum-Bayesian Coherence," `arXiv:0906.2187`.
2. C. A. Fuchs and R. Schack, "A Quantum-Bayesian Route to Quantum-State Space," `arXiv:0912.4252`.
3. C. A. Fuchs, "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209`.

06-05-10 *Växjö Abstract, 2* (to M. Schlosshauer)

Schlosshauerism 29: *Judging from the abstract and what you've told me before, you surely have a point: the final step of "selection" is the notorious achilles' heel of the decoherence program. But at the same—I'd say—the program (understood properly!) never claimed to remedy this worry. Instead, it assumed that one would tack on in the end whatever interpretation of QM one wanted, in order to effect selection. In other words, decoherence (again, properly understood) assumed that one works in a picture where selection happens at some stage of the vN chain.*

Understood. But the way I said it was this:

> But then what of the mysterious "selection"? Decoherence theorists usually leave that question aside, implicitly endorsing one variety or another of an Everettian interpretation of quantum mechanics.

And you have to admit that that is *usually* the case. Furthermore, the Oxford Everettians think that decoherence is precisely the remedy—that selection is never needed and decoherence is precisely the missing link.

In any case, I'll make this claim: It may be true that, logically speaking, the decoherence program is noncommittal on the interpretive program, but my suspicion is that, for all those who take it seriously as a foundational ingredient, the die is loaded. That von Neumann Type 2 (unitarity) is considered the more *natural* thing for physics to be talking about than von Neumann Type 1. The assumption is, unitarity is no mystery—that's the starting point.

Schlosshauerism 30: *Now, to my mind, the new aspect that the QBist approach is bringing to the table is to say: all unitaries are subjective. So the whole entanglement business of decoherence appears in a much less physically relevant light. I think most (if not all) other interpretations would ascribe some sort of objective character to unitaries (and thus the process of entanglement à la vN), and thus there'd be a real place for decoherence in this picture.*

06-05-10 *Movie Title?* (to S. Savitt)

It was good talking to you again. May I ask you to send me the movie title you mentioned that brought up some imagery of the Jamesian theory of truth? I'd like to watch that.

I read the Gerard Manley Hopkins poem: It is indeed powerful! I wish I could read it with the same verve you demonstrated the other night!

Steven's Reply

> The movie is *Playtime*, which fans on the IMDB say should be watched only in a theatre in the 70mm version, which one never has a chance to do. Every film by Tati is wonderful in its own way.

10-05-10 *The Urgleichung as a Normative Rule, 2* (to R. Schack)

[Here's the completion of the note I had started last week.]

You do have several good points in this note. [See R. Schack's Preply to note titled, "The Urgleichung as a Normative Rule," dated 6 May 2010.] And I do think that Zurich is going to be a grand time to think about it deeply. Here are some shorter, preliminary replies at the moment.

Schackcosm 133 : *What I am struggling with is where exactly between the normative and the empirical it is located.*

My answer at the moment is that I don't want to see it as "between" at all. The urgleichung is a normative statement, full stop. It is that a normative statement (a recommendation for behavior) has been latched onto for empirical reasons. The usual vision of a physical law is that it is a) intended to be descriptive (in a correspondence sense) and b) empirically motivated. What we QBists do is simply replace a) by "intended to be normative."

Schackcosm 134 : *One way to bring in calibration is through a game in which the agent announces probabilities for the ground both for the case that the measurement in the sky is counterfactual and the case that the measurement in the sky is factualized. An opponent then chooses if the measurement in the sky is performed.*

Yes, like you, I don't like this. It is similar to Itamar's way of posing coherence in `quant-ph/0208121` and `quant-ph/0510095`. As you always emphasize, coherence should be posed as a purely internal criterion. That's a first consideration, but then there is also this further crucial point that you make (specific to quantum theory): "If a measurement is an extension of the agent, how could an opponent choose which measurement is performed?" And finally, on calibration, I'd say we can't waiver: "Probability is single case or nothing!"

My feeling is that if we get an argument for the specific form of the urgleichung, we'll at the same time start to understand what the agent is supposing of his world so that he would view any other connection between the probabilities as ill-advised. And for this, we're going to have to dig deep into our souls again. I wrote this to Max yesterday:

> Rüdiger and I are now set for Växjö. We're in Zurich the week before, and then we transfer to Sweden, via Copenhagen, on Sunday, June 13. At the moment, we're scheduled to stay in Växjö 'till the end, because I'd like to talk with Anton if he does actually show up. But on the guess that there's a significant chance that he'll be a no-show with only a few days notice (as he often is at conferences), we're contemplating uprooting to Copenhagen for a day or more. Two weeks of sublime thought! I'm really looking forward to it. The main project on the table is to find a convincing motivation for the urgleichung. Consistency, like always, should carry the day! It's just a question of following its instructions.

It must be that there is something in our conception of quantum measurement that pushes us to the urgleichung (or at least the linear form of it).

I wish I had more to say, but at the moment, I don't.

10-05-10 *Reference and Weyl* (to W. G. Demopoulos)

Demopoulosism 40 : *I'll be copying the Weyl today and should be able to put it in the mail very soon. Do you have the page number for your quote from Feynman about the atomic hypothesis?*

I wish I had marked that page number down! For, this morning, I haven't been able to find it. However, I am quite certain I found it in that book—I believe I pulled it off my shelf and tracked it down when I was writing that section of the paper. Anyway, a web search this morning shows me that it can (also?) definitely be found in *The Feynman Lectures on Physics*, Volume 1, beginning Section 1-2. Unfortunately, I cannot see the page number on that. . . . Wait! I can do better, that

IS the page number, 1-2 (that's the convention of the book). The Section is titled "Matter is Made of Atoms."

... Interesting. Just as I was putting my copy of *The Character of Physical Law* back on the shelf, I noticed that it was sitting exactly beside Gerald Feinberg's book, *What is the World Made Of?* It's been about 30 years since I've read that one, but my guess is, his answer wasn't, "The world is made of this and this and this. The world is constructed of every particular there is and every way of carving up every particular there is."

10-05-10 *1003.5209, 2* (to C. H. Bennett)

Beautiful article on pre-mineralarian mineralarianism! On Rob, I think he was just temporarily knocked for a loop. I had just commented that I swallowed six pills at once. He seemed surprised, "Six at once!?" I said, "No gag reflex; I'd be great in the porn industry." Then I was so tickled with myself, I couldn't stop laughing. Rob had difficulty recovering himself as well.

I'm jealous of your visit to Seven Pines. You and I were there together previously in 2004.

The 200th anniversary celebration sounds perfect. Kiki and I would have loved to have come (i.e. we would definitely have done it), if I weren't already scheduled for Europe on the same day. I'll be in Zurich on the 12th just finishing Renato Renner's meeting, and then traveling to Sweden on the 13th for Max Schlosshauer's special session on "quantum decoherence" at the annual Växjö meeting. So, from Zurich to Zurek for me. See attached trouble-making abstract. [See 06-05-10 note "Växjö Abstract" to M. Schlosshauer.] I get to confront Wojciech directly with this.

Charlie's Preply

> I think of you often, and not mainly in connection with bad philosophical papers. I suspected it was bad, just needed you to tell me. I am at a conference on decoherence in Minnesota with, it turns out, a lot of talk about quantum foundations, the Seven Pines Symposium, sponsored by a wealthy gentleman physicist whose hobby is refurbishing 1910 era cars, especially Mercedes-Benz. Others include Bill Unruh, Amit Hagar, Phil Stamp, Hans Briegel, David Wallace, and Alastair Rae. In the pictures you sent, why is Rob holding his book upside down? Is that part of the SIC-POVM philosophy? In reading the suggested part of your latest tract, I especially liked the new uses of diamonds and the ability to calculate the additional attraction of a hungry child toward an apple.
>
> I have been waging a quixotic battle of my own, trying to persuade the curator of the Museum of Hoaxes to correct his site's entry on Mineralarianism, where he says minearlarians eat rocks. In fact they eat synthetic chemicals made from rocks (and air and water), but most people are surprised to hear this is possible. I have found an amazing number of otherwise well educated people who think that human nutritional requirements are so complex that a person could not possibly survive on an all-chemical diet—surely there must be some undiscovered substances in natural foods without which humans would sicken and die, just as they do when deprived of known vitamins or essential amino acids. In the course of this I uncovered a piece of research done by NASA exploring just this kind of diet. See attached article.
>
> Theo and I are celebrating the 200th anniversary of our house in June—see attached invitation. It's maybe a bit far for you and Kiki to come, but I think it's the sort of event you would enjoy.

12-05-10 *Correct Woody Allen Quote* (to the QBies)

You might want to know the correct quote as well:

> "I hate reality, but, you know, where else can you get a good steak dinner?"

12-05-10 *Today's Colloquium* (to the QBies)

Today's colloquium speaker Verlinde strikes me as potentially interesting: He strikes me as having a good "idea for an idea" (but it probably shouldn't be thought of as more rigorous than that). Anyway, the talk might give you some food for thought in relation to QBism, or "additionism" as Marcus calls the broader program.

12-05-10 *Definitely Over the Limit* (to M. Schlosshauer)

Schlosshauerism 31: *I discovered [this album] just the other day: it's pianist Jacky Terrasson with vibes player Stefon Harris locked in spontaneous interplay. Another great example for how standards are just some very vague foil for creating something entirely new.*

And so too of space itself. See this talk we just had at PI today, `http://pirsa.org/10050022/` (it's not posted yet, but should be by tomorrow morning my time . . . or probably by the time you read this note). Space, like the jazz standard, is the foil in the background—or at least that's a thought that intrigues me. You can also see this paper by Verlinde, `http://arxiv.org/abs/1001.0785`. It's all vague and hand-wavy at the moment (dimensional arguments really), and from the wrong philosophical point of view, but I can't help the feeling that there is something really deep here. It says that space is the stuff on the "outside," and I quite like that. It is connected (I think or hope) to what I meant in the last paper when I said that QBism "tinkers with spacetime."

But on to the music! There is deep stuff here as well. I loved this line in the album you suggested: "Our two instruments became one instrument with four hands." That, I think, is the right way to think of Alice and Bob in most quantum information protocols. When in collaborative communication, two (ostensible) agents are really one with respect to the concerns of personalist, Bayesian probability theory. I must have sent you William James's notion of the self before:

> In its widest possible sense, however, a man's self is the sum total of all that he can call his, not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works, his lands and horses, and yacht and bank account.

Thanks a million again for the suggestion. It pushes me to new thoughts, and for that I am always grateful.

13-05-10 *Contact from the Stellenbosch Institute for Advanced Study* (to H. B. Geyer)

It looks like it's been seven years since we've "seen" each other! Great to hear from you. It sounds like a lot has been happening down there.

Thanks for the flattering remarks on my latest paper. I am intrigued by your proposition, but let me try to understand it better. Are you wondering whether I would be interested in an existing project? Or are you asking whether I might consider being a "project leader" (phrase you used)?

It is true that I look for every opportunity possible to promote progress on understanding these magical quantum measurements, the symmetric informationally complete (SIC) positive operator valued measures. (See all the references in this paper by Scott and Grassl, if you want to get a sense of the breadth of the problem: `http://arxiv.org/abs/0910.5784`. You might also get a laugh out of the "press release" on my group's involvement with these things; attached. And here's a meeting I organized on the problem a couple years back: `http://pirsa.org/C08025`.) Would a program like that be something of interest to STIAS?

It would indeed be nice to give you a shorter visit and see what it is like there. Thank you for the invitation. But with all the travel I'm already committed to this summer, I couldn't possibly do it before September or October. Would something like that work for you?

Hendrik's Preply

> A voice from the past – you may recall that you helped guide Robert Schumann 10 years ago through a thesis on Quantum Information Theory which I supervised; I think we sporadically exchanged a few e-mails after that.
>
> Much has happened here since; I/we managed to establish a National Institute for Theoretical Physics in South Africa, funded by the SA Dept of Science and Technology. Neil Turok will be able to tell you something about it; amongst other things Neil was instrumental in getting Stephen Hawking here in 2008 at the inauguration (see `http://www.nithep.ac.za/`).
>
> My own career has recently somewhat diverged from theoretical physics; I am presently the director of the Stellenbosch Institute for Asdvanced Study (STIAS) – see `http://www.stias.ac.za/`. (Lenny Susskind has been a long time supporter, by the way, and spent time here in 2009 – `see http://www.stias.ac.za/news.html`). STIAS has an ambitious programme covering all disciplines, with a focus on inter-/transdisciplinary projects. Finding projects within, or primarily rooted in, the natural sciences proves to be quite a challenge.
>
> I recently read your papers on Qbism, Perimeter etc., finding them stimulating, became aware of your work at Perimeter and wondered whether you would be interested to explore a quantum information related project at STIAS. Such projects typically entail that a project leader and a group of researchers identified by/in consultation with him/her are invited as STIAS Fellows who then spend time here simultaneously – from one to several months (not unlike the Kavli Institute setup, but on a smaller scale of course).
>
> Perhaps you would consider first coming to STIAS on your own to get a feel for the place. I can invite you as my guest for a week or two (or longer, if your programme would allow it). This will also provide an opportunity to interact with people at NITheP, apart from whoever is here as STIAS Fellows at the time.
>
> Please give it a thought, and let me know.

14-05-10 *Definitely Over the Limit, 2* (to M. Schlosshauer)

Schlosshauerism 32: *As for considering Alice and Bob as one agent: do you then look at this agent as possessing two independent prosthetic hands with which "he" can perform two different—and potentially synchronous—actions upon the world, at different points in space?*

Your question on Alice and Bob is good one, a deep one, an important one. I promise you an answer in Växjö! Monday, I go back to DC, this time to report on the QBies to ONR. So I'm scrambling once again!

14-05-10 *Qutrit SIC POVM experiment* (to L. A. Rozema)

Rozema-ism 1: *Is there anything that makes the 3d sic povm more interesting than the 2d sic?*

There's a quote by Richard Feynman that goes: "A poet once said, 'The whole universe is in a glass of wine.' We will probably never know in what sense he said that, for poets do not write to be understood. But it is true that if we look in glass of wine closely enough we see the entire universe."

Similarly one can say of the qutrit: The whole of the probabilistic structure of quantum mechanics is already in it. What I mean by this less poetically is that Gleason's theorem, which underlies the Born Rule for quantum probabilities, makes crucial use of the qutrit in its proof. Particularly, Gleason shows that if the theorem is true in dimension three, then it is true in all dimensions (even infinite dimensions), and as it turns out, the hard part of the proof is precisely the dimension-three part. So everything stands or falls on the qutrit case.

Now, we Quantum Bayesians want to take the "quantum law of total probability" (i.e., the Born rule written in SIC language) as a new foundation for quantum theory. So, it is just nice to see it firmly established (experimentally) in dim=3, and by way of this, one can make a direct comparison (in word and deed) to Gleason's theorem when advertising the new point of view.

That is to say, Gleason (effectively) shows the QLTP as a *theorem* in dim 3. (I say "effectively" because he didn't know of SICs at the time, and thus could not rewrite the Born Rule into that language.) We on the other hand want to take the QLTP as an axiom. So, it really ought to have some experimental grounding. And showing that it has experimental grounding in dim 2 is of no great interest, because Gleason's theorem doesn't even work there: I.e., the QLTP has no competition from Gleason in dimension 2. All the direct competition is in dim 3.

Finally, let me attach one of Gelo's very pretty equations. See Eq. (2) in the attached file. The "QBic equation" along with the "Bloch-sphere-type" equation uniquely specifies which probability distributions for SIC outcomes correspond to pure quantum states. Only in dimension 3, so far as we know, does the QBic equation take such a simple and beautiful form. If simplicity unlocks secrets, it's going to be found there. Still another reason for looking at qutrits. (And once again, another reason that qubits are too trivial. In the qubit case, only the Bloch sphere equation is active—for the QBic equation reduces to it in that case. Quantum life only becomes interesting in dimension 3.)

Hope that helps some.

17-05-10 *Chris and the Navy* (to D. H. Wolpert)

Wolpertism 2: *Navy? Related to things like quantum computing/encryption?*

No, quantum foundations. They fund my QBism (Quantum Bayesian) effort. Specifically, the technical part of it—to develop a *clean* formalism for quantum theory that dispenses with quantum states and instead uses probabilities only. The key part of the apparatus is the SIC (symmetric informationally complete) positive operator valued measures—an object we believe (but have not yet proven) to exist in all finite Hilbert-space dimensions.

See attached glossary. [See 07-04-10 note "Up Front" to Å. Ericsson *et al.*]

20-05-10 *Civil War Times* (to D. B. L. Baker)

Yesterday, on my drive between Shepherdstown, West VA and Dulles Airport, I took a small side jaunt to the Antietam Battleground. The view was breathtaking; I wish you could have been there. Such a beautiful place, juxtaposed with the idea of so much killing. I cried a bit before getting back in the car.

20-05-10 *My Weyl Quote* (to H. Price)

I'll show you mine; now you show me yours ...

Unfortunately, I don't know the original source (where Weyl wrote it). I do know however that I got this from Paul Forman's article, "Weimar Culture, Causality, and Quantum Theory, 1918–1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment." Presumably (hard to remember, it being 11 years since I copied this down) the phrase "[regarded as a four-dimensional continuum of events]" was Forman's addition (i.e., not mine, but I can't guarantee it at the moment).

> Finally and above all, it is the essence of the continuum that it cannot be grasped as a rigid existing thing, but only as something which is in the act of an inwardly directed unending process of becoming ... . In a given continuum, of course, this process of becoming can have reached only a certain point, i.e. the quantitative relations in an intuitively given piece $\mathcal{S}$ of the world [regarded as a four-dimensional continuum of events] are merely approximate, determinable only with a certain latitude, not merely in consequence of the limited precision of my sense organs and measuring instruments, but because they are in themselves afflicted with a sort of vagueness ... . And only "at the end of all time," so to speak, ... would the unending process of becoming $\mathcal{S}$ be completed, and $\mathcal{S}$ sustain in itself that degree of definiteness which mathematical physics postulates as its ideal ... . Thus the rigid pressure of natural causality relaxes, and there remains, without prejudice to the validity of natural laws, room for autonomous decisions, causally absolutely independent of one another, whose locus I consider to be the elementary quanta of matter. These "decisions" are what is actually real in the world.

21-05-10 *About Changing the World* (to L. Freidel)

Returning to last night's discussion ... The part about "changing the world" (literally) with the aid of a scientific theory. I expand on some of what I said in Sections 6, 7, and 8 of this paper: `http://arxiv.org/abs/1003.5209`.

But more directly, you might enjoy reading the first few paragraphs of the attached proposal I wrote for the Templeton Foundation. It says it all a bit more flowingly, and I was pretty upfront about it with them. I.e., I actually believe this stuff!

21-05-10 *Workshop Talks* (to O. C. O. Dahlsten)

Let's give it the following provisional title:

"Some Properties of QBist State Spaces, whichever Ones Rüdiger Schack Does Not Cover"

How does that work for you?

21-05-10 *A Third Way* (to M. Gleiser)

I was reading your piece in the *Harvard Divinity Bulletin* from 2005. I just want to quickly respond to these lines of yours near the beginning:

> After reading hundreds of creation myths I realized they all fall within a simple classification scheme, based on how each answered the question "Did the world come to be at a specific moment in the past?" That is, "Was there a moment of creation?" The answer can only be "yes" or "no." A "yes" means the universe has a finite age, just as we do; it appeared some time in the past and is still around today. A "no" can mean two things: either the universe has existed forever, an eternal, uncreated cosmos, or it is created and destroyed in a cyclic succession that repeats itself throughout boundless time.

But there is a third way to envision things ... at least, and probably more than that, but one I care about. It is that creation comes in "spots and patches" as William James puts it:

> Our acts, our turning-places, where we seem to ourselves to make ourselves and grow, are the parts of the world to which we are closest, the parts of which our knowledge is the most intimate and complete. Why should we not take them at their facevalue? Why may they not be the actual turning-places and growing-places which they seem to be, of the world—why not the workshop of being, where we catch fact in the making, so that nowhere may the world grow in any other kind of way than this?
>
> Irrational! we are told. How can new being come in local spots and patches which add themselves or stay away at random, independently of the rest? There must be a reason for our acts, and where in the last resort can any reason be looked for save in the material pressure or the logical compulsion of the total nature of the world? There can be but one real agent of growth, or seeming growth, anywhere, and that agent is the integral world itself. It may grow all-over, if growth there be, but that single parts should grow *per se* is irrational.
>
> But if one talks of rationality—and of reasons for things, and insists that they can't just come in spots, what *kind* of a reason can there ultimately be why anything should come at all?

I like it because it tracks with what I think is going on with quantum measurement. But we can discuss.

I can't wait until this meeting is over, so that I get a chance to read some of these writings of yours that I just discovered!!

22-05-10 *The Coffeemaker Revisited* (to L. Hardy)

I just got the coffee brewing this morning and imagined the following. A devotee of Sarah Palin says, "God created the world just as it is. All the complexity, all the detail you see, to its last detail, was in God's great plan." And that of course, includes the bad Sunbeam coffeemaker design I deal with everyday (the carafe always drips onto the counter). A devotee of Daniel Dennett says, "The way to understand the world is as Conway's Game of Life. All the complexity, all the detail you see, to its last detail, was in the game's initial condition." And that of course, includes the bad Sunbeam coffeemaker design I deal with everyday (the carafe always drips onto the counter).

26-05-10 *Invitation to Speak at Science and Nonduality 2010* (to S. E. Sobottka)

I have been thinking hard about whether I might take you up on your invitation—the conference distinctly intrigues me—but in the end I must say "no." It is not that I am not sympathetic to *some* aspects of what I read at the conference website. In my own opinion, quantum theory might well imply, or at least hint at, a kind of pantheism. But when so, it does as a kind of pluralism (something along the lines of ideas in William James's book *A Pluralistic Universe*). So, it would be a kind of spiritualistic universe, but not one of "oneness"—instead a kind of "manyness," a pluriverse. However, that's just a line of thought and research—one that I'm sure will take me several years and several developments in the formalism to see through clearly. Not something written in stone—but a research direction—and ideally it might be good for the general public to know something about this.

The reason I say "no" to the invitation is what I perceive as the hodgepodge nature of the meeting and the speakers there. The ideas I read are really all over the map: It feels to me like a million stories by a million lonely souls. Take F. David Peat, Stuart Hameroff, Amit Goswami, and me. The *only* thing we'd really have in common is the *word* "quantum"—there's no common substance beyond that. Goswami's website contains nearly the opposite of everything I'd say. There's nothing wrong with debate of course: But a debate at the service of a pre-established cause (say "nonduality") troubles me. Moreover, I worry about the level, or to be more honest, lack, of serious critical thinking in many that I see in the speaker list.

In all, I think the conference is just not the right forum for me. At Perimeter Institute, I carry a coffee cup that says, "Life is not about finding yourself, but about creating yourself." All the evidence in front of me indicates a conference largely consisting of people who've spent years "finding themselves" rather than ones who've jumped into the stream of life and contributed to making the world "go." In the end, I'd likely be pretty uncomfortable there.

But thanks for your consideration, and best wishes.

27-05-10 *Stream of Life* (to J. B. Lentz)

Just yesterday, I turned down an invitation to speak at one of the strangest conferences I've ever been invited to. If you have a look at the other speakers, you'll understand why. See: `http://www.scienceandnonduality.com/speakers.shtml`. (The main page of the conference is `http://www.scienceandnonduality.com/`.) As it turns out, I ended my note to the organizers with these words:

> In all, I think the conference is just not the right forum for me. At Perimeter Institute, I carry a coffee cup that says, "Life is not about finding yourself, but about creating yourself." All the evidence in front of me indicates a conference largely consisting of people who've spent years "finding themselves" rather than ones who've jumped into the stream of life and contributed to making the world "go." In the end, I'd likely be pretty uncomfortable there.

Before meeting Kiki and meeting you, I'm not sure I could have written some of those sentences. The way you live has been an inspiration to me since the beginning: You more than most others I know have certainly "jumped into the stream of life and contributed to making the world go."

Happy 70th birthday Brad! I think now is when life gets really good.

29-05-10 *God Wouldn't Scam, Would He?* (to H. B. Dang & the QBies)

Dangism 4: *I've bought many things on eBay, and this is a principle I've been always followed to avoid scams: if it sounds too good to be true it probably is.* [...]

As Ingemar-Kate pointed out, in $d = 6$, among the 984 normal vectors, we can find sets of 4 vectors whose squared inner products are all 1/3. Moreover, in each such set, those four 6-component vectors span a 2-D subspace, so they indeed form a 2-D SIC. This is very interesting, because one doesn't need to start with a 6-D SIC fiducial. Start from any vector in the Zauner subspace (which we know how to calculate), we will still get the same linear dependency structure and still get 2-D SICs out of it.

The big question is, of course, will this kind of miracle happen in $d = 9$? [...]

In $d = 9$ there are 79767 normal vectors (corresponding to that many hyperplanes), and there are over 3 billion inner products to be checked. Instead of taking a long walk like Ingemar did, I slept, played pool, talked to Chris, etc. while waiting for 20 hours for my desktop and the QBism server to finish the calculation. Indeed, there are pairs of normal vectors that have the right inner product. Those pairs come from 54 normal vectors.

Next small question: among those 54 vectors, can one find set of 9 vectors whose squared inner product between any 2 of them is 1/4? [...]

Last small question: for those set of 9 vectors, do they live in a 3-D subspace?

The answer is yes! So they do form a 3-D SIC!

God? It is *our* efforts and actions that complete nature! You have added to nature, and it has tolerated it. (See one of my favorite William James quotes attached.) [See quote in 05-01-09 note "What I Really Want Out of a Pauli/Fierz-Correspondence Study" to H. C. von Baeyer & D. M. Appleby.]

Mighty good work! This gives me a very happy feeling this morning.

31-05-10 *References* (to N. D. Mermin)

Merminition 218: *I'm refereeing a paper that claims that virtually nobody but Fine and de Munyck dissent from the view that quantum mechanics is nonlocal. Could you send me a few references to published papers by yourself et al. that take issue with nonlocality.*

I'm sorry, I forgot to reply to this one! My "inbox" is an absolute mess. Is this still relevant to you? If so, I'll put a list together a little later in the day. Also, Zeilinger and Brukner and Zukowski emphasize that there is nothing nonlocal about QM. Griffiths and Gell-Mann as well. And I'm sure more names will come to mind if I think harder.

31-05-10 *References, 2* (to N. D. Mermin)

Merminition 219: *Any other names that come to mind (me, for example) should be added to my list.*

With regard to yourself, I'll let you make your own judgements. With regard to me, these three are fairly explicit:

1. C. A. Fuchs and A. Peres, "Quantum Theory Needs No 'Interpretation'," Physics Today **53**(3), 70–71 (2000).

2. C. M. Caves, C. A. Fuchs, and R. Schack, "Subjective Probability and Quantum Certainty," Studies in History and Philosophy of Modern Physics **38**, 255–274 (2007).

3. C. A. Fuchs, "QBism, the Perimeter of Quantum Bayesianism," (2010), `arXiv:1003.5209v1`.

With regard to Brukner and Zukowski, see: `http://arxiv.org/abs/0909.2611`. They tell me they are writing a masterwork on the subject, coming flat out against nonlocality, but I don't see it on the `arXiv` yet. You might contact them directly. Here's an older one by Marek: `http://arxiv.org/abs/quant-ph/0605034`. As I recall, here's one where Duvenhage came out fighting for locality: `http://arxiv.org/abs/quant-ph/0203070`. There were a gazillion things by Peres. Here's one of the later ones: `http://arxiv.org/abs/quant-ph/0310010`. Is that enough to get you started?

It sounds like Griffiths exaggerated, but I concur with the gist of what he was trying to get after. The vast majority of quantum info people, at least, seem to buy spooky action at a distance out of hand.

31-05-10 *Another on the Side of Locality* (to N. D. Mermin)

This one from Itamar Pitowsky: `http://arxiv.org/abs/quant-ph/0510095`.

31-05-10 *And Bill Demopoulos* (to N. D. Mermin)

Also, Bill Demopoulos has a paper coming out that is sympathetic with the side of locality. It's not on the web yet, but I'll cc this note to him and maybe he'll send you the present draft.

31-05-10 *Caves Talk* (to N. D. Mermin)

You might enjoy this talk of Carl's "What are the laws of physics? Resisting reification" at `http://info.phys.unm.edu/∼caves/talks/talks.html`.

31-05-10 *The Gleason-Weyl Thing* (to D. B. L. Baker)

Speaking of bad mental places, sorry I've kept you so long with a reply. This Spring has turned to insanity for me. Some men can't keep their pants on; my problem is I can't stay off a damned airplane. I'll try my best to write you a longer note from Zurich next week; the town is the kind of place that makes me think, and I'll surely be lonely in the soul once again.

I certainly still want to steal away with you, but the demons have me booked up all the way into the Fall. Maybe when (more carefully, if) my position becomes permanent here, I can use that as a good excuse to blow some money on a celebration with you.

When I was in Portland in March, I couldn't keep myself out of Powell's Bookstore. It was the most amazing place I had ever seen. And you want to talk about hippie chicks! First off the town is full of them, but then the concentration at Powell's reaches truly dangerous levels. Nonetheless, I nearly single-mindedly kept my attention on the books: I came home with over $400 worth!

But my best purchase of all from them had to wait until my return home—it came from the internet. When I got home, I asked my three students, "Did you go to Powell's." "Only to the technical book annex a few blocks away from the main store," they responded. I was like, "You losers! You missed the greatest bookstore ever. All the history, art, architecture, philosophy, biography ..." Then I said, "Well, did you buy anything?" They all responded, "Nah." But Matthew, the youngest, said, "Well, I did see an old book on group theory, with a stamp on the first page that said, 'Andrew M. Gleason'." I said, "Which book on group theory?" He said, "An old book by Weyl." I said, "*Theory of Groups and Quantum Mechanics*?" He said, "Yeah, that was it; an old hardcover version of it." I said, "You came across Gleason's copy of Weyl's book and you didn't buy it?!?!" "How much did they want for it?" He said, "$17.95." My jaw dropped. I said, "What on earth were you thinking?" He said, "Well, I didn't know if that was really Gleason's stamp or not; somebody else might have just put it there to sell the book more easily." I said, "Now who would do that?!?! Who on earth besides us even knows who Gleason is?" Well, I got on the internet and found that Powell's had one hardcover copy of the book for $17.95 and bought it immediately. Now, the mingling of Gleason and Weyl's spirits hanging out in my study have been giving me all kinds of inspirations. Report of one such below. (And still further below, John Wheeler's story of how he learned quantum mechanics from Weyl's book when he was 19.) [See 27-04-10 note "Midnight Reading" to H. C. von Baeyer and 24-04-10 note "Weyl's Book and John Wheeler" to the QBies.]

In another note, I'll tell you about who Gleason was and why anyone would care, and I'll record a funny story about the one time I met him.

01-06-10 *SIC POVMs and the Law of Total Probability* (to B. R. La Cour)

I got your note and have read it. Thanks for your interest in these matters. Near the closing, you write,

La Courism 3: *The use of SIC POVM and projection measurements leads to a result which, at first, appears to be at variance with the law of total probability. A more careful examination reveals that the apparent discrepancy is due to ...*

I think, implying that *I* think [sic] that there is some discrepancy between the quantum rule and the law of total probability. But I do not think that. Rather I view the Born Rule as a *supplement* to raw probability theory that allows probability assignments in *one* experiment to be expressed in terms of the probability assignments in another (incompatible) experiment (where there need not have been any a priori connection). Please see Section IV of my paper "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`. Particularly, the part below Eq. (8) where it says,

> But beware: One should not interpret Eq. (8) as invalidating probability theory itself in any way: For the old Law of Total Probability has no jurisdiction in the setting of our diagram, which compares a 'factual' experiment (Path 1) to a 'counterfactual' one (Path 2).

If you'll be in Växjö, we can discuss then. I'm just getting ready for it, by way of Zurich the week before.

01-06-10 *URGENT QCMC Invited Speakers ABSTRACT* (to QCMC 2010)

Attached is the abstract for my talk, both in LaTeX and as a PDF file.

Charting the Shape of Hilbert Space

The space of quantum states for a d-level system is usually thought of as a smooth, featureless place—simply, a linear vector space $\mathcal{H}_d$ over the complex field. But in fact, the space of quantum states corresponds to the convex set of trace-1 positive semi-definite operators *on top of* $\mathcal{H}_d$: This is a body that is anything but smooth and featureless. And in its shape may lie the key to a deeper conceptual understanding of quantum mechanics—at least this is the point of view of the Quantum Bayesian approach to quantum theory developed by Carlton Caves, Rüdiger Schack, the author, and others [Fuchs10].

The reason for this is that Quantum Bayesianism, or QBism, is based on the idea that quantum theory is best understood as an *addition* to probability theory, not as a theory separate from standard probability theory or one including standard probability theory as a special case. It is an addition invoked in a situation where one wants to analyze a given physical experiment in terms of another, never-actualized (or counterfactual) experiment. As such, the first step to finding a deeper understanding quantum theory is to find a manageable representation of quantum states purely in terms of probabilities, without amplitudes or Hilbert-space operators. In this talk, we review the efforts the Perimeter Institute QBism group has made to find such a good representation.

The best candidate so far involves a mysterious entity called a "symmetric informationally complete positive-operator-valued measure," or SIC (pronounced "seek") for short. This is a set of d^2 operators $H_i = \frac{1}{d}\Pi_i$ on $\mathcal{H}_d$, where the $\Pi_i = |\psi_i\rangle\langle\psi_i|$ are rank-one projection operators such that

$$|\langle\psi_i|\psi_j\rangle|^2 = \frac{1}{d+1} \quad \text{whenever} \quad i \neq j\ . \tag{100}$$

We say mysterious because, despite 10 years of growing effort since the definition was first introduced [Zauner99,Caves99] (there are now nearly 50 papers on the subject), no one has been able to show that SICs exist in general finite dimensions. All that is known firmly is that they exist in dimensions 2 through 67 [Scott09]. Dimensions $2-15$, 19, 24, 35, and 48 are known through direct or computer-automated analytic proof; the remaining solutions are known through numerical simulation, satisfying Eq. (100) to within a precision of 10^{-38}.

What is most intriguing about a SIC is that the probabilities $P(H_i)$ for its outcomes uniquely determine the system's quantum state ρ, and they do so through an amazingly simple formula,

$$\rho = \sum_{i=1}^{d^2} \left((d+1)\, P(H_i) - \frac{1}{d} \right) \Pi_i\ . \tag{101}$$

Making us of this formula, one finds a novel way to think of the Born Rule for quantum probabilities. For instance, consider a von Neumann measurement with outcomes $D_j = |j\rangle\langle j|$ (the vectors $|j\rangle$ forming an orthonormal basis), and let $P(D_j|H_i)$ be a conditional probability for finding outcome D_j if the system had been prepared in state Π_i. Then the probability for D_j given by the standard Born Rule

$$Q(D_j) = \mathrm{tr}(\rho D_j) \tag{102}$$

becomes

$$Q(D_j) = (d+1) \sum_{i=1}^{d^2} P(H_i) P(D_j|H_i) - 1\ . \tag{103}$$

Compare this to the expression one would expect from classical probability theory (i.e., the Law of Total Probability),

$$P(D_j) = \sum_{i=1}^{d^2} P(H_i) P(D_j|H_i) \; . \qquad (104)$$

What a tiny modification to the classical law! In fact, the Born Rule seems to be nothing but a kind of Quantum Law of Total Probability.

Recent work at Perimeter Institute has been devoted much to the SIC existence problem, rewriting the problem in Lie algebraic terms [Appleby10], and trying to see how much of the shape of quantum-state space is implied by the very consistency of Eq. (103) [Fuchs09a,Fuchs09b]. Surprisingly, one can glean quite a bit about the structure of quantum states from the requirement that $Q(D_j)$ always be a proper probability distribution. The talk will end with a list of open problems and avenues for further research.

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1. C. A. Fuchs, "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209`, (2010).
2. G. Zauner, *Quantum Designs – Foundations of a Non-Commutative Theory of Designs* (in German), PhD thesis, University of Vienna, 1999.
3. C. M. Caves, "Symmetric Informationally Complete POVMs," University of New Mexico internal report (1999).
4. A. J. Scott and M. Grassl, "SIC-POVMs: A New Computer Study," `arXiv:0910.5784`, (2009).
5. D. M. Appleby, S. T. Flammia, and C. A. Fuchs, "The Lie Algebraic Significance of Symmetric Informationally Complete Measurements," `arXiv:1001.0004`, (2010).
6. C. A. Fuchs and R. Schack, "Quantum-Bayesian Coherence," `arXiv:0906.2187`, (2009).
7. C. A. Fuchs and R. Schack, "A Quantum-Bayesian Route to Quantum-State Space," `arXiv:0912.4252`, (2009).

01-06-10 *Antietam* (to D. B. L. Baker)

Thanks for the lesson. That was great!

It's funny, I know so little about the Civil War myself, but I could still envision a vague sense of the battle. Probably old memories from nearly forgotten movies replaying in my head. But it was real enough at the moment to pull the emotions forth. Things were probably already set in motion by the massive cemetery I drove past in Sharpsburg before getting to the battleground. The weather, dreary and drizzly, was probably conducive as well.

I didn't have much time because I felt I was running late for the airport, but I also stopped at an oak grove in Sharpsburg where Robert E. Lee had camped (for three days I think). It was a beautiful setting—big, massive trees. Very peaceful. It goes to show you the depth of things hidden in the calm airs around us.

01-06-10 *Flattery Will Get You Everywhere* (to J. Ismael)

You flatter me (but I bet you say such things to all the boys). In any case, it worked! I printed out your papers and will be carrying them to Europe with me. I hope to get back to you soon with my thoughts on them.

I too enjoyed our conversation. I feel like I'm learning a lot on Weyl, Ramsey, and the embedded perspective.

Excerpt from the Draft "Volition, Time, and Becoming" Jenann Had Sent

> What makes the world itself appear to be in the process of becoming as it is experienced (rather than simply the otherwise natural idea that we simply have a changing appearance of a fixed reality) is that we observe the results of our own actions. The world partakes in the unsettled, unresolved, remaining to be decided character of volition because what I freely decide to do changes the future course of experience. If we just were just observers of history, there would be no reason that becoming wouldn't be a feature of our experience of time, rather than a feature of time itself. It's the discovery that what happens depends on my will, that makes time itself partake in the contingency of my present and future willings. I can no more regard time itself as a static dimension of which I have a varying appearance than I can regard my own volition that way. Of course there is a transcendent perspective from which both my decision processes and their downstream consequences are part of the fixed, eternal manifold of events. But from any given temporally embedded perspective with a life, as it is being lived, there are any number of futures, any one of which might yet be chosen.
>
> In sum, then, our conception of time differs crucially from our conception of space in that we don't think of ourselves as simply 'viewing' different parts of time in temporal sequence as we think of ourselves as viewing different parts of space. We think of the future as existing only in potential until experienced because the future depends on the movements of my will, and the movements of my will are up for grabs, from my own perspective, up until the very moment of choice.

03-06-10 *Traunkirchen Again* (to the QBies)

On the other workshop, I'm fine with that for anyone who wants to. I myself had to turn down their invitation since I'm going to be in Australia at another meeting. It doesn't look quite so exciting as the second meeting, but it'd be interesting to see how people react to Richard Healey's talk, since he's turning partial QBist. It would give you more time in the Alps to think, contemplate, calculate, get inspired, eat schnitzel, whatever. (The urungleichung, after all, was born in the Alps.)

03-06-10 *Padmanabhan* (to L. Smolin)

I found the talk by your friend yesterday extremely exciting. (And much better/plausible than Verlinde's.) It is hard not to feel that there is something really deep in pursuing this direction.

When he talked about observer-dependent entropy, I wanted to say, "Entropy is *always* observer dependent in a Bayesian understanding of statistical mechanics." I.e., the kind of Ed Jaynes thermodynamics/stat-mech that Caves and I have been promoting all these years. But I was too shy: It was too trivial a point in comparison to the other deep ideas I was hearing.

I feel though that there is something wrong about the "micro-state counting" part of Padmanabhan's picture. What came through loud and clear to me instead yesterday is that an "accelerating observer" is an "agent taking an action upon space" (like in the picture on my office door—it is just that the cube is replaced with empty space). To accelerate is to initiate a quantum measurement (i.e., taking an action on the external world and suffering its consequence).

Anyway, huge foods of thought here!

03-06-10 *Padmanabhan, 2* (to L. Smolin)

Thanks; that was fun! Who could ever say that the standard quantum foundational issues aren't absolutely crucial for issues to do with gravity. The Rindler observer *is* Wigner's friend!

03-06-10 *Gravity and Wigner* (to L. Hardy)

By the way, yesterday you missed one of the best talks I've ever heard coming through the quantum gravity group here. You should watch it as you get a chance: `http://pirsa.org/10060000/`.

The reason I think I'm so excited is that I saw more clearly than previously that one should think of an accelerating observer as one who is taking an *action* (quantum measurement) on empty space (the quantum system). It starts to fit QBist terms for me now, and that always makes me feel happy.

03-06-10 *Elementary Question* (to W. G. Unruh)

I have a probably stupid question, but maybe you have an elementary explanation. As I understand it, a uniformly accelerated observer will see black-body radiation in a case where an inertial observer will see none. People say this arises from the existence of a horizon, blah, blah, blah. But what of the case of an observer standing on the surface of the earth? In the idealized case of a free-falling observer seeing *no* blackbody radiation, would the stationary observer on the surface see some? There's no horizons here, are there? Yet, what I understand of the equivalence principle, it would seem that the fellow stuck on the surface of the earth should see the same thing as the fellow in the elevator.

Confused in Waterloo,

03-06-10 *A (really, really) Recommended Talk* (to G. L. Comer)

Which email address should I use for you now? I'm confused; I didn't see your university one come up in my address book.

Anyway, there is a link to what I thought was a fantastic talk below. And the reasons why I thought it fantastic too. [See notes to L. Hardy and L. Smolin from earlier in the day.] Still one more conceptual link: I saw Bill Unruh give a talk on (your old) dumb holes last month in DC. Afterward I made a comment to him that when two things look too much like each other, one might should stop calling one the analogy of the other. For instance, one should stop thinking of the numerical identification between inertial and gravitational mass as accidental, but rather the consequence of a deep principle. I said, "Maybe you should be looking for your own equivalence principle." He joked that he asks his students, "What do elephants and steam ships have in common?" and then replies, “They both carry trunks. But that doesn’t make them the same thing, does it? Well, it’s amazing how often it works in physics to act as if they are.”

But I think I’m not joking. I think the principle here is that acceleration (through any medium) enacts a quantum measurement upon it. It is an action upon it.

05-06-10 *A Question on Informational Gravitation* (to H. Poirier)

I definitely remember you. And I definitely feel a resonance between Verlinde’s latest turn and my QBism program. See, for instance, this paper of mine: `http://arxiv.org/abs/1003.5209`. See the end of Section VI on page 25; I cite Verlinde’s paper there. Particularly, I am intrigued with the way he treats spacetime as having emerged “outside of the boundary”, while inside the boundary there is not yet such a thing as spacetime.

It is all very early in this stuff still, though. What he seems to have is really more of “an idea for an idea” (as John Wheeler would say) than any truly solid idea, much less an actual “derivation” of Newton’s laws, etc., as he claims in the paper.

Have a look at the papers by this guy, Padmanabhan. For instance,

- `http://arxiv.org/abs/0911.5004`
- `http://arxiv.org/abs/0911.1403`
- `http://arxiv.org/abs/0910.0839`

They support a similar point of view, but I think are much more solid.

I fly to Zurich tomorrow morning, and so may be delayed in my emailing for a day or two, but if you have any further questions, let me know.

05-06-10 *The Consequences of Tommy* (to the QBies)

Saturday morning philosophizing. Thinking about how the urgleichung fits in with Unruh radiation and the principle of equivalence. Listening to The Who’s album *Tommy*. And at this very moment, it’s going by the words:

> Right behind you I see the millions.
> On you I see the glory.
> From you I get opinions.
> From you I get the story.

I like that line, “Right behind you I see the millions.” For I do: It’s one of those days when I feel like we’re doing really very big things. Small, small steps, all on the path toward something really big. And from you guys, “I get the story.”

Let me recommend this interview with John Wheeler for your own Saturday philosophizing: `http://www.bigear.org/vol1no4/wheeler.htm`. Among other things, I think it characterizes well, the role you guys play in my life, or at least it’s what I’d like to strive for:

> **Interviewer:** Do you have some thoughts about educating students?
>
> **Wheeler:** Shouldn’t you rephrase your question? After all, I’m sure that it is really the students who educate me! We all know that the real reason universities have students is to educate the professors. But, in order to be educated by the students,

> one has to put good questions to them. You try out your questions on the students. If there are questions that the students get interested in, then they start to tell you new things and keep you asking more new questions. Pretty soon you have learned a great deal.

Thought of the day in the attachment:

> It is difficult to escape asking a challenging question. Is the entirety of existence, rather than being built on particles or fields of force or multidimensional geometry, built upon billions upon billions of elementary quantum phenomena, those elementary acts of "observer-participancy," those most ethereal of all the entities that have been forced upon us by the progress of science?
>
> — John Archibald Wheeler

07-06-10 *Time Flies* (to J. D. Norton)

Greetings from Zurich, where I happened to bring a few John Norton papers with me (most on historical matters in GR). The physicist in me thinks that before using the words "quantum" and "gravity" in the same sentence, we should take all that we've learned about quantum mechanics and go back to 1907.

Anyway, not quite related to that project, but because I was intrigued, I read your "Time Really Passes" and I enjoyed it very much. I'm glad you've now joined those who believe time flies. For your enjoyment a little story about John Wheeler below drawn from my email collection (it was originally a letter to Greg Comer).

From a 17 December 1997 note to Greg Comer, "It's a Wonderful Life"

> Good holidays to you. This morning, as I was driving to work, it dawned on me that roughly this day 10 years ago, I was conferred my degrees at the University of Texas. Time does fly.
>
> It made me think of a little anecdote about John Wheeler that I heard from John Preskill a few days ago. In 1972 he had Wheeler for his freshman classical mechanics course at Princeton. One day Wheeler had each student write all the equations of physics s/he knew on a single sheet of paper. He gathered the papers up and placed them all side-by-side on the stage at the front of the classroom. Finally, he looked out at the students and said, "These pages likely contain all the fundamental equations we know of physics. They encapsulate all that's known of the world." Then he looked at the papers and said, "Now fly!" Nothing happened. He looked out at the audience, then at the papers, raised his hands high, and commanded, "Fly!" Everyone was silent, thinking this guy had gone off his rocker. Wheeler said, "You see, these equations can't fly. But our universe flies. We're still missing the single, simple ingredient that makes it all fly."

07-06-10 *Time Flies, 2* (to J. D. Norton)

Nortonism 2: *Nice to hear from you and thanks for the Wheeler story. There have been just a few extraordinarily people like Wheeler, full of ideas and able to enthuse students.*

I expect to be excommunicated from the Church of Philosophers of Physics!

Well, in my eyes, that represents genuine progress. Here's the way William James once put it with regard to German philosophy:

> In a subject like philosophy it is really fatal to lose connexion with the open air of human nature, and to think in terms of shop-tradition only. In Germany the forms are so professionalized that anybody who has gained a teaching chair and written a book, however distorted and eccentric, has the legal right to figure forever in the history of the subject like a fly in amber. All later comers have the duty of quoting him and measuring their opinions with his opinion. Such are the rules of the professorial game — they think and write from each other and for each other and at each other exclusively. With this exclusion of the open air all true perspective gets lost, extremes and oddities count as much as sanities, and command the same attention ...

And I must say, I find myself thinking that of the Philosophy of Physics a lot. Extremes and oddities sit side by side with sanities, and it is all tolerated in the forum. Your essay is an example that makes it clear—why should you fear that you'll be excommunicated but that the subject has gone so very wrong. Don't get me wrong, I think I am personally deeply philosophical, and it is a very worthy business, but most of my influences have not come from the clique of philosophy that calls itself Philosophy of Physics.

Thinking of the ending remarks in your essay, it strikes me that the time is right in your life to rethink James's *Principles of Psychology*, particularly the bit on the perception of time, if you haven't read it recently. If nothing else, I'll guarantee that the writing will captivate you.

All the best (and I wish you were here for dinner time discussions as I make a bit of progress in your old papers).

09-06-10 *Appleby's Mud Hut* (to R. W. Spekkens)

With regard to the conversation we just had, here are some excerpts from a ridiculously long email from Marcus to Howard and me:

Applebyism 40: *The usual attitude, which not only drives the Bohmians and Everettians, but is also dominant among physicists generally, is to regard ordinary language/common-sense as a kind of medieval mud-hut. A way of thinking which was useful in the past, but whose day is now gone. Survivals from the past often induce reverence. One seeks to preserve them. And indeed that is the attitude of archaeologists to actual medieval huts found in the course of excavation. However, there are limits. No archaeologist would want to live in a medieval mud-hut, all dank and dark and smelly (except perhaps for a week or two, just to see what it is like). Similarly with common-sense, in the minds of most physicists. It may have a place in the museum. But so far as serious thinking is concerned the only thing to do is tear it down and replace it with some modernistic structure, all glass and polished steel.*

One of the difficulties with this programme is that a building has to rest on something. Actual modernistic buildings rest on the same wormy, fungus and bacteria-impregnated earth that the medievals used to build their mud-huts on. With the modernistic structures we build in our minds it is even worse: they don't simply rest on the same soil, they actually rest on the mud hut itself, entire and unreconstructed. Despise the mud-hut at the bottom of the whole thing, as contemporary physicists almost all do, and you are despising the foundations on which everything else depends.

[...]

The dominant tendency among physicists is to identify physics with the developed mathematical theory. With the equations, in other words. But if you were only allowed to write equations, and *nothing else at all, then I doubt that it would be possible even to get started on the problem of explaining (in response to a question from an undergraduate, for example) just what is so puzzling about quantum mechanics. To get puzzled by quantum mechanics in the first place you have to draw on ideas which reach all the way down to that poor old primitive mud-hut. Dank and dark and smelly it may be. But there is no getting away from it. Trying to get away from it: this would be like a tree trying to pull itself up by the roots. The attempt is unlikely to succeed. But if it did it could only end in disaster.*

Moreover, I don't think it is only quantum foundations which has this dependency on common-sense and ordinary language. I think the same is true of every other area of physical thinking, without exception. Walk along the corridors of any physics department and, though you will certainly see people writing on chalkboards, you will also see them talking and writing, using ordinary words. Moreover if you take the trouble to inspect what has been written on the chalkboards, though you will doubtless see equations, you are likely also to see simple-minded drawings, assimilable at the level of mud-hut thinking. Again, any half-way decent talk includes an attempt to provide background, and intuitive motivation: and those are things which simply can't be done just by writing equations. It takes words. Ordinary words.

12-06-10 *Pauli, Four, and Me* (to R. Renner)

Thanks again for a wonderful, thought-provoking conference. Schack and I are soon to head out to explore the city and the hills nearby.

I wanted to show you one thing that Heisenberg wrote on Pauli in his essay "Wolfgang Pauli's Philosophical Outlook":

> In the alchemistic view "there dwells in matter a spirit awaiting release. The alchemist in his laboratory is constantly involved in nature's course, in such wise that the real or supposed chemical reactions in the retort are mystically identified with the psychic processes in himself, and are called by the same names. The release of the substance by the man who transmutes it, which culminates in the production of the philosopher's stone, is seen by the alchemist, in light of the mystical correspondence of macrocosmos and microcosmos, as identical with the saving transformation of the man by the work, which succeeds only 'Deo concedente.' " The governing symbol for this magical view of nature is the quaternary number, the so-called "tetractys" of the Pythagoreans, which is put together out of two polarities.

If you compare that to what I wrote for the back cover of the 10$^{\text{th}}$ anniversary edition of Nielsen and Chuang:

> Nearly every child who has read Harry Potter believes that if you just say the right thing or do the right thing, you can coerce matter to do something fantastic. But what adult would believe it? Until quantum computation and quantum information came along in the early 1990s, nearly none. The quantum computer is the Philosopher's Stone of our century, and Nielsen and Chuang is our basic book of incantations. Ten years have passed since its publication, and it is as basic to the field as it ever was. Matter will do wonderful things if asked to, but we must first understand its language. No book written since (there was no before) does the job of teaching the language of quantum theory's possibilities like Nielsen and Chuang's.

you might see why I have a fascination with Pauli! (I have a big portrait of him on my wall at PI.)

Anyway, thanks again. Schack and I have done some good science in this city. It seems to bring out the best in people.

14-06-10 *Displacements All the Way Down* (to A. Plotnitsky)

Here is this paper on Weyl that has interested me greatly. [Erhard Scholz, "Weyl Entering the 'New' Quantum Mechanics Discourse," conference contribution to *History of Quantum Physics 1*, Berlin, July 2–6, 2007.] The main reason is that my beloved SICs seem to be the dual structure to Weyl's "phase space." And that phase space goes all the way back to at least 27 September 1925!

I'd like to look up the dates (or narrow the range) of when Heisenberg and, particularly, Born found the unitial commutation relations.

Best wishes from just behind you ...

15-06-10 *Human Observations* (to R. Schack)

BTW, did you notice how the guy from the Austrian Academy of Sciences seemed to insist on invading the space between you and me? What was up with that? When I would rotate toward you, he would reinsert himself between us, only looking toward me, keeping the back of his head more toward you.

Very strange.

15-06-10 *How To Make SICs Exist* (to J.-Å Larsson)

Attached is my spiel on willing SICs into existence. It starts at the heading titled "Conceptual Barrier!" [See 13-03-10 note "Conceptual Barrier!" to the QBies.] (You'll see that I'm not crazy ... just very close to being so.)

15-06-10 *Gravity as Thermodynamics* (to F. De Martini)

Following on from our conversation on the bus this morning: The guy I was telling you about is Padmanabhan. Here a couple of links to his papers:

- `http://arxiv.org/abs/0911.5004`
- `http://arxiv.org/abs/0910.0839`.

And here is a link to a video of the talk he gave us at PI:

- `http://pirsa.org/10060000/`.

It'll be good to talk more when you're at PI.

21-06-10 *QBist Interference* (to J. I. Rosado)

Rosado-ism 3: *I read with great interest your paper "QBism, the Perimeter of Quantum Bayesianism". I think the ideas exposed in this paper not only are useful to find a QBist explanation of the Born rule but also to explain the fundamental concept of quantum interference.*

Thanks for your continued interest during all these years. I'm glad you are working out some of these ideas yourself. I make a bit of a more thorough analogy to interference in `http://arxiv.org/abs/0906.2187`. You might have a look at that paper if you have some interest.

24-06-10 *Idea This Morning* (to R. Renner)

This morning I was thinking about a proposal I'll be writing to the Templeton Foundation in a month or so, and I had an idea that might involve you if you have any interest. Attached is the pre-proposal I had written for them earlier, and they have given me the go ahead for writing a full proposal. I think my chances for obtaining the funding are quite good, given their mission and my subject matter ... [yeah, right][226].

If you read through this thing, you'll note a certain emphasis on Wolfgang Pauli's thoughts being brought into the modern context. And particularly, in Sections 3 and 6 you'll find mentions of a workshop on Paulian ideas. The tentative title for the meeting is, "A Malleable World? Wolfgang Pauli's Dreams for Physics in the Light of Quantum Information." It doesn't say so in the proposal yet, but I've been thinking I'd like to have that meeting in Switzerland, perhaps in a place like the small village where Harald Atmanspacher lives, Amden. (Beautiful setting.) The point is to have an inspiring setting, and I'll make the argument we'll more easily get some Pauli scholarly work going in Switzerland than anywhere else. I.e., that this is the natural place for such a meeting.

The idea I had this morning is this: Might the Pauli Center have any interest in being involved in sponsoring such a workshop? Or at least lending its name to the proposal, even if no dollars? As I say, I think my chances of getting this funding are already fairly good, but it never hurts to get as much legitimacy as possible before pursuing these things. And who knows, maybe the Pauli Center would even get something out of my efforts in return.

Just an idea. I'd welcome your thoughts pro and con and even sideways.

26-06-10 *We're in the Top Ten!* (to the QBies)

Have a look at this old blog entry of Scott Aaronson's:

`http://scottaaronson.com/blog/?p=112`

Particularly, look at the rules in Number 10. It was fun to learn that already in 2006 someone voted our research subject one of the "ten most annoying questions in quantum computing."

26-06-10 *Things to Read* (to P. Wells)

Here are a couple of the blogs I was telling you about, by Perimeter-style thinkers, even if some are not presently at Perimeter:

- Scott Aaronson's "Shtetl Optimized":

  `http://www.scottaaronson.com/blog/`.

  Scott had been a postdoc with us some years ago; now he's a professor at MIT. (Funny too: I was pleased to learn in flipping through it a few minutes ago, as I retrieved the address for you, that my group's main research problem was voted one of the "ten most annoying questions in quantum computing." Quite an honor!)

[226]Not in original note; addition for the purposes of this samizdat.

- Michael Nielsen's blog:

  `http://michaelnielsen.org/blog/`.

  Michael wrote the major textbook in quantum computing, was a professor at U. Queensland, and finally on faculty at Perimeter until he decided to drop out of physics entirely.

- John Baez's "This Week's Finds in Mathematical Physics" can be found here:

  `http://math.ucr.edu/home/baez/`.

  John is a cousin of Joan Baez the singer, as I was telling you, and apparently is just about to make a career change as Nielsen did. (Unrelated, but did you know that Olivia Newton-John is a granddaughter of the great physicist Max Born, one of the founders of quantum mechanics? I learned recently that he had his beautiful equation

  $$pq - qp = \frac{h}{2\pi i}$$

  carved into his gravestone.)

- Sabine Hossenfelder's BackReaction:

  `http://backreaction.blogspot.com/`.

  Sabine had been a postdoc with us; she works in quantum gravity.

- Finally, let me mention Howard Barnum's "Wine, Physics, and Song":

  `http://winephysicssong.com/`.

  Howard is a visiting researcher with us, on leave from Los Alamos National Lab, and working in the quantum foundations group. He hasn't written on his blog in a while, but some of the entries in it on wine at the Black Hole Bistro are real gems. What amazes me in reading some of these entries, is that I can well remember being with him at the moments the entries describe . . . and to all outward appearances we were having technical discussions about details of quantum mechanics. But in the back of his mind, he was clearly writing up his tasting notes!

I myself am not a blogger, but I do fancy myself something of a writer. In prep for our discussions next week, I might point you to Sections 1 and 6 of this paper:

`http://arxiv.org/abs/1003.5209`

and Section 1 of this one:

`http://arxiv.org/abs/quant-ph/0205039`.

I think they're written in a way that a general reader can get the point from, and they tell the story of why I think quantum foundations is important.

Also, let me attach the intro to my Cambridge University Press book: It tells the story of why I'm in physics in the first place (not for the search of truth and beauty, but for science fiction). You might find it a bit entertaining.

30-06-10 *Two Pictures from Copenhagen* (to the QBies)

The Niels Bohr Institute, one innocent day in June, 2010—what was happening on the outside, and what was happening on the inside. You'll find the eight new isms I think QBism is pointing us to:

1. interiorism
2. agentialism
3. meliorism
4. jazzism
5. nonreductionism
6. empiricism / "the world is not sentence shaped"-ism
7. additivism
8. experientialism

02-07-10 *Version 6, Reply 1* (to H. C. von Baeyer)

OK, organize my thoughts, I need to do. (Spoken like Yoda in my head.) It's not easy!

I liked your lede (except for the little-bit distracting parenthetical detail on Einstein vs Bohr). And I wondered what came next! To make the transition from the (ancient) wave vs particle to the (modern) probability vs click is probably a good way to go for Sci Am readers.

A long time ago when I first thought about writing an article for them, I thought about starting off with something like this:

> In the holiday movie classic *It's a Wonderful Life*, the protagonist George Bailey proclaims in a moment of anguish, "I suppose it'd have been better if I'd never been born at all." It was the very idea his guardian angel Clarence needed to save his soul. "You got your wish. You've never been born." The story develops with George seeing how disturbingly different the world would have been without his presence. As Clarence told it, "You've been given a great gift, George—a chance to see what the world would be like without you." George came to realize how integral his life and his actions were for the very shape of the world around him. There was great wisdom in that movie: For in the 85 years since the advent of quantum theory, we have started to learn that this may be just be its greatest lesson. That our actions and their consequences are so integral to what the theory is about, to try to imagine what the theory is saying about a world without us simply cannot be done.

This certainly was not the vision of science before quantum theory, and is very much the source of why quantum theory often seems to mysterious even today. Science, and certainly physics, was once thought to be *exactly* about what the world is like without *us* playing any active role in its course. A whole cottage industry has developed . . .

Anyway, that's old stuff. As I wrote you at the start of this project, "Trouble is, I don't think I know how to write something that is *not* a manifesto—it is a character flaw." And that remains true. I think you're probably much more right on your track. After getting back from dropping the kids off at the Toronto airport tomorrow morning, I plan to put much more quality time into your old outline and give you some proper feedback, and also to try to figure out what my role can be here—i.e., how I can help you, while letting your seasoned science-writing instinct reign.

Kids off to Texas tomorrow! And as I say, I proper letter back to you tomorrow.

04-07-10 *Idea This Morning, 2* (to R. Renner)

Thank you for your thoughtful message. Maybe it is best not to push the issue too far with the Pauli Center.

Still!, given what you have written, you make my mind turn: I start to wonder whether it might be useful to consider eventually writing a Pauli-Center-specific proposal. Maybe for a follow-on conference for the year after the Templeton one (if that proposal flies). I can easily imagine a theme that a) would be politically safer than the Templeton stuff, but b) still in line with what I think is most worth developing in quantum foundations. For instance, a meeting with a title like "Quantum Contextuality: From Wolfgang Pauli to Kochen-Specker and Beyond." Maybe you'd have some interest even? It could be partially historical, partially philosophical, and partially technical. I write all the time that I think quantum mechanics is just the beginning of something much bigger; the workshop could be on that—giving theorists with good technical skills some better perspective (historical and philosophical) on why they should be pursuing the mathematics that they are pursuing.

Just the "idea this evening"! Let it roll around in your head a bit and see if we might do anything with it in the next year. I, of course, would do most of the legwork if you have any interest. And if you don't have any interest, that is OK too: As always, I'm just trying to see what I can help make happen.

06-07-10 *Comments* (to C. Ferrie)

Ferrie-ism 10: *Are you still around the IQC? If not, this is just an e-mail to remind myself to follow up on your question in my talk.*

Sorry, I'm gone already. Here's the kind of thing I was thinking—please allow me to use a SIC representation of quantum states to get the problem started. Suppose I have a measurement whose clicks are labeled j. Then the quantum probability $q(j)$ given by the Born rule can be written in terms of the SIC basis according to:

$$q(j) = \sum_i \left[(d+1)p(i) - \frac{1}{d}\right] r(j|i)$$

where $p(i)$ is the probability distribution for SIC outcomes and $r(j|i)$ is the probability for a j outcome given that the preparation was the i SIC vector.

In this context, I would call the quantum state corresponding to $p(i)$ "classical" in the case that

$$(d+1)p(i) - \frac{1}{d} \geq 0.$$

for all i. The reason I would call it that is because then $q(j)$ is calculated according to the classic Law of Total Probability for any measurement at all.

This defines a convex set of the $p(i)$. In this case, a regular simplex.

Anyway, I was thinking the "classical states" must always correspond to an affine transformation of that basic set.

07-07-10 *Leipzig* (to Å. Ericsson)

I'm thinking harder of cleaning my schedule of the Leipzig meeting, and you did a fantastic job with your talk in Växjö. I.e., I trust you now. I thought about giving a talk titled, "Probably the Best Statistical Manifold for Quantum Theory," as a play on the Carlsberg beer advertisement, but if you could give a talk expressing the same idea, even if not the same title!, this might be something to consider. Here is a link to the conference venue:

> `http://www.mis.mpg.de/calendar/conferences/2010/infgeo/`
>
> and
>
> `http://www.mis.mpg.de/calendar/conferences/2010/infgeo/program.html`.

Please let me know your thoughts. Is it something you have any interest in going to? Would you "strive to" do an effective sales job on our program while there (not only during your talk, but lunches and dinners and coffee breaks … a bit like I would do)?

11-07-10 *Marley Lyrics of the Morning* (to Å. Ericsson)

Lyrics to "Put It On" by Bob Marley:

> Feel them spirit
> Feel them spirit
> Feel them spirit
> Lord, I thank you
> Lord, I thank you
>
> Feel alright now
> Feel alright now
> Feel alright now
> Lord, I thank you
> Lord, I thank you
>
> I'm gonna put it on, I put it on already
> I'm gonna put it on, and it was steady
> I'm gonna put it on, put it on again
> Good Lord, help me
> Good Lord, help me
>
> I'm not boastin'
> I'm not boastin'
> I'm not boastin'
> Feel like toastin'
> Feel like toastin'
>
> I rule my destiny, yeah
> I rule my destiny
> I rule my destiny
> Lord, I thank you, yeah
> Lord, I thank you

No more cryin'
No more cryin'
No more cryin'
Good Lord, hear me
Good Lord ...

I really like that line about ruling my destiny.

12-07-10 *Dentist Office* (to P. Wells)

It was part of this:

> For my own part, I imagine the world as a seething orgy of creation ... There is no one way the world is because the world is still in creation, still being hammered out. It is still in birth and always will be ...

I think the dictionary only used the middle sentence of that.

12-07-10 *Interpretation* (to Å. Ericsson)

After you left, I had a read through some of the discussion on that Marley song.
One person wrote:

> *his voice sounds "otherworldly" here*

Another one wrote:

> *I know exactly what you mean. I have felt a spiritual presence while listening to his music. It's as if when he sang he became a vessel for something beyond even his own comprehension. This is probably my fav song of his. It's so bare.*

And earlier, still another had written:

> *1 year ago 2 I had just left a graveyard after visiting an old beau's headstone with his mom. I had this song on in the car and felt my seat belt constrict for no apparent reason. The song completely spoke to me and that moment and I realized so much about my relationship with God and my old boyfriend. "Put on the spirit of Christ." The reason I love Bob and reggae! "Good Lord, thank you!"*

12-07-10 *Interpretation, 2* (to Å. Ericsson)

My own interpretation is something like the last one. By "feeling them spirit" and "putting on (something like) Christ" (i.e., like putting on clothing), he could rule his destiny. And like the other commentator, one of my own attractions to the song is how bare it is. I think it is basically a prayer.

12-07-10 *Interpretation, 3* (to Å. Ericsson)

"put it on" = take, receive, try out. "I'm going to wear it." "I'm going to make it part of me."

12-07-10 *Interpretation, 4* (to Å. Ericsson)

Here, actually listen to this version: `http://www.youtube.com/watch?v=McJu3cRcjeo`. (It is slower than some other versions.) Definitely has the sense of a chant or a prayer. I think he was asking for strength and received it.

12-07-10 *QBism, Installments 1, 2, and 3* (to H. C. von Baeyer)

I like your introduction even better now.

I'm not sure how I should tackle giving you feedback. Perhaps recording my "stream of thought" might be the most efficient way to go forward at the moment. At present, I'm finding it extremely difficult to *organize* any thoughts.

1) As you know, I love it when I learn something just from the very act of writing. And I love it too when I learn something from my co-author's very act of writing! These lines were quite thought provoking for me: "The resolution of the incongruity between waviness and graininess was effected by a compromise. Quantum mechanics avoids the description of an electron as a particle or a wave. Instead, it introduces the new, somewhat nebulous notion of the quantum state." I think that is a really good way to put it, and I don't think I've ever quite thought in terms of a compromise before.

2) Whenever/wherever possible, might we use "a quantum state" rather than "the quantum state"? Remember these lines from my introduction of *Coming of Age*:

> Better writing, not just any writing, made for better understanding. ... I started to realize that the major part of the problem of our understanding quantum mechanics had come from bad choices of English and German words (maybe Danish too?) for various things and tasks in the theory. Once these bad choices got locked into place, they took on lives of their own. With little exaggeration, I might say that badly calibrated linguistics is the predominant reason for quantum foundations continuing to exist as a field of research. Measurement? I agree with John Bell—it is a horrible word and should be banished from quantum mechanics. But it is not because it is "unprofessionally vague and ambiguous" as Bell said of it. It is because it conveys the *wrong* image for what is being spoken of. *The* quantum state? That one is just about as bad. Who would have thought that so much mischief could be made by the use of a definite article?

3) I know I've got to fight my manifesto-izing instincts, but I wonder if we couldn't come off more strongly in the closing paragraph of the introduction? I think it would be nice to give the reader a sense that something thunderous is happening in our understanding of the quantum before he plunges into the rest of the article. Somehow I'd like to send a big rush of enthusiasm. Also, I think it'd be good to take a swipe there at the science-fiction solutions to the quantum conundrum the regular readers of that magazine have surely seen. I'm thinking particularly of parallel universes and action-at-distance. Remember, Musser had written this to me just before I thought of proposing the project to you:

> We'd need a more straightforward account: here are the mysteries of quantum mechanics; HERE ARE THE USUAL RESPONSES; here is why those responses fail; here is a better way. I suspect that such a reader will actually be more receptive to the notion that quantum states represent a form of gambling odds than a physicist or philosopher steeped in the subject is; we have less baggage to shed. But we do share the desire

> to know ultimately what the information is about and, even if this question cannot be answered, will want to know how the Bayesian approach advances that goal.

I all-capped the part I was thinking of particularly. I wouldn't want to dwell on those other "responses," but I think I would like to say something to the effect, "those sensationalistic responses are all well and good for a Hollywood movie, but in the end they're not very imaginative (like a Hollywood movie) and don't come close to expressing how really cool/awesome/nifty/exciting/unusual the quantum world really is." The world is wired in such a way that our actions matter; and that is certainly not the world of Banville's left-hand group.

4) I'm OK with 1/2 of Banville's distinction, but I'm not sure his right-hand group is the right way to put where I sit personally. ... [MORE TO COME]

I'll come back to that point in minute. In the meanwhile, let me send this much to you: I'm not sure when you go to bed, and I want to make sure you have *something* from me today before you close your eyes.

Picking back up ...

4) I'm OK with 1/2 of Banville's distinction, but I'm not sure his right-hand group is the proper way to identify where I (or QBism) sit(s) in the spectrum. To pinpoint it, maybe I can say this: I'm not so sure I like to think that a physical theory imposes *order* on an unruly world, so much as it imposes our will (to the extent that it can presently be imposed). Remember the description I gave of William James's view in the Templeton pre-proposal:

> Of scientific theories, the philosopher William James once wrote, "You cannot look on any such [theory] as closing your quest. You must set it at work within the stream of your experience. It appears less as a solution than as a program for more work, and more particularly as an indication of the ways in which existing realities may be changed. Theories thus become instruments, not answers to enigmas. We move forward, and, on occasion, make nature over again by their aid." On this conception, a theory is not a statement about what the world is, but a tool, like a hammer, to aid in making the world what we want it to be. The world may resist, but to some extent it is malleable.

I'm not saying not to work with Banville's distinction or use it as a hook, I'm just pointing the difference of flavor in my own thought.

5) I did however like Banville's phrase (and your use of it) of a "boiling chaos". It's a bit similar to Chris Timpson's description of my view:

> The four-dimensional pattern is too unruly: it does not admit of any parsing into laws, or even weaker forms of generalisation, not even statistical ones.
>
> This, then, is the micro-level we have dubbed unspeakable; to which we are denied direct descriptive access. The picture is of a roiling mess. Fuchs adds:
>
> > For my own part, I imagine the world as a seething orgy of creation... There is no one way the world is because the world is still in creation, still being hammered out. It is still in birth and always will be... (To Sudbery-Barnum 18.8.03)

Part of that quote of me, by the way, made it into the *Oxford Dictionary of American Quotations*. (Surely I've told you this before, but my memory gets worse and worse.)

6) "Mathematically it is described ..." In line with what I said previously, "the function" $\longrightarrow$ "a function"? "can usually not be visualized" $\longrightarrow$ "can almost never be visualized"

7) "Max Born" Worth parenthetically mentioning (grandfather of the pop singer Olivia Newton-John)? Probably not, but I thought I'd throw it in for consideration.

8) “psi should be interpreted in terms of a probability” $\longrightarrow$ “psi is a tool that can be used to calculate probabilities”? The reason I bring this up is because (part of) QBism's contribution has been to emphasize that it is “absolutely nothing more” than probabilities; it has no existence beyond them.

9) “negative or even imaginary” $\longrightarrow$ “negative or even an imaginary number”?

Since I just got a response from you, I'll chop this much off now and send it. Back later with stuff on section 2 and beyond.

Picking up again with the random stream of thought (this time more random and vague than previously).

10) “Atom of Information,” let me just play around that phrase, or use it as a focus, for a while. A journalist from *Science et Vie* this morning wrote me, “what is for you the meaning of information (in a physics sense) — a physical substance?” To which I plan to reply, “God no!” One of the key things I want to stress with him, and I guess with you, is that when Shannon quantified information, he quantified it in terms of “surprise.” To receive a “bit of information” is to say one will be completely surprised by the answer to the relevant question. (Or, in the quantum case, the consequence of the relevant action.) If I write down a 0, as opposed to a 1, on a piece of paper, and then announce to you that I wrote down a 0, when I finally give you the piece of paper you will not receive any bits at all: The paper gives no information because it gives no surprise. Somehow I'd like to see that crucial point worked into the story: Information (in a Bayesian understanding) is not a new fluid that we pour into receptacles.

Still, “atom of information.” It is a phrase I rather like. Particularly because it helped me think of qubit as an “atom of surprise.” No matter how I take an action on it, it always has the ability to surprise me with the response I receive. And that is a crucial piece of QBism.

And maybe too emphasizing that “bit” quantifies “surprise” rather than “substance” or “fluid content” can help make the transition to what Bayesian probability is all about, “disciplined uncertainty accounting.”

11) Actually I wouldn't mind having a small box, not particularly on SICs, but on the urgleichung. A little visual on the sky vs ground measurements, and the simple rule that connects them. It is a statement that one can do without quantum states completely. Instead the Born rule is really about “taking probabilities as inputs and returning probabilities as outputs.” Does it go too far for *Sci Am* to actually show one equation? Kind of in the way that I did in the attached?

But the way, the SIC was brought out of the sky and placed a little closer to the ground a couple weeks ago. See: `http://arxiv.org/abs/1006.4905`.

12) Going back to point 10 above. After laying down what Bayesian probability is about (uncertainty, surprise, not objective features “already there”), maybe then it is a good time to come to the “puzzles of qm” (item 4 in your old outline)? One could contrast their standard accounts with the QBist stories of the same.

13) Maybe I'll leave it at this for now, not giving an unlucky point 13.

After I hear some of your responses, I'll think harder about a more “look ahead” outline.

Time to start heading for home so that I can start heading for the airport to pick up the kids—those wonderful disciplinary resources for keeping old dad working rather than slacking off.

13-07-10 *QBism, Installment 4* (to H. C. von Baeyer)

von Baeyerism 62: *Do you think we should go through the list of current “interpretations” of q.m. in the text, the way you did in “Perimeter”, or should that be a box? I agree that the list is* *necessary, but am on the fence about the issue of a box. Whatever the answer, the list has to come early in the piece.*

14) I tend to think it ought to come in the text directly. This is part of what I was trying to suggest in Point 3 (Installment 1). I think that, with some good planning, one should be able to knock out a couple of them relatively quickly. Or perhaps, another way to go is to put a sentence or two in the text (I would prefer taking a little swipe at them for some spice), but then have a larger box saying a little bit about each mentioning a short-coming: A) Many worlds, the beloved one of science fiction writers, could have been imagined under any physics, and indeed was. Aka, it is contentless, despite all the bluster about it. And even those who think it has content still can't agree whether it can generate probability. B) Bohm. Must have faster-than-light "signals" but yet, miraculously, they cannot be used to actually signal. C) GRW-Flash. The "flash" events can always be adjusted to be just out of experimental reach. Furthermore, as Spekkens emphasizes, the flashes are ad hoc additions that give no unifying power. I.e., supposing them doesn't help us understand the structure of quantum theory in any way. D) Consistent Histories (Griffiths version). Despite its bluster, it doesn't go an inch beyond Copenhagen. E) Decoherence (Zurek version). Many worlds in disguise. Disguised mostly by all the bling it adds to the costume.

15-07-10 *Wednesday Soirée* (to P. Wells)

Similar to Latham, I want to make an addendum to last night's discussion.

Actually a corrigendum. When I went onto the porch for my coffee this morning, I realized to my horror that there was a typo in the equation I wrote for you!

I wrote

$$q(j) = \sum_i p(i) \left[(d+1) r(j|i) - \sum_k r(j|k) \right]$$

when I should have written

$$q(j) = \sum_i p(i) \left[(d+1) r(j|i) - \frac{1}{d} \sum_k r(j|k) \right] \ .$$

Sorry!

15-07-10 *Wheeler's 20 Questions* (to P. Wells)

Here's an extract (less than five pages) from a big, fat document (194 pages) of mine. The first page records Wheeler's 20-questions-in-reverse story, the other pages (pages 183–186) expand on it a little more. It looked like you were interested in the story last night, so I thought you might enjoy reading it in the old man's own words.

> The Universe can't be Laplacean. It may be higgledy-piggledy. But have hope. Surely someday we will see the necessity of the quantum in its construction. Would you like a little story along this line?
>
> Of course! About what?
>
> About the game of twenty questions. You recall how it goes—one of the after-dinner party sent out of the living room, the others agreeing on a word, the one fated to be a questioner returning and starting his questions. "Is it a living object?" "No." "Is

it here on earth?" "Yes." So the questions go from respondent to respondent around the room until at length the word emerges: victory if in twenty tries or less; otherwise, defeat.

Then comes the moment when we are fourth to be sent from the room. We are locked out unbelievably long. On finally being readmitted, we find a smile on everyone's face, sign of a joke or a plot. We innocently start our questions. At first the answers come quickly. Then each question begins to take longer in the answering—strange, when the answer itself is only a simple "yes" or "no." At length, feeling hot on the trail, we ask, "Is the word 'cloud'?" "Yes," comes the reply, and everyone bursts out laughing. When we were out of the room, they explain, they had agreed not to agree in advance on any word at all. Each one around the circle could respond "yes" or "no" as he pleased to whatever question we put to him. But however he replied he had to have a word in mind compatible with his own reply—and with all the replies that went before. No wonder some of those decisions between "yes" and "no" proved so hard!

And the point of your story?

Compare the game in its two versions with physics in its two formulations, classical and quantum. First, we thought the word already existed "out there" as physics once thought that the position and momentum of the electron existed "out there," independent of any act of observation. Second, in actuality the information about the word was brought into being step by step through the questions we raised, as the information about the electron is brought into being, step by step, by the experiments that the observer chooses to make. Third, if we had chosen to ask different questions we would have ended up with a different word—as the experimenter would have ended up with a different story for the doings of the electron if he had measured different quantities or the same quantities in a different order. Fourth, whatever power we had in bringing the particular word "cloud" into being was partial only. A major part of the selection—unknowing selection—lay in the "yes" or "no" replies of the colleagues around the room. Similarly, the experimenter has some substantial influence on what will happen to the electron by the choice of experiments he will do on it; but he knows there is much impredictability about what any given one of his measurements will disclose. Fifth, there was a "rule of the game" that required of every participator that his choice of yes or no should be compatible with *some* word. Similarly, there is a consistency about the observations made in physics. One person must be able to tell another in plain language what he finds and the second person must be able to verify the observation.

— *John Archibald Wheeler*
Frontiers of Time, 1979

16-07-10 *IGAIA III – Missing Titles and Abstracts* (to IGAIA III)

I am sorry to keep you waiting. Here it is.

Title: Rewriting Quantum State Space as a (Nice) Statistical Manifold

Abstract: Recent work in the field of quantum information theory has pointed to the significance of a very special kind of quantum measurement—the so-called symmetric informationally complete quantum measurements (or SIC). This structure is only known to exist for finite-dimensional quantum state spaces with dimension $d = 2$ to 67, but they are widely believed to always exist for all finite dimensions. If they do exist, then quantum state space (i.e., the convex set of trace one positive semi-definite matrices) can be rewritten as a d^2 dimensional (very symmetric) statistical manifold within the d^2 dimensional probability simplex. Furthermore, in this language, the Born Rule for calculating probabilities for all other quantum measurements transforms into a very simple variant of the classic Law of Total Probability. In this talk, I will present all the above in greater depth and list several open questions that the information geometry community may be in a position to answer. Some details can be found at: `http://arxiv.org/abs/0906.2187` and `http://arxiv.org/abs/0910.2750`.

16-07-10 *Where Women Roar and Men Thunder* (to D. B. L. Baker)

It's been a long time since I've written you from a long flight. I've got nothing particular to say, but *it is a long flight* that I am on, and I suppose I hope that this will inspire me to say something. I'm on my way to Los Angeles from Toronto, the first leg of a trip to Brisbane, Australia. I arrive in LA at 9:10 PM, and then depart for Brisbane at 11:30—two hours of misery in a way. My body will be thinking it's between the hours of 12:10 and 2:30, but yet I dare not sleep. At least once I'm on the plane, 14 hours later, I'm in Brisbane.

I believe this is my sixth trip to Australia, though only my second to Brisbane. The other ones have been to Sydney. I'm going to the QCMC conference (Quantum Communication, Measurement, and Computation), where I pick up my \$3K award and make an after-dinner speech. I hate making after-dinner speeches: For some reason they put me on edge like a scientific talk never does. Maybe it has something to do with something Paul Simon said about song writing. He said, in writing a song, he strives to start with a statement of truth; after that everything flows smoothly. Maybe that's why I'm disoriented with the after-dinner thing: I don't know where the truth lies in those contexts.

We were talking about having a walk-about together. I still think that's a good idea. Just got to figure out how to pull it off. I tabulated the other day that I've flown nearly 49,000 miles already since January 1 (46K of it on American Airlines where it earns me free flights more efficiently). Immediately after this trip, I take the family to Munich (separating to go first to a conference in Austria and then a conference in Leipzig). Then in October, I go to Cape Town, South Africa (Stellenbosch, a wine region, actually). And finally in December I go back to Brisbane. So, I plan to have a butt-load of frequent-flyer miles at the end of this year. We just have to do something once I get some of this work cleared out of the way.

My kids took their first great Texas adventure last week. Emma (11) and Katie (8) flew for the first time by themselves. My sister Cathy picked them up in Houston, and then got them back to the airport at the end of the week. Next year, they fly to Europe for the first time without Kiki and me. It's all part of the accelerated growth plan. I was 30 already before I said anything new and useful about quantum mechanics, or natural philosophy more generally. My thinking is that they ought to beat me by at least 10 years.

After reading all the *Harry Potter* books three times, and the *Percy Jackson* series, I've finally bamboozled Emma into reading the *Lord of the Rings* trilogy. She's fought it tooth and nail the last couple of years. But I offered to pay her 10 cents a sentence for every email she writes me about what she's read in the books while I'm away in Australia, and that seemed to tip the scales. She's been saving for an iPod Touch. Ah, priorities!!

Hey, check out this blurb I wrote for the back cover of Nielsen and Chuang's textbook on quantum computing (10th anniversary edition). It made me feel literary when I put it together:

> Nearly every child who has read Harry Potter believes that if you just say the right thing

or do the right thing, you can coerce matter to do something fantastic. But what adult would believe it? Until quantum computation and quantum information came along in the early 1990s, nearly none. The quantum computer is the Philosopher's Stone of our century, and Nielsen and Chuang is our basic book of incantations. Ten years have passed since its publication, and it is as basic to the field as it ever was. Matter will do wonderful things if asked to, but we must first understand its language. No book written since (there was no before) does the job of teaching the language of quantum theory's possibilities like Nielsen and Chuang's.

If Emma hadn't become so fascinated with *Harry Potter*, I'd have never been able to write that. There's so much to learn from our kids. How are your three doing? What sorts of growth and what sorts of mischief have they been up to?

PS. I must say, I still haven't had a vegemite sandwich.

18-07-10 *Where Women Glow and Men Plunder* (to D. B. L. Baker)

It seems my memory failed me yesterday.

Oh well . . . head full of zombie,

20-07-10 *An Honest Attempt* (to C. M. Caves)

OK, an attempt at an actual answer.

It has to do with the trail of thought I've been following since 2001 or so. The only way I know how to express it is in analogy to Newton's law of universal gravitation. Newton's law is that every two masses in the universe attract each other with a force proportional to a product of two numbers intrinsic to each: Their gravitational masses. Newton identified a property common to all matter. But beyond that, it was silent on what is real of the world. It was not a nuts and bolts ontology in the sense that one usually seems to want of quantum mechanics—i.e., that the wave function is real, or the Hamiltonian is real, or the Bohmian trajectory is real, or the multiverse with its universal wave function is the sole reality, etc., etc. All Newton's theory does is say, "Look here. Here is a property common to all matter, in all its manifest and yet to be discovered forms. Every piece of the world has gravitational mass, and the significance of that number is blah, blah, blah."

Now, I think that is the best way to understand quantum mechanics. I.e., to scale back our reductionist dreams for it and say, "It is not at all a theory tailor made for supplying an ontology in the traditional sense. Instead it is like Newton's law in identifying a single, important aspect of all pieces of the world." That aspect, I think, is what we conventionally call Hilbert space dimension, d. Newton's law was a great achievement for physics even though it didn't pretend to be a theory of everything, and the same I say of quantum mechanics. If it can solidly identify one—just one—aspect of this world common to all things, even if it says nothing beyond this, that is enough for me.

So, my whole thought is geared toward pinpointing what the essence of this quantity — dimension — really is.

That is why I am attracted so much to the urgleichung. This is because it displays Hilbert space dimension front and center *as* "deviation from a naive (or unjustified) application of the law of total probability." When we deal with a certain piece of matter, quantum theory advises that we should deviate—by a particular amount, d—from the law of total probability. The more d a system is hypothesized to have, the more we should deviate from a naive application of the LTP in the diagram I always draw. That is something that the usual form of the Born Rule just buries away; it isn't seen at all in the usual complex-Hilbert-space way of writing the rule.

Now, for my reason for pursuing a deep detailed study of the urUNgleichung. These "maximal consistent sets" seem to have a rich enough structure to hint of the possibility that the full structure of quantum-state space might just come from the preservation of the consistency of the urgleichung. In other words: It is that the principle that brings dimensionality out front and center as a deviation from the LTP is the same principle that sets the full structure of quantum state space.

If that *turns out* to be true, then I see it as a great unifying step for QBism. Our thought hangs together better than it used to, and such a theorem would clear out just that much more underbrush. And with that, the hope is we'll be able to see the full forest much better for it.

To a person who wants to build a better gravity wave detector *today*, I would guess all of this will make no difference to his life. But to the person who wants to go the next step in physical theory (say, to talk about gravity and quantum in the same sentence, but not necessarily this), my gut says it will make all the difference in the world. My gut says, despite all the erosions in confidence the live-for-today physicist tries to dole out upon me, that this is the right way to go. At least it is the right way for me. My gut says the fruits of this effort will have lasting value that will long outlive me.

I honestly believe what John said, "Until we see the quantum principle with this simplicity [in one clear, simple sentence] we can well believe that we do not know the first thing about the universe, about ourselves, and about our place in the universe." To the extent of my limited ability, that is what I want to be about. And I feel our emphasized rewriting of the Born Rule is a step in that direction.

20-07-10 *An Honest Attempt, 2* (to C. M. Caves)

Cavesism 84: *Thanks for the answer. I was hoping for something that would, without appealing to the esoterica of quantum gravity, tell me why you're working on this particular program.*

But I wasn't at all appealing to quantum gravity for any kind of justification. I was appealing to the idea of the value of identifying one property common to all things. I.e., nonreductionism. Gravitational mass just happened to be the example.

21-07-10 *Those James Quotes* (to G. J. Milburn)

Here they are. The first concerns how I think you should view your new center, as an addition to your "self":

> In its widest possible sense, however, a man's self is the sum total of all that he can call his, not only his body and his psychic powers, but his clothes and his house, his wife and children, his ancestors and friends, his reputation and works, his lands and horses, and yacht and bank account.

With this new arm, you may do things you had never previously imagined possible.

The second quote concerns German philosophy in the early 1900s, but my experience tells me it is completely apropos of the present-day "philosophy of physics" (considered a professional field).

> In a subject like philosophy it is really fatal to lose connexion with the open air of human nature, and to think in terms of shop-tradition only. In Germany the forms are

so professionalized that anybody who has gained a teaching chair and written a book, however distorted and eccentric, has the legal right to figure forever in the history of the subject like a fly in amber. All later comers have the duty of quoting him and measuring their opinions with his opinion. Such are the rules of the professorial game — they think and write from each other and for each other and at each other exclusively. With this exclusion of the open air all true perspective gets lost, extremes and oddities count as much as sanities, and command the same attention ...

The quote on the self comes from his *Principles of Psychology*. I've never read the whole book, but Scott Aaronson wrote me once: "I'm working through it now. I've decided to recommend it to people as 'the most up-to-date, state-of-the-art book about consciousness' (without telling them the publication date)."

The quote on German philosophy is from his book, *A Pluralistic Universe*.

The first book that really made an impression on me was simply titled, *Pragmatism*.

23-07-10 *Subjectivism in Classical Statistical Mechanics* (to P. G. L. Mana)

A key point (I think in agreement with what you say), is to be found in the second and third paragraphs after Merminition 150 of "Extract for Luca." Also something to think about is Keynes' remark on Ramsey in Extract 2, that the "basis for our degrees of belief ... is ... perhaps given merely by natural selection." [See 08-11-06 note "November 8th" to N. D. Mermin, Mermition 155 now, and the subsection titled "The Projection Postulate as Bayesian Updating" in the 24-06-06 note "Notes on 'What are Quantum Probabilities' " to J. Bub.]

Luca's Preply

This question became more interesting to me after I finally proved to myself that the maximum-entropy principle is just as 'subjective' as the rest of plausibility logic `http://arxiv.org/abs/0911.2197`.

It seems as if there is much to say, and very little to say, about it. The most honest books on statistical mechanics make clear that the various distributions used are just postulated — they are used because they work. Any 'derivation' is only an a posteriori complacency.

Indeed, one could say that we have arrived at them through a sort of historical Bayesian updating: we have tried some until we arrived at those that work, i.e., that are not further updated by new data. This is corroborated by the fact that the usual distributions do not work in some cases, for granular materials e.g., and new distributions are used there — trial and error again.

23-07-10 *What GK Say* (to C. M. Caves)

From G. K. Chesterton's book, *Heretics*:

There are some people—and I am one of them—who think that the most practical and important thing about a man is still his view of the universe. We think that for a landlady considering a lodger it is important to know his income, but still more important to know his philosophy. We think that for a general about to fight an enemy

> it is important to know the enemy's numbers, but still more important to know the enemy's philosophy. We think the question is not whether the theory of the cosmos affects matters, but whether in the long run anything else affects them.

26-07-10 *Just Noticed!* (to A. Kent)

I just noticed the name change:

Centre for Quantum Information and Foundations (formerly Centre for Quantum Computation)

That's great! Congratulations.

03-08-10 *Noncolorable Sets* (to A. Cabello)

Cabello-ism 3: *Would it be interesting to have a finite non-(KS)colorable set of:*
(a) trines
(b) regular tetrahedrons?

Regular tetrahedrons for sure. (Assuming you are talking about for a qubit.) More generally, it would be quite nice to know if in arbitrary finite dimension d, there are always noncolorable SIC sets.

Cheers from Munich (working at my in-laws' house),

04-08-10 *Help* (to C. M. Caves)

Cavesism 85: *I gave a public lecture here the other night on why quantum mechanics is strange and tried to show that it means that things don't have objective properties. Afraid I offended my officemate, Cyril Branciard, who is pointing me to Gisin's work contending that nonlocality is the only assumption in proving Bell inequalities. I don't have time to read and come up with good answers, so give me some help. I will read it, but I'm sure you've thought about it much more deeply than I will.*

Yeah indeed people say that … all the time. Read for instance this "very clear" exposition of Travis Norsen: `http://arxiv.org/abs/quant-ph/0601205`.

My own opinion is that each and every time this claim is made, the claimer is either explicitly or implicitly (and in a roundabout way) assuming the EPR criterion of reality. And an essential piece of that is that CERTAINTY implies TRUTH (in the ontic or correspondence sense).

I'll send you some of my conversations with Spekkens, Mermin, etc., on the subject if I can dig it up.

I think my own best exposition on the subject is Section V (starting page 14) of `http://arxiv.org/abs/1003.5209`. I think that section can be read independently of the rest of the paper.

07-08-10 *Scientific Report* (to M. A. Graydon)

Graydonism 3: *I want to thank you again for making the trip to Traunkirchen possible. I had the pleasure of meeting and discussing physics with some really interesting people while I was there — making the long trip well worth its while. After you left, I gave a short informal talk to a small* *group on quaternion qt and the general Jordan-Banach algebraic structures that have been on my mind lately. Now back home, I am realizing just how beautiful our surroundings were. It's left me with a sense of optimism for the future, both personally and work-wise.*

They were fantastic surroundings. Mother earth is big, and she brings her size and grandeur up front for your inspection in the Alps. I hope you'll use every opportunity like this to see the world from a different angle. There'll be many opportunities in your future. The mind is an amazing connected web of thoughts, plans, feelings and facts, and one shouldn't think for a minute that an emotion in the Alps may not play a role in a mathematical theorem somewhere down the road in Waterloo or anywhere else.

10-08-10 *Whaddya Think?* (to T. Jacobson)

Every new paper like this gives me occasion to recall my wisdom teeth being pulled. No matter how many times it's recalled, it never becomes more pleasant.

Ted's Preply

`http://arxiv.org/abs/1008.1066`

12-08-10 *Slide Permissions Question* (to J. Gleick)

Your publisher sent the note below to me, assuming that you had already written me. I'm not quite sure which slide she is talking about. Maybe you can shed light. Also, it'd be nice to know what your book is about.

I read your *Chaos* book many years ago and enjoyed it much. And your Feynman book has sat on my shelf for a couple years calling me, calling me.

James's Reply

> Oh, gosh. I've been meaning to call or write. Thanks for emailing me directly.
>
> The book is a sort of history of information and information theory. It has Claude Shannon as its core, and goes back to the invention of writing and forward to quantum information science, and meanwhile explores many issues that are more cultural than scientific.
>
> The slide of yours that I want to use, by way of illustration, is attached. Contrary to what Jill says, I don't think a higher resolution version is needed. In the text, I quote something you said in "Quantum Mechanics as Quantum Information (and Only a Little More)"—the passage about holy tumult over quantum foundations, and your comment, "Quantum mechanics has always been about information; it is just that the physics community has forgotten this."
>
> Of course I'll send you the book when it's released. For that matter, would you want to see it in galleys? I'd certainly welcome any comments or corrections you might have. I'd ask only that you keep it entirely to yourself for now.
>
> Thank you also for the kinds words about *Chaos*, and I'm glad the Feynman book has at least made it as far as your shelf.

16-08-10 *Whaddya Think?, 2* (to T. Jacobson)

Sorry for the delay: I wrote you my "smart ass" note just as I was preparing to come home from Europe, and then I needed a few days of "recovery from the vacation" before looking at email again.

I'm going to refer you to Carlton Caves for this one, since he and Rüdiger Schack have a nice paper on the subject:

`http://arxiv.org/abs/quant-ph/0409144`

(I'd refer you to Rüdiger too, but I know he's hiking in France for the next two weeks.)

Ted's Preply

Let's take it out of the cosmology context.

Do you object a priori to the crazy idea that there *are* an infinite number of copies of the system, and that the composite *is* always an exact eigenstate of the relative frequency operator?

If not, that leaves the question whether and in what sense it implies the Born rule for *us* (one of those copies), when *we* do the experiment a *finite* number of times, say 100. A brief perusal of their paper gave me the impression that they were analyzing this latter situation by positing that we sample the infinite number of copies with equal "statistical" probability, so we measure, statistically, the relative frequency operator. But I'm not sure this is what they were saying. And it is suspect, since our 100 repetitions of the experiment either breaks the symmetry between us and the rest of the subsystems, or all subsystems do it, i.e., the whole infinite composite does the experiment 100 times. Then one can ask about the relative frequency operator of just our outcomes, and we know the system cannot be in an eigenstate of this, because that would preclude the 1/sqrt(100)=10% fluctuations that we know QM gives … which suggests that we can't deduce the Born rule after all …

16-08-10 *Slide Permissions Question, 2* (to J. Gleick)

Thanks for the information. (Hee hee.) Sorry to contact you quickly and then leave you high and dry, but I wrote you just as I was traveling back home from Europe … then there was the weekend.

Your new book sounds interesting. Thanks very much for your kind offer to send me a copy! Attached is a little "obituary" I had written on Shannon for the participants of a meeting I was organizing when he died. I wonder now whether you were the author of whatever *New York Times* article I linked to then? [See 27-02-01 note "Claude Shannon's Death".]

And thanks for letting me know which slide you need. I think I can supply you nonetheless with a cleaner scan than the one you have. Just give me a couple of days to dig it up.

Your publisher asked me about "licensing fees" for the slide, but how about we just do this? If you'd cite a couple of my papers on the subject (along with the old one you quote), as well as the upcoming Cambridge University Press reprint of my book *Notes on a Paulian Idea* (in this incarnation it is called *Coming of Age with Quantum Information*), I'd be most appreciative. Attached is a copy of the Introduction to the new edition of the book; you might enjoy some of the character sketches of quantum information figures in it (like Landauer, Bennett, Peres, etc). And the more up-to-date paper I'm thinking for the citing is this one: `http://arxiv.org/abs/1003.5209`. I would think that Section IV sounds particularly relevant to the thing from the old paper that you're already quoting. For instance, this paragraph would seem to indicate so:

> In this regard, no question of QBism tests nature's tolerance more probingly than this. If quantum theory is so closely allied with probability theory, if it can even be seen as an addition to it, then why is it not written in a language that starts with probability, rather than a language that ends with it? Why does quantum theory invoke the mathematical apparatus of complex amplitudes, Hilbert spaces, and linear operators?

Anyway, sound like a deal?

James's Reply

> No need to make a deal. I don't really think you want to charge a licensing fee for the slide, whatever my publisher says, and in any case I cite papers I have consulted and found useful, which includes your wonderful *Notes on a Paulian Idea* among several others. Would you please send me the correct bibliographic information for the CUP book, so that I can update the reference?
>
> Thank you also for the QBism paper; I can see that it's going to cause me to update the book. Luckily it's not too late.

17-08-10 *Hitler's Fireplace* (to D. B. L. Baker)

Thanks for the BBC story. I've been meaning to write you for some time. Particularly, I wanted to write you from Germany, but then things got in my way—I'm certainly not the correspondent I used to be.

What I had wanted to tell you about was my trip to Berchtesgaden, and the emotions it brought with it. Chances are, it's too late to express them properly now. Probably one of those things where you had to be there. It hit me as I was explaining to my girls that Hitler's Eagle Nest (or Kehlsteinhaus) was more than just a place to have a very fancy tea party or to simply impress the visiting diplomats. It was intended to put fear into the hearts of those visiting diplomats as well. It was a subtle way of saying, "Look what the power of this nation can do. Take note. What we can build for ourselves, we can destroy of yours." You really get a sense of this as you drive up the mountain, going through about five tunnels. Then you get to the level where Hitler's Berghof used to be, and you walk into a tunnel deep into the mountain crafted by Italian stone cutters, finally entering Hitler's original (massive) bronze plated elevator to get the rest of the way to Kehlsteinhaus. So, I was really feeling this sense of the Third Reich's strength and power ... when I ran across a picture of Eisenhower and Montgomery viewing a room I had just been standing in. I think it was the room with Hitler's fireplace, now quite damaged from all the chips of marble soldiers had broken from it. There was Eisenhower, standing in the damage, a symbol of America. And I turned to the girls and said, "All that power, and America's in the end was greater." "They put all these stones together on this high, high mountain to show their power and start a war. And we ended the war by putting 14 pounds of plutonium in just the right place. And just 14 pounds! What subtlety. The thousands of beautifully cut stones meant nothing in comparison; power is measured by what you can make happen. 14 pounds in just the right place."

For a few minutes, I was very proud again to be an American and to think of those old victories of mind and industry.

If you want to remind yourself of Hitler's Eagle Nest, this seems to be a pretty good site: `http://www.songsofwar.info/hitlermountain/Kehlsteinhausnew.html`.

Attached are a couple of pictures from the trip. The first is of the whole family, taken at the level of Hitler's Berghof and Goering's home (i.e., not all the way at the top). The second is of

Katie and me at a biergarten back in Munich. (Don't worry: That's an apfelschorle in Katie's hand, not a beer!)

18-08-10 *Your Paper* (to J. I. Rosado)

Congratulations! You did it all without me. *Foundations of Physics* sent me the paper to referee, and I didn't accept to review it: I thought it would be better for everyone if you had impartial reviewers. And it succeeded even without my help. This is a very good sign for the science! [See J. I. Rosado, "Representation of quantum states as points in a probability simplex associated to a SIC-POVM," Found. Phys. **41**, 1200–1213 (2011).]

21-08-10 *The Chartreuse Microbus* (to H. C. von Baeyer)

von Baeyerism 63: *One of the important ingredients of our paper is going to have to be an analogy for the SIC Born rule. Your measurement in the sky or at NIST won't do it, because analogies must not refer back to the concept being explained – quantum measurements in this case. Analogies must be to images that are instantly familiar. Even Fourier series fails on that count, tho it's closer. I'll suggest alternatives later. However, for now, I seem to recall that you wrote a paper with a student about the Fourier analogy but I can't find it in the* `arXiv`*. Does it exist?*

I think you're thinking of the conversation below. [See 11-01-10 note "Analogy?" to H. C. von Baeyer.] The equation numbers must refer to the version of the paper I had sent you just before. I'll re-attach that version for reference.

Just like Fourier expansion is a very special functional expansion (into sine waves), so is a SIC expansion (into a nice set of positive semi-definite operators).

For argument's sake though, I don't think the bureau of standards conception is circular at all. It is more like the Lord of the Rings:

> *Three Rings for the Elven-kings under the sky,*
> *Seven for the Dwarf-lords in their halls of stone,*
> *Nine for Mortal Men doomed to die,*
> *One for the Dark Lord on his dark throne*
> *In the Land of Mordor where the Shadows lie.*
> *One Ring to rule them all, One Ring to find them,*
> *One Ring to bring them all and in the darkness bind them*
> *In the Land of Mordor where the Shadows lie.*
> *He paused, and then said in a deep voice,*
> *"This is the Master-Ring, the One Ring to rule them all."*

I'll expand and explain once I can get my hands more firmly on the keyboard. (I'm presently having cheese and sausage and sliced turkey for lunch—finger food all.)

21-08-10 *One Ring to Bind Them, One Ring to Rule Them All* (to H. C. von Baeyer)

OK, my fingers are clean.

Here's what I meant by saying that the SIC is a bit like the Lord of the Rings. Like all quantum measurements, it is an action that an agent can take on a system with some number of unpredictable consequences for him—so it is not special in that regard, or in any way explanatory of the concept of quantum measurement. However, it does have a special defining feature that sets it apart from all other measurements. It is an action (and the only one) for which the agent can judge the following: That if he contemplates performing it twice on his system (getting outcomes i and j respectively), no matter what his Bayesian probabilities for the first outcome i, he will assign an almost flat conditional probability $p(j|i)$ for the second outcome j. That is, he will assign a $p(j|i)$ of the form $(a, a, \ldots, a, b, a, a, \ldots, a)$, where the value b inhabits the i'th slot of the probability vector, no matter what his prior $p(i)$ was.

And that one simple defining feature—in the end—gives the SIC all its expressive power for the Born Rule for all other measurements. What a nice primitive for a world believed to be indeterministic to its core.

... Ah, now that I've written that I don't feel I've shed the light that I wanted. I'm not getting to the key point in the right way. I may try again later in the day.

22-08-10 *Things from the Middle of the Night* (to H. C. von Baeyer)

Let me think on the new draft. Two broad thoughts at the moment.

1. Something like the red paragraph at the end of Section 2 could work, but it should be re-written to be more in Hans style than Chris style. At present it kind of stands out a bit too obviously (and even when the color was changed from red to black).

2. Section 3 is more sympathetic with frequentism presently than I can personally sign on to. W. K. Clifford once wrote, "It is wrong always, everywhere, and for anyone, to believe anything upon insufficient evidence"—which is not something I subscribe to (give me James's "will to believe" any day). But I do find Clifford's wording appropriate for frequentism: "It is wrong always, everywhere, and for anyone." And that opinion will cause havoc for using the Nagel quote as a pivot as well. In the game of probability, one cannot give an inch without losing a mile. I think these two papers by our friend Appleby make the point better than nearly any other source that I've read:

   `http://arxiv.org/abs/quant-ph/0408058`
   `http://arxiv.org/abs/quant-ph/0402015`.

   "Probability is single case, or nothing."

I shall meditate a bit.

23-08-10 *Probability* (to H. C. von Baeyer)

2:15 AM, at the moment; I had started to become restless at 12:39. I.e., it's another one of those nights.

Thank you so much for this note. I don't think I can adequately express the relief in seeing it. When I finally roused myself from bed five minutes ago, I thought that I would be writing you a small essay that started like this:

A frequentist says probability is long run frequency (always). A Bayesian says probability is judgment *always*. ... A young man has been tossing a favorite coin for a long time, 10,000 trials and has noted the frequency of heads and tails is almost exactly 50/50. Now it comes time to gamble on the next toss—now it comes time to put his observations to use. The young man confidently writes down 50/50 for his probability assignment … BUT WAIT! Just before he's had to act on that assignment he notes the strangest feature of his sequence of coin tosses: By some miracle, it is a binary representation of the digits of π! He uses *Mathematica* to check that there's not one error. How could that be? Is there some hidden mechanism behind the toss? "Hogwash, it can't be," he says, "I've been doing the tossing myself." Still a bookie walks from around the corner, and offers a lottery ticket on the young man's next toss. "Receive $1,000,000 if heads" for the price of $65,000. Will he buy the ticket? Or not? That will depend upon his judgement. He thinks to himself, "It could have been extraordinary luck. If probability is frequency, then this very string of heads and tails was among the possible. But it has no hold over the precise detail of the next coin toss. I will use my study of the frequency to temper myself. It would be foolish to buy such a ticket. That is my salary for a whole year; I would be devastated if I lose that much money." But if instead he judges the finding of π significant, miraculous even—for instance that God is with him in some way in his coin tossing (and I would think myself very likely to judge something like that in such an extreme case)—then he would accept the ticket. All his experience in the past—not just some coarse grained variable like frequency—tells him that it is the right thing to do. He lives by the dictum, "Never throw away information," and proceeds to buy the ticket. Which judgement is the right one? The young man will have time to contemplate that when the moment to buy or not to buy the ticket has passed and the outcome of the next coin toss is in hand.

Moral: A (past) frequency can be used to determine one's (future) probability assignment if one *judges* it to be relevant to that assignment. The judgement, however, is always there in the background; it cannot be gotten rid of. The Bayesian is often considered shrill for pointing this out over and over, but losing sight of it is the great original sin of frequentism. Past frequency is one thing; future gambling (i.e., that which is measured by one's probability assignment) is another.

Anyway, I thought I was going to have to write you an essay like that, but a much more careful one, one that tried to anticipate your every countermove. "But what about this? But what about that?" It would have taken a lot of thought. And then perhaps a long email debate would have ensued—it has happened to me before. … And I must say I was dreading it! BUT you saved the day!!! AND I bet I can fall back asleep now!!!

Hans's Preply

> OK, I see, I'll pour the weak tea down the drain. Somehow I thought that we could define the two prevailing approaches to probability before launching into QBism, but I see now that we must enthrone Bayesian probability before we get to q.m.
>
> Don't bother to save parts of section 3. I'll start it over, taking comfort only in the implication of Appleby that it IS possible to derive probabilities from finite ensembles, the way everybody does, provided only that one admits to sloppy thinking:
>
>> Frequentists are impressed by the fact that we infer probabilities from frequencies observed in finite ensembles. What they overlook is the fact that we do not infer probabilities from just any ensemble, but only from certain very carefully selected ensembles in which the probabilities are, we suppose, constant (or, at any rate, varying in a specified manner). This means that statistical reasoning makes an essential appeal to the concept of a single-case probability: for you cannot say that the probability is the same on every trial if you do not accept that the probability is defined on every trial.
>
> This will take a little longer.

23-08-10 *Probabilismo!* (to H. C. von Baeyer)

I don't know how much prodding to cap off your apparent ascendancy to personalist Bayesianism, but in case you need just a bit more, let me give another reading recommendation. This one is de Finetti's soul-searching article Probabilismo. More than any other thing I've read in the foundations of probability, this article affected me the most deeply. (It took four readings, I should add, before it had this effect on me, but the final result was nonetheless real.)

I hope I didn't cause you too much grief yesterday—as Rüdiger and I know too well, schisms in our thoughts are always painful even if necessary.

23-08-10 *B. Russell You Might Like* (to D. M. Appleby)

I'm having a horribly boring day going through the final proofs of my Cam U Press book, but I came across a quote of Bertrand Russell that I had sent to Bill Wootters in 1999 that you might quite like:

> Physics is mathematical not because we know so much about the physical world, but because we know so little: it is only the mathematical properties we can discover.

Looking forward to your return to Waterloo . . .

24-08-10 *Your Eloquent Point* (to R. W. Spekkens)

I remember you once making the point that to really hope to contribute to quantum foundations, one should know as much of modern physics as possible. And I thought you made the point rather eloquently. Have you ever written that kind of thing down anywhere? I want to use it to help give my student Matthew some guidance on choosing his courses.

25-08-10 *Particle Physics Course* (to M. A. Graydon)

Sorry to keep you waiting. I remembered that my colleague Spekkens had something rather elegant to say on the issue, and I wanted to get that quote from him and send it in my reply to you. But Spekkens has no clue what I'm talking about. So, let me just wing it.

Rob says that one can't really hope to make a deep contribution to quantum foundations unless one has a good working knowledge of all the other things going on in modern physics. (He said it more elegantly.) I think there's some truth in that. And from that perspective, a high energy course would be just the right thing.

25-08-10 *Time for Templeton* (to D. C. Lamberth)

I hope you remember our brief encounter from April. I'll place my original letter of introduction below, as well as your reply to it. You might also go to the Perimeter Institute website `http://www.perimeterinstitute.ca/` if you want to get a picture of me in your head; there happens to be a banner on the homepage at the moment picturing me for a recent award I won.

This time I'm writing you because the Templeton Foundation pre-proposal that I sent you got past their first round of refereeing, and I have been invited to write a full proposal. (I'll attach the pre-proposal again for your convenience.) Thus I'm gearing up to do that—I just started today, and it is due September 15.

In that regard, I'd like to explore again the idea that you join me as a co-organizer in planning a workshop on pure experience and quantum measurement. I presently envision a workshop of about 15–20 people, for about 10 days, in a nice setting. Its participation would consist of a carefully chosen combination of quantum foundational physicists, pragmatist philosophers (possibly a smattering of other neutral monist philosophers), and theologians, with plenty of time for long discussions—a way for the communities to get to know each other and get to know each other's considerations. I am imagining the meeting sometime in 2012, and ideally I think I would like it to be held somewhere in New England, where the spirit of James is particularly strong: Harvard, near Chocorua, don't know.

If you are amenable, I would like to list you among the "personnel" in this proposal, and start up a dialog, even as I write it in the next twenty days, over some of the details of the shape of such a workshop. The only obligation on you with regard to the grant would be in having an active participation in the organization of the workshop. I would try to get enough money from the Templeton Foundation so as to completely cover all the expenses for the meeting. Also, I would of course give you an opportunity to edit any piece of the proposal that has to do with you personally.

I understand it is quite bold of me to contact you like this without either of us knowing each other: But life is short, and I want to make things happen. It is time for physicists and pragmatist philosophers to start talking to each other, and I finally see an opportunity that can give that some possibility of happening.

I hope to hear back from you soon, whatever your opinion.

27-08-10 *Excerpts* (to L. Freidel)

Here's a link to the New York Times article on how the structure of one's native language influences what his attention is drawn to:

> MAGAZINE — August 29, 2010
> Does Your Language Shape How You Think?
> `http://www.nytimes.com/2010/08/29/magazine/29language-t.html?emc=eta1`
> By GUY DEUTSCHER
> The idea that your mother tongue shapes your experience of the world may be true after all.

On the other hand, here is an excerpt from the introduction to my book:

> I started to realize that the major part of the problem of our understanding quantum mechanics had come from bad choices of English and German words (maybe Danish too?) for various things and tasks in the theory. Once these bad choices got locked into place, they took on lives of their own. With little exaggeration, I might say that badly calibrated linguistics is the predominant reason for quantum foundations continuing to exist as a field of research. Measurement? I agree with John Bell—it is a horrible word and should be banished from quantum mechanics. But it is not because it is "unprofessionally vague and ambiguous" as Bell said of it. It is because it conveys the *wrong* image for what is being spoken of. *The* quantum state? That one is just about as bad. Who would have thought that so much mischief could be made by the use of a definite article? So many of these things came to mind as I would strive to be clear and entertaining in my writings to David ...

As I came to realize while talking to you, I think that's why I was so interested in the language article this morning.

Finally another excerpt related to our discussions—this one from one of Rob Spekkens' papers:

> We shall argue for the superiority of the epistemic view over the ontic view by demonstrating how a great number of quantum phenomena that are mysterious from the ontic viewpoint, appear natural from the epistemic viewpoint. These phenomena include interference, noncommutativity, entanglement, no cloning, teleportation, and many others. Note that the distinction we are emphasizing is whether the phenomena can be understood conceptually, not whether they can be understood as mathematical consequences of the formalism, since the latter type of understanding is possible regardless of one's interpretation of the formalism. The greater the number of phenomena that appear mysterious from an ontic perspective but natural from an epistemic perspective, the more convincing the latter viewpoint becomes. For this reason, the article devotes much space to elaborating on such phenomena.
>
> Of course, a proponent of the ontic view might argue that the phenomena in question are not mysterious if one abandons certain preconceived notions about physical reality. The challenge we offer to such a person is to present a few simple physical principles by the light of which all of these phenomena become conceptually intuitive (and not merely mathematical consequences of the formalism) within a framework wherein the quantum state is an ontic state. Our impression is that this challenge cannot be met. By contrast, a single information-theoretic principle, which imposes a constraint on the amount of knowledge one can have about any system, is sufficient to derive all of these phenomena in the context of a simple toy theory, as we shall demonstrate.

It is that point that really drives me: By adopting a way of thinking of quantum mechanics so that it seems to come from a single, very far reaching principle—rather than simply coming out of the blue—I feel we'll more quickly get to the next stage of physics.

30-08-10 *Some Bayesian Crap* (to P. G. L. Mana)

Manalogue 19: *Bayesian crap apart, what still surprises me is how students don't ask "but what determines the individual outcomes?", as any good physicist should do. How many of such questions do you get? Are we just teaching them to become good parrots? Attached is the image of a 'vacuum noise' current measurement, from Leonhardt's book. How can people be happy to know what's the distribution of the peaks without asking why and how it's going up and down that way?*

It still baffles me.

How could Bohr, Heisenberg, and other philosophical clowns have such a big influence?

Well, the philosophical clowns influenced me deeply. I had no illusions that they "had all the answers" or even "most of the answers," but—to me at least—they did have interesting directions of thought. Here is a recent formulation of my own:

> Most of the time one sees Bayesian probabilities characterized (even by very prominent Bayesians like Edwin T. Jaynes) as measures of ignorance or imperfect knowledge. But that description carries with it a metaphysical commitment that is not at all necessary for the personalist Bayesian, where probability theory is an extension of logic. Imperfect knowledge? It sounds like something that, at least in imagination, could be perfected, making all probabilities zero or one—one uses probabilities only because one does not know the true, pre-existing state of affairs. Language like this, the reader will notice, is never used in this paper. All that matters for a personalist Bayesian is that

> there is *uncertainty* for whatever reason. There might be uncertainty because there is ignorance of a true state of affairs, but there might be uncertainty because the world itself does not yet know what it will give—i.e., there is an objective indeterminism. As will be argued in later sections, QBism finds its happiest spot in an unflinching combination of "subjective probability" with "objective indeterminism."

It comes from `http://arxiv.org/abs/1003.5209`.

Appleby as well has some interesting points on the clowns in connection with Bayesianism in the introduction to `http://arxiv.org/abs/quant-ph/0402015`.

31-08-10 *From the Vault* (to A. Kent)

Kentism 12: *But scientifically I'm a little puzzled. I'd thought probably most people at this point would guess that SICs exist in all dimensions. (Am I wrong?)*

No, you're right. It was just one of those things that had been nagging my soul. At the end of a talk I would often get the question, "But what if they don't exist in all dimensions? What of QBism then?" I found that I would always get into apologetic mode, squirming around a bit, and saying something like, "Well, it'd really make no philosophic difference to QBism, but the ultimate formalism would be uglier. The world might have been better, but it wasn't." Maybe the audience couldn't see it, but I'd always shrink a bit inside—sweaty palms and all that. Now instead, I feel I'd rise to the occasion. When someone asks, "But what if they don't exist?" I'll shoot back, "Even better!! Then I'd have a theory—not quantum mechanics—of my very own! I'd be complete in a way Richard Feynman never felt he was!" (From a story in *Surely You're Joking* or somewhere like that.) You get the point.

01-09-11 *Opening the Mind of Max* (to M. Schlosshauer)

BTW, ever since you called my house a palazzo, I've kind of fancied to the term and use it all the time. This morning I coined Palazzo del QBismo and am thinking about modifying the QBism glossary accordingly.

02-09-10 *No Laughing Matter* (to R. W. Spekkens)

OK, I'm procrastinating from my Templeton theological efforts … *but*, two points for me tonight. (I'll make a rule—first person to five gets a free beer.) I looked up the twins story. (1) It wasn't laughs, it was smiles. And (2) Sacks interjected an 8-digit prime. Corroborating passages below.

> The second time they were seated in a corner together, with a mysterious, secret smile on their faces, a smile I had never seen before, enjoying the strange pleasure and peace they now seemed to have. I crept up quietly, so as not to disturb them. They seemed to be locked in a singular, purely numerical, converse. John would say a number—a six-figure number. Michael would catch the number, nod, smile and seem to savour it. Then he, in turn, would say another six-figure number, and now it was John who received, and appreciated it richly. They looked, at first, like two connoisseurs wine-tasting, sharing rare tastes, rare appreciations.

and

> I returned to the ward the next day, carrying the precious book of primes with me. I again found them closeted in their numerical communion, but this time, without saying anything, I quietly joined them. They were taken aback at first, but when I made no interruption, they resumed their 'game' of six-figure primes. After a few minutes I decided to join in, and ventured a number, an eight-figure prime. They both turned towards me, then suddenly became still, with a look of intense concentration and perhaps wonder on their faces. There was a long pause—the longest I had ever known them to make, it must have lasted a half-minute or more—and then suddenly, simultaneously, they both broke into smiles.

03-09-10 *Three Rabbis in a Bar* (to H. C. von Baeyer)

By the way, I have read the document they're wanting comment upon in that question, "The Philanthropic Vision of Sir John Templeton," very thoroughly and twice. I do realize his disciples will know him inside and out, but my reading of that document is that QBism really is on his side (and vice versa) in many, many of the matters expressed there.

But as you say, best not to speak directly of the tetragrammaton.

Still, quoting Sir John himself:

> Possibly, we can become servants of creation or even helpers in divine creativity. Possibly, we are a new beginning, the first creatures in the history of life on earth to participate consciously in the ongoing creative process.

03-09-10 *The Strip Tease* (to H. C. von Baeyer)

Well, Kiki and the kids finally got home from Buffalo. So I guess I have to go now. If I follow your suggestions in the quickest fix way, just stripping out the presumption about Sir J, and rearrange the remark on radical pluralism (see below), that leaves me with 115 more characters to fill in. But now, what to say!?!?

I'll have to think on it, but Kiki's here now. If there's anything that you'd like to see said, suggest away!

> A student who heard the applicant give a guest lecture on QBism in a graduate course on quantum foundations sent an email saying, "I like your theory because it returns to me as much freedom as I feel that I have. Such freedom is lost or partially lost in other interpretations [of QM]." This in fact is the heart of the matter for QBism's development of quantum theory and the heart of the matter, as we see it, with respect to Sir John's Donor Intent.
>
> By solid science, QBism has arrived at a stage of seeing human freedom as no less illusory and no less crucial to quantum theory than an electron's freedom as it "passes through" a Stern-Gerlach device. We feel that we have a window on quantum theory that has never been peered through before. Through the tools of quantum information theory and a sense that Wolfgang Pauli and John Wheeler were on the right track to understand the meaning and significance of "quantum measurement" (our two answers to the two Big Questions of this Funding Priority), we have come to a vision of quantum

theory that finds its most natural expression in the humanistic terms William James used to convey his radically pluralistic ontology.

As we hope to make clear in the more detailed parts of this proposal, what is within grasp through a methodical rewrite of quantum probabilities into Bayesian probabilities is that, at the same time as they bring relief to the quantum foundational conundrums, they indicate a world not "one" in its composition, but "many"–a partial community of partially independent powers, a pluriverse.

Will Durant put it most beautifully, "The value of a [pluriverse] as compared with a universe, lies in this, that where there are cross-currents and warring forces our own strength and will may count and help decide the issue; it is a world where nothing is irrevocably settled, and all action matters."

03-09-10 *The Strip Tease, 2* (to H. C. von Baeyer)

von Baeyerism 64: *I'm sorry, but I still don't really understand what you mean by freedom in your opening passage. Freedom from what? Could your 115 characters help here?*

Freedom of will, of course. The boy was saying QBism rejects the block universe picture of things, and he liked that. No other interpretation of quantum mechanics gives him that, he says. Would it help if I simply put the first and second paragraphs back together (as they were in the preproposal and didn't seem to set of any alarm bells in you then)?

I'm trying to pin down what exactly is confusing you.

Strange morning: For the first time ever, our big golden retriever decided he was frightened of walking down the staircase and wouldn't go done. He wouldn't let himself be dragged either without bringing the house down with him. Even the bribery of food wouldn't work. He was absolutely frightened. Now, where did that come from?

03-09-10 *Putnam Story and Others* (to D. C. Lamberth)

Hilary Putnam was your PhD advisor, wasn't he? I've been looking for a little story I wrote up on him once ... expressing my near horror over his turn toward the Bohmian interpretation of quantum mechanics in 2008 (in a talk at the Bill Demopoulos Fest), but so far I've had no luck. When I find it—if it's nice and joking enough!—I'll send it to you. As I recall, he called information theoretic approaches to quantum theory like my own, "instrumentalism with a postmodern sauce."

Anyway, I like writing stories on people. Attached are some I wrote for the introduction of my Cambridge University Press book. A couple of the stories are about philosophers, I don't know if you know any of these guys—Jeffrey Bub, Allen Stairs, Richard Healey (another student of Hilary's), and Abner Shimony. You might know Abner through proximity. I tried to display his sweetness in a story starting on page 30, and he plays a bit part in the less serious Zeilinger story on page 34.

Hope you enjoy—the Zeilinger story is particularly relevant for my interactions with you:

> Personal anecdotes aside, Anton Zeilinger is certainly a philosophical uncle to the very core of this book, the Paulian idea, and maybe he deserves the title for that alone. Here's the way he put it in a recent interview:
>
> > I call that the two freedoms: first the freedom of the experimenter in choosing the measuring equipment—that depends on my freedom of will; and then

> the freedom of nature in giving me the answer it pleases. The one freedom conditions the other, so to speak. This is a very fine property. It's too bad the philosophers don't spend more time thinking about it.

Simple and clean the notion is: Too bad indeed the philosophers don't spend more time thinking about it!

04-09-10 *House Warming Bubbles* (to H. C. von Baeyer)

Of course I wouldn't want you to enter an empty house! This time I hope I have successfully navigated your concerns.

The references in the Donor Intent section refer to these passages in one of Sir JT's books:

> Although we seem to be the most sophisticated species at present on our planet, perhaps we should not think of our place as at the end of cosmogenesis. Should we resist the pride that might tempt us to think that we are the final goal of creation? Possibly, we can become servants of creation or even helpers in divine creativity. Possibly, we are a new beginning, the first creatures in the history of life on earth to participate consciously in the ongoing creative process. [*Possibilities*, p. 41]

and

> The laws of the spirit refer to patterns of voluntary human behavior, not to the involuntary behavior of physical objects. A person is free to choose to act in accord with these spiritual laws or to try to defy them. This being the case, the patterns which these laws express are not uniformly exhibited by humans at all times. Rather, they represent the ideal patterns to which humans may aspire.
>
> Conformity to the laws of the spirit is a free choice of all responsible humans. So perhaps, to avoid misunderstandings, we should call them spiritual principles. [Possibilities, p. 154]

In the latter case, think of interpreting the Born Rule as a normative rule, and my figure with the tablet of the ten commandments (which I know you still hate). Anyway, they're solid, defensible references for me. In the terminology of Sir JT, QBism says that the Born Rule is a law of spirit or spiritual principle!

06-09-10 *On Dice, Divinity, and Telephone Companies* (to D. M. Appleby)

I walked over to tell Hulya that Rogers (or Bell or whoever you used for your telephone line) said that they'd be by to install it sometime between 11:00 and 2:00 today. She said that she'd be there. Kiki told the telephone people that if for any reason they could not get into your apartment, they should give us a call here at home.

By the way, I reread your paper "Concerning Dice and Divinity" over the weekend. I liked your remark near the end, "By contrast, the epistemic interpretation obliges us to concede to the cat a degree of metaphysical privacy." Looking back, that remark probably had quite some influence on me, expressed today, for instance, in my long rants in Sections 3 and 6 of the long recent QBism paper, `1003.5209`.

08-09-08 *Research Proposal* (to H. B. Dang)

Dangism 5: *Maybe this would cheer you up if I tell you that I start to understand the "ladder of faith" now.*

When I first looked at the Vanier Scholarship it was like "I want this, but this seems hopeless". Then when I read about the selection criteria more carefully: "I do think I have some chance". After I talked to you about it: "I might actually get it". After you submitted your letter: "I ought to get it". I'm not yet at "I must get it", but I will fight for that!

Indeed, James's ideas in use always cheers me up. I am impressed that you paid attention to the faith ladder.

09-09-10 *Your Manuscript LT12790 Medendorp* (to A. M. Steinberg & Z. E. D. Medendorp)

I will return to a civilized state (as opposed to the very furry Neanderthal-ish cave-dwelling persona I've taken on in the last few days), when my full proposal to the John Templeton Foundation has been turned in September 15. (It's on SICs and theology, of course.) In the meantime, maybe someone else will have some useful input on Aephraim's question about how to respond to the instrument matrix thing. Sounds like it's too late to reconstruct it now. It seems the question is now how to find the most positive and politic way to say, in essence, "Well we didn't do it, and we ain't gonna either."

07-09-10 *Agony* (to D. M. Appleby)

In agony over this letter writing (long story that I'll have to make meaningful for you eventually), I can't sleep ... and I can't write either. I turned to the *New York Times* for diversion. Anyway, here is something I just read that struck me as worth recording for myself:

> Science too has its share of mysteries (or rather: things that must simply be accepted without further explanation). But one aim of science is to minimize such things, to reduce the number of primitive concepts or primitive explanations. The religious attitude is very different. It does not seek to minimize mystery. Mysteries are accepted as a consequence of what, for the religious, makes the world meaningful.

In a sense, I think, QBism incorporates religion into science at just that point: Every quantum system is a little core of mystery.

Marcus's Reply

> You rang just as I started to reply to this. I absolutely agree. I think this question of "mystery" and "explanation" is extremely important. People in the Spekkens-Hawking-Weinberg mould invariably regard mystery rather in the way that catholics regard a black mass, as an evil entire and unadulterated. And explanation as a corresponding good. That is a good in the absolute sense. For myself I certainly wouldn't want to reverse the emotional loading. Obviously, explanation is OK in its place, and there are mysteries which are crying out to be resolved (for instance the mystery of why SHW believe what they do). However, it really does need to be kept in its place. The word

"to explain" means, literally, to lay something out flat. The idea being that when one lays it out flat one can see what is going on. Which can often be an interesting and useful thing to do. But of course SHW aren't thinking of just explaining this thing or that thing, in some limited context, as the occasion may arise. They want to explain everything, in some absolute way. EVERYTHING in capital letters. And that I think is a perversion. Laying the entire world out flat: if that were possible wouldn't it mean that the world was very boring? Indeed, the word "flat" is often used more or less a synonym for "boring".

Explanation is certainly a good. But only in the way that food is a good. How would it be if someone got so obsessed with eating that they came to see stuffing their face as the meaning of existence? I think the way SHW regard explanation is similarly pathological.

Incidentally, I think the definition of mystery as something that "must simply be accepted without further explanation" misses the point. The religious attitude of mind is not only willing, it positively desires to contemplate a mystery. Roll it around the mind rather in the way that one rolls wine around the mouth in order to appreciate it. Verbal articulation has a place in this process. Just as Howard will articulate the meaning that a wine has for him, so there are any number of religious writers who will articulate the meaning of a religious mystery. What makes it mysterious is not the fact that it just has to be accepted without further words or thought. Rather it is the fact that it is recognized that there is more to it than words can fully capture. That is obviously true of wine (reading Howard's wine tasting notes is one thing; drinking the wine quite another). Why can we not approach quantum mechanics in a similar spirit? —not as a "mystery" in the pejorative sense that SHW attach to this word, but rather in the way that anyone, them included, appreciates a glass of wine? (it may, incidentally, be apposite to remark that wine is an essential part of one of the central Christian mysteries).

We should talk about this some more, once you have finished your letter and Templeton writing. Also those articles you sent me on language and artificial intelligence. Perhaps over a glass of wine?

08-09-10 *Mixed Signals* (to H. C. von Baeyer)

Uh oh. I thought the guy went out of his way to say *no* manifesto. So when I show him a piece of another manifesto (i.e., a piece of the JTF proposal), I didn't quite expect a reaction like this:

Musserism 29: *Damn, this is great:*

> The research program of Quantum Bayesianism (or QBism) is an approach to quantum theory that hopes to show with mathematical precision that its greatest lesson is the world's plasticity. With every quantum measurement set by an experimenter's free will, the world is shaped just a little as it takes part in a moment of creation. So too it is with every action of every agent everywhere, not just experimentalists in laboratories. Quantum measurement represents those moments of creation that are sought out or noticed. If this vision of quantum theory stands scrutiny, it will mean that modern physics itself already speaks of a humanistic world—a world of hope & struggle & possibility & change. That would be the project's enduring impact.

It almost makes me angry that you haven't written the Sci Am article yet! Because it's going to be a great article.

Maybe he's just bowled over by the ideas.

What are you going to be up to in Spain? How long will you be gone? I don't think I've been to Spain for 12 years, but I still have an overpowering memory of garlic soup, red beef, and Rioja wines.

Bit. Stone. Qubit. Philosopher's Stone. We shall come back to all that after Spain. Have you ever watched the Dustin Hoffman movie, *Mr. Magorium's Wonder Emporium*? My family thinks it's a really wonderful movie, despite all the critics' pans of it. Recommended viewing for all QBies, in fact. Note the similarity between the Congreve Cube and the QBist qubit.

Magorium: Come with me. This, my lovely, is for you.

Mahoney: Thank you. What is it?

Magorium: It's the Congreve Cube.

Mahoney: It looks like a big block of wood.

Magorium: It is a big block of wood. But now, it's your big block of wood.

Mahoney: Thank you. I was just saying last night I don't have enough big blocks of wood.

Magorium: Unlikely adventures require unlikely tools.

Mahoney: Are we going on an adventure?

Magorium: Well, my dear, we're already on one. All I will say is this: With faith… love… this block… and a counting mutant, you may find yourself somewhere you've never imagined.

08-09-10 *Two Dialogues on QBism* (to the QBies)

Both from the movie *Mr. Magorium's Wonder Emporium* with Dustin Hoffman as Magorium and Natalie Portman as Mahoney.

Part 1)

> **Magorium:** Come with me. This, my lovely, is for you.
>
> **Mahoney:** Thank you. What is it?
>
> **Magorium:** It's the Congreve Cube.
>
> **Mahoney:** It looks like a big block of wood.
>
> **Magorium:** It is a big block of wood. But now, it's your big block of wood.
>
> **Mahoney:** Thank you. I was just saying last night I don't have enough big blocks of wood.
>
> **Magorium:** Unlikely adventures require unlikely tools.
>
> **Mahoney:** Are we going on an adventure?
>
> **Magorium:** Well, my dear, we're already on one. All I will say is this: With faith… love… this block… and a counting mutant, you may find yourself somewhere you've never imagined.

Part 2)

Mahoney: Then why are you leaving?

Magorium: It's my time to go.

Mahoney: That's it?

Magorium: What else could there be?

Mahoney: What are we gonna do without you?

Magorium: Run the store.

Mahoney: Sir, I don't know how.

Magorium: That's why I gave you the Congreve Cube.

Mahoney: But it just sits there.

Magorium: What have you done with it?

Mahoney: I don't know what to do with it. It's a block of wood.

Magorium: Can you think of nothing?

Mahoney: Well, I'm sure I could think of a million things to do with it.

Magorium: There are a million things one might do with a block of wood, but, Mahoney, what do you think might happen if someone just once ... believed in it?

Mahoney: Sir, I don't understand.

10-09-10 *Ouch!* (to S. Hartmann)

I was just thinking it'd be nice to have the book on Nancy Cartwright's Phil Sci that you helped edit on my bookshelf . . . and then I saw the price! Ouch; maybe not.

Lately, I've taken an interest in her (never noticed her before) because of my newest thoughts on Hilbert space dimension as a "capacity". In that regard, Sections 6, 7, and 8 of this paper `http://arxiv.org/abs/1003.5209` might amuse you.

By the way, is it true that Cartwright doesn't take email (as it advertises on her home page)? In my files, I do actually have an email from her (turning down a conference invitation). I guess I ask because I am thinking about writing her, but I'd like to understand beforehand how to interpret the silence I'll most likely get in response!!

How are things going in your new chair? You must be getting settled in comfortably by now.

12-09-10 *Interpreting the Universe after a Social Analogy* (to D. C. Lamberth)

I guess because you're on my mind, I reread your article in the *Cambridge Companion* last night. It was as nice as I remembered it to be. I hope you realize from all that I've sent you (especially my last paper, "QBism, the Perimeter . . . ") that your title (that Jamesian phrase)— Interpreting the Universe after a Social Analogy— is exactly what I'm after and what I think our QBistic analysis of "Wigner's Friend" in quantum theory (in Section III) leads straight away to. There's a world of interesting physics in front of us, and the time is ripe—I've just got to muster the resources and sustain the community's attention long enough for this line of thought to sink in!

There's one sentence in your paper that quite captures a distinction between my own approach to quantum foundations and an opposing theme that gets more and more airplay—relationalism (though so far, no one has put much substance behind it). See, for instance: `http://en.wikipedia.org/wiki/Relational_quantum_mechanics`. You pointed out that "James favors a philosophy in which all our dynamic relations in the world are cast (metaphysically) as reciprocal rather than merely relational." Certainly for me. The relational interpretations of quantum theory are all monistic to the core, and all too block-universey for my taste.

I liked your remarks on "compenetration" on page 246 and "novelty" on pages 256–257 as well.

David's Reply

> Thanks for this. I am amused, because this piece has received the most mixed reviews of things I've written, and most readers don't really know what to make of it. The comments you make are right on the mark with what I was after, and what I find remarkable and useful in James's understanding of experience. He had to resort to the language of panpsychism, which is confusing at base because most panpsychists understood consciousness precisely in the way James criticized in his "Does Consciousness Exist." So most have missed the point. This connection, and whether it can be ramified in terms of physics, is what interests me about your proposal here and from the outset.
>
> I expect to be sending you the two documents needed by tomorrow afternoon—I have time mapped out to finish them then

12-09-10 *New Questions* (to D. C. Lamberth)

I've already asked you where you might think a good location for the conference might be (and I hope you'll answer!), but I'm gearing up to write the "conference section" of the proposal, and I'm starting to realize that I'll need more information.

Could you supply me with a list of 5 to 10 or more (the more the better for sure at this stage) people that we can list as potential invitees. Range your mind over:

1. Philosophers of pure experience
2. Philosophers of neutral monism
3. Pragmatists
4. Disciples of the new realists (are there any left?) like R. B. Perry, W. T. Marvin and that lot
5. Panpsychists
6. Interesting Deweyians who have something to say on "experience"
7. Anyone who has anything interesting to say on Mach's world elements (I for instance have been quite taken with Erik Banks' work, maybe there are others)
8. Any Bergsonians?
9. Pragmatists you have ongoing debates with? (Like the trouble-in-river-city guy!?)
10. Any disciples of Russell's neutral monism?

I know plenty of standard philosophers of science who know a little about pragmatism (and even say they are pragmatists or turning pragmatist) like Huw Price, Arthur Fine, Richard Healey, and some others—and they, I imagine, will provide some good connective material for the meeting (go-betweens on quantum and philosophy), but they're not quite optimal for the things I really want to get at.

12-09-10 *I Don't Know That I Had Ever Seen This One Before* (to R. Schack)

Richard Jeffrey, "De Finetti's Probabilism," *Synthese* **60**(1), 73–90 (1984).

12-09-10 *Nor This One* (to R. Schack)

Richard Jeffrey, "Carnap's Voluntarism," in *Logic, Epistemology, and Philosophy of Science IX*, edited by D. Prawitz, B. Skyrms, and D. Westerståhl, (Elsevier, Dordrecht, 1994).

12-09-10 *Books: Free To Good Homes* (to T. Slee)

Slee-ism 5: *We have finally accepted that there are many books on our shelves that we are not going to read again.*

For the next few days these books will be on our driveway at 178 Herbert Street, free to good homes. Please feel free to browse and to take as many as you want.

I just discovered them and gave some Jungian readings (I promise) a good home!

13-09-10 *Pauli's Dreams* (to T. Slee)

I just noticed that I should probably thank Lynne more particularly for the Jung books: I noticed the name Shelagh Supeene is signatured in the front. (A relative? Or another name for Lynne?)

Anyway, on page 116 of the Dreams book, the following line can be found: "The material consists of over a thousand dreams and visual impressions coming from a young man of excellent scientific education."

That young man was Wolfgang Pauli (revealed years later). And I learned from Atmanspacher—one of the personnel in the proposal I'm writing—that another 1,000 pages of Pauli's dreams are soon to be published. (This time, not 1,000 dreams, but 1,000 pages! Or at least that's what he said.) Another one of the personnel in the proposal is my friend Hans Christian von Baeyer. (Maybe you've seen some of his semi-popular books on physics? The one on 'information' is excellent.) I attach one of his articles on Pauli, the man, "Wolfgang Pauli's Journey Inward," in case you're interested.

Thanks to Lynne again!

14-09-10 *The Latest Cleanest Latest!* (to H. C. von Baeyer)

von Baeyerism 65: *The project description reads well. My only negative reaction is to the Timpson bit, which sounds a little immature. After all, when Feynman gave a lecture to physicists,* *he would go out of his way to describe Newton's second law. So to accuse philosophers of science (no less) of not knowing their subject, on the basis of one man's careful definition of terms, seems wrongheaded.*

Well ... as an initial compromise to throw on the table, I took out the "no less!" ... which, after your remark, I started to think was a bit uppity my own self.

... BUT ... BUT ... At the same time I discovered that he had misspelled Peirce (as Pierce). Now what does that tell you about these philosophers of science?

Here's the present version of the passage (I took the liberty of fixing the spelling on Peirce):

> Crucial to our whole effort is fielding the ideas behind QBism more thoroughly in a larger community of expertise. No research effort with a hope to grow and flower can be an island unto itself. But as should be clear by now, QBism is in quite a predicament when it comes to placing itself in a community of appropriate peers. Part of its roots are in quantum information, but how many quantum information theorists have ever read the correspondence between Wolfgang Pauli, Markus Fierz and Carl Jung, where so many of Pauli's ideas on "background physics" were worked out? How many pragmatist philosophers and experts on William James know the distinctions between the frequentist and Bayesian conceptions of probability? How many know the details of quantum mechanics? Vice versa, how many quantum information theorists are there out there who think the philosophy of pragmatism simply means, "Shut up and calculate!"? Maybe to the greatest surprise, it is telling that C. G. Timpson in his review article on QBism in *Studies in History and Philosophy of Modern Physics*—a philosophy of science journal—felt compelled to include a footnote stating, "Pragmatism is the position traditionally associated with the ... American philosophers Peirce, James and Dewey; its defining characteristic being ..." Apparently even the philosophers of science who lay judgment on quantum theory are not particularly familiar with pragmatism!

14-09-10 *From Coleman to Cuero* (to D. C. Lamberth)

Many thanks for this! [...]

It was interesting learning that you were born in Coleman. I know I've traveled near it, if not through it, on the small-road crazy route I used to take between Albuquerque and my own hometown, Cuero. Apparently we were born just 262 miles from each other. And probably temporally close as well; I was born October '64.

The new name I'm toying for the conference is

From Pure Experience to Elementary Quantum Phenomena:
William James, John Wheeler, Quantum Interpretation

and I've tentatively put it "near Chocorua."

14-09-10 *The James Meeting* (to R. Schack)

Lamberth pointed out this place as a possibility for the James meeting: `http://www.asticou.com/` say, in June. (He said tourists don't start coming until July.)

Looking in Richardson's biography of James, I see that I can give as a narrative that it was just after the completion of this time in Maine, at the Northeast Bay, that James brought to a close and bound his notes on radical empiricism.

Or at least that's the general target area, and maybe the prices are typical for something we'd find up there.

> For summer jaunts James went twice in 1906 to Maine. The first time, in July, he went to Northeast Harbor and Mount Desert, and the second time, in August, to Penobscot Bay, Rockland, North Haven, Isle au Haut, and Swan's Island. But it was pragmatism that was on his mind. Between trips he wrote exultingly to Schiller that 'things are drifting tremendously in our direction.' Never hamstrung by modesty, James wrote, 'It reminds me of the Protestant reformation!'
>
> There was one casualty of James's rush toward pragmatism: somewhere along the way he benched radical empiricism. ...
>
> ... Perhaps it was his turning away from what he scornfully called 'technical' work, perhaps it was the force of 'The Miller-Bode Objections,' perhaps it had something to do with his fervid new interest in pragmatism. Whatever the reason, radical empiricism moved to the background as a program and a professional pursuit for William James ...

Actually that latter set of places are much closer to where John Wheeler's home was: High Island, Maine. It might be an interesting theme if we could find a good hotel closer to that region.

15-09-10 *Your Offer Taken!* (to R. Schack)

Schackcosm 135: *I attach my attempt at the budget narrative, at the moment excluding the workshops. Regarding the project timeline and the cost effectiveness sections, they should refer to the outcomes and outputs sections. Do we have such sections?*

Regarding outputs, it's relatively easy. 2 books, say 4 papers each on Jamesian things, crypto and randomness things, SICish things, and 2 workshops.

Outcomes, I am almost tempted to say that it is all towards the one big outcome, a new conception of the world or something like this. The cost effectiveness factor would be something like $720,000 per revolutionary world view, which is a bargain indeed.

I loved that!!!! I laughed and laughed. The family already thinks I'm going crazy because I stood in the yard yelling and wagging my finger at a squirrel the day before last. They almost had a look of horror when I kept laughing and laughing at your "$720,000 per revolutionary world view, which is a bargain indeed."

I dare say I like the idea too! It meshes well with something Hans said, late, just before I went up to bed. Placed below, and cc'd to Hans, so each of you can see each other's thoughts on telling Templeton "we're important."

I'm on my first cup of coffee and getting into the groove ...

15-09-10 *Outcome 1* (to R. Schack)

What will be different and who will be affected:
Mathematical Research:

> QBism is one of the first efforts in quantum interpretation that we are aware of that has said, "A proper interpretation of quantum mechanics should almost close its eyes to the existing mathematical formalism of the theory, and instead IMPLY what

the formalism OUGHT to be." In that regard we have already had some success–our rewriting of the Born Rule is an example of an interpretation-driven change in the very formalism itself–but of course, we want more. In the end, we expect of a Quantum Bayesian interpretation of quantum mechanics that it should actually imply the full mathematical formalism of the theory. An interpretation that did that alone would already be revolutionary in the quantum foundations wars.

Who will be affected? The whole quantum foundations community, for it will hold up an example of how things ought to be done: Interpretation first, quantum formalism next. (Not like, say, the Many Worlds Interpretation views the order of affairs.)

What are the indicators of the difference and the measurable change you expect:

Recently, we got a referee report for a submission to *Reviews of Modern Physics* that had these lines in it, "This is as strikingly novel a way of looking at the quantum theory as Feynman's sum of amplitudes over histories was when it appeared in the late 1940s. Whether it will prove as fruitful and durable remains to be seen, but it ought to be known to a broad audience ...." When we get referee reports like that, we certainly get the sense that we are making a useful contribution to the physics world as a whole.

We want more referee reports like that! And we will strive our best to get them.

But an obvious indicator of how much a scientific result is being noticed is how many times the result is cited per year. We will be watching that closely, but also noticing simply how much "buzz" the view gets as develops ever more.

15-09-10 *Outcome 2 (half of it)* (to R. Schack)

Conceptual Research:

We feel that in the last few years we (QBists) have turned a corner in our understanding by realizing that the deepest import of QBism is that it implies a significantly nonreductionist reading of quantum theory. One of the two great, fundamental theories of physics by its own structure, puts a stop to the reductionist dream that is usually identified with "science itself." This, we feel, instates a humanistic face to physics that has the potential not only to affect physical practice, but also the worldview of even lay scientific enthusiasts (readers of *Scientific American* and such).

Also with the success of this reading of quantum theory, we expect to see the philosophy of physics community for the first time take note of old-style American pragmatism, and perhaps even squeeze their own golden nuggets from it.

Now the question is, what to write for the "indicators" half of this!!

15-09-10 *Outcome 2's INDICATOR* (to R. Schack)

Indicator:

- Count the number of philosophers who stop talking so seriously about Many Worlds (more generally, views that take quantum states as objective properties) and start talking about Pauli's ideas, Bayesianism and pragmatism instead. This is not a joke. It is the method we have used for years to measure the power of our view. We can list (and point fingers to appropriate publications of) philosophers Bub, Demopoulos, Healey, and Stairs, as examples exhibiting themselves as cross-overs in the Bohr-Einstein debate, adopting one or another element close in spirit to the QBist view (even if not remotely the whole QBist package).

We certainly want more of that and will have our eyes open for it and pass on any anecdotal information to JTF.

- More generally the usual markers: invitations to conferences, keynote speeches, citation counts, and reports in the lay press. Also invitations to write articles ourselves for the lay public (as in our recent invitation from *Scientific American*).

15-09-10 *Outcome 3 and Indicators* (to R. Schack)

If you can see anything to add, please suggest.

Workshops:

> The key goal of the two workshops is to get parts of the disparate communities of quantum information physicists, Bayesians, pragmatist philosophers, and Pauli scholars all talking to each other and becoming aware that there is overlap in their interests and concerns. With this, we hope to start to build a community capable of understanding QBist ideas, and even capable of building upon them (at least in spirit, even if not completely in letter). QBism needs a wider community to interact with–to educate and to learn from itself–and the time appears ripe for making this happen.
>
> The conference proceedings arising from this activity will be invaluable resources for the worldwide Bayesian, Paulian, quantum, and pragmatist communities to get to know each other.

Indicators:

- As in the example provided, pre- and post-questionnaires, testing the various community's knowledge of each other is a very good idea, actually.
- Collect anecdotal evidence of friendships and professional ties made at the meetings.
- Collect data six months and a year down the road from the meetings on the network of active email ties between participants.
- Track sales and professional reviews of the conference proceedings.
- Collect data on conference participants starting to take part in one of the other community's conferences.

15-09-10 *Unruh's Talk* (to the QBies)

You can imagine I'm still doing Templeton things—it is literally the hardest grant proposal I've ever written. But I did want to encourage you all to go to Unruh's talk today in my place! The great WG is a man who has made a very deep contribution to physics (that walking through space heats it up), and it's actually rare that we get physicists coming through PI of that stature in nature's eyes.

16-09-10 *The Enduring Impact* (to R. Schack)

> Realistically, we hope the efforts of this project, when all is said and done, will be identified with a small span of history when a tide was turned. It would be nice to change the world fully and deeply in two years, but such things can never be planned for (and perhaps dare not be hoped for). A turning of the tide of this 85-year-old debate on quantum foundations would already be a revolution in itself. QBism points in a direction that says quantum theory's great lesson is not that IT is incomplete (that there is another theory waiting to be uncovered from underneath it), but that the world itself is unfinished—it is still in the furnace of creation. If our efforts contribute to getting our fellow physicists to take this possibility seriously, even if only enough that its residue shapes their work and their questions ever so slightly below the threshold of awareness, then the results of this project will be very pleasing indeed. Physical thought is a collective effect; one physicist affects another in turn with every paper and every experiment. If QBism does point in the right direction, as we believe it does, and if a tide is turned, things will eventually take care of themselves.

16-09-10 *The Final Submission* (to H. C. von Baeyer)

FYI, here's the final thing, warts and all. You'll certainly note, if you care to amuse yourself, that my answers become more and more uncouth as the deadline drew nearer (particularly in the budget justification section, where some of their questions became truly ridiculous). Story below to Rüdiger; I got the thing in just 2 minutes before its deadline!

Wow, what work! And what work for nothing but a chance! Still, there were lessons learned from the exercise, and maybe that's what in the end counts:

1) Let me attach Appleby's letter of support. In it he makes a good point about quantum theory being initially based on a mechanical analogy, and that a wrong turn was taken for interpretation just right there. I liked it and had never quite thought about it that way. It'd be nice if we could squeeze something about that into the *Sci Am* piece in some way.

2) Like with a good joke, I amuse myself sometimes. Here's something I wrote for one of their more worthless fields of questioning:

> QBism is one of the first efforts in quantum interpretation that we are aware of that has said, "A proper interpretation of quantum mechanics should almost close its eyes to the existing mathematical formalism of the theory, and instead IMPLY what the formalism OUGHT to be." In that regard we have already had some success—our rewriting of the Born Rule is an example of an interpretation-driven change in the very formalism itself—but of course, we want more. In the end, we expect of a Quantum Bayesian interpretation of quantum mechanics that it should actually imply the full mathematical formalism of the theory. An interpretation that did that alone would already be revolutionary in the quantum foundations wars.

That really is a key point, and I don't think I've said it that way before. So I should thank the JTF. Or maybe I *have* said it that way. Anyway, the key thing is this. What sets QBism apart from Many-Worlds, say, is that

1. *they* take the formalism as given (they've got no clue why Hilbert space vectors) and try to make up a story for it w.r.t. what it could mean in common life, but

2. *we* take a story, and try to show how it implies the mathematical theory.

There's no doubt our job is harder than theirs: It's easy to make up stories to fit something one doesn't understand. Or another (slightly sophomoric) way to put it: We're doing what physics is all about—noting things, expressing them, and then trying to find a mathematical description that captures their essence—while they're obsessed with a *homework* problem. (Imagine the voice of Darla in *Our Gang*.) "Teacher said 'Hilbert space'. Now we've got to make a story that uses the word five times."

Anyway, like with Appleby, it'd be nice to try to really bring this point home in the *Sci Am* article: I.e., The story has to come first, and then the equations second, or it is not *deep* physics, it's just a homework problem. (Taylor and Wheeler?)

I do hope you're still engaged with QBism and not getting fed up with all my shenanigans. Thanks again (for the near hundredth time) for all the help you've given me. You're teaching me discipline in writing; even Mermin didn't do that; and I do appreciate (even when I stray otherwise). You're my mentor.

Good night, I say, ... as you're probably nearly about to wake up ...

16-09-10 *New Religion* (to D. B. L. Baker)

It's nearly 2:00 AM for me, and I've been celebrating getting this thing (attached) in literally just two minutes before the bloody deadline! (I.e., a wee dram of Scotch and such things.) But, not lying; two minutes. Have a read. Is it just Methodism all over again? Or is it something else? I'd almost like to think it's a new religion. And I've got credentials too ... I'm asking for money! (Not even based in Houston, TX, but I'm asking for money. How else would you define "religion"?) But to Sir John Templeton, I told him it's science.

Religion or science? Not such a clean line is it? Who would expect otherwise from two Cuero degenerates?

17-09-10 *Free Will Saved James's Life* (to the QBies)

William James's Diary Entry, 30 April 1870

> I think that yesterday was a crisis in my life. I finished the first part of Renouvier's second "Essais" and see no reason why his definition of Free Will—"the sustaining of a thought because I choose to when I might have other thoughts"—need be the definition of an illusion. At any rate, I will assume for the present—until next year—that it is no illusion. My first act of free will shall be to believe in free will. For the remainder of the year, I will abstain from the mere speculation and contemplative *Grüblei* in which my nature takes most delight, and voluntarily cultivate the feeling of moral freedom, by reading books favorable to it, as well as by acting. After the first of January, my callow skin being somewhat fledged, I may perhaps return to metaphysical study and skepticism without danger to my powers of action. For the present then remember: care little for speculation; much for the form of my action; recollect that only when habits of order are formed can we advance to really interesting fields of action—and consequently accumulate grain on grain of willful choice like a very miser; never forgetting how one link dropped undoes an indefinite number. *Principiis obsta*—Today has furnished the exceptionally passionate initiative which Bain posits as needful for the acquisition of habits. I will see to the sequel. Not in maxims, not in *Anschauungen*, but in accumulated

acts of thought lies salvation. *Passer outre*. Hitherto, when I have felt like taking a free initiative, like daring to act originally, without carefully waiting for contemplation of the external world to determine all for me, suicide seemed the most manly form to put my daring into; now, I will go a step further with my will, not only act with it, but believe as well; believe in my individual reality and creative power. My belief, to be sure, can't be optimistic—but I will posit life (the real, the good) in the self-governing resistance of the ego to the world. Life shall [be built in] doing and suffering and creating.

17-09-10 *Motivated By Our Pluriverse Discussion* (to the QBies)

`http://en.wikipedia.org/wiki/Universal_set`

20-09-10 *Back from the Mountaintop* (to P. Wells)

Thanks for the article! Sorry to take so long to say "thanks," but when I got your note Thursday I was exhausted, and my writing fingers were broken. Not an easy trip back down from the mountaintop: I had just spent several days in a frenzy trying to get a proposal written for the John Templeton Foundation—toughest grant proposal I had ever written.

I loved your James 1878 quote (and your play on it at the end of the article); where did you get it from? Was it "The Sentiment of Rationality"? I liked too your "read like a forecast of quantum theory." That's the heart of the matter; that's why there's a shrine to him. I was impressed by your sensitivity (at least in my case) to what was going on around you.

Great article.

20-09-10 *Small Vision* (to H. Barnum & R. Schack)

In broadest outline, here's the way I think maybe I can envision how this could become a report.

1) It could start with a section saying, here is how the convex-operational framework is understood. States, effects, linear maps, and all that. Cones, blah, blah, blah. Maybe most of it can be cut and pasted from one of Howard's papers.

2) On the other hand, here's the QBist framework. Not states and effects, but modified law of total probability. No mention of cones directly at all! The QBist hope is that consistency of the modified law alone will imply a significant nontrivial restriction on the parts of the simplex that should be used (i.e., state space), and similarly with the conditional probabilities through the principle of reciprocity.

3) Now show that the QBist framework can be rewritten in convex-operational terms and comes automatically with these statements about the cone of effects.

4) Speculation on next steps.

Enough for a conference proceedings and we'll have a resource for others who are scratching their heads over whether QBism and Coneheadonism have any connections.

21-09-10 *Whitehead on James* (to D. M. Appleby)

I just haven't been able to concentrate so much today, so I took the easy way out reading "people magazines" for the most part.

Here is that quote from the letter Whitehead wrote Hartshorne in January 1936:

> European philosophy has gone dry, and cannot make any worthwhile use of the results of nineteenth century scholarship. It is in chains to the sanctified presuppositions derived from later Greek thought .... My belief is that the effective founders of the renascence in American philosophy are Charles Peirce and William James. Of these men, W. J. is the analogue to Plato, and C. P. To Aristotle, though the time-order does not correspond, and the analogy must not be pressed too far. Have you read Ralph Perry's book (2 vols.) on James? It is a wonderful disclosure of the living repercussions of late 19th century thought on a sensitive genius. It is reminiscent of the Platonic Dialogues. W. J.'s pragmatic descendants have been doing their best to trivialize his meanings in the notions of Radical Empiricism, Pragmatism, Rationalization. But I admit W. J. was weak on Rationalization. Also he expressed himself by the dangerous method of overstatement

And here is another passage I came across; it comes from Whitehead's book *Modes of Thought*. I'll place it below. Personality drivel mostly, but still I wanted to record it, and you seemed like the most natural repository.

> In Western Literature there are four great thinkers, whose services to civilized thought rest largely upon their achievements in philosophical assemblage; though each of them made important contributions to the structure of philosophic system. These men are Plato, Aristotle, Leibniz, and William James.
>
> Plato grasped the importance of mathematical system; but his chief fame rests upon the wealth of profound suggestions scattered throughout his dialogues, suggestions half smothered by the archaic misconceptions of the age in which he lived. Aristotle systematized as he assembled. He inherited from Plato, imposing his own systematic structures.
>
> Leibniz inherited two thousand years of thought. He really did inherit more of the varied thoughts of his predecessors than any man before or since. His interests ranged from mathematics to divinity, and from divinity to political philosophy, and from political philosophy to physical science. These interests were backed by profound learning. There is a book to be written, and its title should be, 'The Mind of Leibniz'.
>
> Finally, there is William James, essentially a modern man. His mind was adequately based upon the learning of the past. But the essence of his greatness was his marvelous sensitivity to the ideas of the present. He knew the world in which he lived, by travel, by personal relations with its leading men, by the variety of his own studies. He systematized; but above all he assembled. His intellectual life was one protest against the dismissal of experience in the interest of system. He had discovered intuitively the great truth with which modern logic is now wrestling.

22-09-10 *The Quote on Sacred Matter!!* (to P. Hayden)

> To anyone who has ever looked on the face of a dead child or parent the mere fact that matter *could* have taken for a time that precious form, ought to make matter

sacred ever after. It makes no difference what the principle of life may be, material or immaterial, matter at any rate co-operates, lends itself to all life's purposes. That beloved incarnation was among matter's possibilities.

22-09-10 *The Tube Alloys Project* (to B. W. Schumacher & M. D. Westmoreland)

I greatly enjoyed our conversation on "pure indeterminism" after your talk. (Ben will understand the allusion in the title; he can explain.)

Attached are two notes from my collection somewhat to do with the issue of "probability + x" that we were talking about. The first one is a (rather nice) note to Abner Shimony introducing the second one. The second one is a rather rabid one to a Princeton professor who annoyed me. (He called me a pansy, basically.) Anyway, it happens to be the one with the key point (starting right after McDonaldism 6). [See 18-10-06 note "Real Possibility" to A. Shimony and 30-01-06 note "Island of Misfit Toys" to K. McDonald. It is McDonaldism 12 in the present compilation.]

I liked Ben's image of the alloy: I think it's much more of the right flavor than the simple "+" I had been using.

22-09-10 *Tube Alloys 3!* (to B. W. Schumacher & M. D. Westmoreland)

Actually I do a much better job on the point in the attached page, drawn from a recent paper. The good stuff is in the left-hand column, particularly Footnote 14:

> Most of the time one sees Bayesian probabilities characterized (even by very prominent Bayesians like Edwin T. Jaynes) as measures of ignorance or imperfect knowledge. But that description carries with it a metaphysical commitment that is not at all necessary for the personalist Bayesian, where probability theory is an extension of logic. Imperfect knowledge? It sounds like something that, at least in imagination, could be perfected, making all probabilities zero or one—one uses probabilities only because one does not know the true, pre-existing state of affairs. Language like this, the reader will notice, is never used in this paper. All that matters for a personalist Bayesian is that there is uncertainty for whatever reason. There might be uncertainty because there is ignorance of a true state of affairs, but there might be uncertainty because the world itself does not yet know what it will give—i.e., there is an objective indeterminism. As will be argued in later sections, QBism finds its happiest spot in an unflinching combination of "subjective probability" with "objective indeterminism."

27-09-10 *Pauli's Eye Quote* (to H. Atmanspacher & H. C. von Baeyer)

Greetings from Stockholm!

Can either of you quickly pin down where I can find an English translation of the nice thing that Pauli wrote Heisenberg, just a bit before Heisenberg found the uncertainty relation? It is something about being able to look through the x-eye or the p-eye, but not being able to open both eyes at once.

I want to use that as a paradigm example that not all great contributions to physics are explicit and based on publication. A lot goes on behind the scenes.

Harald's Reply

It's in P to H Oct 19, 1926 (letter 143, p. 347 in P's corr vol.1), actually in a discussion about collision processes (my translation):

> It's always the same thing: due to diffraction, there are no arbitrarily thin rays in the wave optics of the ψ-field, and it is illegitimate to assign ordinary 'c-numbers' to 'p-numbers' and 'q-numbers' simultaneously. One may then view the world with the 'p-eye' and one may view it with the 'q-eye', but if you want to open both eyes at the same time this drives you crazy.

Note that P does NOT say it is IMPOSSIBLE to open both eyes. He says (in German: "dann wird man irre") that it drives you crazy, or maybe a bit weaker, that it is mind-boggling. [Compare the perception of ambiguous figures like the Necker cube.] The letter was sent to H at Copenhagen, and generated a lot of enthusiasm among H, Bohr, Dirac and Hund who were all there at the time (as H reports in his reply of Oct 28). H's paper "Ueber den anschaulichen Inhalt der quantentheoretischen Kinematik und Mechanik" in ZPhys was received there March 23, 1927.

Another example would be P's postcard of 4 Dec 1930 to the Tuebingen conference on radioactivity where he predicts the neutrino (my translation):

> ... in connection with the 'wrong' statistics of the N and Li6 nuclei as well as the continuous β-spectrum, I hit upon a desperate remedy for saving the 'alternation law' of statistics and the energy law. This is the possibility that there may be, in the nuclei, electrically neutral particles, which I shall call neutrons [later: neutrinos], which have spin 1/2, obey the exclusion principle and, moreover, differ from light quanta by not traveling with the velocity of light. Their mass should be of the same order of magnitude as the electron mass and not greater than 0.01 proton masses. The continuous β-spectrum would then be understandable under the assumption that in β-decay a neutron will be emitted together with an electron, such that the sum of their energies is constant. ... (letter 259, p. 39–40, in P corr vol.2)

29-08-09 *Eight-Port Homodyne Detector* (to M. Sasaki)

I almost forgot, here is the reference I was telling you about `http://arxiv.org/abs/0708.4094`. For myself, it is far too technical to read, but I believe I understand the conclusion: By suitably adjusting the "parameter" mode, one can measure any Weyl-Heisenberg covariant observable. In finite dimensions (up to $d = 67$ at least), one can always construct a Weyl-Heisenberg SIC-POVM by suitably choosing the right fiducial state. (See: `http://arxiv.org/abs/quant-ph/0310075`.) The question then would be, for continuous variables, what sequence of "parameters" would converge to something like a SIC-POVM.

As I say, the Kiukas/Lahti reference is too mathematical for my tastes, but it should have references in it that are more understandable.

29-08-09 *QBism House* (to N. Waxman)

Waxmania 3: *All the best (and remind me to send Kiki pictures of my mom's 150 year old house on a cliff here—it was once an epic fixer upper she would truly appreciate!),*

OK, I'll get in touch with you Monday. And it'll be fun to see the pictures. I myself am thinking about doing a feature for the newsletter if you'll take it, titled "QBism House Open for Business". I'd like to use a couple pictures of our house in it, particularly one showing our porch with chalkboard installed and my floor to ceiling library. I'd explain how my group of six (myself, Appleby, Ericsson, and three grad students) will be using the areas as an annex to PI for research. Say some things about what the research is about, and where we want to go with it. And welcome anyone else who might have an interest in the subject to join us: In good weather, the plan is to have a weekly group meeting on the porch.

You should see the house, if you haven't seen it in a while: It's just about finished on the outside now (except for planting some better grass). And 95 percent finished on the bottom floor. I'm quite proud of it.

Let me know when you're available next week, and we can get together on the webpage stuff.

29-09-10 *Quantum Foundations in the Light of Quantum Information III* (to G. Brassard)

Absolutely! Of course! Yes! Wonderful! Thanks! Please! Cool! Count me in!

29-09-10 *Perfect Contrast* (to H. C. von Baeyer)

I am in the middle of Göran Lindblad's talk, where he had the most lovely (and arrogant) quote by Steven Weinberg. Here's an extension of it that I just dug off the web:

> Bohr had presided over the formulation of a "Copenhagen interpretation" of quantum mechanics, in which it is only possible to calculate the probabilities of the various possible outcomes of experiments. Einstein rejected the notion that the laws of physics could deal with probabilities, famously decreeing that God does not play dice with the cosmos. But history gave its verdict against Einstein—quantum mechanics went on from success to success, leaving Einstein on the sidelines.
>
> All this familiar story is true, but it leaves out an irony. Bohr's version of quantum mechanics was deeply flawed, but not for the reason Einstein thought. The Copenhagen interpretation describes what happens when an observer makes a measurement, but the observer and the act of measurement are themselves treated classically. This is surely wrong: Physicists and their apparatus must be governed by the same quantum mechanical rules that govern everything else in the universe.

The reason I say it is the perfect contrast to us is because QBism rejects exactly what Weinberg takes as unquestionable: "Physicists and their apparatus must be *governed* by the same quantum mechanical rules that govern everything else in the universe." QBism says quantum theory doesn't "govern" anything in the physical world, in the same way that raw probability theory doesn't *govern* anything in the physical world. Quantum theory is something physicists *use*—that was implicit in Bohr's view of QT, as any careful reading of him would show. This is why I say Weinberg is one hell of an arrogant guy when he calls Bohr's view "deeply flawed". He didn't understand a word of it. Humbug!

01-10-10 *Great Minds Think Alike* (to S. Hossenfelder)

Marek reminded me last night of Wigner's remark on von Neumann. I had indeed known that once (long ago), but forgot about it. Sorry I didn't bring it up yesterday. Anyway, it looks like you're thinking like von Neumann. See Footnote 1 in the attached paper. [E. Wigner, "On Hidden Variables and Quantum Mechanical Probabilities," Am. J. Phys. **38**, 1005–1009 (1970).]

03-10-10 *Scaling, of a QBist Flavor* (to M. A. Graydon)

Just a small break from the logistical matters. I think your pushing me on the Hardy axioms has been quite needed. In the following form, I think I might find the composite system law more palatable from a QBist philosophy.

The way I'd like to transform the question is this: For a given number of outcomes in the sky (for a given measure of how much matter we have), how much of a deviation from the law of total probability should we expect in an urgleichung? How does the urgleichung scale with the amount of matter?

We could say as an axiom, β should be multiplicative in those cases when n is multiplicative. And my guess is that that would force upon us the same functional equation Hardy explored.

OK, but now why that particular choice of parameters to assume mutual multiplicativity for? Why not not n and α, for instance? Or could one show that any other choice of pairs for multiplicativity would lead to a contradiction with some other axiom?

I'm reminded of a speculation I've told of Bohr. Somewhere I've read that when Bohr got the idea of quantizing the hydrogen atom, he locked himself away for like three weeks to do the calculations. But you know that it's no more than a night or two of homework to figure out the energy level spacing from Bohr's assumption of simple integer quantization of angular momentum. How could it take Bohr three weeks? I've speculated that at the beginning, he simply didn't know which variable to quantize in terms of simple integer values. And thus he set out on a random walk to find out which scheme would be simple and believable and would give the needed energy level spacing.

Maybe that's the way to approach this problem. "Distinguishable states" and "degrees of freedom" … humbug! (It's nearly hidden-variable talk. Or at least evocative of the imagery.) But "how much stuff" (capacity) and "how much deviation from a naive use of law of total probability"? Now we're cookin'!

05-10-10 *The Old Bayesian Motivations* (to P. J. Lahti)

Thanks very much for your curiosity about my research program. I was very flattered and very much enjoyed my conversations with you.

Let me point you to three papers in case you have any continuing curiosity.

The first really tries to make a connection between the Bayesian motivations and the direction of formalism I seek: `http://arxiv.org/abs/1003.5209`. The second lays out some (provisional) axioms: `http://arxiv.org/abs/0912.4252`. And the third tells something about how to get some of the structure of quantum-state space back out of all of this: `http://arxiv.org/abs/0910.2750`.

Much of it, in the end, will have to do with the story you tell over and over: For the SICs are just a particularly crisp way of simultaneously measuring position and momentum (in Weyl's discrete phase space).

I hope you will view our efforts as complementary!

05-10-10 *Quote of the Day* (to the QBies)

I've rigged Google to give me a "quote of the day" every day now. Here was today's pick:

> Equations are the devil's sentences.
>
> — Stephen Colbert

05-10-10 *Your Newest Book* (to J. W. Moffat)

I read about your new book in the newspaper after getting home from Sweden Sunday (thus I'm not sure which day's newspaper I was looking at). Anyway, it sounds quite interesting, and you can be sure I'll be going to the bookstore once I hear that it is out.

John's Reply

> Yes, there was an article in last Saturday's *Waterloo Record* about me and my new book called: *Einstein Wrote Back*. It will be published by Thomas Allen & Sons, Toronto, about the middle of this month. My first book was: *Reinventing Gravity*, published 2008 by HarperCollins NY and Thomas Allen, Toronto.
>
> Thanks for your interest in my book.

05-10-10 *Galoshes* (to M. A. Graydon)

I won't tell you what I put in my letter, but I will tell you this much: You have some damned big shoes to fill now. I expect to see some really important physics from you.

The more I think about it, the more I'm extremely pleased with the thought direction you've pushed us to. The game is to get in a position of having Hardy's functional equation *without* speaking of composite systems and local measurements. I like having this image in my mind. I throw one rock into a bag and place it on a scale; the scale goes down a bit. I throw two rocks into a bag and put it on the scale, and the scale goes down a bit more. Now I ask myself, if confronted with a bear, do I want to whack it in the head with the first bag, or the second? The second of course—it has more oomph! It's got nothing to do with any local tomographic properties, etc. It's all about how the oomph scales. It's all about how the oomph scales with the stuff.

So, here's a question. Suppose *before* getting to Assumption 6 we make the assumption that n is a function of β. And that when

$$\beta = \beta_1 \beta_2$$

that

$$n(\beta_1 \beta_2) = n(\beta_1)\, n(\beta_2) \; .$$

The sort of thing I'm wondering is, will this allow us to clean up Assumption 6 any? Will it allow us to put something much better in its place?

07-10-10 *Wolfgang Pauli and Alchemy* (to W. R. Newman)

I enjoyed meeting you last night. I feel a bit embarrassed that I was not so coherent in our conversation, but wine does that to me.

I just placed an order with Amazon for your *Promethean Ambitions* book. I look forward to reading it.

Lollygagging away a little time today, I decided to re-read some things in my accumulated notes on the intersection of Pauli and alchemy. I'll share those with you, in case you have any interest. They're in the attached file. Here are the relevant parts:

1) The first quote from Werner Heisenberg's article, "Wolfgang Pauli's Philosophical Outlook" [217].

2) The long quote from Pauli's "unpublished" draft "Modern Examples of 'Background Physics'" [336].

3) "The Influence of Archetypal Ideas on the Scientific Theories of Kepler" [339] (This one is quite good reading.)

4) "Ideas of the Unconscious from the Standpoint of Natural Science and Epistemology" [343].

5) Letters from Pauli to Jung [354].

08-10-10 *History* (to A. Wilce)

Wilce-ism 8: *I still owe you (and the world) a screed. Meanwhile, a question: do you know of a good book on the history of probability theory and/or statistics that talks at any length about early attitudes towards the Normal distribution? Any pointers towards material of this kind would be most welcome.*

Sorry for the delay. Probably the best possible things are Ian Hacking's two books, *The Taming of Chance* and *The Emergence of Probability.* Louis Menand's book *The Metaphysical Club* (one of the best books ever, in any case) has a nice chapter or two on Quetelet and his reception.

12-10-10 *The APS March Meeting* (to G. Chiribella)

Thanks so much taking me up on the invitation to speak. The information on the session is below, and the meeting will be March 21-25 in Dallas, Texas. Anyway, in this, you join some famous company: Lucien Hardy was the invited speaker one year, Rob Spekkens another, and I think Bill Wootters another. [...]

I never could find the right Feynman quote, by the way. It's one about how it takes a generation or two to get used to a new physical theory. The only thing that's really broken in quantum interpretation is just our intuitions, or something like that. If you know which quote I'm talking about, I'd be most grateful if you could send it to me.[227]

[227] R. P. Feynman, "Simulating Physics with Computers", Int. J. Theor. Phys. **21**, 471 (1982): "Might I say immediately, so that you know where I really intend to go, that we always have had (secret, secret, close the doors!) we always have had a great deal of difficulty in understanding the world view that quantum mechanics represents. At least I do, because I'm an old enough man that I haven't got to the point that this stuff is obvious to me. Okay, I still get nervous with it. And therefore, some of the younger students ... you know how it always is, every new idea, it takes a generation or two until it becomes obvious that there's no real problem. It has not yet become obvious to me that there's no real problem. I cannot define the real problem, therefore I suspect there's no real problem, but I'm not sure there's no real problem."

23.13.4: Quantum Information for Quantum Foundations
Organizer: Christopher Fuchs (Perimeter Institute for Theoretical Physics)

Description:

Richard Feynman once famously said, "..." This may or may not be true, but if one supposes it to be getting at the right idea, it behooves us to ask, "Then what would it take to readjust our intuitions so that the discord is alleviated?" Starting with John A. Wheeler, several in the last two decades have suggested that re-exploring the foundations of quantum theory in the language of quantum information may be the first step in a long needed therapeutic process. This focus session will be devoted to assessing the impact of quantum information theory in diffusing the perennial quantum mechanical mysteries, as well as providing novel ways to rewrite the theory in terms of informational structures and functions.

13-10-10 *Congratulations* (to C. G. Timpson)

Timpsonism 14: *Sorry you've not heard anything from me for* ages. *It's been an eventful year: Jane just gave birth to our baby daughter Catherine Jane on 5 August — a most wonderful thing! Such a joy to be a father; though it's interesting trying to teach logic on very little sleep ... Oddly, it seems ok trying to teach QM ... not sure what that says (about QM, or me ...)*

Many congratulations on your daughter; that is wonderful news! Children are the greatest classroom ever for learning about the world. Please give my best wishes to Jane, and of course to Catherine too!

I liked your quip on teaching logic. I think it means logic is forced and strained in our natural humanity, whereas quantum theory is actually a gut feeling we all already have! (It being simply a refinement of everyday life — a story of the consequences of agents taking actions upon the world.) It'd be fun to revive one of F. C. S. Schiller's polemics on logic for a short class reading, like the introduction (as I recall) from his *Formal Logic, a Scientific and Social Problem*. It'll make everyone laugh out loud. (No matter how much inspiration I draw from him, the guy was really and truly a nutcase ... and probably mostly should be kept in the closet.)

13-10-10 *Up in the Air* (to D. B. L. Baker)

Bakerism 7: *I have to admit I haven't been able to completely read your "New Religion" paper. I'm still working on it. Have you seen this entire quote by Samuel Butler?*

> A belief in human progress is a matter of faith. Progress does not necessarily imply a monotonically increased advance, but rather an advance that will eventually occur within the limits of mankind's collective morality and knowledge of its respective environment. It is common to hear both philosophers and non-philosophers complain that philosophy makes no progress. Whether such a complaint is justified depends, or course, on one's understanding of the nature of philosophy, and on one's criteria of "progress". All progress is based upon a universal innate desire on the part of every organism to live beyond its income.

Thanks for the recommendation. Sounds great. Don't worry about the "new religion" thing—it wasn't a paper at all, just a grant proposal ... all geared toward extracting money. (Though I definitely was honest in it with regard to my physical and philosophical beliefs. ... Now, with regard to the "outputs" and "outcomes" that they required me to divine, that might be a different story!)

I didn't know the Samuel Butler quote, and in fact I didn't know about Samuel Butler at all before your bringing him to my attention. So thanks! I just looked him up on Wiki, and will learn more as time goes on. I've got to read more of this guy. The quote certainly resonates with my "meliorist" tendencies. I'll match your Butler with a James or two. I like the little piece that starts in the right-hand column on the first page of "Meliorism 1" at the words, "Free-will is thus a general cosmological theory of promise ..." [See 20-09-07 note "Free Will" to Å. Ericsson.] I also like entry 247 in "Meliorism 2". [See "Pragmatism and Religion" section in 30-06-09 note "A New Name for Some Old Ways of Thinking" to M. Schlosshauer.]

In fact, I just opened up a nice new collector's item this morning that arrived at the door yesterday—a 1933 printing of Henry Call Sprinkle's Yale PhD thesis, *Concerning the Philosophical Defensibility of a Limited Indeterminism: An Enquiry Based upon a Critical Study of the Indeterministic Theories of James, Renouvier, Boutroux, Eddington, Bergson and Whitehead*.

So, the evidence you present to me (and the corroborating evidence I send to you) say that maybe this "new religion" is not so new after all! It's all old stuff really; it's just that science as usually practiced has forgotten it.

14-10-10 *Temporarily Imposed Titles* (to Å. Ericsson, B.-G. Englert, S. T. Flammia, & W. K. Wootters)

Dear Sessioneers!

Since I haven't heard back from most of you, I've gone ahead and made up some tentative titles for you and placed them into the APS system for the purpose of the planning meeting which I leave for tomorrow afternoon: 1) in Steve's case, the title of his last paper on the subject, 2) in Åsa's case, a minor variation of the title she sent me, 3) in Berge's case, the title of his second-to-last paper, and 4) in Bill's case, the title of his last talk on the subject. I.e.,

- The Lie Algebraic Significance of Symmetric Informationally Complete Measurements
  – Steven T. Flammia, California Institute of Technology

- Quantum States as Probabilities from Symmetric Informationally Complete Measurements
  – Åsa Ericsson, Institut Mittag-Leffler

- On Mutually Unbiased Bases
  – Berthold-Georg Englert, Centre for Quantum Tech., National University of Singapore

- States with the Same Probability Distribution for Each Basis in a Complete Set of MUBs
  – William K. Wootters, Williams College

I hope you don't mind me taking this liberty. But please don't forget that these are just tentative titles and can be changed up until November 19. For the remaining speaker, I'm aiming for an *experimentalist* doing MUB and SIC experiments. (I figure if we have an experimentalist, we might just have someone besides ourselves attending the session!) Keep your fingers crossed!

14-10-10 *(Even More Urgent) APS March Meeting Invitation* (to A. M. Steinberg)

I'm the APS March-Meeting program chair for the Topical Group on Quantum Information this year, and I'm presently in the hurried process of planning out three invited sessions for it. On two of them, I've been completely selfless (one theoretical, one experimental), but on one I've been quite self-indulgent (as I have noticed has been the tradition in previous years ... so I don't feel so bad). Anyway, as you might nearly guess, the title of that session will be "Symmetric Discrete Structures for Finite Dimensional Quantum Systems". (A little bit more detail can be found below in the sample invitation I had sent to Berge Englert.)

Here's a listing of four of its five speakers:

- The Lie Algebraic Significance of Symmetric Informationally Complete Measurements
  – Steven T. Flammia, California Institute of Technology

- Quantum States as Probabilities from Symmetric Informationally Complete Measurements
  – Åsa Ericsson, Institut Mittag-Leffler

- On Mutually Unbiased Bases
  – Berthold-Georg Englert, Centre for Quantum Tech., National University of Singapore

- States with the Same Probability Distribution for Each Basis in a Complete Set of MUBs
  – William K. Wootters, Williams College

The trouble is, I've been having a hard time thinking of a perfectly appropriate fifth speaker. Most of the guys that come to mind are either already burdened with coming to my Tulane U. "Clifford Lectures" the week before, or too mathematical to really be good salesmen for the subject at the March meeting, or both.

But today I had an idea! (I know, shoot me.) An *experimentalist* in the list would probably be the very best thing we could do for the fame and fortune of the subject! (I.e., that way somebody beside the speakers might actually attend the session.) Would you come and talk on your SIC and MUB experiments?

Please, please, please! (And please let me know as soon as possible. I fly to DC for the planning meeting tomorrow afternoon, and really should have all proposals in the online system before I arrive.)

16-10-10 *(Doubling the Enjoyment* (to A. M. Steinberg)

Read from bottom to top. I got tickled at your email "signature"

> "Shut up and measure." — Jeff Lundeen (after Mermin)

and sent it to David Mermin.

Aephraim's Reply

> To be honest, I would've attributed it to Feynman too, but Jeff used it in his talk with the attribution to Mermin, and the web seemed to agree with him.
>
> I guess the point is that it seems to be what Feynman believed, but in David's words. I have to go back to the original essay ... is that what he was saying?

I should mention that I have a soft spot in my heart for Mermin (whom I've only met about twice, and don't really know personally). I doubt I ever told you, but when in '94 I was first applying for postdoc positions, I wrote a "chain letter" job application, along the lines of "Karl Müller received a copy of this application and forwarded it to 300 colleagues; he received the Nobel Prize the following week. Stefan Schmidt ignored the letter, and his entire division of IBM-Zurich was closed two days later." I sent it to people I knew, and to some people I though might possibly have a job for me but probably wouldn't, and to people I expected would have senses of humor — notably Dan Kleppner & David Mermin, based on their Reference Frame columns. My advisor later ran into Kleppner at some meeting, and Dan said, "Oh, is that your student who sent me this ridiculous email? You should tell him to be careful if he wants people to take him seriously." Prof. Mozart, on the other hand, responded very kindly, and even suggested that I apply for a job with Alex Gaeta, who is indeed an excellent researcher I hadn't previously known about.

I actually really wish I could find that email! Since I never knowingly throw anything (even a backup of the disk from an old computer which probably contains the backup of the disk from an older computer which probably contains all the mailboxes from my 1994 unix account) away, it's hard to believe that I can't find it!

Perhaps it's a WriteNow file unreadable on some disk anyway, since I'm not sure how many of the recipients actually had email addresses at the time!!

22-09-10 *Modal Quantum Theory* (to M. D. Westmoreland & B. W. Schumacher)

This sounds interesting. I'm especially intrigued by your remark, "In fact even if you allow some possible results (in the modal sense) to receive a probability of zero (so that zero probability is not identified with impossible), ..."

The reason, of course, is that the distinction between probability zero and impossibility, and between probability one and truth, is crucial to the QBistic view of quantum theory that Carl Caves, Rüdiger Schack, and I are trying to build up. So the distinction would be crucial (in my mind) for your modal models as well. Here's some of the original reading in case you're interested: `http://arxiv.org/abs/quant-ph/0608190`, and I tried to do a better job of some of the arguments here `http://arxiv.org/abs/1003.5209`, particularly in Section V.

I'll look forward to really understanding your papers once I fight off some of this week's upcoming fires.

Mike's Preply

Ben and I have posted a paper on the material Ben covered in his colloquium talk on modal quantum theory. Here is the `quant-ph` link: `http://arxiv.org/abs/1010.2929`

We should have a paper on the open systems analogues up in two to three weeks.

We are currently in the midst of writing a paper on the quantum analogues to the Kochen-Specker and the Conway-Kochen (Free Will) results. We hope to have that paper posted within a week.

We have also discovered that, at least in some cases, these modal systems cannot be embedded in any probabilistic model. That is, in the mobit model, there is a state and measurements such that no probability distribution on the measurement results can assign a positive probability to the results labeled as possible in the modal system. In fact even if you allow some possible results (in the modal sense) to receive a probability of zero (so that zero probability is not identified with impossible), the only possible probability assignment gives a Popescu-Rohrlich system.

It was great to visit with you last week. I hope it will not be another seven years before we see each other again.

17-10-10 *QBlue's Fall Quantum Information Prize* (to the QBies)

QBies,

I had a wacky idea yesterday in my exhaustion from the March Meeting planning session that I might start instating prizes for little non-SIC puzzles (for God's sake!) that I'd like to see solved. (An effort to broaden us all, if you will.)

So here's my first challenge: Figure out an application for free will. More specifically, consider the lovely nine bases for a $d = 4$ system found by Cabello et al., and described between Eqs. (14) and (17) of my paper `http://arxiv.org/abs/1003.5209`. Challenge: find some quantum information protocol built around these nine bases, and for which these nine bases give the optimal answer. For instance, one can certainly build a quantum crypto protocol of the style of BB84 out of these. But is there any advantage to these 9 bases for that task, rather than some other 9 bases? What is significant about these 9 bases is that they cannot be colored in the Kochen-Specker way. But what is the practical cash value of that observation? Is there any cryptographic use for the way these bases lock together? Is there something unique and interesting about the Gram matrix of the associated 18 vectors? Little questions. Add to the list if you can think of anything.

Anyway, I'll award the finder of the best observation, protocol, or theorem (by my judgement) with some old, nice book on the subject of free will and a good heap of Kiki's cooking.

For the prize and award committee,

QBlue

19-10-10 *Intersections* (to L. Hardy)

Just taking a two minute break (before my nap!). Anyway, when all this writing and politicking is over with, it'd be nice to talk to you about some physics. Most recently (after Matthew's pushing me on the subject), I've been taking your remark from a few months back more seriously—i.e., that maybe we should take an intersection of some of your axioms and some of mine. Below is the note that started the thinking off. [See 05-10-10 note "Galoshes" to M. A. Graydon.]

We've now got a rigorous argument that our modified law of total probability (MLTP) demands that $K_{12} \geq K_1 K_2$. I.e., that that much of your Axiom 4 is not a postulate for us, but required by consistency. Now we're (well, not me really at the moment) trying to see if the MLTP similarly demands that the parameter β (in eq 10 of `http://arxiv.org/abs/0912.4252`) be multiplicative (or submultiplicative or supermultiplicative) as well.

As I say below, the question from the QBism point of view is how the oomph scales with the stuff.

Keeping my fingers crossed . . .

21-10-10 *Genetic Engineering of Nature Itself* (to M. A. Graydon)

Sections 4 and 5 are the relevant parts. [See "The Anti-Växjö Interpretation of Quantum Mechanics," `quant-ph/0204146`.]

22-10-10 *Crazy Talk* (to C. Smeenk)

Thanks for the invitation to the do last night, if you were responsible for it. Immediately after asking my question to Kitcher, I started regretting how I posed it. I should have just asked more crisply, "What would John Dewey have said about Wikipedia?"

Attached is the article from Maclean's Magazine that I was telling you about last night. The reporter sure didn't do me any favours, juxtaposing my thinking with our director's views on the matter!

22-10-10 *Clarence Irving Lewis* (to P. Wells)

I was just forwarding your PI article to a philosopher friend at Western, and I noticed for the first time that you said I had C. S. Lewis on my bookshelf. Not at all! It's C. I. Lewis. Let me introduce you to him:

> `http://en.wikipedia.org/wiki/Clarence_Irving_Lewis`.

22-10-10 *Qualities of Matter* (to C. Smeenk)

By the way, if it's in readable form already, I wouldn't mind getting a copy of your "qualities of matter" paper with Biener. Funnily enough, I've recently adopted a bit of terminology like that for "Hilbert space dimension"—seeing it as a quality or capacity of matter ... not sure which term is best and what all the existing connotations are. Perhaps reading you will help. There's a bit of my present thinking on the matter in Section VI, starting page 19, of this paper: `http://arxiv.org/abs/1003.5209`. And I even give my own—almost surely distorted—view of Newton there. It'd be nice to learn about the real man.

25-10-10 *Paul Krugman's the Man!* (to R. W. Spekkens)

From today's *New York Times*:

> What we do know is that the inadequacy of the stimulus has been a political catastrophe. Yes, things are better than they would have been without the American Recovery and Reinvestment Act: the unemployment rate would probably be close to 12 percent right now if the administration hadn't passed its plan. But voters respond to facts, not counterfactuals, and the perception is that the administration's policies have failed.

25-10-10 *Tentative Title* (to H. B. Dang)

Dangism 6: *The title is actually the hardest line, which I was planning to leave as the last thing to write, with the hope that along the way I will come across a title that best captures this paper.*

You should always write the title long before the writing the paper! I never do otherwise myself.

26-10-10 *A Paper and a Fragment* (to R. Schack)

Sorry for all the silence. I've had a grueling month. Presently I'm hurriedly working on my tenure application and I've finally come to the "Future Research" section ... and thus have started thinking of what I will say in the subsection, "Decoherence and the Classical-to-Quantum Transition (sic)". Anyway, to make a long story short, I discovered the attached paper on the web. It looks useful for our purposes (in case you haven't seen it before): Mitchell S. Green and Christopher R. Hitchcock, "Reflections on Reflection: Van Fraassen on Belief," *Synthese* **98**(2), 297–324 (1994).

Everything is kosher, by the way, with Meir Hemmo. He's waiting on us relatively patiently. I hope to push your draft to the highest possible priority next week while I'm in South Africa getting things set up for a big QBism meeting at STIAS.

26-10-10 *That Old Free Will Again* (to D. M. Appleby)

Yes, I'm still working on the sordid self-aggrandizement exercise—it's up to 45 pages now. But I have a small hope that I will finish it today.

Just writing on Bell-Kochen-Specker stuff ... and came across this reference:

`http://arxiv.org/abs/1002.1410`.

It looks like there's some interesting commentary on your work in there. Were you aware of it?

30-10-10 *Hilbert Space Dimension as an Occult Quality ...* (to C. Smeenk)

I thought you might enjoy a couple of sections near the end of the completed draft (or at least I think I just finished it!) where I speculate on HS dimension as an occult quality! It's a joke of course, but I am a bit serious too. At least to the point that it seems useful for me study some of the historical literature, like your paper, and some other nice looking things I've found on the web.

Once I get a chance to read and think about all this esoteric stuff a bit more deeply, I'm sure to be back in touch!

01-11-10 *And Conway Wasn't Conway* (to A. Karlsson)

You probably learned complex analysis from J. B. Conway's book. The Conway here is John Horton Conway, the very, very powerful mathematician:

`http://en.wikipedia.org/wiki/John_Horton_Conway`

I have a story about Conway and SICs. I told John that it looks like the Welch bound can be achieved in the complex case. He was a bit shocked to learn that—for you know, he knows the real case very well, making very important contributions to it. Anyway, he was very pleased with this new knowledge, thought for a minute, and then declared, "I do believe the bound can be achieved in the complex case, but it would be very difficult to prove it."

02-11-10 *Probable Delay* (to R. Schack)

From New York JFK. Just to warn you: They're presently showing my flight as delayed by 40 minutes. My scepticism says that that'll turn into a much larger delay before it's all over with. Anyway, stay on the lookout so that you don't waste too much time in the airport.

I've been reading some very nice stuff on alchemy this evening. I've learned that Wheeler's "now fly!" metaphor is every bit as old as Aristotle himself. From a 1997 note of mine:

> In 1972 [John Preskill] had Wheeler for his freshman classical mechanics course at Princeton. One day Wheeler had each student write all the equations of physics s/he knew on a single sheet of paper. He gathered the papers up and placed them all side-by-side on the stage at the front of the classroom. Finally, he looked out at the students and said, "These pages likely contain all the fundamental equations we know of physics. They encapsulate all that's known of the world." Then he looked at the papers and said, "Now fly!" Nothing happened. He looked out at the audience, then at the papers, raised his hands high, and commanded, "Fly!" Everyone was silent, thinking this guy had gone off his rocker. Wheeler said, "You see, these equations can't fly. But our universe flies. We're still missing the single, simple ingredient that makes it all fly."

Compare:

> Plato's distrust of the mimetic arts reflects a widespread ambivalence toward imitation in antiquity. This mistrust is rooted in the idea that the painter or sculptor, by producing a replica of something natural, is engaging in a sort of counterfeit. The same attitude existed with regard to the *technai* more broadly. Although they might be clever simulacra of nature, they could not themselves be natural. A clear formulation of this distinction between the products of nature and the products of artifice appear in Aristotle's *Physics*, where the Stagirite distinguishes natural products from artificial ones on the basis of the fact that the natural have an innate principle of movement (or change), whereas the artificial have no inherent trend toward change. For this reason, Aristotle says, "men propagate men, but bedsteads do not propagate bedsteads." The artificial product is static, having received no intrinsic principle of development.

On other matters: The key issue of Weyl's phase space must be that for the measurements associated with it (i.e., X and Z) every SIC vector will give a give a probability distribution for those two observables with identical Rényi 2-entropy. Every SIC vector (WH SIC at least) will generate an entropy of $\log\left(\frac{2}{d+1}\right)$ for either of those measurements. It strikes me that that might well be an axiom we'd want to take. For instance, I have this pet idea (since 4:00 AM this morning) that this might give us the right bound on the number of zero components in a probability vectors. Our bounds previously have been horrible (only saturated in $d = 3$). But this looks like a much tougher constraint to me.

But I cannot get too distracted this week: My purpose is to write, write, write of QBism from the bottom of the world (not derive, derive, derive). And when we all come back next year, we will turn QBism on its head there! That's got to be the purpose of the place.

05-11-10 *Sabbatical in South Africa* (to R. Schack)

I'm now starting to understand what they want: Ideally about 5 people (interdisciplinary) to stay for 6 to 8 weeks and work in their facility. Travel and accommodation covered, as well as a small stipend for food. But no salary support. It is stunningly beautiful here; very nice facilities with nice gardens, vineyard, etc. (Much more my style than Perimeter.) And a very pleasant community with lots of restaurants and cafés.

05-11-10 *Heisenberg's Microscope and Battery* (to H. B. Geyer)

It looks like we were both right. See this excerpt from Cassidy's book:

`http://www.aps.org/publications/apsnews/199801/heisenberg.cfm`

The relevant lines are:

> As the 21-year-old Heisenberg appeared before the four professors on July 23, 1923, he easily handled Sommerfeld's questions and those in mathematics, but he began to stumble on astronomy and fell flat on his face on experimental physics. In his laboratory work Heisenberg had to use a Fabry-Perot interferometer, a device for observing the interference of light waves, on which Wien had lectured extensively. But Heisenberg had no idea how to derive the resolving power of the interferometer nor, to Wien's surprise, could he derive the resolving power of such common instruments as the telescope and the microscope. When an angry Wien asked how a storage battery works, the candidate was still lost. Wien saw no reason to pass the young man, no matter how brilliant he was in other fields.

I do, however, think Cassidy's article is wrong near the end of the webpage. My understanding is that Bohr found his complementarity they same week Heisenberg found his uncertainty principle, and the two of them were in different places at the time.

06-11-10 *QBies for the BQs* (to N. D. Mermin)

Merminition 220: *You've had much more experience with philosophers than I've had.*

Here's a general comment from my draft replies to Max's 17 questions. (He gave me another 15 day extension. I gather you are also delinquent.)

> When I got into this business I had hoped that philosophers would bring to the conversation their historical expertise in the Big Questions. What is the nature of human knowledge? How do people construct a model of the world external to themselves? How does our mental organization limit our ability to picture phenomena? How does our need to communicate with each other constrain the kinds of science we can develop? Those kinds of questions.
>
> But, to my disappointment, it seems to me the professional philosophers prefer to behave like amateur physicists. They don't try to view the formalism as part of a Bigger Picture. On the contrary, they seem to prefer to interpret it more literally and less imaginatively than many professional physicists. Because they are less proficient than physicists in using the tools of physics, they tend not to do as good a job on these narrower matters. Sometimes they strike me as naive and unsophisticated.
>
> So I would say that up to now professional philosophers have not played a significant role in advancing our understanding of quantum foundations. I would not (and could not) discourage philosophers from working in quantum foundations. But I would urge them to keep their eyes on the Big Questions.

So go for somebody who worries about BQs — somebody who brings BQism to QBism.

Oh, I wish you hadn't sent me that! I was only a little way into answering that question, but I was going to make a point much like you. Now, I'll have to think hard so that I don't feel like I'm sounding too much like you.

I was going to base the answer on two recent rants. One to John Norton and one to Max himself. I'll put the rant to Norton below. [See note to J. D. Norton, titled "Time Flies" and dated 07-06-10.]

Here were the words I had constructed so far for my answer to Q14, "What is the role of philosophy in advancing our understanding of the foundations of quantum mechanics?"

> If you catch me on a bad day, I'd say "no role." But I'd be lying if I left it at that. What I mean more particularly is that a large fraction of the philosophers of science who hang out at quantum foundations meetings have never seemed to me to bring much to the table that is forward looking or creative. Except for their willingness to engage in foundational questions in a way most physicists will not, they almost represent an impediment. There is no doubt that my views would not be what they are today if I had not had a sustained interaction with this community, but their role has always been a negative and resistive one; what I have gotten out of the deal is that it has been a kind of whet stone for sharpening my own thoughts. I'd rather say that I've learned something *directly* from them—that my eyes were opened by this one's or that one's considerations—but it hasn't been much so.
>
> The trouble is they advertise that what they are up to is all about the logic of what the physicists present them with, but it has been my experience that it is most often logic used in the service of *their own* prejudices. The manipulations of logic work just as well on false values as they do on truth values . . .

Don't send me any more of your answers!

Off for drive out into the wine regions.

07-11-10 *(Urgent) APS Invitation !!* (to B. W. Schumacher)

I write you (from South Africa!) as a *desperately running late* organizer for the Topical Group on Quantum Information's portion of the APS March Meeting. It's going to be in Dallas, March 21–25.

It's not quite a 10 year anniversary of the old PhysComp '92 meeting we were at, but I did find a way to make it an important anniversary for quantum information. Among the sessions I'm organizing, there'll be one titled "20 Years of Quantum Information in Physical Review Letters" (I used Ekert's QCrypto paper as the excuse) and through it, I'm hoping to draw broad attention to the TGQI by having (somewhat) historical talks about the golden age of quantum information. So far, for that session, I've secured Bennett, Shor, DiVincenzo (who will be at Aachen U by then), and Ekert.

I'm writing . . . *urgently!* . . . to ask if you'd be willing to join the crowd? It'd be a 30 minute talk with 6 minutes of questions. It would be wonderful to have a talk about the origin of the qubit, quantum compression, and quantum channel capacities by you.

In other sessions, so far, I've secured Bill Wootters, Anton Zeilinger, Jeff Kimble, Chris Monroe, Dick Slusher, Richard Hughes (giving a talk titled "27 Years of Quantum Cryptography!"). And present for the executive meeting will be John Preskill and Dave Bacon. I'm sure I'm forgetting others. The main thing I'm trying to impress you with is that you should have some fun people to talk to if you would join us! Oh, and a talk by Till Rosenband on one of the coolest experiments on earth: `http://www.nist.gov/pml/div688/clocks_092810.cfm`. (They can measure GR effects on clock synchronization over just 33 cm!!)

Also there'll be lectures by at least one (and maybe both) of the graphene Nobel prize winners. And since it's a 100th year anniversary of superconductivity, there'll be a special Nobel session with Giaver, Ketterle, Leggett, Mueller, and Wilczek speaking. Finally, David Mermin has said that he might come just to hang out. It'll be a big, buzzing zoo with likely 7,500 attendees (I'm told).

Anyway, I hope you can tell me you'll come! (And I hope you can tell me really *quickly* whatever the answer be. I've really got to get an announcement out to the general membership tomorrow or the next day prodding them to submit abstracts before the Nov 19 deadline, and I want to have just as much firepower as I can by then!)

It is a general policy of APS to not extend travel grants or fee waivers to invited speakers, trying to keep the "exceptional cases" to where the speaker is a nonphysicist or needing to make a transoceanic flight. So far, I've only used this power for Peter Shor because of his maths affiliation. But I don't recall your having a travel grant like all my other guys, so if you need airfare or fee waiver or both, I think I will be able to argue the case (with Ivan Deutsch and Christine Lenihan).

Mostly I want you there! Please let me know just as soon as possible. And at the same time, tell me your financial needs … and your title!

From Stellenbosch,

Chris … (oh, and there'll be a focus session "Quantum Information in the Service of Quantum Foundations" where you and/or Mike your modal stuff for a 12+3 talk)

07-11-10 *Ellis on Time* (to H. B. Geyer)

Here is a short summary of Ellis's view on time that we were discussing yesterday:

`http://www.fqxi.org/community/forum/topic/361`

07-11-10 *Another Meaning of Truth. Or The Usual One?* (to D. M. Appleby)

These lines struck me deeply tonight:

> Everybody loves the sound of a train in the distance
> Everybody thinks it's true

They come from Paul Simon's song "Train in the Distance":

> She was beautiful as southern skies
> The night he met her
> She was married to someone
> He was doggedly determined that he would get her
> He was old, he was young
> From time to time he'd tip his heart
> But each time she withdrew
> Everybody loves the sound of a train in the distance

Everybody thinks it's true
Well eventually the boy and the girl get married
Sure enough they have a son
And though they both were occupied
With the child she carried
Disagreements had begun
And in a while they fell apart
It wasn't hard to do
Everybody loves the sound of a train in the distance
Everybody thinks it's true
Two disappointed believers
Two people playing the game
Negotiations and love songs
Are often mistaken for one and the same
Now the man and the woman
Remain in contact
Let us say it's for the child
With disagreements about the meaning
Of a marriage contract
Conversations hard and wild
But from time to time
He makes her laugh
She cooks a meal or two
Everybody loves the sound of a train in the distance
Everybody thinks it's true
Everybody loves the sound of a train in the distance
Everybody thinks it's true
What is the point of this story
What information pertains
The thought that life could be better
Is woven indelibly
Into our hearts
And our brains

Meliorism is woven into our hearts and our brains.

Greetings from beautiful/ugly, rich/poor, full/empty ... sad/hopeful ... South Africa. This country feels bigger than anything I've ever encountered before.

08-11-10 *Bill's Functionalism* (to W. G. Demopoulos)

Two African meals have passed, and I have finished reading your new version of the paper. It was a pleasure this, as every time before.

I think I caught one typographical error, page 10, 6th line from the top: "to the question whether" $\rightarrow$ "to the question of whether"?

As you can probably guess, I'm still not very sympathetic with (what I view as) your ontological dualism. "Effects are the traces of particle interactions on systems which admit a theoretical description in terms of propositions which belong to them." I don't like the idea of the world consisting of two fundamentally different kinds of entity (in reductionistic character at least). I'm much happier to think “propositions never belong to systems” quantum or classical. If, as opposed to me, you’ll have nothing of agents, then I’d much rather see you develop a view where it’s effects all the way down. A particle’s effect on a device is that its own effects on anything else can now be something that they would not have been otherwise. A world that is “ever not quite” (Benjamin Paul Blood).

My favorite part of the paper continues to be how “a particle’s characterization has the logical form of a function.” A broad suggestion for some next-step explorations/research. It would be nice if you would compare and contrast what you’re talking about here with with some of the (so-far only partially developed) relational views on quantum mechanics. For instance, Rovelli’s `http://arxiv.org/abs/quant-ph/9609002`. What are the differences between Rovelli’s “relationalism” and Bill’s “functionalism”? (It seems your dualism is one thing.) But is there anything in common between them?

Another thing I’d like to hear is a more lingering discussion on, “What are these eternal properties you speak of?” Say, an electron: What are its eternal properties? And is there any connection between the eternal properties and the set of effects a particle can produce.

I liked Itamar’s remark, “The lesson of EPR is not about locality but about how their criterion of reality is insufficient.” However, I don’t agree with one of his lines a few previous to that—that “EPR’s criterion is at best necessary.” That gives it too much credit for me. But that’s all tied up with my personalist Bayesianism even for probability 1.

When you feel like writing again, let me know how you’re doing.

08-11-10 *There and Back Again* (to H. B. Geyer)

I didn’t catch the name of the NITheP director, and I couldn’t find it on their webpage. You might forward this to him. It looks like the Google Swedish tool might be a bit better than the German one after all! Or maybe it’s just that the Swedes have a certain sympathy to Jim’s (old) way of looking at the world.

Here is a passage from a 1968 paper of Jim Hartle’s:

> A quantum-mechanical state, being a summary of the observers’ information about an individual physical system, changes both by dynamical laws and whenever the observer acquires new information about the system through the process of measurement. The existence of two laws for the evolution of the state vector becomes problematical only if it is believed that the state vector is an objective property of the system. Then, the state vector must be required to change only by dynamical law, and the problem must be faced of justifying the second mode of evolution from the first. If the state of a system is defined as [our information of it], it is not surprising that after a measurement the state must be changed to be in accord with [any] new information. The “reduction of the wave packet” does take place in the consciousness of the observer, not because of any unique physical process which takes place there, but only because the state is a construct of the observer and not an objective property of the physical system.

Here it is after Google’s attempt to translate it into German:

> Ein quantenmechanischer Zustand, wobei eine Zusammenfassung der Beobachter Informationen über eine einzelne physische System, Veränderungen sowohl durch dynamische Gesetze und wenn der Beobachter erwirbt neue Informationen über das System durch den Prozess der Messung. Die Existenz von zwei Gesetze für die Entwicklung des Zustandsvektors wird problematisch, wenn man glaubt, dass der Zustandsvektor eine

objektive Eigenschaft des Systems ist. Dann muss der Zustandsvektor verpflichtet, nur ändern, indem dynamische Gesetz sein, und dieses Problem muss zur Rechtfertigung der zweite Modus der Evolution von der ersten konfrontiert werden. Wenn der Zustand eines Systems wie [unsere Informationen davon] definiert ist, ist es nicht verwunderlich, dass nach einer Messung den Zustand geändert werden muss, um im Einklang mit [alle] neue Informationen sein. Die "Reduktion des Wellenpakets" findet statt im Bewutsein des Betrachters, nicht wegen irgendeiner einzigartigen physikalischen Vorgang, der dort, aber nur, weil der Staat ist ein Konstrukt des Beobachters und nicht eine objektive Eigenschaft des physikalischen Systems.

And here it is after Google's attempt to re-translate it back into English:

> A quantum mechanical state, with a summary of information obtained on a single physical system, with dynamic changes in both law and if the observer acquires new information about the system through the process of measurement. The existence of two laws for the development of the state vector is problematic if one believes that the state vector is an objective property of the system. Then the state vector required to change only by law to be dynamic, and this problem needs to justify the second mode of evolution are confronted from the first. If the state of a system like [our information like this] is defined, it is not surprising that change after a measurement the state needs to be in accordance with [any] new information. The "reduction of the wave packet" will take place in the consciousness of the viewer, not because of any unique physical process that there, but only because the state is a construct of the observer and not an objective property of the physical system.

Not so good indeed!

Now, I go through the same process, but with Swedish rather than German. First to Swedish:

> En kvantmekaniska tillstånd, är en sammanfattning av observatörernas information om en enskild fysisk systemet, ändringar av både dynamiska lagar och när Observer förvärvar ny information om systemet genom processen av mätning. Förekomsten av två lagar för utvecklingen av statens vektor blir problematisk endast om det kan antas att staten vektorn är ett mål egendom av systemet. Sedan måste staten vektor måste ändra endast genom dynamiska lag, och problemet måste mötas av att motivera det andra sättet för utvecklingen från den första. Om tillståndet hos ett system definieras som [vår information om det], är det inte förvånande att efter en mätning staten måste ändras för att vara i överensstämmelse med [några] nya uppgifter. Den "minskning av den våg paket" tar plats i medvetandet hos den observatör, inte på grund av unika fysikalisk process som äger rum där, men bara för att staten är en konstruktion av observatören och inte en objektiv egenskap hos det fysiska systemet.

And now back again:

> A quantum state, is a summary of the observers' information about a single physical system, changes to both the dynamic laws, and when the Observer acquires new information about the system through the process of measurement. The existence of two laws for the development of the state vector becomes problematic if it can be assumed that the state vector is an objective property of the system. Then, the state vector must change only through dynamic team, and the problem must be met to justify the other type of development from the first. If the state of a system is defined as [our information

> on it], it is not surprising that after a measurement the state must be modified to be consistent with [any] new data. The "reduction of the wave packet" takes place in the mind of the observer, not because of the unique physical process that takes place there, but just because the state is a construct of the observer and not an objective property of the physical system.

I'm not quite sure, but I think Sweden does a little better!

09-11-10 *The Universe on a Social Analogy* (to P. Cilliers)

I am looking forward to dinner with you all tomorrow evening. At tonight's dinner, I read your paper with Heylighen and Gershenson, "Complexity and Philosophy." Now I understand much better where you're coming from.

You should know that I am quite sympathetic with many things you wrote in your paper. In fact, my foundational effort in quantum theory (which I call QBism) is based on a *denial* of one of the sentences in your paper ... but it is a denial that I think you will be pleased with once you come to understand it:

> While the notion of uncertainty or indeterminacy is an essential aspect of the newly emerging world view centering around complexity (Gershenson & Heylighen, 2005; Cilliers, 1998), it is in itself not complex, and the physical theories that introduced it are still in essence reductionist.

The denial is that actually for quantum theory to make any foundational sense, it must ultimately be read in nonreductionist terms—in fact, it is what QBism is all about! Moreover, it gives some of the main statements within quantum theory a normative reading. You can read about this view in this paper of mine (particularly in Section VI, starting on page 19): `http://arxiv.org/abs/1003.5209v1`. It is written in an entertaining manner and meant to be easy reading, so I hope you'll give it a go.

If I were to organize a project at STIAS, it would be on the themes in this paper, and thus, I think would have quite a natural overlap with your concerns.

I have always been taken with this quote of William James,

> Why may not the world be a sort of republican banquet of this sort, where all the qualities of being respect one another's personal sacredness, yet sit at the common table of space and time?
>
> To me this view seems deeply probable. Things cohere, but the act of cohesion itself implies but few conditions, and leaves the rest of their qualifications indeterminate. As the first three notes of a tune comport many endings, all melodious, but the tune is not named till a particular ending has actually come,—so the parts actually known of the universe may comport many ideally possible complements. But as the facts are not the complements, so the knowledge of the one is not the knowledge of the other in anything but the few necessary elements of which all must partake in order to be together at all. Why, if one act of knowledge could from one point take in the total perspective, with all mere possibilities abolished, should there ever have been anything more than that act? Why duplicate it by the tedious unrolling, inch by inch, of the foredone reality? No answer seems possible. On the other hand, if we stipulate only a partial community of partially independent powers, we see perfectly why no one part controls the whole view, but each detail must come and be actually given, before, in any special sense, it

> can be said to be determined at all. This is the moral view, the view that gives to other powers the same freedom it would have itself.

It sounds similar to some of the ideas in your paper.

Paul's Reply

> I look forward to finally get some time to talk, I have been running around too much. There seems to be a very good basis for a good discussion.
>
> The sentence you "deny" was actually aimed primarily at certain forms of complexity theory, mainly those relying heavily on chaos theory. I have been quite consistent (I hope) in avoiding all reference to quantum theory, mainly because of my own lack of understanding, but also because of a deep suspicion of chicanery when quantum ideas are used by philosophers and other social scientists. This is also something I would like to talk about.
>
> I will probably not have time to read your paper before we meet for dinner, but let us see.
>
> Thanks for taking the trouble to get the discussion going.

13-11-10 *Quantum Foundations at the APS March Meeting* (to J. N. Butterfield)

Word has gotten out that you maintain a mailing list of foundations folks in the UK. May I ask that you distribute the announcement below (along with the showy, crucial attachment of invited speakers) to your list? Nicely, we've already gotten a few European submissions; so it is not out of the question that a European would want to come. Any submission of quality work would certainly help the cause of making foundations a respected field within the broader APS audience.

And a test of your philosophical prowess: What phrase below was snatched from a philosopher, and who was it? Talking about what?

Jeremy's Reply

> Thanks Chris for this! Im afraid I don.t have such a list now, but saw your message go out next day through Bob Coecke.s mail list, which would have covered at least most of my addressees. May I say dear friend: it is: William James on the stream of consciousness?!

13-11-10 *Quantum Foundations at the APS March Meeting* (to B. Coecke)

I agree with Rob, your idea is an excellent idea and long overdue. Having that list would have been very convenient for this year.

In the meantime, I am running out of time to try to get submissions for the present meeting. So if you could forward the announcement below to your existing list, that would be much appreciated. Surprisingly, I have actually gotten a few European and Australian submissions. So, I guess it is not out of the question that Europeans will come.

Please note that the note below and the attachment are slightly different than the previous ones I sent you. Some typos have been corrected, etc. So if you could please use the present versions . . .

This is much, much appreciated. And it'd be great to meet someone from your group (or you!) at the March Meeting!

> ==
>
> **Deadline: 19 November!**
>
> Dear quantum-foundational colleagues,
>
> When Danny Greenberger and Anton Zeilinger first solicited the American Physical Society to form what later became the Topical Group on Quantum Information (GQI), they also intended it to be a representative body for quantum foundations research. Our mission statement makes this explicit:
>
>> The Group is committed to serving as the home within the American Physical Society for researchers in the foundations of quantum mechanics. The Topical Group will promote a continuation of the active and beneficial exchange of ideas between quantum foundations and quantum information science.
>
> I write to you as the chair-elect of the GQI executive committee and 2011 March Meeting program chair to ask you: **PLEASE DO** support quantum foundations in the APS by **SUBMITTING A TALK OR POSTER** on your work for the upcoming March Meeting in Dallas, Texas, 21-25 March 2011.
>
> To stay viable, quantum foundations research must stay visible. It must also contribute to the larger project of physics more generally. Bringing our work to the attention of the APS and showing its respectability is crucial to that project. It helps physics, and it may help create foundations-oriented faculty positions for the younger of us out there.
>
> This year we will have a focus session titled "Quantum Information for Quantum Foundations" with Giulio Chiribella (Perimeter Institute for Theoretical Physics) as our invited speaker. His talk will be titled, "Toward a Conceptual Foundation of Quantum Information Processing." Also, Anton Zeilinger (University of Vienna) will be giving a symposium talk, "Quantum Information and the Foundations of Quantum Mechanics: A Story of Mutual Benefit."
>
> It is very important that we have many other quality submissions to our focus session. I hope that you too will recognize the importance of this meeting and do what you can.
>
> More details of the full program can be found below and in the attached schedule that I have previously sent to the full GQI membership. With all the Nobel prize speakers (6+) and so many of the fathers of quantum information, this promises to be one of the most memorable March Meetings in quite some time.
>
> To register for the meeting and submit an abstract, please go to
>
> `http://www.aps.org/meetings/march/`
>
> and note that the **DEADLINE** for abstract submission is **19 November**, just 6 days away.
>
> Please join us in this "blooming, buzzing confusion" of 7,500 physicists and show them what good quantum foundations work is all about!
>
> Best wishes,
>
> Chris Fuchs

(more details on program below and attached)

==

Dear GQI Membership,

I write to you as the chair-elect of the GQI executive committee and the program chair of our portion of the 2011 APS March Meeting. This coming year the meeting will be in Dallas, Texas, 21-25 March 2011.

We believe we have put together an exciting venue of invited talks and focus sessions. Please have a look at the attachment and you will see. There will be some astounding experiments reported, and you will also have a chance to meet several of the founders of our field. 2011 is a hallmark year for quantum information as a field within physics Also we are pleased to announce that one of our talks will be given by one of the two LeRoy Apker Award winners for "outstanding achievements in physics by undergraduate students."

I should further mention that the meeting will host a talk from one of this year's Nobel-Prize winners for the discovery of graphene, Konstantin Novoselov. (Andre Geim may also speak, but has not yet confirmed.) Moreover, there will be a recognition of the 100th anniversary of the discovery of superconductivity with a session of historical talks devoted to the subject, as well as a Nobel-laureate session on it. Speakers will include Ivar Giaever, Wolfgang Ketterle, Sir Anthony Leggett, K. Alexander Mueller, and Frank Wilczek, and there is word that there may be more.

In all, it should be a more-than-usual memorable meeting, with some quite wonderful GQI invited and focus sessions. The executive committee and I hope the venue will be exciting enough to tip the scales for you if you have been indecisive about attending.

Particularly, we encourage you to submit a talk or poster on your latest research. The better showing GQI makes at this meeting, the greater the chance we have of increasing general APS awareness of our field, the better the chance the topical group may recruit enough members to attain APS Division status, and, **MOST IMPORTANTLY**, the better the chance we have of convincing American physics departments that it is worthwhile to create faculty and research positions for all of us. Your participation is really, truly vital. Quantum information needs you!

> This story shall the good man teach his son;
> And Crispin Crispian shall ne'er go by,
> From this day to the ending of the world,
> But we in it shall be remembered—
> We few, we happy few, we band of brothers;
> For he to-day that sheds his blood with me
> Shall be my brother; be he ne'er so vile,
> This day shall gentle his condition;
> And gentlemen in England now-a-bed
> Shall think themselves accurs'd they were not here,
> And hold their manhoods cheap whiles any speaks
> That fought with us upon Saint Crispin's day.

Please note that the deadline for abstract submission is **NOVEMBER 19** (less than 6 days away!). Please submit an abstract yourself; please get your students to submit an abstract too! Please get your associates to submit an abstract as well!! The place to go to submit and register for the meeting is here:

`http://www.aps.org/meetings/march/`

The GQI executive committee and I hope to see you in Dallas. It'll be a whoppin' good time!

Chris Fuchs
Chair-elect of APS Topical Group on Quantum Information
GQI Program Chair for 2011 APS March Meeting

15-11-10 *Quantum Foundations at the APS March Meeting* (to S. L. Braunstein)

I hope you have a good student or postdoc who needs some speaking practice or exposure that you can send. Better yet, come yourself. Your work on Heisenberg groups would fit right in (perfect for the foundations session, and would complement the discrete structures symposium). Beside everyone in the lists below and above, I know that Gerard Milburn, Danny Greenberger, several Zeilinger associates, . . . , will be there.

It really promises to be a very good meeting this year.

18-11-10 *Quantum Foundations in Dallas!* (to I. T. Durham)

OK, I give up. I'm off to have some dinner. I tried to get this thing posted on FQXi myself, and I just couldn't get it to stick. I don't know what I'm doing wrong. But this is the last you'll hear from me tonight. Please if you don't mind, use the exact wording I've given below (modulo adding your own title and adjusting to 36 if you wish). I was given hell from a visiting professor earlier today because of my previous wording (he was offended)—I hope this time I have made it clear that these titles "can be considered" foundations and that they are not all in one session, but "culled from all sessions".

===

Dear quantum foundations folk,

As I write to you, 3400 abstracts have already been submitted for the APS March Meeting, with 140 of those earmarked for the Topical Group on Quantum Information. Very importantly for quantum foundations, **35 of those abstracts** (culled from all sessions) can be considered with good justification to be **quantum foundations submissions**!! In other words, at the moment, we've got 1% of the whole meeting thinking about the foundations of physics!

Have a look at some of the titles and speakers below; there are going to be some very good talks at this meeting. It will be a grand opportunity for everyone in our community to mix and mingle and learn from each other.

Please don't forget that the abstract submission deadline is tomorrow, November 19, at 5:00 PM EST.

I really encourage everyone who wants to see quantum foundations thrive and be memorable to please submit a talk to this meeting. Encourage your colleagues and students too. Let's build a critical mass. Your voice will count.

The place to go is: `http://www.aps.org/meetings/abstract/instructions.cfm`. You must have an APS membership before submitting ($128 regular, $64 for recently completed PhDs, and $0 for students first joining), but you can still submit an abstract even if you don't have your membership number yet–the instructions at the link explain how to do it. (It is not necessary, but please do spend the extra $8 to join the Topical Group on Quantum Information, the official home within the APS for quantum foundations research.)

Sincerely,

Chris Fuchs

20-11-10 *How Foundations Did* (to D. M. Greenberger & A. Zeilinger)

I thought I'd give you a summary of how foundations ended up doing. There were 7,700 submissions for the March Meeting as a whole. The topical group for quantum information got 352 submissions. That is up from 256 last year (and 227 the year previous to that). The impressive thing for us is that we got 66 talks that should be of foundational interest. This means we will commandeer 4 sessions for the submitted foundational talks alone! I'll put all the titles below; and you can notice that there's not even any crazy ones in the list! The best we'd ever done on foundations before was two sessions (one mostly solid stuff, and one mostly crazy).

I've got to say, I'm very proud of what we accomplished this year. Thanks Anton for getting so many of your guys there. Our community can stand and rise!

Long Talks

1. A Brief Prehistory of Qubits
   Benjamin Schumacher

2. Quantum Information and the Foundations of Quantum Mechanics: A Story of Mutual Benefit
   Anton Zeilinger

3. Toward a Conceptual Foundation of Quantum Information Processing
   Giulio Chribella

4. Pairwise complementary observables and their mutually unbiased bases (MUBs)
   Berthold-Georg Englert

5. Quantum States as Probabilities from Symmetric Informationally Complete Measurements (SICs)
   Åsa Ericsson

6. The Lie Algebraic Significance of Symmetric Informationally Complete Measurements
   Steven T. Flammia

7. Experimental access to higher-dimensional discrete quantum systems, towards realizing SIC-POVM and MUB measurements, using integrated optics
   Christophe Schaef

8. Isotropic States in Discrete Phase Space
   William K. Wootters

Short Talks:

1. Physics as Information
   Giacomo Mauro D'Ariano

2. Quantum theory cannot be extended
   Roger Colbeck, Renato Renner

3. The quantal algebra and abstract equations of motion
   Samir Lipovaca

4. Scaling of quantum Zeno dynamics in thermodynamic systems
   Wing Chi Yu, Li-Gang Wang, Shi-Jian Gu

5. Mathematical Constraint on Realistic Theories
   James Franson

6. Uncertainty Relation for Smooth Entropies
   Marco Tomamichel, Renato Renner

7. Quaternions and the Quantum
   Matthew Graydon

8. A Linear Dependency Structure Arising from Weyl-Heisenberg Symmetry
   Hoan Bui Dang, Marcus Appleby, Ingemar Bengtsson, Kate Blanchfield, Åsa Ericsson, Christopher Fuchs, Matthew Graydon, Gelo Tabia

9. Proofs of the Kochen-Specker theorem based on the 600-cell
   P. K. Aravind, Mordecai Waegell, Norman Megill, Mladen Pavicic

10. Proofs of the Kochen-Specker theorem based on two qubits
    Mordecai Waegell, P. K. Aravind

11. Quantum Theory for a Total System with One Internal Measuring Apparatus
    Wen-ge Wang

12. The thermodynamic meaning of negative entropy
    Lidia del Rio, Renato Renner, Johan Aaberg, Oscar Dahlsten, Vlatko Vedral

13. Pseudo-unitary freedom in the operator-sum representation
    Yong Cheng Ou, Mark S. Byrd

14. Quantum Computational Geodesic Derivative
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15. Hardy's paradox and a violation of a state-independent Bell inequality in time
    Alessandro Fedrizzi, Marcelo P. Almeida, Matthew A. Broome, Andrew G. White, Marco Barbieri

16. Topos formulation of History Quantum Theory
    Cecilia Flori

17. Quantum Darwinism in an Everyday Environment: Huge Redundancy in Scattered Photons
    Charles Riedel, Wojciech Zurek

18. Redundant imprinting of information in non-ideal environments: Quantum Darwinism via a noisy channel
Michael Zwolak, Haitao Quan, Wojciech Zurek

19. Foundational aspects of energy-time entanglement
Jan-Åke Larsson

20. A Bigger Quantum Region in Multi-Party Bell Experiments
Matty Hoban, Dan Browne

21. Qutrits under a microscope
Gelo Noel Tabia

22. Quantum systems as embarrassed colleagues: what do tax evasion and state tomography have in common?
Chris Ferrie, Robin Blume-Kohout

23. Modal Quantum Theory
Michael Westmoreland, Benjamin Schumacher

24. On the Experimental Violation of Mermin's High-Spin Bell Inequalities in the Schwinger Representation
Ruffin Evans, Olivier Pfister

25. Measurement backaction and the quantum Zeno effect in a superconducting qubit
Daniel H. Slichter, R. Vijay, Irfan Siddiqi

26. A derivation of quantum theory from physical requirements
Markus Mueller, Lluis Masanes

27. Quantum simulation of time-dependent Hamiltonians and the convenient illusion of Hilbert space
Rolando Somma, David Poulin, Angie Qarry, Frank Verstraete

28. Time-asymmetry and causal structure
Bob Coecke, Raymond Lal

29. Large violation of Bell's inequalities using both counting and homodyne measurements
Valerio Scarani, Daniel Cavalcanti, Nicolas Brunner, Paul Skrzypczyk, Alejo Salles

30. Simulating Concordant Computations
Bryan Eastin

31. A generalization of Noether's theorem and the information-theoretic approach to the study of symmetric dynamics
Iman Marvian, Robert Spekkens

32. MUB Entanglement Patterns by Transformations in Phase Space
Jay Lawrence

33. Regrouping phenomena of SIC POVMs covariant with respect to the Heisenberg–Weyl group
Huangjun Zhu

34. Quantum networks reveal quantum nonlocality
Daniel Cavalcanti, Mafalda Almeida, Valerio Scarani, Antonio Acin

35. Interpreting quantum discord through quantum state merging
Vaibhav Madhok, Animesh Datta

36. Long-range spin-coupled interactions: a Gedankenexperiment on the nature of spin
Ian Durham

37. Affine Maps of the Polarization Vector for Quantum Systems of Arbitrary Dimension
Mark Byrd, C. Allen Bishop, Yong-Cheng Ou

38. Inadequacy of von Neumann entropy for characterising extractable work
Oscar Dahlsten, Renato Renner, Elisabeth Rieper, Vlatko Vedral

39. Causality, Bell's theorem, and Ontic Definiteness
Joe Henson

40. Entanglement in Mutually Unbiased Bases
Marcin Wiesniak, Tomasz Paterek, Anton Zeilinger

41. Experimental Violation of Two-Party Leggett-Garg Inequalities with Semi-weak Measurements
Justin Dressel, Curtis Broadbent, John Howell, Andrew Jordan

42. Testing spontaneous localization with ultra-massive cluster interferometry
Stefan Nimmrichter, Klaus Hornberger, Markus Arndt

43. Experimental non-classicality of an indivisible system
Radek Lapkiewicz, Peizhe Li, Christoph Schaeff, Nathan Langford, Sven Ramelow, Marcin Wiesniak, Anton Zeilinger

44. Violation of local realism with freedom of choice
Johannes Kofler, Thomas Scheidl, Rupert Ursin, Sven Ramelow, Xiao-Song Ma, Thomas Herbst, Lothar Ratschbacher, Alessandro Fedrizzi, Nathan Langford, Thomas Jennewein, Anton Zeilinger

45. Matter wave interferometry with large and complex molecules
Stefan Gerlich, Sandra Eibenberger, Mathias Tomandl, Jens Tüxen, Marcel Mayor, Markus Arndt

46. Surface based detection schemes for molecular interferometry experiments – implications and possible applications
Thomas Juffmann, Adriana Milic, Michael Muellneritsch, Markus Arndt

47. Integrable matrices with a given number of commuting partners and their exact solution
Haile Owusu, Emil Yuzbashyan

48. Closed Systems that Measure Particles
Michael Steiner, Ronald Rendell

49. Causal Tapestries
William Sulis

50. Measures of non classical correlations
Matthias Lang, Anil Shaji, Carlton Caves

51. Information geometric approach to foundations of quantum theory
Ryszard Kostecki

52. A non-local quantum eraser
X. Ma, J. Kofler, A. Qarry, N. Tetik, T. Scheidl, R. Ursin, S. Ramelow, L. Ratschbacher, T. Herbst, A. Fedrizzi, T. Jennewein, A. Zeilinger

53. Homogeneous Self-Dual Cones and the Structure of Quantum Theory
Alexander Wilce

54. Quantum Measurement, Correlation, and Contextuality
Masanao Ozawa

55. Operational interpretations of quantum discord
Marco Piani, Daniel Cavalcanti, Leandro Aolita, Sergio Boixo, Kavan Modi, Andreas Winter

56. Multipartite Entanglement: Classification, Quantification, Manipulation, Evolution and Applications
Gilad Gour

57. Decoherence Free Neutron Interferometry
Dmitry A. Pushin, David G. Cory, Michael G. Huber, Mohamed Abutaleb, Muhammad Arif, Charles W. Clark

58. Time-Reversal Symmetry and Temporal Coherent Back-Scattering in a Driven Two-Level System
Simon Gustavsson, Mark Rudner, Jonas Bylander, Leonid Levitov, Will Oliver

25-11-10 *Pauli's Dreams + Dinner + Additionism* (to F. De Martini)

And let us join forces. From our brief conversation after my return from South Africa, I got the sense that there is a good bit in common between our views of the "nature of classicality." It is a view that Appleby and I call "additionism"—that quantum behavior is something that comes "in addition to" to classical. As you write on the subject and have your lab perform relevant experiments, please keep me informed.

26-11-10 *Qbism* (to W. C. Myrvold)

Myrvoldism 7: *Did you know that "Qbism" is an iPhone game? See*

`http://www.blowfishstudios.com/`

Very cool! But too bad they misspelled QBism.

29-11-10 *Mermin's Cautionary Tale* (to D. M. Appleby)

Below is David's cautionary tale.

Merminition 221: *You've forgotten that my interest* [in SICs] *goes back at least to our independent, but strongly correlated interactions with Gabe Plunk. Why is it so damned hard to show that such sets exist, or to find a counterexample? (A cautionary tale is unique factorization in the cyclotomic integers—remember the videotape of my lecture at Bell?—which holds for every degree up to 22, before breaking down at 23. But that's a much more subtle issue. Why is this apparently trivial one so stubborn?)*

(Independent, poetic) question of the day: What is the shape of a philosopher's stone? Prompted by this passage that I just wrote:

> There is an anecdote I sometimes use for starting my talks on quantum foundations that is particularly relevant to the proposal at hand. It is about a conversation I had with my (then) seven-year-old daughter. For some time over the last year or two she had been asking me what is my favorite color. But one day she took me quite by surprise by asking, "Dad, what is your favorite shape?" At first, I started to answer glibly, "A ball." But I caught myself and became quite delighted with the possibilities. "It's Hilbert space," I said, "My favorite shape is Hilbert space." "What does it look like?" she asked. The only honest reply I could give was *I don't know!* "Then how do you know it's your favorite shape?" "Because we know enough about it to know that it is the most beautiful shape ever imagined. We can already see that much even if we don't know all its details. When I go to work at Perimeter Institute every day that's what I work on—trying to understand this beautiful shape." Two days later, my daughter announced that when she grew up she would work where I work!
>
> What the anecdote really refers to is the convex body that makes up the set of quantum states (pure and mixed) for a finite-dimensional quantum system. It is easy enough to say what this body is in conventional, algebraic terms: It is the set of trace-one positive semi-definite operators over a complex inner-product space of finite dimension d. But what is the object geometrically? There a great mystery lies and coming to grips with its answer is of the utmost importance for the foundations of quantum theory.
>
> In the broadest terms, this is what Dr. X's proposal to you is all about: Understanding this wonderful shape that empowers quantum information processing and quantum computing like a philosopher's stone.

02-12-10 *Your Notes* (to D. M. Appleby)

I have read your notes now, and they were quite interesting to me. I like this stress of yours between a probability assignment on the one hand, and a "decision" on the other.

Also, I liked these lines very much:

> And what is that objective something? It would be fair to say that finding out what that objective something truly is was one of the central projects of classical physics. Trying to answer that question eventually led to the destruction of classical physics. And we still have the problem now, in the shape of interpreting quantum mechanics.

I am glad you have decided to write a paper on all this. Now that I've read your notes, I am even more convinced that you'll make a real contribution with this. Your emphasis that "when one judges two samples to have two distinct half lives, one is ipso facto committed to the belief that there is an objective difference between them" is more and more pressing on me. It is right, and it needs fleshing out. At the moment, it remains beyond my ability, but somebody has to start the process of unwinding this issue somewhere.

If the topic comes up, as you wish, I will reveal your identity.

Happy birthday, yesterday. I wish that I had known. I will try to make it up to you when I am back in town.

02-12-10 *QI Meets GR in a Real Way* (to N. Waxman)

I'm on my way to Washington for a planning meeting for the APS March meeting (I'm program chair for the 400 talks on quantum information), and I was thinking about one of my invited speakers, Till Rosenband from NIST. It dawns on me that doing a story on his experiment for *Inside the Perimeter* would be a good way to highlight the potential for fruitful interchange between quantum information and general relativity (and gravitational experiments)—i.e., PI so well representing both fields.

Till has built the world's most accurate clock, one so accurate that he can see two such clocks come out of synchronization because of GR effects over only 33 centimeters of height difference! And the cool thing is that the clock mechanism is based on quantum computing techniques. NIST calls them "quantum logic clocks". You can read a bit about them here:

- `http://www.nist.gov/pml/div688/clocks_092810.cfm`
- `http://www.nist.gov/pml/div688/logicclock_020410.cfm`
- `http://www.nist.gov/pml/div688/logic_clock.cfm`

Just an idea that I thought I'd record.

02-12-10 *The Size of Quantum Information* (to N. Waxman)

How do you like that title? Thinking further. Once everything gets settled on planning this meeting, I might write something for you on the whole breadth of quantum information at this meeting. The number of talks in quantum information might actually far exceed the 400 I've already told you about. (The GQI that I chair had 360 submissions, and then there were at least 22 invited talks from other divisions that are on the subject of quantum information as well. I have no idea how many submitted talks from other divisions will be on quantum information until I see the titles, but I was quite confident there would be far in excess of 20 if there were already 22 invited speakers.)

Then if you wanted to write a little accompanying piece on the Rosenband/Wineland experiment (the one I mentioned earlier)—the two pieces might go together well.

02-12-10 *Probability Distributions* (to S. Gharibian)

Good question. And your intuition is right: For a general POVM with $|M|$ outcomes and an arbitrary probability distribution P over $|M|$ outcomes, one cannot find a quantum state that will give rise to P.

Take for example the SIC POVMs that I always talk about. This is a POVM on a d-dimensional Hilbert space with d^2 outcomes. It turns out that no quantum state can ever give too sharply peaked a probability distribution for its outcomes. For instance, no probability can ever be larger than $1/d$. More generally, the probability distributions arising from quantum states are always confined within a very interesting convex subset of the full simplex of probability distributions. See the story I wrote below about this shape (from a review that I was writing on someone's proposal). [See 29-11-10 note "Mermin's Cautionary Tale" to D. M. Appleby.]

So, I think this answers at least your question 2. For instance take the valid probability distribution $P = [1, 0, 0, 0, ..., 0]$ over d^2 outcomes. The closest distribution one can get to that with a SIC POVM is $Q = [1/d, 1/d(d+1), 1/d(d+1), 1/d(d+1), ..., 1/d(d+1)]$. You could for instance measure the distance between these two vectors with the standard Euclidean distance.

You can read a bit more about the geometry of this convex set in this paper of mine: `http://arxiv.org/abs/0910.2750`. Of course, it is particular to the SIC POVM but up to an affine transformation it is generic behavior.

Toy theories where your question can be answered in the positive are called "classical". You can read plenty about that in some of Howard Barnum's papers.

If you want to talk more about this, drop by my office some time. I think it's one of the most important questions in all of quantum foundations.

04-12-10 *Wave Function Volume* (to A. Ney)

The paper you mention is dated, and I have evolved much since then. Would you consider this one instead: `http://arxiv.org/abs/1003.5209`. How does its length look for your needs? How much would it have to be trimmed?

So, that is a potential "yes", depending upon what you say. Thanks for thinking of me.

PS. It is very funny: When I first looked at your email title, I thought that you had some question about the "volume of Hilbert space". See story below that I had just written up for a referee report. [See 29-11-10 "Mermin's Cautionary Tale" to D. M. Appleby.]

05-12-10 *Quantum Information at the March Meeting* (to the American Physical Society)

You asked for a heads-up of some newsworthy items at the March meeting. I've worked hard to put together a good program on the 20th anniversary of quantum information and quantum computing in APS journals. Attached is my list of speakers. Particularly, the 20th anniversary session is filled with several founders of the field. For instance, Bennett is one of the authors of the original quantum teleportation paper; Ekert is one of the discoverers of quantum cryptography; and Schumacher is the very inventor of the qubit (word and idea). Wootters, another session, was a discoverer of the no-cloning theorem, and also one of the authors of the quantum teleportation paper. Two of the guys in the experimental session, Jeff Kimble and Anton Zeilinger, performed the first two quantum teleportation experiments back in 1997/98.

Let me also flag this guy in the experimental session: Till Rosenband from NIST. Till, most recently, has built the world's most accurate clock, one so accurate that he can see two such clocks come out of synchronization because of general relativistic effects over only 33 centimeters of height difference! And the cool thing is that the clock mechanism is based on quantum computing techniques. NIST calls them "quantum logic clocks". You can read a bit about them here:

- http://www.nist.gov/pml/div688/clocks_092810.cfm
- http://www.nist.gov/pml/div688/logicclock_020410.cfm
- http://www.nist.gov/pml/div688/logic_clock.cfm

Anyway, needless to say, I would love to see you work up a bit of a feature on quantum information and computing with the press. I do think it would be of general interest.

08-12-10 *Receipt and Bill* (to O. J. E. Maroney)

OK, I was a bit late, but both letters are off now. In the case of Pitt, I sent the signed version by snail mail, but I also sent an electronic copy to John Norton and asked him to put it into your file.

I wrote the letters in a way to be intellectually honest ... but you shouldn't worry in any way. They both came off very strong I believe. I wish you all the luck.

If you haven't already surmised however, you should know that nothing from me ever comes for free. Thus, for my labors, let me bill you this: A reading of two of Marcus Appleby's papers: `http://arxiv.org/abs/quant-ph/0402015` and `http://arxiv.org/abs/quant-ph/0408058`. As an un- or non-initiate in personalistic probability, I'd like to know what you find convincing in them (if anything) and what you don't (if anything).

Thanks for the comments last week on Bennett's TIDY classical computers and their potential quantum analogs. I think it would be good to make that explicit in your research proposal—i.e., that you are talking about those computers particularly. I am cautious, however, in my belief of you on this one. To my nose, it smells a bit too much of the stories one used to hear of quantum error correction before Shor, i.e., that the no-cloning theorem would forbid it. But the key point was the clean subspaces, not the redundancy of $|\psi\rangle \to |\psi\rangle|\psi\rangle|\psi\rangle$ (which can't be made generally). Anyway, you might be right; I'm just saying I'm cautious of believing you. Also, when thinking of the circuit models of quantum computation that everyone uses, my mind goes back to the computer geeks I used to know in college who would laud the beauties of the C programming language. "Dude, it's so structured, it won't let you be inefficient!" It strikes me that the circuit models might already be like that—they won't let you be inefficient (in comparison to Bennett's original FORTRAN). If the quantum circuit models are inefficient in bit usage, then in what way?

Anyway, this note counts as your receipt—your letters are off; I hope you do well. And my bill is that you give Appleby some serious thought.

08-12-10 *Diracula!* (to D. Lynch)

I hope you understood: Diracula was sheer genius! I never heard that one before.

Here's the quote from David Deutsch at the bulletin board associated with his book *Fabric of Reality*, dated 16 July 2000. It refers to Asher Peres and me:

> A. Carvalho wrote, *"'Contrary to those desires, quantum theory does not describe reality ... ' I wonder what it can possibly describe. Is there anything else beyond reality?"*
>
> No, but that's not what they think. They think it describes our observations, but that we are not entitled to regard this as telling us anything about a reality beyond our observations. Why? Just for the Bohring old reason that they don't like the look

> of the reality that it would describe, if it did describe reality. Why? — I have many speculations, but basically I don't know. I don't understand why.
>
> It's sad enough when cranks churn out this tawdry old excuse for refusing to contemplate the implications of science, but when highly competent physicists — quantum physicists — dust it off and proudly repeat it, it's a crying shame.
>
> They really need to read FoR, don't they?

By the way, David Deutsch is sometimes described as being vampirish in his habits (being reclusive, only receiving visitors at his home at night, after midnight, etc). And he was a winner of the Dirac prize in 1998! So maybe you're on to something.

Mischievous wishes,

09-12-10 *The Shape of Quantum Theory* (to N. Waxman)

Attached are a few paragraphs from a referee report I wrote for the Guggenheim Foundation last week. Definitely, consider it *only* a first pass on the question you asked me. I'm sure for the annual report we'll need to sober it all up significantly. But it is a true story (about Katie) as it presently stands, and conveys in an essential way what's really driving me on the day-to-day basis at PI.

We can fill out, subtract, rearrange, etc., later. I just wanted to record this now since you asked tonight.

> There is an anecdote I sometimes use for starting my talks on quantum foundations that is particularly relevant to the proposal at hand. It is about a conversation I had with my (then) seven-year-old daughter. For some time over the last year or two she had been asking me what is my favorite color. But one day she took me quite by surprise by asking, "Dad, what is your favorite shape?" At first, I started to answer glibly, "A ball." But I caught myself and became quite delighted with the possibilities. "It's Hilbert space," I said, "My favorite shape is Hilbert space." "What does it look like?" she asked. The only honest reply I could give was, *"I don't know!"* "Then how do you know it's your favorite shape?" "Because we know enough about it to know that it is the most beautiful shape ever imagined. We can already see that much even if we don't know all its details. When I go to work at Perimeter Institute every day that's what I work on—trying to understand this beautiful shape." Two days later, my daughter announced that she when she grew up she wanted to work at Perimeter Institute too!
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> What the anecdote really refers to is the convex body that makes up the set of quantum states (pure and mixed) for a finite-dimensional quantum system. It is easy enough to say what this body is in conventional, algebraic terms: It is the set of trace-one positive semi-definite operators over a complex inner-product space of finite dimension d. But what is the object geometrically? There a great mystery lies and coming to grips with its answer is of the utmost importance for the foundations of quantum theory.
>
> In the broadest terms, this is what Dr. [X]'s proposal to you is all about: Understanding this wonderful shape that empowers quantum information processing and quantum computing like a philosopher's stone.

09-12-10 *Time Stories* (to N. Waxman)

By the way, your son seems to be quite some young physicist! Below is a little story I wrote up nearly 13 years ago (it's drawn from one of my samizdats). You can pass it on to your son if you wish and tell him what I think the missing ingredient is: It's time. [See story "It's a Wonderful Life" in 05-12-01 note "Lucky Seven" to B. W. Schumacher.]

09-12-10 *My Clifford Lectures* (to R. Penrose)

When you were last visiting Perimeter Institute, you showed some interest in the problem of the "maximum number of equiangular lines in a complex vector space of dimension d". Also my postdoc Åsa Ericsson told me that you had expressed a potential interest in participating in a meeting on the subject she is organizing in Banff for 2012.

Anyway, the reason I write you now is that (by some miracle) I was chosen to be the Clifford Lecturer at Tulane University for 2011 (see `http://www.math.tulane.edu/activities/clifford/`) and a small workshop will be built up around my own lectures, March 14–17. There should be some quite interesting people there, and maybe some old friends of yours. Beside whoever else is at Tulane, Lane Hughston, Samson Abramsky, +6 more, for instance will be there. And particularly, my colleague Marcus Appleby, I think you would enjoy meeting:

`http://arxiv.org/find/quant-ph/1/au:+Appleby/0/1/0/all/0/1`.

Most recently we have found a lovely connection between this equiangular line problem and cyclotomic number fields, at least in dimensions 2, 3, 4, 5, and 7. So we certainly get the sense that some real progress can be made on the problem in the near future, and that this once very specialized problem has tendrils into all sorts of mathematics.

Anyway, in all, would you have some interest in participating? You could speak on any subject you wish. But mostly, personally, I would like to get any input and feedback on this interesting problem in algebraic geometry you could give in our personal discussions outside the talks.

If you have an interest, I'll get an official invitation sent to you. All expenses paid, of course.

10-12-10 *Texan Roots* (to M. E. L. Oakes)

You probably don't remember me. I was in two of your courses in 1984/1985, I believe.

Last night I was up with a bout of insomnia, and somehow in all the web surfing, I ended up wandering (first) to the *Daily Texan* and (then) to the UT Physics Dept. It was quite a stroll down memory lane.

I suppose it was a natural consequence of my working on an interview article the last few days for a book someone is compiling. One of the questions caused me to be semi-autobiographical again, and I suppose Texas has been on my mind. In fact, this is the third round of autobiography I've had to do this year: The first was for the introduction to my book *Coming of Age with Quantum Information* (Cambridge U. Press, to be released in about a week), and the second was for my tenure application here at Perimeter Institute.

I'll attach both of these in case you have some interest. They tell the story of how one Texas boy ended up doing physics, and even making a mark in the crazy (usually markless) world of quantum foundations.

I guess I'm saying I haven't done too badly, and seeing your picture last night reminded me of what a great teacher you were. Without exaggeration, your lectures were the best I encountered in my four years of physics at UT. They kept me going at a time when I had doubts of why I was even there (the opening pages of the *Coming of Age* intro explain why I say this).

There's a line on the second page of the Perimeter research statement that says, "To the best students who came asking for a research project, graduate and undergraduate alike, John [Wheeler] would say, 'Derive quantum theory from an information theoretic principle.' In my case, I was given a more workaday project in the Regge calculus . . . " The student I had particularly in mind when I was writing that was Drew Debelack; he was also in your classes at the same time. I looked for him on the web but didn't find much—it looks like maybe he dropped out of physics.

Anyway, thanks again for all the time you put into the students (like me) way back when and the ones more recently. Seeing your title of "Distinguished Teaching Professor Emeritus" last night, it was clear that it is the perfect title for you.

12-12-10 *Dallas* (to A. Wilce)

Wilce-ism 9: *I'm trying to sort out my spring travel plans. It would be helpful to know when, approximately, we'll be hearing about the acceptance of talks submitted to the APS foundations meeting in March. Do you have a sense of when we might hear about this?*

You, of course, are accepted! And your talk will be in the featured foundations session, along with the invited speaker Chiribella, and submitted talks by D'Ariano, Masanes, Greenberger, and 9 others. I think the session was called "Axiomatics and Toy Models". In total, we have four foundations sessions this year! A total of 58 talks in those sessions. Then there's the discrete structures invited session with Wootters, Englert, Flammia, Ericsson, and Schaeff (an experimentalist) that might interest you. And the seven Nobel prizers' talks.

Attached is the latest list of all QI relevant invited talks. In total, there were over 360 GQI submissions. And I would guess at least 20 to 40 more quantum-information talks that were not classified so.

I can't tell you exactly when yet, but the session you'd speak in will most likely be on the Monday or Tuesday. And for your reference, I'll be staying in the Adolphus Hotel (with its ghost).

14-12-10 *The Archeology of SICs* (to A. Karlsson & L. Piispanen)

Two things to read on the archaeology of SICs:

- W. A. Fedak and J. J. Prentis, "The 1925 Born and Jordan paper 'On quantum mechanics'," Am. J. Phys. **77**, 128–139 (2009).
- Erhard Scholz, "Weyl Entering the 'New' Quantum Mechanics Discourse," conference contribution to *History of Quantum Physics 1*, Berlin, July 2–6, 2007. `http://quantum-history.mpiwg-berlin.mpg.de/eLibrary/hq1_talks/mathematics/35_scholz`

18-12-10 *Greetings and Thanks!* (to F. De Martini)

Thank you for the kind words. We much enjoyed having you over.

I'm writing to you from a café in a small ski-resort town. We came here to celebrate Katie's 9th birthday, and the girls are out on the slopes. They have all the fun. For myself, I am using the time to write an "interview" paper for a volume that Max Schlosshauer is putting together. I'll surely send you a copy when it is complete. One of the questions in it is, "If you could choose one experiment, regardless of its current technical feasibility, to help answer a foundational question, which one would it be?" I wonder what your answer would be. At the moment, I have no idea how I'm going to answer it.

Please give my best regards to Fiorenza, and a very merry Christmas to both of you.

19-12-10 *The Qubit Story* (to A. Stairs)

Stairsism 3: *The usual lore seems to be that Ben Schumacher coined the term 'qubit' in 1995. But the wonderfully nifty NGram tool from google suggests otherwise. The vertical axis is percent of all words in books in English that Google indexed between 1975 and 2008. (You can also look for several words; separate by commas.)*

So if not Ben S., who coined it?

It was definitely Ben. Story of its origin below. [See 28-04-04 note titled "Quantum History" to B. W. Schumacher and W. K. Wootters.] But I believe it is also an allowed spelling for cubit. That might explain why Google shows the word in use before 1992/93.

I believe Daniel Gottesman coined the word qudit. And it would be fun to learn who was the first to write qunit. I believe I coined the term rebit, for real-Hilbert-space qubits. I wonder what statistics Google gives on that one!

22-12-10 *Did I Do You Good?* (to R. W. Spekkens)

Spekkensism 52: *Incidentally, the example of nonlocality without entanglement that has the closest analogue in the toy theory is the one using three qubits due to Tal Mor.*

Yes, I knew that it had to be: Your toy model has no qutrits. I troubled over this a bit, and your remark causes me to feel (again) that maybe I'm being a bit too disingenuous in my presentation. The reasons I opted for the qutrit example are 1) that it's closer to my heart—it was the example for which we made the name, 2) it was the only example we had a genuine proof for (but maybe that has changed in the intervening years, I didn't look), and 3) the qutrit example was genuinely more ugly, and I wanted to make a point of that.

But then I also wanted particularly to make the point, "he can pull a little conceptual model from his pocket and gain quick insight into any number of technical questions in quantum theory, just by having started with the right conception of quantum states! That is physical insight ..."

Part of me says, "Ah, let them eat cake." The other part of me says I should add a sentence to soften the transition.

Spekkensism 53: *My only minor quibble is when you say: "The toy theory is not quantum theory itself (nor does it pretend to be a start for deriving the real theory)." I guess I did think about it as a kind of start to deriving the real thing in the sense that a proper axiomatization is likely to include an epistemic constraint (and maybe the knowledge is even about a pre-existing reality!).*

The thing that always struck me was how much power you got from a *simple* epistemic constraint. That really did hit me over the head and is probably a significant part of the source of my own faith that, in the right language (SIC language for instance), the ultimate epistemic constraint (which in turn, for me, must arise from some new Bayesian rule for consistency between probability assignments) will itself be simply stated. Of course, you know that I have my problems with the parenthetical part of your sentence. Anyway I will think about how I might modify my sentence so that a little bit of "the toy model being that much of a start" is on display.

I'm listening to a great album of Ozzy Osbourne doing covers of all kinds of songs. At the moment, the Beatles' "In My Life". Kind of bizarre.

22-12-10 *More Honesty* (to R. W. Spekkens)

The attached draft should do your quibble some justice (I hope). Also, it slips in a clause about the triple-qubit example so that my soul might rest more peacefully tonight.

26-12-10 *Egg Nog* (to L. Hardy)

Thanks for the sympathetic note. In the end, I'll get over it — chalking it up to a learning experience. When I get my head around what happened, I'll probably end up thinking the new understanding is something we ought to incorporate into our next physical theories!

. . . semi-seriously. Already I've become more keenly sensitive to something I've been repeating for quite a while, that "the world is not sentence shaped." Take van Fraassen's example, "Why did Adam eat the apple?" What does it ask? Depending upon how you read it and where you put the emphasis, it can be asking any number of things. Two days ago, I said some nasty things about [Professor X], but if I were to present a transcript of the conversation we had to a court of law, they would be completely puzzled over why I felt slighted. I've come to realize that most of the "conversation" wasn't conveyed through the "transcript words" but through the intonation, emphasis, and tone of speech. Even [Professor X]'s eye movements and chuckle were part of it. This is really interesting and makes the point very concrete for me, where it was mostly academic before.

06-01-11 *Quadits Inside SIC Simplexes* (to M. A. Graydon)

Graydonism 4 : *I have been wondering about the shape of quaternionic quantum state spaces inside complex SIC probability simplexes. In case you are interested, I have attached some notes from the past few days on that subject. The calculations are routine, but the results could serve as food for thought. I must admit that I am finding the idea of dimensionality as an ontic property harder to swallow these days, and I have included some brief thoughts on that in the notes (I also quote one sentence of yours that I am at odds with at the moment.) Today, at least, I feel like there is a lot more to do before I would be happy saying that dimension is agent-independent.*

Take a coin, and imagine flipping it. We generally write down a (subjective) probability distribution over two outcomes to capture our degrees of belief of which way the flip will go. But of course it is a subjective judgement that it can only go two ways. Steven van Enk would say it could always land on its side; so he would always write down a probability distribution over three outcomes.

If one takes (p_0, p_1) as a subjective assignment, the number 2 is objective with respect to it. Something we imagine or hypothesize about the coin. If one takes the (p_0, p_1, p_2) as a subjective assignment, then the number 3 is objective with respect to it—something we imagine or hypothesize about the coin, that it can fall three ways. I.e., it will fall one of three ways regardless of what we believe of which of the three ways it will fall.

So objectivity/subjectivity comes in layers. We call something objective, and then make probability assignments in the subjective layer above it. But of course, the first "calling something objective" was a subjective judgment in itself.

What if I had said it like this:

> Dimension is something [I hypothesize] a body [to hold] all by itself, regardless of what an agent thinks of it.

Much like I hypothesize that a coin can fall out two ways, while van Enk hypothesizes that it can fall out three ways.

06-01-11 *Invitation to xQIT Conference May 3–4* (to S. Lloyd)

Yes, I can come. I haven't been to Cambridge in a few years, and it would be nice to see it again. You can give my talk the title: "Still Seeking Those Damned SICs".

Please keep me up to date on where I should book a room, etc.

07-01-11 *Paper for Itamar's Volume* (to W. G. Demopoulos)

Demopoulosism 41: *A quantum question: Would it be fair to say that instead of effects in my sense (whatever that may be) you prefer "effects" in the sense of changes in subjective probability assignments by your "agents" in response to their "probing" the world? Thus an effect is not any kind of property of anything, but a probability assignment or judgement.*

Congratulations on the reprieve! I'm glad to hear it. Very best of luck on all that's to follow. Your friends' thoughts are with you, and my heart is definitely with you.

On your quantum question: Yep, that's it. I have a very old discussion of it somewhere in `http://arxiv.org/abs/quant-ph/0205039`. But the key point is this: Take a POVM consisting of operators E_k. I.e., these are positive semi-definite operators that sum to the identity operator. Thus they might seem like mysterious abstract entities (perhaps properties of the system, or properties of the 'measuring device', or maybe something still weirder, though surely agent-independent). But, via a fiducial SIC POVM in the sky (as I talk about in my papers), one can map these operators to a set of conditional probabilities $p(i|k)$ in a one-to-one fashion. That is, these operators contain nothing over and above the prescription of a probability assignment. There is nothing to them mathematically but that—nothing is lost by thinking of them in these terms, as probability assignments. Similarly, the philosophical point should be this: That there is nothing conceptually more than this either.

So, what should one call the "outcome" of a quantum measurement? Well, for the agent it is an experience "k" ... but what meaning does that have for him? How could he articulate it? What does it mean to him? How will he change his behavior with regard to it? The answer is: It is $p(i|k)$. It is a probability distribution conditioned on k. The only operational handle the agent has on k is through the assignment he makes, $p(i|k)$.

But all probabilities (for the personalist Bayesian) are subjectively given (i.e., functions of the agent only, his history and experiences, not the object). And thus, I guess, my resistance to part of your way of thinking about effects. (Of course, as you should know by now, I am in serious agreement with you in other ways ... in fact, probably with you more than with any other living philosopher.)

Attached is a little thing I've just finished for Schlosshauer's interview book. I hope I'm not too harsh on your friends in it. You'll see that I'm my usual belligerent self. Still I hope it will give you a smile from time to time (I can always hope).

08-01-11 *Strange Einstein Remark* (to R. W. Spekkens)

Having finally watched the video of Ernst Specker that Adán gave me, I wanted to learn a little bit more about this guy Gonseth that Specker mentions (and whose name I had known through a couple of remarks by Wolfgang Pauli). Anyway, a Google search landed me on one of Mehra and Rechenberg's volumes of *The Historical Development of Quantum Mechanics* (I can't quite figure out which one because of an inconsistency in Google Books, and I'm too lazy to figure it out).

Anyway, it's talking about the 1948 issue of Dialectica that Pauli edited and says:

> Einstein, on the other hand, used the opportunity to repeat again his objections that had been familiar since the Einstein-Podolsky-Rosen paper (1935), recalling that 'if in quantum mechanics we consider the ψ-function as (in principle) a complete description of a real physical situation we thereby imply the hypothesis of action-at-a-distance, a hypothesis which is actually hardly acceptable,' and 'if, on the other hand, we consider the ψ-function as an incomplete description of a real situation, then it is hard to believe that, for this incomplete description, strict laws of temporal dependence hold' (Einstein, 1948, p. 323).

Of course, their commentary about "familiar since EPR" is not exactly right, but I'm struck by the second Einstein quote. Surely Einstein was familiar with Liouvillean mechanics. I wonder whether he was really trying to get at something else, or just made a mistake? Have you ever run across Einstein saying something like this before? Do you know the wider context?

Here's the link in case you have an interest in looking yourself:

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http://books.google.ca/books?id=kn6mb0ltm0UC&pg=PA1198&lpg=PA1198&dq=
pauli+gonseth+quantum&source=bl&ots=v07etN9maM&sig=q92eq_GyRanHzwEv8D
f4hSQ2KBk&hl=en&ei=VHYoTaHBG6TenQfo4dC9AQ&sa=X&oi=book_result&ct=resu
lt&resnum=7&ved=0CEQQ6AEwBg#v=onepage&q=pauli%20gonseth%20quantum&f=f
alse
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Another thing that was kind of interesting was a little discussion on Max Born a few pages later, talking about a 1955 paper of his where (apparently) he first emphasized the similarity of quantum mechanics to Liouvillean mechanics (and sounded very Bayesian as well, I might add):

> It is misleading to compare quantum mechanics with the deterministically formulated classical mechanics; instead, one should first reformulate the classical theory, even for a single particle, in an indeterministic, statistical manner. After that some of the distinctions between the two theories disappear, [while] others emerge with great clarity. Amongst the first is the feature of quantum mechanics, that each measurement interrupts the automatic flow of events and introduces new initial conditions (so called "reduction of probability"); this is true just as well for a statistically formulated classical theory.

08-01-11 *Rampant Anti-Realism* (to R. W. Spekkens)

I wonder what this guy would think of me?

`http://www.sfu.ca/content/dam/sfu/philosophy/docs/bradley/anti_realism.pdf`.

Anyway some quotes from Max Born's correspondence with him in there.

08-01-11 *Strange Einstein Remark, 2* (to R. W. Spekkens)

Spekkensism 54: *I love the Born quote. I hadn't realized he had ever made statements that so clearly espoused an epistemic view of the quantum state. The view attributed to Einstein in the statement: "it is hard to believe that, for this incomplete description, strict laws of temporal dependence hold" is a bit puzzling. Of course, it's hard to take something that isn't a direct quote too seriously. Many of his contemporaries couldn't even understand what Einstein was getting at, so I wouldn't be surprised if they failed to summarize it adequately. If Einstein did say something like this, it doesn't follow that he was endorsing indeterminism at the level of epistemic states for isolated systems. Perhaps he just meant that discontinuous changes in the epistemic state as a result of measurement on a correlated system are natural.*

No, I think that was a direct quote of Einstein. (Well, a translation from the German.)

08-01-11 *Strange Einstein Remark, 3* (to R. W. Spekkens)

Spekkensism 55: *Okay, then it is somewhat puzzling. I take it you think that he may here be endorsing indeterminism of some variety?*

No, not endorsing indeterminism.

The more I think about it, it is maybe this: That he found a puzzling incongruity in being stuck with epistemic states for the systems themselves, but presumably "complete" information about their evolutions. I.e., that we could still interpret unitary time evolution (Hamiltonians) in ontic-style terms.

I sure would like to read his real article. How's your German?

08-01-11 *Strange Einstein Remark, 4* (to R. W. Spekkens)

Spekkensism 56: *Ah, the question of the status of unitaries . . . I see.*

Here is essentially the extent of my German: Ein grosse bier, bitte. You can get by pretty well in Germany knowing only that . . .

Actually, it hadn't dawned on me until now that that might be the issue. I was just genuinely perplexed that Einstein would say something like that. At least if he knew about Liouvillean mechanics, that is. But maybe he didn't. I don't know. I guess it also seems strange to me that Max Born didn't seem to understand the similarities between QM and LM until the mid-fifties.

08-01-11 *Strange Einstein Remark, 5* (to R. W. Spekkens)

Spekkensism 57: *Ah, the question of the status of unitaries … I see.*

That does seem to be an important paper of Einstein's that I'd like to get hold of (and get translated). It also apparently contains this quote:

> The following idea characterises the relative independence of objects far apart in space A and B: external influence on A has no direct influence on B; this is known as the Principle of Local Action, which is used consistently only in field theory. If this axiom were to be completely abolished, the idea of the existence of quasienclosed systems, and thereby the postulation of laws which can be checked empirically in the accepted sense, would become impossible.

08-01-11 *Strange Einstein Remark, 6* (to R. W. Spekkens)

And you even cite the paper in your paper with Harrigan.

08-01-11 *Mehra and Rechenberg* (to R. W. Spekkens)

I had heard before that one should be careful with the Mehra and Rechenberg books—that they are not always that accurate. Well, I have just had my first case in point.

In the piece I pointed out to you this morning, they first report Pauli's commentary on the Einstein paper, in which Pauli says the following:

> According to my opinion one cannot draw from the particular cases of correlated systems any new conclusions which are not already contained in the previously mentioned requirement in quantum mechanics of giving up the general predictability, of the results of individual observations on a single atomic system in a given state. In view of both the empirical facts and the existence of the logically consistent quantum mechanical formalism it seems to me that only this renouncement enables us still to use in physics the concept "closed system" and the usual perception of space and time, which are so closely connected with each other. It is in this sense that I consider the quantum mechanical description to be complete.

I've now read the original Pauli article and he was clearly talking about the issue of locality here.

On the other hand, Mehra and Rechenberg then go on to say:

> A more explicit description of 'closed systems' was provided by Heisenberg (1948) in the same issue of the *Dialectica*.

And I've now read the Heisenberg article too. (It was reprinted and translated in his book *Across the Frontiers*, on my shelf.) That article is titled "The Notion of a 'Closed Theory' in Modern Science" and had nothing whatsoever to say about "closed [atomic] systems" in the Pauli and Einstein articles. It was instead about how theories don't actually reduce to one another—how for instance classical mechanics and quantum mechanics live side-by-side, both distinct, both valid. (Which is a position somewhat like QBism's present one. Makes me wonder whether I ever actually say anything new. I must have read this article 25 years ago.)

So! Be careful with those guys.

09-01-11 *Sunday Letter* (to W. G. Demopoulos)

Thanks for the new draft I like the increased discussion on the timeless properties; I have much agreement with that, you know. You have a little typo at the end of your footnote 6; it ends with a ",".

I didn't see that you strengthened your defense against my charge of a kind of dualism. Maybe I'm still missing something. When you write, "Such systems are epistemically accessible to an extent that systems which are characterized only in terms of their eternal properties and their effects are not," I would think that you're admitting that there are (at least) two distinct kinds of systems in the world: Those that have a certain type of epistemic accessibility and those that do not, and that that epistemic accessibility is not characterized by things to do with the observer's situation, but rather the with system's itself.

Another typo on page 17, by the way, "does not precludes".

And you don't weaken my accusation any either by writing on page 18: "This kind of conceptual dependence, does not preclude the application of quantum mechanics to systems that record effects. Although it is largely a matter of convenience which systems are, and which are not, taken to record effects, it is not wholly a matter of convenience." The phrase 'not wholly a matter of convenience' again points me to an ontic distinction between two types of system. Again, a reason for my saying, "dualism". Similarly with respect to your sentence, "This leaves entirely open the empirical question of why it is that some systems appear to be amenable to descriptions that are expressible in the framework of classical mechanics." I.e., you don't explain why, but there is an empirical distinction between two kinds of system.

I'm not saying there's anything necessarily philosophically wrong with a dualism; mostly it is that it just doesn't match my taste, and doesn't feel like the right direction for moving physics forward. It adds a bigger burden on the physicist than I'd rather him have: For now, for each lump of matter, he'll have to—in some mysterious way—come to a conclusion of whether it supports dynamical properties or not. How does he do that? Where you leave us is where Bohr left us—as far as I can tell, in just telling us "it must be so, but I'm not going to tell you where/how to make that distinction" ... i.e., that there must be two kinds of system for us to build our evidentiary base for the quantumly treatable ones at all.

Imagine my going up to Rainer Blatt and saying, "Rainer, I just found this nice rock on the beach. You think you might be able to use it as a component in that quantum computer you want to build?" Asher Peres would say, "Of course he can use it as a component; it is just a matter of money. With enough money, any old rock can be polished into a quantum computer." But if it ain't so, then it must be a burden on physics to say when it can and when it cannot be done. My guess is that Rainer will never be able codify and make explicit such a criterion of distinction; he'll never be able to tell me which tests he must perform to certify my rock ineligible for quantum computation.

On the question of Rovelli, I don't know what to say. I leave it to you to come to your own conclusions of whether there's any (strained) similarity between your "functionalism" and his "relationalism". I just like to flounder with all kinds of ideas; every now and then one emerges as providing some insight.

On Israel, I go between Feb 13 and 19.

Oh, how's your French? I was reading through the 1948 issue of Dialectica edited by Pauli yesterday, and came across this description of his of the Gonseth paper in it:

> Many of the articles mention possible applications of the idea of complementarity outside physics, as for instance to questions connected with biology or psychology. I shall

> not discuss these questions in this introductory survey but wish to draw the reader's attention to the interesting attempts of Gonseth to formulate the idea of complementarity so generally that no explicit reference is made anymore to physics in proper sense. This is, of course, only possible by use of a language to which the physicists are not accustomed, which uses expressions like "horizons of reality", "profound horizon" and "apparent horizon", "events of a certain horizon". The word "phenomenon", however, is used in this article strictly in the above mentioned sense given to it by Bohr. To the "profound horizon" of Gonseth belong the symbolic objects to which conventional attributes can not be assigned in an unambiguous way, while the "traces" of Gonseth are identical with the "phenomena" in our sense. I wish again to stress here the circumstance that the free choice of the observer can produce either the one or the other of two "traces" and that every phenomenon or "trace" is accompanied by an unpredictable and irreversible change in the "profound horizon".

I was struck by the use of the word "trace" since I know that you also use it. Maybe that's where the similarity stops. Still, I attach the Gonseth paper in case you care to find out.

One last thing with regard to my nasty accusations of dualism, as you get a chance, I'd like to know what you think of Heisenberg's article in that same issue of Dialectica. It was reprinted and translated as "The Notion of a 'Closed Theory' in Modern Science" in his book *Across the Frontiers*.

09-01-11 *The Gonseth Connection* (to W. G. Demopoulos)

Specker credits a seminar of Gonseth's (attended by Pauli) for his interest in quantum mechanics. I have a little video of him telling a bit of the story to someone last year or so that I was listening to yesterday morning. Below is the conversation it spurred on other matters. If your German is better than your French you might be interested in the attached Einstein paper. You might be interested in the part about Einstein's general methodological considerations for requiring locality (a bit reported below).

09-01-11 *The Gonseth Connection, 2* (to W. G. Demopoulos)

Demopoulosism 42: *Isn't this paper translated and re-printed in the Born-Einstein Letters?*

Is it???? Certainly he's given variations of the locality argument all over the place—in papers and correspondence. What intrigued me most was the sentence I was curious about on time evolutions—I had never noticed him saying something like this before.

09-01-11 *Mine! Yours?* (to N. D. Mermin)

"I'll show you mine, if you'll show me yours!" Attached is the interview I finally sent off to Max last Wednesday. I think I wrote you in November, "It's just 17 questions for god's sake." Ha! That'll teach me. At least it sounds like Zeilinger was every bit as late as I was. So, if you throw darts in your mind, don't just throw them at me. [...]

But enough of these bad things. Now that I've got the nerve to read it, I hope you'll send me your own interview for Max's book. It'd be a pleasure to read it, and it'd make my Sunday a brighter, happier one. My three girls are out skiing now, and as you can see, I'm catching up on long waiting correspondence etc. Actually, I should be writing the cover story for the GQI newsletter, The Quantum Times, bragging about the March Meeting … but that's only three days overdue! So my cycles repeat …

09-01-11 *Mine! Yours?, 2* (to N. D. Mermin)

Reading through your answers slowly, as Kiki and the kids tell me of this week's ski adventure. (There is a new one every week.) What is this, "Adán Cabello's demonstration that, whatever the sense in which correlations have physical reality, it cannot be that their values are EPR 'elements of reality'." What paper does that refer to?

Merminition 222: *I look forward to the day when some clear-headed gifted writer has spelled it out so lucidly that everybody is completely convinced that there is no such problem.*

There go my credentials …

Merminition 223: *This adds a word to Asher's famous title: "Unperformed experiments have no conceivable results." That addition makes his point just a little harder to swallow.*

But swallowing becomes easier again if I expand Asher's title further to "Many different sets of unperformed experiments have no conceivable sets of results with the result for each local test being the same for every member of the set of results in which that particular local test appears." (The expanded title, however, is itself harder to swallow.)

I loved that!!!

Merminition 224: *My intuitions about the nature of quantum mechanics are not coherent enough to add up to anything I would dignify with the term "interpretation". Admittedly, shortly after turning 60 I did write a few papers setting out what I called the "Ithaca Interpretation" (see also my answer to Q6). But I was young then, innocent, and overly willing to sacrifice an accurate phrase for an entertaining one.*

Loved this one too!!! Compare to my, "Switch sides to what?"

Merminition 225: *It remains entirely possible that some wise, imaginative, and readable person may in the future lure me away from the position I am trying to sketch in my answers to these questions.*

There go my credentials again …

Merminition 226: *What I don't like about consistent histories is the reluctance—verging on refusal—of many of its practitioners to acknowledge the utterly radical nature of what they are proposing. The relativity of time was a pretty big pill to swallow, but the relativity of reality itself is to the relativity of time as an elephant is to a gnat.*

Three cheers!

Merminition 227: *I would nominate for the most important recent development the application of quantum mechanics to the processing of information, starting with the invention of quantum cryptography by Bennett and Brassard in 1984, and continuing with the development of quantum* *computation. As runner-up, I would cite the study of pre- and post-selected ensembles by Aharanov and his collaborators, and (perhaps—I still lack a good feeling for it) the ensuing notion of weak measurement. In third place I would put the consistent histories point of view, as put forth by Bob Griffiths.*

... There go my credentials for the final time of the evening!!!
Good night!!!

09-01-11 *One Endorsement* (to M. Schlosshauer)

The usual: I should thank you for everything you've done for me. In the present case, forcing me to finish the interview for you. There was a time when I genuinely wanted you to let me off the hook, but you wouldn't do it. If Appleby is genuine below, at least he got something from my answers—and maybe that ought to be enough for me and my spirits. But if Appleby is right to the point of there being one really clever, really technically-capable student inspired to develop QBism far beyond what we old guys have been capable of, then I should thank your persistence ever that much more.

Marcus's Preply

I think your interview is great. Really, really good. You sound depressed about it. But I think this is an example of an author not necessarily being the best judge of his own work. I think it could have a big impact. I don't even know who the other contributors are, let alone what (exactly) they will say. But I would be willing to bet a lot of money that someone uncommitted—a student say—reading this volume will be deeply impressed by your contribution, and will promptly forget all the others. And it is the uncommitted, above all the students, whom we most want to catch. I think this must be the first time that qbism has been set out in this way, side by side with all the other presently (but not for much longer) more fashionable approaches. At any rate, if it has been done before, it can't have been done often. There is only one possible winner from such a comparison. It is going to be just as it was with the course at the University of Waterloo, where the students were asked to vote on their favorite interpretation. It is not simply the force of the argument (important though that is). It is a deep down intuitive thing. You are just so much more alive than everyone else. People (people with a bit of youth in their soul, that is) feel that: can sense at a level below words the difference between the fresh, new-baked bread that you are giving them, and the stale old crusts which is all that anyone else has got to offer.

Actually, I found it thought-provoking myself. This may sound surprising, as I am obviously familiar with the general argument. Even so, it didn't read like yet another reiteration of the same old thing. The trouble with familiarity is that one develops intellectual habits. One's mind starts to run along rails. But reading your interview I found my mind kept jumping the tracks—following a train of ideas different from the one always suggested before. I am not sure why this was exactly. I think it was partly because the writing really is fresh—it isn't simply a cut and paste job. It is also because I happen to have been reading Schweber's book on the history of QED. Not that Schweber's book is all that good. But there are a few things in it which somehow hooked up with your interview and caused me to look at things from a different angle.

It is what Schweber says about Feynman which is relevant. It started with what you say about telling a story. Reading that I thought that telling a story is exactly what Feynman wanted to do. Nowadays the robots all think of Feynman diagrams as a mere calculational device. And perhaps that is how Feynman himself came to think of them in the end. But it is not how he thought of them in the beginning. In the beginning he was trying to tell a story. I think that is the main reason everyone loves him—why he stands out from all his contemporaries. He was the only one trying to do that (or nearly the only one—I guess Wheeler should also be included as it was he, not Feynman, who originally came up with the picture of electrons zig-zagging backwards and forwards in time). And this got me to wondering about the differences between the story Feynman told, and the one we want to tell. Which in turn led to me to thoughts about space-time. On the face of it Feynman was thinking in very blockist terms. Positrons moving back in time, advanced and retarded potentials—it is hard to see how such ideas could be meaningful if one was not thinking of the future being somehow given, along with the past, all in one go, as one great block. On the other hand, it is obvious that Feynman didn't take the idea entirely seriously. He didn't think it was the literal truth about things. Only that it was, for certain limited purposes, a useful way to think. So I don't think that Feynman was really a blockist. I digress, however. Although Feynman didn't think of his pictures as the literal truth about things, they clearly do, so far as they go, take the space-time manifold for granted. And that got me to thinking: what does a qbist say about space-time? Well, we have asked ourselves that question before of course. But on this occasion I suddenly had the sense of new vistas opening up.

I am still feeling very confused, and I am certainly not up to writing out a coherent argument. Really I want to talk about this, not write about it. Perhaps we can do that when I get back to Waterloo? But I think I would like to jot down a few thoughts while they are fresh. Not a connected piece of Jamesian prose. Just a few markers, which may serve as the jumping off point for future discussion.

The basic principle of qbism (so far): QUANTUM STATES DO NOT EXIST. I think we should supplement that with the principle: SPACE-TIME DOES NOT EXIST. In particular THE PAST DOES NOT EXIST. THE PRESENT DOES NOT EXIST. THE FUTURE DOES NOT EXIST.

Perhaps this sounds rather wild to you? But I think we are strongly pushed to it. Obviously, if one believes in free will one can't believe the future exists. If one believes in relativity one can't believe the present exists (there is no preferred space-like slice). And once one has gone that far isn't it strongly suggested that one should go the rest of the way, and also deny that the past exists?

I think we have discussed this idea before. But for me, this time, it is different. Before I somehow felt blocked from pursuing this train of thought. But now I have a sense of inner conviction.

What was blocking me? Two things, I think.

The first was Minkowski's suggestion that relativity forces us to think of space-time as a single entity. I have somehow been brain-washed by that into thinking that relativity naturally suggests a blockist conception. But I don't think it does. For sure relativity obliges us to abandon the Newtonian conception, of space as a physically existent thing. But once you have taken that step there are two possible directions in which to go. One possibility is to take the Minkowski route, and to think of space-time as a physically existent thing. The other is to reject the idea that either space or time are physically existent things.

The second was the phrase "frame of reference". The word "frame" suggests something physical. Indeed Einstein himself strongly encouraged that, with his talk of measuring rods and clocks. But now a different characterization occurs to me. Hartle (it was Hartle wasn't it?) characterized the quantum state as a "catalogue of probabilities". I think space-time is similar to that. Except it is something still more tenuous—something even less like a real existent thing. Space-time isn't the catalogue itself, only the cataloguing system. Something like the Dewey system used in libraries. Not the actual cards. Only the abstract principle used to organize the cards.

For instance, we have the actual book *Galois Theory* by Harold M. Edwards. Then we have the card which has printed on it "Harold M. Edwards, *Galois Theory* [various additional bibliographical information] MATHS E30 EDW". (I am looking at the copy I took out from University College London, who it seems don't use the Dewey system—but never mind that). Now consider the actual event, of Obama being elected president. This actual event is analogous to the actual book. Corresponding to the catalogue card we have the proposition "Obama was elected president in 2008". The proposition describing the event is as different from the event itself as the catalogue card is from the book itself. And finally, corresponding to the catalogue mark MATHS E30 EDW we have the temporal marker 2008.

Temporal markers are really useful. The temporal marker 2020 will cause me to think: well, this hasn't happened yet and it is incumbent on me, either to try to ensure that it really does happen (if I think it desirable), or to ensure that it doesn't really happen (if I think it undesirable). By contrast the temporal marker 2008 will cause me to think "well, like it or loathe it, there is nothing I can do about it". Like probabilities, temporal markers play a crucial role in organizing our thoughts, and deciding on our actions. But, also like probabilities, that doesn't mean they are actually existent parts of the world. Similarly with the concepts of past and future. Being in the past isn't a property of the event. Just a feature of the way I think about the event.

The single most important message of quantum mechanics (as I see it) is that we need to introduce some distance between our concepts and the things they are concepts of. We are used to the idea that our usual idea that everything has a position doesn't apply to electrons. I think maybe we need to go much farther than that. It isn't only microscopic things like electrons which slip through our conceptual net. It is also macroscopic things, such as Obama (or me, or you). Obama in his own nature is neither past nor future.

In a way this is not so different from the standard Minkowskian view, according to which the concepts of past and future have no absolute significance. On that view one can talk of event A being past relative to event B, but it makes no sense to talk of an event being past absolutely and intrinsically, in its own nature. However, Minkowski then introduces the concept of space-time as an absolutely existent entity. And the associated, truly horrible idea that everything has already happened.

I was intending to go on. There is certainly quite a lot more in my mind. But I think I won't because, although it is in my mind, it is very confused. All I set out to do here is to put down enough on paper to start a conversation when we next meet (I will be back in Waterloo a week today). And I think I have achieved that already.

But there is one other thing I would like to get down before I forget. It concerns the Pauli-Jung idea of synchronicity. I was never very impressed by this. But now I am suddenly wondering if there might not be something in it. Consider Feynman's definition of science as "a method for, and a body of information obtained by, trying to answer only questions which can be put in the form: If I do this, what will happen?" (Schweber, pp. 462–3). I rather like that definition (I like it because it is so strongly agent-relative: what will happen if I do this?). I also think it captures the essence of the concept of causation. Causation is all to do with control. For instance, turning the ignition key causes the car to start (causal thinking is mechanical thinking, as is acknowledged by such phrases as "mechanical materialism", or "the clockwork universe"). I think this relates to non-locality. In your discussion of non-locality you focus on "unperformed experiments have no results". Suppose, however, that one has performed the experiments. One has this great long sequence of measurement outcomes which display certain striking correlations. This often leads people to ask "what is the explanation? How could those correlations have come about?" And they feel compelled to postulate the existence of some superluminal causal influence. It seems to me, however, that it isn't a causal influence by definition, because there is no possibility of control. Nevertheless one is still left with the existence of those undoubted correlations. What would be a good name for them? Well, how about "synchronicity"? And is it possible that it was something like this that Pauli actually had in mind?

10-01-11 *Freedom = "Each with a Fire of Its Own"* (to W. K. Wootters)

I never came back to answering your wonderful note below. I know I still owe you and need to work my way through the exercise of answering your query 3—it would be very good for me to do that; I know I'll learn from it. But in the meantime, I wonder if the attached thing I just wrote up for Max Schlosshauer's interview book might lay some groundwork and context for how I should eventually answer. I think I get close to your question in my answer to Max's question 5. Unfortunately, that too relies on some of the groundwork in the pages previous to it. But if you can stomach reading the first 10 pages or so, as you get a chance, please let me if you think this might be working us toward an answer to your question.

It must be the beginning of the semester for you, and I know you're awfully busy. Best of luck for the new year.

Bill's Preply, "Nate Stories"

Greetings from Kigali! Things here are going well, though I confess we haven't yet tried banana beer.

Thanks for sending me your paper. I read it immediately but it has taken a while for it to sink in, and I know for a fact that it still hasn't fully sunken in! You have some really good arguments here, and you do a particularly good job of fending off likely misconceptions.

I have a few comments for now, maybe more later.

1. It's cool that you were able to use the story about me and Nate and the flower.

2. A trivial comment regarding the end of Section VI: I'm not sure I can think of a freshman physics problem that treats the earth's gravitational mass as infinite. Sometimes we treat the earth's *inertial mass* as infinite. That is, we say that the earth doesn't move when a force acts on it. We do this even though we also say, in the same problem, that the earth exerts a finite gravitational force on, say, an orbiting satellite.

3. I'm struggling with this important sentence: "Sometimes one will have no strong beliefs for what will result from the creation (as with the measurement of H), and sometimes one will have very strong beliefs (as with the subsequent measurement of H^{T}), but a free creation of nature it remains." I completely agree with the claim that every quantum measurement creates a new fact that was not there before. It's the word "free" that I'm struggling with. Let me begin with this question: Is there any possibility of quantifying nature's freedom? Of course an agent can associate an entropy with the outcome of a measurement he is about to perform, and thereby get a personal estimate of nature's freedom for that measurement. For example, the agent might assign entropy zero to a certain measurement, as in the scenario you imagine. In that case the agent will say he believes that nature is not free to choose the outcome of that particular measurement. But you, who are acting not as an agent but rather as a theorizer of agents, say that nature is free even in that circumstance. Is there any way for you, acting in this capacity, to quantify the freedom of nature? Or is nature always simply free—there is no "more free" or "less free"? I think the QBist answer must be that there is no way to objectively quantify nature's freedom in choosing a particular outcome of a measurement. To do so would suggest a propensity interpretation of probability. So nature is simply free to choose whatever measurement outcome it wants.

But even in QBism, surely nature is not totally free. There is order in the world; it is not total chaos. How does one express the constraints on nature's freedom? It can't be directly through probabilities, since probabilities belong to the agent, not to nature. So what does one say?

10-01-11 *Q3, Part 1* (to N. D. Mermin)

I quite enjoyed reading your interview last night, despite all the noise around me. But I would guess you woke up to already find that this morning!

Here's the first of my promised answers:

Merminition 228: *In Q3, Bob Griffiths and Niu* did *discover their spectacular simplification of the Shor algorithm (replacement of the 2-Qbit gates with 1-Qbit gates in the quantum Fourier transform) by viewing it from the Consistent Histories point of view, but that's the only example I know of. I note that it did not convert you.*

This is true, and you have brought this up with me before. For instance at the PIAF meeting panel discussion a couple of years ago.

But let me tell you another Griffiths story. I am quite sure I have written this one up before, but I have not been able to find it in my email system, so I guess there is nothing to do but write it again.

In 1996, Bob and I were office mates at the ITP in Santa Barbara, and we ended up collaborating on a problem that Asher and I had started up. Something on optimal eavesdropping in BB84. You can read about it in these two papers: `http://arxiv.org/abs/quant-ph/9701039` and `http://arxiv.org/abs/quant-ph/9702015`. You'll note that "part 1" of the paper has Fuchs, Gisin, Griffiths, Niu (a student of Bob's), and Peres as authors, while "part 2" has just Griffiths and Niu. The reason the paper bifurcated into two parts was because after all the first calculations, Niu had found a quantum circuit for enacting the eavesdropping strategy, and Bob said that the only way to *understand* its operation and *motivate* it was through the consistent history picture. Well you can imagine that Peres and Gisin would have none of that and didn't want any such thing mentioned in the paper. Bob was adamant that it needed to be in the explanation. So we decided on an amicable split.

Anyway, a couple years later Niu came by Caltech and gave me a visit. As it so happens, I was in correspondence with Bob at the time about CH, and I remembered back to our old collaboration. So I asked Chi-Sheng about how exactly consistent histories made an impact on his thinking when he discovered the circuit. He revealed to me that he hadn't used consistent histories at all when finding it. He "just played around" until he got the right unitary. I got a great laugh out of that one!

Let me tell you another story. I once did something rather sneaky for a talk I had to give at a "50 years of Everett" meeting. I sent out a questionnaire to various people asking if Everett imagery helped them make their discovery (whatever it was) in quantum computing. For instance, I asked Shor about his algorithm, Simon about his own, and the pièce de résistance was asking Deutsch and Jozsa individually about the Deutsch-Jozsa algorithm (I had a suspicion about the answers I would get). Their replies are attached. Be sure to read them in the order: Deutsch, Jozsa, Shor, Simon. It's fun.

10-01-11 *Q3, Part 2* (to N. D. Mermin)

Continuing,

Merminition 229: *Also in Q3, it wasn't clear to me (as it is clear in the case of Griffiths) how the epistemic leads directly to the answer. Is it just that Spekkens has analogous simple examples?*

Yes, you're on to me. What really happened was that Spekkens noticed the three-qubit example we had in the paper and thought, "I bet I can find the same phenomenon in the toy model." Sure enough, it was so. I thought for a while about being less ingenuous in my presentation, but our three-qubit states didn't have the same spark of *ugliness* about them that the original 9 two-qutrit states did. I wanted the ugly factor to be there.

A better story might have been the time I thought the SIC states were the only sets of states in Hilbert space to have a certain property. Rob said, "Really?" Then he showed me a set of states in his toy model that had the same property. A much closer to the process I advertised in the interview: He just thought for a moment, and then exhibited the set. Sure enough, once I checked on my own in regular quantum mechanics, I found that I had been wrong.

I should ask Rob if he has an analogous example to our 9 two-qutrit states in one of his later toy models.

10-01-11 *Born-Einstein* (to R. W. Spekkens)

Bill Demopoulos thinks that the Einstein paper we were talking about a couple of days ago was translated and reprinted in the Born-Einstein letters. You don't have that book, do you? I was kind of shocked that I don't have it on my shelf. I believe I've read it twice in the past.

11-01-11 *Mr. Physics, professional sidekick* (to R. W. Spekkens)

I realized I didn't explain my story so well yesterday. As Katie was announcing SuperMom, SuperOma, and SuperOpa, I was in the process of pouring their wine. I suppose that's how I became Wine Guy. But as I say, the next morning she relayed to me that she had thought it over a bit, and that I should be called Mr. Physics.

This morning at breakfast, she announced that there was nothing wrong in being Mr. Physics — it really is a important role. "Mr. Physics is SuperMom's sidekick!"

Maybe it's the best I could have hoped for anyway.

11-01-11 *Jaynes* (to R. W. Spekkens)

> But our present quantum mechanical formalism is not purely epistemological; it is a peculiar mixture describing in part realities of Nature, in part incomplete human information about Nature — all scrambled up by Heisenberg and Bohr into an omelette that nobody has seen how to unscramble. Yet we think that the unscrambling is a prerequisite for any further advance in basic physical theory. For, if we cannot separate the subjective and objective aspects of the formalism, we cannot know what we are talking about; it is just that simple.
>
> — E. T. Jaynes

Spekkensism 58: *Incidentally, I presented this slide to the PSI students yesterday. It reminded me of a comment that Graeme Mitchison once made about the same picture of E. T. Jaynes (which was hanging in my office in Cambridge). "That's a haircut that you can set your watch to!"*

And Carl Caves tells me that his politics was just like his hair.

11-01-11 *For January Issue, Part 1* (to I. T. Durham)

Here's the first piece, along with the relevant attachment. How does it look to you?

March Meeting, Take Note of Quantum Information!

Put on your cowboy hats, it's time to go to Dallas! This year we will have an impressive line-up of quantum information talks for the APS March Meeting in Dallas, Texas, and you won't want to miss it . . . or at least you shouldn't!

This will be the biggest year yet for quantum information and computing at an APS March Meeting. There will be over 400 talks on the subject, covering nearly every aspect of the field you can imagine—superconducting qubits, semiconducting qubits, optical qubits, ion traps, entanglement, coherent control, decoherence, quantum error correction, and much, much more. (For gosh sakes, there's even 60 talks on the foundations of quantum mechanics!) On the back of this page, you can find a full list of GQI invited speakers and topics, as well as a list of speakers and topics from other divisions that are relevant to GQI concerns. The full schedule of GQI-sponsored talks can be found here: `http://meetings.aps.org/Meeting/MAR11/sessionindex2?SponsorID=GQI`.

Two of the invited sessions particularly stand out. "20 Years of Quantum Information in Physical Review Letters" on the Wednesday will feature five retrospective talks from five of the field's founding fathers. Want to see the man who invented the word qubit (and lived to watch it become an entry in the Scrabble Players' Dictionary)? He'll be there! We're hoping this session will be a big draw for the whole APS and serve to entertain and educate them about our growing field. Before that though, on the Tuesday, there'll be a session titled "Quantum Information: Featured Experiments". Who in the general (non-GQI) APS would have thought that the world's presently most accurate clocks are directly reliant on quantum-computing techniques? We hope it'll be a lesson. Quantum information is not only crucial for the future of computing; it is forcefully relevant for weights and measures today!

Beyond the talks, GQI will also sponsor a tutorial "Quantum Simulation and Computing with Atoms" (given by Ivan Deutsch), two "Graduate Student Lunches with the Experts" (hosted by Benjamin Schumacher and Anton Zeilinger), and a "pizza and beverage-of-your-choice reception" before the business meeting. (Yes, we'll have beer; it's Texas.)

At the risk of sounding like a Slap Chop infomercial, "But wait, there's more!" The March Meeting in general this year is just going to be an exceptionally good one. For instance, there'll also be a celebration of the 100th year anniversary of superconductivity, with a Nobel laureate session associated with it. Speakers include Ivar Giaever, Wolfgang Ketterle, Sir Anthony Leggett, K. Alexander Mueller, and Frank Wilczek. Furthermore, one of this year's Nobel prize winners for the discovery of graphene, Konstantin Novoselov, will give a special talk.

There are all kinds of reasons to come. We hope that nearly every member of the GQI will be there. We have a real chance to show our relevance and importance to the APS this year. Going to the March Meeting is, of course, about learning and communicating physics, but it is also about building community and friendships and establishing a base for career paths in our field. Young people need jobs, and one way to see to it that there will be some jobs out there is to get the world physics community to take note of quantum information.

So, please tell your students, tell your teachers and friends, that important things will happen in Dallas this year. Here's the website to go to for all the details on the meeting: `http://www.aps.org/meetings/march/`. Don't forget, January 21 is the deadline for early registration—thereafter registration fees go up. Get everyone you can to come. Texas (like Hilbert space) is a big place and always accommodating!

11-01-11 *For January Issue, Part 2* (to I. T. Durham)

And how does this sound?

And probably no need to use it, but I've asked Charlie Bennett permission to use this photo in case we might want to. (But no reply from him yet; so maybe best not to plan around it.) The photo could be called, "The World of Quantum Information, When it Could Fit in One Photo Frame". This was taken just after Shor's factoring algorithm came out.

I really apologize again for delaying you so long. It'll surely be great to get this issue out so that some might act before the early registration deadline is up (Jan 21).

Letter from the Incoming Chair

It's hard to compete with last year's letter from last year's chair, Dave "Pledge Drive" Bacon—I won't even try. But pledge drive is what it's all about! So, I am glad he set the tone.

My guess is that this is going to be a key year for the GQI. Presently our membership is just a bit below 1,100. If we can get it up to 1,450 members and sustain that for two years, we can petition to become an APS division. That might look like a lot, but I don't think it is really—that is why I say this could be a key year. With our upcoming much-larger-than-previous turn-out at the APS March Meeting (our submissions grew by 40% from last year!), I think we will be in a very good position to make it happen. The March Meeting is our crystal. We just need to give the APS the best showing of quantum information it's ever seen, and that is bound to build excitement and a desire in many to be part of the topical group. I'll tell two friends, and they'll each tell two friends, and the same for each of you, and we can make this thing happen.

"APS Division of Quantum Information," doesn't that sound so sweet to the ear? And doesn't it sound so respectable? We would obtain for the first time actual representation within the APS—we'd be respected brothers and sisters to the Division of Condensed Matter Physics, the Division of AMO Physics, and all the others. American physics would respond in part to our membership's desires and needs.

I will tell two (embarrassing) stories from my history in this field, from earlier times when I was trying to get a faculty position. At the conclusion of one job colloquium, one of the professors asked me, "This is nice for weekend physics, but what do you during the week?" Of another interview, it was leaked to me that in the faculty discussion after my departure, one of the professors implored, "Well, if he likes quantum mechanics so much, why doesn't he do anything with it!?" But I was doing quantum information then, as I am doing quantum information now—the subject matter has not changed particularly.

So many of you must have experienced (or will soon experience) something similar in your own careers. But we want that number to be less and less. To the extent that such behavior in hiring committees has abated over the years, it has done so only because of the increased awareness and respect the rest of the physics community has come to for this subject we hold so dear to our hearts. We have made great progress in the 20 years since Physical Review Letters started publishing papers in quantum information—2011 is an anniversary for us—but there is so much further to go.

I look to 2011 to be a bend in our curve of growth within the APS. So, my job for the coming year will be to "tell two friends" every chance I get about the beauty and promise of quantum information. I hope all the GQI will do their best to do the same.

We have a world to change. Let's do it!

12-01-11 *Quick and Dirty (a conversation)* (to M. Schlosshauer)

Schlosshauerism 33: *What are you doing up so late?*

Reading Hardy's interview at the very moment.

Schlosshauerism 34: *It's a good one, isn't it?*

Haven't gotten far enough yet to know. I did jump to the little bit on QBism 'cause he had flagged it—the guy still gets it wrong. Chairs are no more accountable by quantum theory—on QBism's account—than the particular outcome of a Stern-Gerlach measurement. QBism's framework is empiricism; Hardy is still looking at it through reductionist glasses.

Schlosshauerism 35: *Yeah, I know what you mean. I too was surprised by Hardy's insistence on ontology as a necessary part of quantum theory.*

Not quite right. "Ontology is a necessary part of physics," he would say. He's not sure that quantum theory itself has the tools to give a correct ontology. This is because he thinks QT is an effective theory. (Or should that be affective theory? Told you I always get confused.)

14-01-11 *Great Incendiary Fun* (to I. T. Durham)

I'm sorry! First, things didn't go nearly as smoothly as I had expected for our transition from Waterloo to here. (Blue Mountain is in Ontario, on the coast of Lake Huron.) Then, for some reason, I repeatedly would remember that I needed to take care of this for you, but then would forget again. Here's the new by-line:

> Christopher A. Fuchs is a Senior Researcher at the Perimeter Institute for Theoretical Physics in Waterloo, Canada and an Adjunct Professor at the University of Waterloo. He is a winner of the 2010 International Quantum Communication Award and Chair of the American Physical Society Topical Group on Quantum Information. His Erdos number is 3, but so is his Einstein number. Mostly, he is very proud that his academic great-great-great-great grandfather Franz Exner (through the lineage Carlton Caves, Kip Thorne, John Wheeler, and up) believed, long before quantum mechanics was around, that our ultimate physics would be indeterministic. Chris's Cambridge University Press book *Coming of Age with Quantum Information* has just appeared at the bookstores, and he's been told, makes for some "great incendiary fun."

On the picture, I still haven't heard back from Charlie. So, let's just drop it. I'm quite sorry I've already delayed you this much. If the notes are going to be effective at all for getting the populace to the polls before the Jan 21 early-reg deadline, the sooner we can get the issue appearing the better.

You're a saint for putting up with contributors like me!

Hope all the snow shoveling you had to do didn't break your back.

18-01-11 *What Is the Scale at which Quantum Theory Applies?* (to R. W. Spekkens)

Spekkensism 59: *So I spoke to a public audience about quantum theory on the weekend. It was a "café scientifique" in a bar. Each of the four panelists tried to answer, in 10 minutes, the question of what quantum theory was and why the audience should care. After the break, the moderator asked us if we could remind the audience that quantum theory is a theory of stuff at small scales. That idea always sat badly with me because quantum theory applies to all systems, even macroscopic ones. So I got to thinking about how to better describe the regime in which quantum effects are important. Here's what I came up with (and told the audience): If quantum theory were about particles and forces and motion then one might have expected a length scale to be significant, but given that it is actually about information and knowledge, the scale that is important is a scale of certainty. If one is talking about a system about which one has very imprecise information, for instance the position and momentum of a car, then one is nowhere near the regime in which the Heisenberg uncertainty principle is important and classical mechanics suffices. If one is talking about an atom that is being measured extremely precisely, then one can begin to see the effect of* *the Heisenberg limit and quantum theory is important. The reason that quantum effects were first seen in microscopic systems is because these are the systems wherein it is easiest to achieve a great deal of certainty. It is difficult to do so for macroscopic systems because these generally interact more readily with their environment, and this generally leads to a decrease in our degree of certainty about the system because we typically have a great deal of uncertainty about the environment.*

So I have a new slogan: The scale at which quantum theory applies is the scale of small uncertainty. What do you think?

Here, here! I like honesty to the layman, and I can see why the moderator's suggestion wouldn't sit well with you. I wish I had been there to hear the whole story. Who and what were the other points of view? (I had to decline their invitation when they asked me.)

I liked your answer all the way to the bit about "decoherence" (though I resumed liking it again at your slogan). Of macroscopic systems, I would say we never had any certainties to begin with to lose. For instance, for a cat, no one has ever come close to making a pure-state assignment for it for which decoherence would then be relevant. In my mind, decoherence as an "explanation" of classicality is defunct. I lay my money on something closer to what Kofler and Brukner propose: `http://arxiv.org/abs/quant-ph/0609079` and then in more detail in Kofler's thesis.

Of regular "decoherence," (as used for quantum foundational discussion à la Zeh, Zurek, etc.) my new view is that it has essentially nothing to do with interactions with an environment. Instead it has everything to do with how an agent should gamble under the supposition of future certainties. Attached is a presently poorly written draft of a paper where Rüdiger and I finally try to write down this point. Or at least this is our first stab at it. The quantum section will be strengthened as I rewrite it (much comes from an earlier conference abstract of mine), but the essential point is there: Quantum decoherence (in the context of measurement theory) is none other than van Fraassen-ian reflection.

Maybe as a modification to your slogan that fits a bit better my own proclivities: Quantum theory applies to the scale of precision where contextuality and counterfactuals start to matter.

19-01-11 *Born's Rule* (to J.-Å Larsson)

Larssony 4: `http://xkcd.com/849/`.

Thanks, that was funny. Attached is my own latest cartoon. [See `http://arxiv.org/abs/1207.2141`.] I can't post it for a few months, but that doesn't stop me from sending it to friends like you.

At the moment, I'm working on a more Rüdiger-style paper on "decoherence" from the point of view of QBism. I.e., what I disappointed everyone with at Khrennikov's meeting last year. I hope we do a better job this time. Here's the abstract for it:

> The probabilities a Bayesian agent assigns to a set of events typically change with time, for instance when the agent updates them in the light of new data. In this paper we address the question of how an agent's probabilities at different times are constrained by Dutch book coherence. We review and attempt to clarify the argument that, although an agent is not forced by coherence to use the usual Bayesian conditioning rule to update his probabilities, coherence does require the agent's probabilities to satisfy van Fraassen's [1984] *reflection principle* (or the similar constraints due to Goldstein [1983] and Shafer [1983]). Bringing the argument to the context of quantum measurement theory, we show that "quantum decoherence" can be understood in purely personalist

terms—quantum decoherence (as supposed in a von Neumann chain) is not a physical process at all, but an application of the reflection principle. From this point of view, the decoherence theory of Zeh, Zurek, and others as a story of quantum measurement has the plot turned exactly backward.

20-01-11 *Combining Probabilities* (to R. W. Spekkens & M. S. Leifer)

This may be a useful paper for you [C. Genest and J. D. Zidek, "Combining Probability Distributions: A Critique and an Annotated Bibliography," *Statistical Science* **1**, 114–148 (1986)], in case you don't know about it:

> `http://www.jstor.org/pss/2245510`.

21-01-11 *Section 5* (to R. Schack)

And I am confused on the priority of Section 5. Is that an argument purely due to you? Or had someone else given it before, perhaps in different form?

Here's the present version of the abstract. Have I made it too weak with respect to what's actually been done?

> The probabilities a Bayesian agent assigns to a set of events typically change with time, for instance when the agent updates them in the light of new data. In this paper we address the question of how an agent's probabilities at different times are constrained by Dutch book coherence. We review and attempt to clarify the argument that, although an agent is not forced by coherence to use the usual Bayesian conditioning rule to update his probabilities, coherence does require the agent's probabilities to satisfy van Fraassen's [1984] *reflection principle* (or a related constraint due to Goldstein [1983]). Bringing the argument to the context of quantum measurement theory, we show that "quantum decoherence" can be understood in purely personalist terms—quantum decoherence (as supposed in a von Neumann chain) is not a physical process at all, but an application of the reflection principle. From this point of view, the decoherence theory of Zeh, Zurek, and others as a story of quantum measurement has the plot turned exactly backward.

22-01-11 *Woefully Inadequate* (to the QBies)

I felt woefully inadequate yesterday in our discussion of "facts" and "experience". I feel the same this morning. I feel so far from verbalizing whatever it is that's trying to form in my mind. Attached are my two best shots at it so far (from two recent papers). Maybe putting it in the middle of poetry, like in my pseudo-Shakespearean quote, is the most I can hope for at the moment:

> Our revels are now ended. These our actors,
> As I foretold you, were all spirits and
> Are melted into air, into thin air . . .
>
> We are such stuff as
>   quantum measurement is made on.

The young Wittgenstein started off the *Tractatus* with, "The world is everything that is the case. What is the case, the fact, is the existence of atomic facts." Somehow, someway, I want to replace his "facts" with something closer to "birth" in its imagery. Facts, as usually thought, are kind of dead things. (I said something like that yesterday.) In any case, I do very much believe that whatever it is that I'm looking for should be modeled on "quantum measurement" (whatever that is) in its first pass.

Marcus's Reply

> I agree totally with this
>
> > Facts, as usually thought, are kind of dead things.
>
> and I agree also that there is something wrong with that. Except "wrong" is too weak. So let me try again. Black as night. Dark as the grave. Life denying. Soul destroying. A figment of Satan.
>
> But, like you, I am not sure where to go from here.

22-01-11 *Creatia!* (to the QBies)

Figment of Satan! Beautiful. Attached is just a bit more to read. It comes from somewhere in the middle of the proposal I wrote for the Templeton Foundation. In it, I called this new stuff of the world "creatia."

> As important as getting straight the "quantum-theory-as-a-user's-manual conception" has been for Quantum Bayesianism, it is striking that taking that step leads right away to the very next one. The journey is not concluded by settling the foundational conundrums; instead, it is just the start of a greater, more important trek.
>
> What we have learned from the user's-manual conception of quantum theory is that every quantum measurement (by definition, a transaction between an agent and a system) represents a moment of creation. Something new comes into the world that was not there before. A knee-jerk reaction that one often hears as a response to these ideas is that they're a veiled form of solipsism—the idea that the world consists only of *the self* (presumably the Project Leader?). We have argued strenuously, however, in `\cite[Section VI]{Fuchs10b}` that the source of the knee-jerk reaction is mostly a lack of appreciation that physical theories can have any reading other than reductionistic ones.
>
> The key is not to view quantum theory in reductive terms and try to derive the world we commonly see around us out of the theory's mathematical structure, but rather to view the advice of quantum theory as *additive* to the world of (pre-quantum) experience. Here is the way it was put in `\cite{Fuchs10b}`,
>
> > The expectation of the quantum-to-classical transitionists is that quantum theory is at the bottom of things `\cite{Schlosshauer07}`, and "the classical world of our experience" is something to be derived out of it. QBism says "No. Experience is neither classical nor quantum. Experience is experience with a richness that classical physics of any variety could not remotely grasp." Quantum mechanics is something put on top of raw, unreflected experience. It is additive to it, suggesting wholly new types of experience, while never invalidating the old. To the question, "Why has no one ever *seen* entanglement

in diamond before?," the QBist replies: It is simply because before recent technologies and controlled conditions, as well as lots of refined analysis, no one had ever mustered a mesh of beliefs relevant to such a range of interactions with diamonds. No one had ever been in a position to adopt the extra normative constraints required by the Born Rule. For QBism, it is not the emergence of classicality that needs to be explained, but the emergence of our new ways of manipulating, controlling, and interacting with matter that do.

The backdrop for this program of thought is in recognizing that the happiest philosophical home for QBism is a kind of empiricism—the idea that everything experienced, everything experienceable has no less an ontological status than anything else. From this view, an experienced dream and a Higgs-boson detection event at the LHC are equal elements in the filling out and making of reality. Instead of a starkly empty world, as a quantum reductionist might have it ("the world is *nothing but* the universal wavefunction undergoing unitary evolution"), the world of QBism is overflowingly full—it is a world whose details are beyond anything grammatical or rule-bound expression can articulate. In other words, it avoids the great "original sin" of "vicious abstractionism" (as William James called it),

> We conceive a concrete situation by singling out some salient or important feature in it, and classing it under that; then, instead of adding to its previous characters all the positive consequences which the new way of conceiving it may bring, we proceed to use our concept privatively; reducing the originally rich phenomenon to the naked suggestions of that name abstractly taken, treating it as a case of 'nothing but' that, concept, and acting as if all the other characters from out of which the concept is abstracted were expunged.

Previous to QBism's thinking, the seed of that sin was spread over nearly every flavor of quantum interpretation known.

So what are the "salient and important features" QBism singles out to be *added* to the "previous character" of common experience? We have at least one answer to this so far, and it is identified right at the front of

$$Q(D_j) = (d+1)\sum_{i=1}^{d^2} P(H_i)P(D_j|H_i) - 1\ .$$

It is the single number d, called dimension, associated with each quantum system. What is so nice about this equation, as opposed to the usual way of expressing the Born Rule, is that dimension is pulled out and displayed prominently: It represents the *deviation* from the classical Law of Total Probability one should use in one's considerations for a given system. In `\cite[Section VI,VII]{Fuchs10b}`, we have argued that dimension should be thought of as a new *capacity* inherent in all matter, much like "gravitational mass" can be thought of as a capacity inherent in all matter `\cite{Cartwright99}`. It is this new capacity that allows one to do unexpected and wonderful things with quantum systems.

But our thoughts really are nascent in this, and so much work needs to be done to distill it into productive physics. With JTF's support, having Dr. Schlosshauer stationed at Perimeter Institute and Dr. Appleby available for more conceptual work, we will be able to have a devoted conceptual-research group set to get straight to the heart of this matter. (For instance, by taking advantage of the 1,000-book "pragmatism library" already built up by the Project Leader for the purpose of exploring this line of thought; see supplementary material.) Extended visits by Co-Project Leader Schack will also be decisive in this matter.

Chief among our considerations is making a push to pin down exactly which flavor of empiricism surrounds the technical apparatus of QBism. At the moment, we think it is a combination of Wolfgang Pauli's thoughts (where the individual stuff of the world is "neutral," neither matter nor psyche) and William James's "radical empiricism" of "pure experience." QBism says that every quantum measurement is a moment of creation, and the formal apparatus of quantum theory is an aid for each agent's thinking about those "creatia" she is involved with. But surely a Copernican principle applies just as much to QBism as to any other science. QBism's solution starts by saying the last point just that much more clearly: "Quantum measurement represents those moments of creation an agent happens to seek out or notice." It does not at all mean that there aren't moments of creation going all around, unnoticed, unparticipated in by the particular agent, all the time. The larger world of QBism is something aligned with James's vision of a pluriverse where "being comes in local spots and patches which add themselves or stay away at random, independently of the rest."

The biggest goal of the research group, of course, would be to say something decisive on John Wheeler's Big Question, "Is the Big Bang here?":

> Each elementary quantum phenomenon is an elementary act of "fact creation." That is incontestable. But is that the only mechanism needed to create all that is? Is what took place at the big bang the consequence of billions upon billions of these elementary processes, these elementary "acts of observer-participancy," these quantum phenomena? Have we had the mechanism of creation before our eyes all this time without recognizing the truth?

Concretely, in the period of the funding, what will happen is that the total force of our collective thought will make its way into the chapters of Schlosshauer's book—particularly, the parts devoted to the larger worldview of QBism—as well as some papers that Appleby has long been gearing up to write. Finally, Fuchs and Schack will write a detailed, more specialized monograph on the sum total "Worldview of QBism"—the monograph be as thorough as possible, expressing all pros, cons, and obstacles identified by the research group.

23-01-11 *Creatia!, 2* (to the QBies)

A quick example of the trouble that might be caused by a quick choice of phrase. Marcus writes, "the radical empiricism of Mach" in this note. Whereas in lots of recent papers, I cite James's book *Essays in Radical Empiricism*. In the case of James, "radical empiricism" was his own name for his particular doctrine. In the case of Marcus, though, I think "radical" was chosen (I would guess) as a way to express that Mach went further in a certain way than Locke and Hume.

You can get a quick sense that these might be different ideas by looking at the wiki page: `http://en.wikipedia.org/wiki/Radical_empiricism`. I don't believe Mach's own philosophy depended upon "relations themselves" being directly experienceable.

Anyway, here's how James defines his "radical empiricism":

> I give the name of radical empiricism to my Weltanschauung. Empiricism is known as the opposite of rationalism. Rationalism tends to emphasize universals and to make

> wholes prior to parts in the order of logic as well as in that of being. Empiricism, on the contrary, lays the explanatory stress upon the part, the element, the individual, and treats the whole as a collection and the universal as an abstraction. My description of things, accordingly, starts with the parts and makes of the whole a being of the second order. It is essentially a mosaic philosophy, a philosophy of plural facts, like that of Hume and his descendants, who refer these facts neither to Substances in which they inhere nor to an Absolute Mind that creates them as its objects. But it differs from the Humian type of empiricism in one particular which makes me add the epithet radical.
>
> To be radical, an empiricism must neither admit into its constructions any element that is not directly experienced, nor exclude from them any element that is directly experienced. For such a philosophy, the relations that connect experiences must themselves be experienced relations, and any kind of relation experienced must be accounted as real as anything else in the system. Elements may indeed be redistributed, the original placing of things getting corrected, but a real place must be found for every kind of thing experienced, whether term or relation, in the final philosophic arrangement.

Anyway, I'm not wedded to the terms "empiricism" or "experience" myself—I might change to something else radically different in the near future. They just seemed expedient at the time of my writing. I didn't know better words and I had been influenced by James, Schiller, and Dewey.

23-01-11 *I Am Available to Talk* (to D. M. Appleby)

Timing was bad. Your note came a bit after we took off for the movies; we saw the new Narnia movie. (I much liked it.) We just got back now. Maybe we can wait on philosophy till tomorrow.

I read the wiki page on empiricism this morning, and learned that there is a bit of an echo of J. S. Mill in the footnote 37 I sent you yesterday. Mill called matter "the permanent possibility of sensation."

24-01-11 *Putnam* (to W. G. Demopoulos)

Demopoulosism 43: *Apropos Hilary's citation of the quote attributed to Bohr by Aage Petersen (text to Hilary's Fn. 19), I would put things differently:*

> There is no classical world. There is only an abstract classical mechanical description.

Hence, I don't see myself as just recycling Bohr, although I do "echo" him.

I liked your play on the Petersen quote! Nice.

On your lines,

Demopoulosism 44: *We agreed earlier that on your view, it is the effects the would has on Bayesian agents that forms the subject matter of quantum mechanics. I regard the dualism implicit in this view as entirely tenable—it's the dualism between our intentions and expectations about the future on the one hand, and reality on the other. But I can't follow you in your incorporation of this dualism into the thesis that QM is about the consequences of our actions.*

There must be a typo in the first sentence; I would like to know what your exact words would have been. But I think I get the gist. I found myself thinking, "What a strangely asymmetric world. I feel it, but it never feels me."

I have to admit that I've been frightened ever since you told me that Putnam may be in my audience in Jerusalem. I guess I'm still trying to get over the "postmodern sauce" he flung at Jeff Bub and me at your meeting a couple years ago. You think there's any chance I could get him to read my `http://arxiv.org/abs/1003.5209` before my talk, before he comes out with his ladle full?

I will think harder and deeper on your remarks. I know you probably don't think much of my "experience mumbo jumbo" but your play on Petersen rings a bit with something I wrote in that paper:

> The expectation of the quantum-to-classical transitionists is that quantum theory is at the bottom of things, and "the classical world of our experience" is something to be derived out of it. QBism says "No. Experience is neither classical nor quantum. Experience is experience with a richness that classical physics of any variety could not remotely grasp." Quantum mechanics is something put on top of raw, unreflected experience. It is additive to it, suggesting wholly new types of experience, while never invalidating the old. To the question, "Why has no one ever *seen* superposition or entanglement in diamond before?," the QBist replies: It is simply because before recent technologies and very controlled conditions, as well as lots of refined analysis and thinking, no one had ever mustered a mesh of beliefs relevant to such a range of interactions (factual and counterfactual) with diamonds. No one had ever been in a position to adopt the extra normative constraints required by the Born Rule. For QBism, it is not the emergence of classicality that needs to be explained, but the emergence of our new ways of manipulating, controlling, and interacting with matter that do.
>
> In this sense, QBism declares the quantum-to-classical research program unnecessary (and actually obstructive) in a way not so dissimilar to the way Bohr's 1913 model of the hydrogen atom declared another research program unnecessary (and actually obstructive). Bohr's great achievement above all the other physicists of his day was in being the first to say, "Enough! I shall not give a mechanistic explanation for these spectra we see. Here is a way to think of them with no mechanism."

BTW, the rocks on the beach were diamond. Diamond has recently been learned to be a very promising material for quantum computation.

24-01-11 *Putnam, 2* (to W. G. Demopoulos)

Demopoulosism 45: *My talk of there being admissible classical descriptions is just another way of saying that our experience is classical.*

Yes, you're right: There's probably not as much resemblance as I was hoping for. All I was really meaning to say is that I would be willing to mimic part of what you were saying. Maybe my own (not so pithy) modification to Peterson would be:

> There is no quantum world. There is only an abstract quantum mechanical description for some of the things in it (at certain times and places in our relations with it). But there is no classical world either. There is only an abstract classical mechanical description of some of the things in it (at certain times and places in our relations). The former is additive to the latter—in the sense of our operational uses of the world's objects. The world in itself is richer than either schema—classical or quantum—can accommodate.

On Putnam, you are probably right. I will hold my tongue until (and unless) I am forced to.

27-01-11 *Schlosshauer and Mach* (to A. Zeilinger)

Thank you for the kind letter—your continued patience moves me. I apologize for not having written you sooner: I had wanted the latest update from Max Schlosshauer before writing you again, and he had fallen silent. It turns out he was on a small island somewhere editing his book, and he had had no internet connection for several days. [...]

I enjoyed reading your short interview. It was helpful for my orientation to see a new portrait of you. (It was similarly useful with respect to understanding Mermin and Hardy, whose contributions I have read as well.) I particularly liked the way you put a point near the end:

> This is not to say that everything is just information or knowledge. What I mean is that we need a new fundamental concept unifying the notions of information and reality.

There is some amount of similarity between that and the far end of my own QBism program. Or at least I think there is. I say so because of (for instance) this passage from a recent paper of mine:

> [W]e have learned enough from Copernicus to know that egocentrism, whenever it can be shaken away from a weltanschauung, it ought to be. Whenever "I" encounter a quantum system, and take an action upon it, it catalyzes a consequence in my experience that my experience could not have foreseen. Similarly, by a Copernican principle, I should assume the same for "you": Whenever you encounter a quantum system, taking an action upon it, it catalyzes a consequence in your experience. By one category of thought, we are agents, but by another category of thought we are physical systems. And when we take actions upon each other, the category distinctions are symmetrical. Like with the Rubin vase, the best the eye can do is flit back and forth between the two formulations. In the common circles of the philosophy of science there is a strong popularity in the idea that agentialism can always be reduced to some complicated property arrived at from physicalism. But perhaps this republican-banquet vision of the world that so seems to fit a QBist understanding of quantum mechanics is telling us that the appropriate ontology we should seek would treat these dual categories as just that, dual aspects of a higher, more neutral concept. That is, these concepts "action" and "unforseen consequence in experience," both so crucial for clarifying the very meaning of quantum measurement, might just be applicable after a fashion to arbitrary components of the world—i.e., venues in which probability talk has no place. Understanding or rejecting this idea is the long road ahead of us. The development of formalism in this paper we see as at least one promising entrance into that road.

It is probably a bit hard to understand out of the context for which it was originally written.

Anyway: What it is trying to get at is, that on the one hand, the quantum mechanical measurer is an agent (in the sense of probability and decision theory) and, on the other hand, it is a physical system like any other. You use the words "information" and "reality" ... and I use the words "agent" (bearer of probabilities) and "system". I think we are both thinking "complementary aspects of ... something."

In both cases, I think a philosopher of science would say that he thinks we are seeking a "neutral monism." I don't know that that is the best name for it, but we both are certainly seeking something that breaks the cut between Descartes' *res cogitans* and *res extensa*.

We are not the first to want to want something like this, and it explains in part why I find such inspiration in the writings of William James (who aimed for this explicitly with his concept of "pure experience") and the private letters of Wolfgang Pauli (who tended to just call it "psychophysically neutral" stuff).

More recently I've taken a great interest in Ernst Mach's own version of it, with his "sensations" or "world elements". I have to admit that until recently, I had never understood the depths of his thoughts in this regard. I had only known of the logical-positivist leaning extensions of his earlier thoughts by others. So much is hidden to us who cannot read the German language!! The actual man's thinking is so much more interesting to me than the other schools that arose (in Vienna) immediately after him.

At the moment, I am in Texas to spend a bit of time with my mother for her 82nd birthday, and I brought a book with me that I've been reading voraciously: Erik C. Banks' *Ernst Mach's World Elements: A Study in Natural Philosophy*. It is a very useful resource because the whole thing is devoted to giving a "conclusive demonstration" that the answer to this question:

> What one really wants from Mach is some clear statement that he believed in objects or elements that were not human sensations . . . : elements that made up a mind-independent world of nature even when no human being is sensing it, similar to what Russell later called sensibilia . . .

is "Yes." Mach's elements were psychophysically neutral elements (in the way that Pauli and James wanted such a thing), but not explicitly tied to human existence.

It is a very good book, and I would recommend it wholeheartedly. It is a certainly a good source of ideas for me, and I suspect it will be for your own variations on the theme as well.

01-02-11 *How To Remove Yourself from Someone's Xmas Card List* (to M. S. Leifer)

Leiferism 6 : *I have written up some notes on my objection to your treatment of Wigner's friend in the QBism papers. It came out somewhat longer than I had intended, because I was trying to get all my thoughts on the subject straight. Somewhere along the way, it seems to have morphed into a paper. It sort of depends on the papers I am writing with Rob, so I definitely want to get those finished before I do anything further with it. That gives you plenty of time to make comments and maybe convince me that I have got it wrong and should junk the whole thing before I decide whether to post this anywhere.*

On the contrary: That you take me seriously enough to write a paper on my ideas (pro *or* contra, it doesn't matter) is a powerful reason to keep you in my correspondence that much more! (We should all beware of the unintended consequences of our actions.)

Thanks for this. I am sorry it took my so long to get back to you. I was in the backwoods of Texas last week, visiting my 82-year-old mother, and that took most of my energies.

This paper will be very valuable to me, as I work out how to respond to you. The Wigner's Friend issue is the central point for me now as I try to move a step beyond quantum mechanics to the kind of neutral-monism ontology (neutral pluralism really) that I promised at the end of my last few papers ("QB Coherence" and "QBism, the Perimeter of . . . "). Similarly in my defunct Templeton Foundation proposal:

> Chief among our considerations is making a push to pin down exactly which flavor of empiricism surrounds the technical apparatus of QBism. At the moment, we think it is a combination of Wolfgang Pauli's thoughts (where the individual stuff of the world is "neutral," neither matter nor psyche) and William James's "radical empiricism" of "pure experience." QBism says that every quantum measurement is a moment of

> creation, and the formal apparatus of quantum theory is an aid for each agent's thinking about those "creatia" she is involved with. But surely a Copernican principle applies just as much to QBism as to any other science. QBism's solution starts by saying the last point just that much more clearly: "Quantum measurement represents those moments of creation an agent happens to seek out or notice." It does not at all mean that there aren't moments of creation going on all around, unnoticed, unparticipated in by the particular agent, all the time. The larger world of QBism is something aligned with James's vision of a pluriverse where "being comes in local spots and patches which add themselves or stay away at random, independently of the rest."

The key issue in any response to you, my guess is, is going to be in the meaning of Claim 1—the explicit "two levels of personalism" QBism now adopts probably does not mesh well with the current philosophy-of-science usage of the term "objectively real" (with strong connotations toward "publicly accessible"). Maybe just as importantly will be this thing you call an "ignorance interpretation". I don't know yet, but from the sounds of it (i.e., your choice of wording), it would indicate that you're missing a key point in my thinking ... or ... that I'm being grossly inconsistent. You'll forgive me for thinking it'll be the former rather than the latter—but I could be wrong, and time will tell.

I hope to get back to you from Israel the week after next, where I will be giving the Pitowsky memorial lecture. They particularly want an explication of the similarity and distinctions of my Bayesianism and his, and all this should be deeply on my mind.

Best wishes, and honestly, thanks for this!

01-02-11 *Mechanism, Sensation, Texarkana* (to C. Smeenk)

On other matters, I've been studying Erik Banks' book *Ernst Mach's World Elements* (UWO Series) for the last week and have been greatly enjoying it. I honestly hadn't appreciated the depths of Mach's thought (even in the 1860s and 1870s already). It's anything but the logical positivism I had always mislabeled him with. You read some of his passages, and if you didn't know better, you might think he was in the quantum gravity group at PI. Anyway, it's been enlightening, and it teaches me once again that some of the best sources of ideas in physics *for me* are in long forgotten 120-year-old books. I've got to learn much more about Mach now.

Attached is something I just wrote that's bound to get any philosopher of science's dander up. I hope you get a laugh from it every now and then, even as you grimace.

04-02-11 *Mach on James on Wine, Coffee, and God* (to D. M. Appleby)

This comes from a 1911 letter of Ernst Mach to Anton Thomsen. James had just died in 1910.

> My personal memories of William James are very pleasant; he visited me while still in Prague in 80 or 81. [In fact it was 1882. CAF] I remember no one with whom, despite the divergence of viewpoints, I could discuss so well and fruitfully. He opposed me almost everywhere and yet I benefited almost everywhere by his objections. Already at that time he avoided any drop of wine or coffee so that I believed him more of a nervous hypochondriac than a really sick man. The center of his work certainly lies in his excellent *Psychology*. I cannot quite come to terms with his Pragmatism. "We

> cannot give up the concept of God because it promises too much." That is a rather dangerous argument.

Reading this again, it reminds me that here is another noteworthy distinction between Mach and James. Mach was an atheist and wanted to weed out as many "transcendent" terms from science as possible (presumably God was one of them). On the other hand, James's "radical empiricism" strives to be as inclusive as possible with all experiences: "To be radical, an empiricism must neither admit into its constructions any element that is not directly experienced, nor exclude from them any element that is directly experienced." So, now think of what he sought to establish in *The Varieties of Religious Experience*—that the experience of God is a widespread and very real thing for so many people. A radical empiricism, therefore, cannot exclude it.

07-02-11 *The 2011 QBist Challenge* (to the QBies)

I attach the QBist Glossary to remind you of the running theme of our work.

The other day at group meeting, I read you all a passage that I had written for a 2004 lecture at Caltech:

> A lecturer faces a dilemma when teaching a course at a farsighted summer school like this one. This is because, when it comes to research, there is often a fine line between what one thinks and what is demonstrable fact. More than that, conveying to the students what one thinks—in other words, one's hopes, one's desires, the potentest of one's premises—can be just as empowering to the students' research lives (even if the ideas are not quite right) as the bare tabulation of any amount of demonstrable fact. So I want to use one percent of this lecture to tell you what I think—the potentest of all my premises—and use the remaining ninety-nine to tell you about the mathematical structure from which that premise arises.
>
> I think the greatest lesson quantum theory holds for us is that when two pieces of the world come together, they give birth. [Bring two fists together and then open them to imply an explosion.] They give birth to FACTS in a way not so unlike the romantic notion of parenthood: that a child is more than the sum total of her parents, an entity unto herself with untold potential for reshaping the world. Add a new piece to a puzzle—not to its beginning or end or edges, but somewhere deep in its middle—and all the extant pieces must be rejiggled or recut to make a new, but different, whole. That is the great lesson.
>
> But quantum mechanics is only a glimpse into this profound feature of nature; it is only a part of the story. For its focus is exclusively upon a very special case of this phenomenon: The case where one piece of the world is a highly-developed decision-making agent—an experimentalist—and the other piece is some fraction of the world that captures his attention or interest.
>
> When an experimentalist reaches out and touches a quantum system—the process usually called quantum 'measurement'—that process gives rise to a birth. It gives rise to a little act of creation. And it is how those births or acts of creation impact the agent's *expectations* for other such births that is the subject matter of quantum theory. That is to say, quantum theory is a calculus for aiding us in our decisions and adjusting our expectations in a QUANTUM WORLD. Ultimately, as physicists, it is the quantum world for which we would like to say as much as we can, but that is not our starting point. Quantum theory rests at a level higher than that.

To put it starkly, quantum theory is just the start of our adventure. The quantum world is still ahead of us. So let us learn about quantum theory.

As Marcus pointed out, I'm certainly using the word "facts" (in all caps) in an awfully non-standard way there. For instance, one does not usually think of facts as having "untold potential for reshaping the world."

So, I've spent the last few minutes trying to find a better word that fits our group theme. The one that strikes me most at the moment is

- **QBoom** – cf. *kaboom*; a fundamental event in analogy to the outcome of a quantum measurement. "In the QBist weltanschauung, the universe does not come into being with a Big Bang, but instead arises 'in spots and patches' through millions upon millions of little QBooms."

If any of you think of a more elegant exercise in QBistry, I'm open to suggestions.

07-02-11 *More QB Silliness* (to the QBies)

- **QBoom** – cf. *kaboom*; the sought-for deanthropocentrized distillate of a quantum measurement that QBism imagines as powering the world. William James called an early version of something along these lines "pure experience". John Wheeler asked, "Is the entirety of existence, rather than being built on particles or fields of force or multidimensional geometry, built upon billions upon billions of elementary quantum phenomena, those elementary acts of 'observer-participancy,' those most ethereal of all the entities that have been forced upon us by the progress of science?" and, "Is what took place at the big bang the consequence of billions upon billions of these elementary processes, these elementary 'acts of observer-participancy,'?" In other words, in place of the Big Bang, the QBist wonders whether it might not be billions upon billions of little QBooms instead?
- **QBlooey** – cf. *kablooey*; a QBist slur on the usual conception of the Big Bang—i.e., a conception of the universe in which nature already blew its wad at the very beginning.

07-02-11 *Renouvier* (to S. Weinstein)

Thanks for the music; I'll have a listen of it when I get home. And, no, I haven't read Hume. I should be ashamed of myself—with my abiding interest in free will, it would serve me to.

Attached are some of my notes from a book on Renouvier. [See 13-08-03 note "Renouvier" to R. Schack.] For Renouvier, free will was something that had to be fought for. It didn't come for free; unless you really wanted it, you would not have it. I also like (the flavor of) his argument that—surprisingly—the concepts of true and false are crucially dependent on freedom.

08-02-11 *From Screed to Screech* (to A. Wilce)

Thanks for the new paper! I've sent it on to my Jordan-algebra-savvy student Matthew Graydon, to get him to explain it to me. Also dug up your reference 8 since it looked like it might intrigue him. Noticed that you had Brukner's name misspelled as Bruckner, and you're missing some diacritical marks as well (on him and his co-author).

Attached is my own latest. [See "Interview with a Quantum Bayesian," `arXiv:1207.2141v1`.] It won't be posted for two or three months until after the book comes out. And if you read some of the nasty things I say about our philosophical friends, maybe that's a good thing!

I've got a more technical paper coming down the pipe soon, on diachronic Dutch book arguments and their relation to (what is misconstrued as a physical, rather than epistemic) quantum decoherence. I'll let you know when it's finally posted.

I'm off to Jerusalem Friday to give the first Itamar Pitowsky memorial lecture next week. A very flattering thing to do. But I'm also scared stiff: I've been told Hilary Putnam will be in the audience.

It'll be good to see you again in March. I'll make sure I reserve some time for you.

09-02-11 *Questions and Answers* (to R. Colbeck & G. Chiribella)

I enjoyed both your questions yesterday. They were completely on the mark, and I appreciated them.

For Roger's question on "what if they don't exist," I attach the write-up of a little epiphany I had while Howard Barnum and I were at some philosophy of science talk at the university. [See 13-03-10 note "Conceptual Barrier!" to the QBies.] I do well believe that someone will eventually show that SICs always exist, but if they don't I'm prepared for it, and the attached write-up makes the point with some passion. You might find it entertaining reading. I also include for your amusement the write-up of a dream I sent to Anton Zeilinger and its interpretation in terms of the epiphany. [See 01-04-10 note "Strange Anton Dream" to A. Zeilinger and H. C. von Baeyer.]

The "allusion to William James's faith ladder" that I mention in the note refers to this little ditty I had sent to my group and Howard in an earlier email:

> SICs are *fit* to be true. It would be well if they *were* true. They *might* be true; they *may* be true; they *ought* to be true. They *must* be true! They *shall* be true! For me at least!!

Anyway, what is powering my faith ladder is the simplicity of the displayed equation in the note. It's consistency + Bayes' rule + an assumption of maximality . . . these three things alone . . . imply an already very rich convex set structure that gets quite some distance toward quantum theory. If you have some interest, you can read about some of the properties of such sets here: `http://arxiv.org/abs/0910.2750`.

Now on to Giulio's question. I think it would be quite healthy for me to try to tackle some of the things he suggested — to try to understand what quantum-like protocols we can perform with this class of theories. It's a crucial question really. If you have any suggestions or any ideas or any methods for getting at some answers, I certainly invite you to get involved.

In that regard, attached are some notes that Howard Barnum wrote up after one of our conversations where he goes some way toward translating the QBist program into the more commonly-used convex-operational language. The equations don't look so pretty in that framework (to me), but it can be done. One of these days, these notes ought to be extended and written into a proper paper. If either of you have an contributions to make, I would surely welcome that as well.

Finally, let me attach an interview I just wrote up that won't be posted on the web for a while (until the book it appears in has been published). [See "Interview with a Quantum Bayesian," `arXiv:1207.2141v1`.] In question Q10, starting on page 16, I give my opinion on axiomatic reconstruction efforts. All my thinking is really in that kind of "literary" context.

Thanks again guys.

11-02-11 *On My Way* (to W. G. Demopoulos)

I just wanted to say hi before crossing the ocean. I'm writing from JFK airport, on my way to Jerusalem for Itamar's memorial. I accompanied myself with several things on neutral monism to read along the way and there, and I just ran across a reference to a paper by you and Friedman, apparently relevant to a discussion of Russell's particular version of it all.

It would be fun to talk to you again sometime soon. I hope you're doing as well as possible at this point.

12-02-11 *Incompatibility's Allure* (to R. Schack)

Ishtiyaque Haji, *Incompatibilism's Allure: Principle Arguments for Incompatibilism*, PB, 26.95 USD.

13-02-11 *Incompatibility's Allure, 2* (to R. Schack)

Here's a nice review of the book, in case you haven't read it yet. This hotel is fantastic!

`http://metapsychology.mentalhelp.net/poc/view_doc.php?type=book&id=4752`

13-02-11 *Primary Stuff* (to A. Zeilinger)

Greetings from a jet-lagged night in Jerusalem. I am here to deliver a memorial lecture in the honor of Itamar Pitowsky.

I am happy to hear that you have offered a position to Schlosshauer. Thank you so much; I was very worried about him. Let's hope this works out to everyone's advantage.

Some small remarks on the deeper stuff in your note.

Zeilingerism 2 : *I think of Schrödinger, who in his "Meine Weltanschauung" regards himself clearly as a monist. He is an even more radical monist than any of us, because he believes that we are all part of one single all-encompassing consciousness!*

I have read this side of Schrödinger in the past and am very aware of it. (There is a discussion about it between Mermin and me in my book Coming of Age ... which, by the way, you should advertise around your group!) Just to make sure we have a common language for future discussions, though, I want to emphasize the difference between a "monist" — someone who only thinks there is only *one thing* to the world — and those people called "neutral monists". The latter should not necessarily be thought of as a species of the former. "Neutral monism" is usually meant as a doctrine that the world consists of *one kind of stuff* (to put it into contrast, for instance, with the Cartesian mind/matter distinction). For instance, Mach is sometimes called a neutral monist, but he did not take the world to consist of just one thing (as, for instance, the modern-day Everettians do with their universal wavefunction), but the whole mass of "sensations."

My hero William James, went particularly far in the direction opposite to monism (that the world consists of only one thing), and because of that, I almost think the label of "neutral monism" is particularly bad in his case. Thus I tend to call his position "neutral pluralism" (along with Ruth Anna Putnam). This comes about because of these two remarks of his.

First:

> My thesis is that if we start with the supposition that there is only one primal stuff or material in the world, a stuff of which everything is composed, and if we call that stuff 'pure experience,' then knowing can easily be explained as a particular sort of relation towards one another into which portions of pure experience may enter.

But then a bit later:

> Although for fluency's sake I myself spoke early in this article of a stuff of pure experience, I have now to say that there is no general stuff of which experience at large is made. There are as many stuffs as there are 'natures' in the things experienced. If you ask what any one bit of pure experience is made of, the answer is always the same: "It is made of that, of just what appears, of space, of intensity, of flatness, brownness, heaviness, or what not." ... Experience is only a collective name for all these sensible natures, and save for time and space (and, if you like, for 'being') there appears no universal element of which all things are made.

A very readable article on neutral monism can be found in the *Stanford Encyclopedia of Philosophy*: `http://plato.stanford.edu/entries/neutral-monism/`. I think there is one person mentioned in there that might particularly intrigue you because he builds his neutral monism on the concept of information. It is a philosopher named Kenneth Sayre (still alive possibly, but 82 years old). They have a quote of him, for instance, where he says:

> If the project ... is successful, it will have been shown not only that the concept of information provides a primitive for the analysis of both the physical and the mental, but also that states of information ... existed previously to states of mind. Since information in this sense is prior to mentality, but also implicated in all mental states, it follows that information is prior also in the ontological sense ... Success of the present project thus will show that an ontology of informational states is adequate for an explanation of the phenomena of mind, as distinct from an ontology of physical events. [And Sayre adds:] It is a reasonable conjecture that an ontology of information is similarly basic to the physical sciences ...

Zeilingerism 3: *maybe there is a point where we might have some slightly different view. I personally would see (a) our roles of being agents, and, if I understand you correctly, (b) physical systems not as something complementary or mutually exclusive. Rather, I think that we have to find a new vantage point from which we can unify both into one concept. ... So to me, in other words, when I want to unify the notions of information and reality, I do not view them as complementary aspects, but as two aspects or features or manifestations of the same entity. An association which comes to mind is the fact that we have learned that space and time are the same in a deep sense rather than complementary.*

Seeing your remark makes me think that I was probably too sloppy with my choice of the word "complementary," for it certainly does usually carry the connotation of "mutual exclusivity" for anyone who has read Bohr. But I don't think I meant to imply that aspect of Bohr's use of the word. (Of course, I don't really know what I mean yet—I am most definitely groping in the dark for the right concepts.) Thus, I think what I do want to say is something closer to an analogue (but with agent and object) of your "I want to unify the notions of information and reality, I do not view them as complementary aspects, but as two aspects or features or manifestations of the same entity."

The way James pulls it off, for instance, is to make thought and matter relational aspects of his neutral "pure experience". A given "pure experience" when considered in relation to one set of other "pure experiences" is called a thought; and when considered in relation to another set of other "pure experiences" is called matter. Moreover, he imagines every gradation in between (I think), and still other aspects in all kinds of dimensions that are neither thought nor matter.

But of course all of this is just the beginning of my thinking on these things (as expressed in my last three or four papers). I've got a long way to go before I'm going to be able to put some substance to the way I ended my paper `http://arxiv.org/abs/1003.5209` where I wrote:

> Quantum states, QBism declares, are not the stuff of the world, but quantum measurement might be. Might a one-day future Shakespeare write with honesty,
>
> > Our revels are now ended. These our actors,
> > As I foretold you, were all spirits and
> > Are melted into air, into thin air . . .
> >
> > We are such stuff as
> > quantum measurement is made on.

13-02-11 *In Town* (to M. Hemmo)

No need to call. Thank you for the suggestions.

I've had some sleep, and a bit of a walk outside. And I think I'm so taken with the tranquility here, that I'd just like to get some work done for the rest of today and tomorrow (in the compound, with only small breaks out into the neighborhood). If that's OK with you? I'm rereading Itamar's papers, preparing my talk, and finishing up my paper for you. What a place to do these things!

Yemima says that I will go to the Dead Sea Thursday. I will see you Tuesday; is that correct? Maybe by then my head will be clear enough that I'll starting more broadly about other things to do.

13-02-11 *In Israel?* (to N. Argaman)

It is true that Norsen's writings get under my skin, so the idea of having to talk about one of his papers for an hour comes with some trepidation. (See, for instance, our exchange in this panel discussion: `http://pirsa.org/09090098/`, as well as my talk here `http://pirsa.org/09090087/` whose title aims squarely at him.)

I'll make a deal with you. If you'll read Section 5, "The Essence of Bell's Theorem, QBism Style," of `http://arxiv.org/abs/1003.5209` before coming here, we can talk. (Section 5 alone is adequate; I'm not asking that you read the whole thing.) There, I try to say as clearly as I can why taking a view of quantum theory that implies the existence of undiminishing, instantaneous causal connections between distant objects carries a far greater danger epistemically than the opposing point of view ever could (i.e., the one of QBism, that quantum measurement acts create their outcomes, rather than reveal pre-existing properties). Without the idea of an autonomy at some level for all physical systems, how could physics as an analytic science ever get off the ground? That was Einstein's worry; that is mine. Giving up the idea of autonomy for physical systems is an immense can of worms, and Norsen closes his eyes to it. If one gives up on the idea of the autonomy of physical systems, how can one be so self-assured that even experimenters can be autonomous from the systems they are testing? Anyway, that's what I argue in the paper (and in the talk mentioned above).

If we have a deal, I'll ask Yemima how she has me scheduled for Tuesday, and I'll tell you what time is available for us.

14-02-11 *Corrections?* (to S. T. Flammia)

I'm up with jetlag, preparing my talk for Wednesday. Giving one in honor of Itamar Pitowsky, who died a year or two ago. Funny how little things affect you. I remembered well the closing lines of the first paper I had read of his. Seemed like an appropriate way to start the talk.

> More broadly, Theorem 6 is part of the attempt to understand the mathematical foundations of quantum mechanics. In particular, it helps to make the distinction between its physical content and mathematical artifact clear.
>
> — Itamar Pitowsky
> Closing words of J. Math. Phys. **39**, 218–228 (1998)

16-02-11 *Thursday?* (to I. Belfer)

Do you know how we're supposed to meet up tomorrow and at what time?

The Dead Sea would be nice, but it's not essential if things get in our way. I'll follow your advice. Mostly it'd be nice to talk about the history of quantum information.

Roly's Preply

> My name is Israel (Roly) Belfer. I am a PhD candidate (BIU) working under Prof. Silvan Schweber. The subject of my research is the impact of Information Theory and informational terminology on physics.
>
> I was excited to hear that you are coming to speak at the Pitowsky memorial event on the 16th (a friend of mine is the recipient of the prize). If you have the time, I would very much like to speak with you about issues of information in physics, such as the evolution of QIT (your new book — which I cannot get here yet); the different approaches to information in science; philosophy-of-physics concerns about information.
>
> If so, I would love the opportunity to buy you a cup of coffee and discuss these topics.

16-02-11 *Since October* (to D. B. L. Baker)

I looked in my files and see that I haven't written you since last October. Pretty pathetic. Where have I been since then? South Africa, DC twice, and even Texas once. And where am I now? I'm writing from a little café in the Old City part of Jerusalem, behind the wall, I think (hope) near the Jaffa gate. Tonight I put on a jacket and tie and give a memorial lecture for a physicist/philosopher/friend who died last year. But that's five hours from now; at the moment, I'm having a "premium quality" Israeli beer . . . if you can believe such a thing (at least that's what it says on its label). Between typing words I take a bite of chicken schnitzel. Basic stuff.

Yesterday, after some business, I spent three hours in the in the Israel Museum. The highlight was a little building called the "shrine of the book" where I got to see the Dead Sea scrolls. I read every word of every display and tried to let my mind float across the centuries. Funny thing, I thought, "When I get home, I've got to get the kids to watch *The Ten Commandments* again."

It's really an interesting city; I regret not coming here before. (I've passed up two opportunities before.) Tomorrow, I'm taking a drive to the Dead Sea. More than the salt in the water, I'm intrigued by it being the lowest land point on earth. (I think 400 meters below sea level.) I came to Israel 11 years ago, and at that time stayed in Haifa, with visits to Bethlehem, the Sea of Galilee, and the Golan Heights. Jerusalem is a very different area—much prettier and more meaningful to me. So far, the only thing a little spooky is watching a soldier or two walk by from time to time carrying a sub-machine gun. It was well spookier the day I sat outside a café in Stellenbosch watching a black guy getting beaten with dowels or bamboo or something. Old write-up of the incident to Kiki below.

17-02-11 *Symmetries of the Magic Measuement* (to J. D. Bekenstein)

Was it perhaps you who asked the question about symmetries at my memorial lecture for Itamar Pitowsky last night? It's just a hunch that it might have been you; if I mentally erase the mustache I see in your web pictures, it coincides a bit with what I remember of the questioner.

If none of the paragraph above makes sense to you, then I've got the wrong person! On the other hand, if it was you, and you have some interest, perhaps we could chat for a small time about these mathematical questions while I'm still in Israel Friday. It was not emphasized last night, but all of this work is a legacy of John Wheeler's imploring for an information-theoretic understanding of quantum theory. (See attached recent interview.) [See "Interview with a Quantum Bayesian," `arXiv:1207.2141v1`.]

17-02-11 *Symmetries of the Magic Measuement, 2* (to J. D. Bekenstein)

Thanks for your kind offer of time. I'm very tempted to take you up on it, even if you aren't the guy who asked the question last night — it'd be nice to tell you about this math problem I find so consternating, but at the same time holds so much promise as a new formalism for quantum theory. Besides, I've always wanted to meet you.

However, the more I think about it, the more I think I probably ought to take one last advantage of the historical sites here, to see what kind of insights they might plant in my head.

Would you like to come to the Perimeter Institute sometime to discuss information in physics? I could bring you as my guest and take care of all expenses, etc.

Attached is an article for a Canadian Physics-Today kind of magazine that explains a bit of the open math problem in its last section. [See "Quantum Bayesianism at the Perimeter," `arXiv:1003.5182`.]

I hope you'll say "yes" to the invitation. As far as I know, you've never visited us at PI.

18-02-11 *On the Difficulties of Communicating across Professional and Philosophical Lines* (to I. Belfer)

I enjoyed yesterday very much and it was a pleasure to get to know you. You strike me as having a very sound head on your shoulders, and you gave me some good food for thought.

Here are the things on communicating with Leggett and the like that I mentioned yesterday. See [this samizdat] at the letters titled "Slammed by the Closet Door" and "November 8th" starting at pages 1177 and 1181, respectively. Tony Leggett is a very kind and gentle and great man. A great scholar and a penetrating thinker all around. And I would never want to hurt his feelings. But that is not to say he will ever understand the message of the quantum — there my gamble would be against him.

The Rotman Institute that I was telling you about yesterday can be found here:

`http://www.rotman.uwo.ca/index.shtml`.

I think it would be fantastic to have you in the neighborhood for a couple of years. Write a good PhD and get yourself noticed!

18-02-11 *The Chances of a Neutral Monism* (to H. Price)

I saw your cryptic job offer a while ago (why was it announced before it was announced?). Anyway, many, many congratulations! Here's an issue: If you become the Bertrand Russell Professor of Philosophy, will you take neutral monism (even neutral pluralism) as seriously as he did?

18-02-11 *First QBist Church of the Holy Sepulchre* (to H. C. von Baeyer)

As I recall, you were never completely happy with either putting the reference SIC up in the sky or putting it in a vault at the Bureau of Standards. What would you think about putting it in a Holy Sepulchre? It's a counterfactual measurement in any case and was never truly meant to be approached? The only thing I don't like about the image of the sepulchre is that it conveys the idea of something dead, whereas the SIC is really something more like the Holy Spirit.

Greetings from *somewhere* in the Old City of Jerusalem — I have no idea where. I have the hope that it is close enough to the original Church that I'll eventually stumble across it. I've been in town since Sunday to give a memorial lecture for Itamar Pitowsky, who died a bit over a year ago. The talk turned out pretty nice, I think, or at least I got a lot of praise for it. I included many anecdotes of Itamar for his family and his nontechnical colleagues, and I tried to give enough technical details to please his spirit up in the sky (and the 15 people in the audience who might understand it).

This has been a place of thought for me, letting my mind rove back to the writing of the Dead Sea scrolls, trying to feel the fear of Masada, watching as Jesus carried the cross down these small streets ... *Actually, at the very moment I write this, a fight has erupted at the fountain near me.* ... I will return eventually from another café.

... OK, it's a few hours later now. I did find the Church of the Holy Sepulchre, and many other things beside. At the Western Wall, I told an African couple a story about the time I ran into Arnold Schwarzenegger at the Pasadena flea market. When a vendor ran after him with a pencil and pad to get an autograph, he said, "Not now, later, I'll be back."

And so, I too — I am back. I've been wanting to write you for a couple of weeks, but of course things got delayed. At least this is the best place to write you from anyway. I told you that I've been following Marcus's therapy. That's been somewhat successful. We had good philosophical discussions every day at lunch, and I've launched into a new study of "neutral monism" (I hate that term though). I spent some time reading and contemplating what I was learning from Erik Banks's book *Ernst Mach's World Elements*. Before this, I really knew very little about Mach — I was always under the impression that he was a proto- logical positivist, but he really wasn't much like that at all. A much more interesting man.

Of course, it leaves me with a million questions on Mach's ultimate influence on Pauli. For instance, I have long known about Pauli's longing for a psycho-physically neutral language for describing physics. But how much of this is due to his interactions with Jung, and how much more is due to the influence of Mach and Mach's writings upon him? I had not sufficiently appreciated before that such "neutral stuff" was what Mach was seeking/positing with his "sensations" or "world elements". Nor had I appreciated how he ultimately wanted to de-anthropocentrize the idea (or at least Banks holds forth enough quotes along these lines to make one think it's true).

Lately I have not been able to get the fake Shakespearean lines I put in the last paper off my mind:

> Quantum states, QBism declares, are not the stuff of the world, but quantum measurement might be. Might a one-day future Shakespeare write with honesty,
>
> > Our revels are now ended. These our actors,
> > As I foretold you, were all spirits and
> > Are melted into air, into thin air . . .
> >
> > We are such stuff as
> > quantum measurement is made on.

The next step in my education in this regard is to study the work of the "American New Realists," particularly Ralph Barton Perry. Perry was a student, follower, and biographer of William James (the very best one in fact), who tried to move past his master, particularly, by working to carefully de-anthropocentrize James's "pure experience". I've started the bible of the movement this week, *The New Realism: Cooperative Studies in Philosophy*, a collective work by the leading six new realists. And it'll be my main reading on the flights tomorrow . . . all three of them! (From a Clash song circa 1982. "These are your rights . . . Know your rights . . . All three of them!")

Where do we go from here? I don't know at all, really. I wrote confidently in Schlosshauer's interview:

> The first [thing to do] is to find a crisp, convincing way to pose quantum theory in such a way that it gets rid of these trouble-making quantum states in the first place. What I mean by this is, if quantum theory is actually about how to structure one's degrees of belief, it should become conceptually the clearest when written in its own native terms.

What I mean by that, you know, is to re-write the Born Rule as a statement about probabilities based on a SIC representation — i.e., the urgleichung. But does that in any way move us closer to Pauli's psycho-physically neutral language? This has been on my mind all week.

Suppose I were to get all I wanted out of the urgleichung: I.e., that its consistency (in the sense of never generating negative probabilities), the identification of priors with posteriors and vice-versa (as we have been doing in the last papers), and a maximality condition, taken with one further mild assumption (yet to be identified), completely specifies quantum-state space. Suppose I get that holy grail of quantum-state space from the urgleichung predominantly. Of course I'll be immensely pleased by the simplicity of the product. It will mean that I have isolated the essence of the formal structure of quantum theory to a single counterfactual situation. I will have pulled the system's dimensionality to a place of prominence in our understanding — it is the extent of the deviation from the law of total probability in our counterfactual scheme.

All of this is fine. But how does it help us get closer to identifying the next step? That's what I'm not seeing at the moment. Maybe it's just too distant into the future, and we've got so much to do before then that it's not worth thinking about this *yet*. But what better place to try to see farther than one normally can than in a short visit to the Holy Land? (How much damage can be done in a short visit?)

Behind quantum mechanics is the idea that "we could do otherwise" in any experimental situation. *If* all the above is true, through Kochen-Specker type arguments on the final product, we know that in any experimental situation the world can, in return, do otherwise. I.e., there is no hidden-variable model for our urgleichung considerations. "We can do otherwise." "The world can do otherwise." Is that the extent of the psychophysical neutral ground Pauli dreamed for? There's got to be something much bigger, much deeper than this!

And it must play off the particular form of the urgleichung, if it's going to play off anything. But how so?

How so? How so?? How so?!?! I don't want to wait another 10 years for it to come to me. But I am as blank at the end of this note as I was at the beginning.

Yesterday, I went to Masada with a nice grad student, Roly Belfer, who's doing his PhD with Sam Schweber. His thesis will be on (roughly) "information as a style of thinking within physics." I very much enjoyed his company. A cute thing was when he was praising your book on information, I revealed, "Yes I know the book; we are close friends." You should have seen his eyes bug out in only the way the young ones do when they find out something interesting. It was as if he had met someone who had met a demi-god.

We brought up the possibility of your visiting PI in our last conversation. What do you think? It could be really good for giving us re-orientation. (Marcus needs it as much as I do!) Marcus will be in Waterloo March 17 to April 9. And I will be back from the APS March meeting starting on the 27th. I shouldn't be traveling again until April 28th. It'd be nice to get the triumvirate together again; we haven't held congress in a while and, no doubt, our philosophies are lacking for it.

Three prayers from the Holy Land!

19-02-11 *Alas!* (to A. Wilce)

Wilce-ism 10: *I hope your talk in Jerusalem went well – I'll be interested to know what sort of reactions you provoked.*

On a less happy note, in trying to fix what seemed a minor error in the manuscript I recently sent you, I found the entire thing unraveling. In fact, it's a tissue of errors, all stemming from one horrifyingly elementary mistake (the parameter "s" in Lemma 3 is generally negative).

I may yet be able to salvage something from this wreck, but for now, please accept my sincere apologies for troubling you with such a mess! And please pass along both this message, and my apologies, to Matthew.

I'm sorry to hear about this. I'll keep my fingers crossed that you can salvage a proof, and I'll make sure to buy you an extra beer in Dallas.

The talk went quite well. The way I wrote H. C. von Baeyer about it:

> I included many anecdotes of Itamar for his family and his nontechnical colleagues, and I tried to give enough technical details to please his spirit up in the sky (and the 15 people in the audience who might understand it).

It turned out that Hilary Putnam was not in the audience. I looked out across the room not knowing that, as I started to tell of the party where I last saw Itamar. I asked, "Is Hilary here?," mentioning that he too had been at the party. When I found out that he wasn't, I decided to report the conversation I had had with him that night. Hilary said, "Could you hold this for a second," handing me his cell phone. He dug quickly into his wallet for some piece of paper, and then said "thank you" as he took the cell phone back. I nodded.

In depth conversation with the man who sweepingly called *all* information-oriented reconstructions of quantum theory "instrumentalism with a post-modern sauce." I'll probably never recover from the trauma of that weekend: I really lost most of the intellectual respect I had for him. In any case, you can now tell your kids and grandkids, "Chris, bless his soul, respected me more than Hilary Putnam … even after my 4.5 axioms unraveled!"

20-02-11 *Monism Is the Devil* (to H. C. von Baeyer)

Good morning, this time from somewhere in my kitchen. The world is a kind of blooming, buzzing confusion this morning; everything is kind of shiny out of the sides of my eyes. I didn't get back to Waterloo until 2:30 this morning — 31 hours of travel total! It was a tortured route to begin with (just to get American Airlines miles), but then all the delays …

I'm sorry to have caused you confusion. That damned term—"neutral monism"—is trouble, and your letter, along with an earlier one from Anton, convinces me to banish it forever and never use it again! Hereafter "neutral monism" shall be a dead term for me. I should have explained to you like I explained to Anton after I saw his own confusion.

Here follows an excerpt from the note to Anton. … Actually, let me just copy in the whole note below. [See 13-02-11 note "Primary Stuff" to A. Zeilinger.] The hatched parts, of course, correspond to quotes from his earlier note to me. In a nutshell the trouble is that "monism" is the doctrine that the world is "one thing," whereas "neutral monism" is associated the idea that the world is composed of "a stuff of one kind of character" (even though it may be plural to the bones). See the quotes from James below to see how that fits in with his pluralism. Ruth Anna Putnam calls it "neutral pluralism," but I don't like that term so much either. It remains a bit lifeless in comparison to the heavy duty it is meant to be called to. But if "neutral pluralism" is lifeless already, "neutral monism" is surely dead, dead as doornail.

Trying to find the appropriate term has word for whatever is going on here has been a real thorn in my side, even before this nail in the grave of yours. That was the cause of my joking note with the word QBoom. On a more serious note, Marcus has toyed with urspracht … but I don't think I like that one either (or at least he hasn't convinced me yet).

Let me know if all this relieves the confusion I induced. If my family will let me go, I'd like to write you a bit more on the subject. But now they've kicked me off the table so they could set it for breakfast.

What's your physical location now?

20-02-11 *Urspracht* (to H. C. von Baeyer and D. M. Appleby)

From our emails this morning.

Chris said: "Trying to find the appropriate term for whatever is going on here has been a real thorn in my side, even before this nail in the grave of yours. That was the cause of my joking note with the word QBoom. On a more serious note, Marcus has toyed with urspracht … but I don't think I like that one either (or at least he hasn't convinced me yet)."

Hans said: "The word urspracht is gibberish. What do you and Marcus mean?"

Well, if it is gibberish, that might be a partial selling point for it (as you'll see in Marcus's last paragraph, which is a point I definitely agree with, about "keeping the bastards guessing"):

> But I think on reflection that the best option might be to avoid presenting any kind of fixed target for the philosophical labeling guns. The trouble with the two above suggestions is that they might lead to us being labeled as "mystics". And I no more want Hilary Putnam saying that we are just a pair of mystics, than I want him saying we are just a pair of empiricists. I want to keep the b***** guessing. Whilst at the same time being totally clear, and totally explicit.

But anyway, below is Marcus's original note on the sort of thing he was after, at least at that time. (Which, there is probably no need to say, may not be exactly the same thing that I'm trying to get after. But what I am looking for may have a lot of elements of what he is after too—I'm not sure—it's all so much confusion at the moment.)

21-02-11 *Einstein on Religion; Pragmatism on Spinoza* (to Å. Ericsson)

I greatly enjoyed reading through these quotes. My guess is you won't as I did; but still I thought I'd make you aware of them: `http://www.stephenjaygould.org/ctrl/quotes_einstein.html`.

A lot of Einstein's arguments would be my own. On the other hand, Spinoza's worldview in its details is certainly foreign to me. I like the way Ralph Barton Perry put it as the antithesis of pragmatism:

> The perfect antithesis to pragmatism is Spinoza, and it is the perpetuation of Spinozism in objective and absolute idealism that is the real object of the pragmatist attack. Absolutism is other-worldly, contrary to appearances; pragmatism [is] mundane, empirical. Absolutism is mathematical and dialectical in method, establishing ultimate truths with demonstrable certainty; pragmatism is suspicious of all short-cut arguments, and holds philosophy to be no exception to the rule that all hypotheses are answerable to experience. Absolutism is monistic, deterministic, quietistic; pragmatism is pluralistic, indeterministic, melioristic. That which absolutism holds to be most significant, namely, the logical unity of the world, is for pragmatism a negligible abstraction. That which for absolutism is mere appearance — the world of space and time, the interaction of man and nature, and of man and man, is for pragmatism the quintessence of reality. The one is the philosophy of eternity, the other the philosophy of time.

22-02-11 *Flesh and Blood* (to R. W. Spekkens)

Here is that line I was proud of:

> What is the source [of truth-value assignments]? When it comes to formal logic, one is tempted to think of it as the facts of the world. The facts of the world set truth values. But it is not the world that is using the calculus of formal logic for any real-world problem (like the ones encountered by practicing physicists). The "source" is rather a finite subscriber to the service, one with limited abilities and resources; the source is always one of us—flesh and blood and fallible through and through—the kind of thing IBM Corporation is taking its first baby steps toward with its *Jeopardy!*-playing

supercomputer Watson. The source of truth values in any application of logic are our *guesses*. Thus, it would be better to be completely honest with ourselves: Applications of formal logic get their truth values from an *agent*, pencil and paper in hand, playing with logic tables not so differently than crossword puzzles. The facts of the world only later let the agent know whether his guesses were acceptable or unacceptable judgments.

I agree that I should read more Dennett (I have read some) ... I know there's insight to be gained there. But I read a little piece in Perry yesterday about how Spinoza is the "perfect antithesis" to pragmatism (I'll copy it below), and I think it quite captures my pre-conceived notion of Dennett, i.e., that he is my antithesis. [See 21-02-11 note titled "Einstein on Religion; Pragmatism on Spinoza" to Å. Ericsson.] I'm aware that this is what is providing the friction to my reading.

22-02-11 *Solvay Conferences* (to I. Belfer)

Thanks for the Galison paper; it looks like it is going to be a lot of fun.

The Jammer paper I was referring to was: Max Jammer, "The EPR Problem In Its Historical Development," in *Symposium on the Foundations of Modern Physics: 50 Years of the Einstein-Podolsky-Rosen Gedankenexperiment*, edited by P. Lahti and P. Mittelstaedt (World Scientific, Singapore, 1985), pp. 129–149.

Jammer writes,

> ... what Einstein had in mind is confirmed by a letter which Ehrenfest wrote to Bohr on July 9, 1931. As Ehrenfest reports, Einstein uses the photon-box no longer to disprove the uncertainty relation but "for a totally different purpose." For the machine, which Einstein constructs, emits a projectile; well after this projectile has left, a questioner can ask the machinist, by free choice, to predict by examining the machine alone either what value a quantity A or what value an even conjugate quantity B would have if measured on the projectile. "The interesting point," continued Ehrenfest, "is that the projectile, while flying around isolated on its own, must be able of satisfying totally different non-commutative predictions without knowing as yet which of these predictions will be made ..."

The contrary story comes from Don Howard's translation of the Ehrenfest letter:

`http://www.nd.edu/~dhoward1/Early%20History%20of%20Entanglement/sld026.html`.

Don has a more extended discussion of this point in one of his papers but I can't find it in my files at the moment. I think you should be able to find it on his website.

23-02-11 *The Very Improbable Tamworth Iron Works* (to H. C. von Baeyer)

A most improbable thing happened to me yesterday, and I've been racing with adrenaline in my veins ever since. It's a long story, which I will tell below ...

... But first an introduction from Linda Simon's book *Genuine Reality*:

> In September [1886], James set out on a real estate search that ended at the base of Mount Chocorua, in a town called, infelicitously according to James, Tamworth Iron

> Works. The "small farm" that interested him consisted of seventy-five acres, most of it woods, the rest hayfields. The shingled, green-trimmed farmhouse, built in the year James was born, needed considerable work, but it was large enough—fourteen rooms, eleven with doors that opened to the outside—included a big barn, and looked out at Chocorua Lake and the mountain. "It is only 4 hours from Boston by rail and 1 hour's drive from the station," William told Henry. "Few neighbors, but good ones; hotel a mile off. If this is a dream, let me, at least indulge it a week or so longer!"

The very improbable thing that happened to me yesterday was that I set out to make arrangements for the MIT meeting May 3rd and 4th, and I got distracted by the hotel address. "Is that the street William James lived on?" So I did a Google search on "william james", "home", and "cambridge". "Nope, he lived on Irving Street; I should have known that!" But one of the other things that came up was a YouTube video of the house. Curiosity got me, and I watched it. Kind of creepy really; just someone standing outside with his video camera. But, you know, curiosity sometimes doesn't know what's best for it. I thought, "I wonder if the same guy took a video of the Chocorua house?" and so changed "cambridge" to "chocorua" in the Google search. He didn't, it turned out, but I did find a video by a real estate company instead!

What a place it turned out to be. 4,387 square feet, 44 acres, barn, swimming pool, and woods, woods, woods. Have a look yourself: [...] If you click on the little down arrow at the address, a box will open with details on the house. And if you go to this website, [...] you can get a satellite view of the property, right next to state forest and national conservation ares. Zoom out enough, and you'll find the nearby lake. And if you go to this wiki link `http://en.wikipedia.org/wiki/Chocorua,_New_Hampshire` you'll see it described as "among the most beautiful lakes in the White Mountains." Half hour drive from good skiing (lots of skiing in fact), and an hour and a half drive from the seashore, similarly to Portland, etc.

Their asking price had been $895,000, but it's off the market. Curiosity kicked in again, and I called the real estate company. Why off market? The agent I talked to said that she would talk to her colleague who had listed it and get back to me. She said one possibility was the owners simply changed their minds about selling. Another was that when Winter encroached and it had not sold yet, maybe they took it off the market till Spring when the weather would be better.

Anyway, all of that was enough to spur all kinds of crazy thoughts. My imagination has been running wild all night. Ever since Kiki and I uncorked a bottle of wine in 2001 and sat down dreaming over Peter Robertson's book of the early years of the Niels Bohr Institute (including floor plans of the place), we've had a fantasy of a little institute where big things might happen. And wouldn't you know it, the details of the fantasy were precisely this: An old farmhouse somewhere in New England, with some forested land to build a few bungalows and a small conference center on. Kiki would be the caretaker and social director; I would manage the science and philosophy and, somehow, someway, bring the money in.

I remember Dr. Ruth Westheimer in the 1980s saying, "Fantasy is OK, even encouraged, as long as it's not actualized." The trouble now is that this *fact* hits so close to the *fantasy*! I had never imagined that that much of the James land was still intact. 44 acres in an idyllic setting! And surely of all places on earth, this one is the very spiritual home of QBism.

I've just been overwhelmed emotionally. How, how, how could I make it happen? Or at least buy the damned thing for myself and the family, until somewhere down the road a real institute might materialize? Or even just keep the thing off the market for a few years ... so that some "investor" doesn't get the idea of making it a two million dollar home before I can get to it?

Easy enough, right? I just sell this house, and buy that one. Yeah, right! The trouble with a house is that it ought to be close to one's source of income! And what income would I have without the Perimeter Institute.

Unfortunately, even my craziest ideas aren't crazy enough. For instance, I had a little fantasy that I would pay my $200K of it for my in-laws (to sweeten the pot); they sell their house outside Munich and move back to New Hampshire, with the agreement that we eventually inherit it from them, instead of their German house. (My father-in-law had started as an assistant professor of history at UNH in Durham, before joining the business world; they still have lots of old friends in Durham.) But two things count against the idea: 1) I expect them to live a long, long time, and 2) even though my mother-in-law has talked of moving back to the States from time to time, they haven't precisely because my father-in-law loves Germany too much.

Of course, it could be that the house will never come back on market anyway. It is curious the date the house went on market: 27 August 2010. The very day after the 100th anniversary of William James's death. (He had died in the house, by the way.) Two weeks before that, there had been a symposium in Chocorua, with house tours, etc. Thus it has struck me that it could be that the owners put it on market because of the enthusiastic reactions they were getting from the participants, and they thought they might feel out how much money they could get. Maybe they learned that they couldn't get nearly so much in that area; maybe they learned that they could get a lot more if they play their cards right and market it appropriately.

Anyway, dreaming, dreaming. "If this is a dream, let me, at least indulge it a week or so longer!"

There, I had to get that off my chest! The question now is do you have any ideas? Do you know of any philanthropists who might be convinced to help finance an institute on pragmatism and the quantum? Do you know of any physics or philosophy departments near Chocorua that it would be fruitful to inquire into? (Perimeter tenure coming? Who would care about Perimeter who could live and think in William James's home?!) Do you have any wilder ideas still? A thought on how to make a QBist commune viable?

But dreaming, dreaming … Now with too much adrenaline and *too much coffee*!!

Oh, long note on "neutral stuff in the pluriverse" coming soonish; it's been percolating in my head since the weekend. (We need a better name than "neutral stuff"!)

23-02-11 *Whitehead on James, 2* (to A. Hamma)

It was nice to learn of your philosophical inclinations the other day. I don't have *Process and Reality* in front of me to find the lines where Whitehead said that his effort was to extend and make precise James and Bergson. But below are some quotes I sent to Appleby on Whitehead's regard for James. [See 21-09-10 note titled "Whitehead on James" to D. M. Appleby.]

Attached also is a little interview I wrote up recently. In the answers to Q2 and Q10, I made the point that you seemed to like.

Let's indeed talk sometime. I'd love to learn more about Whitehead's thought.

24-02-11 *Whitehead on James, 3* (to A. Hamma)

No, I didn't know that. Bryce had said it when I was an undergraduate taking a class on Lagrangean mechanics from him. It was just something that caught my ear then. If you find the precise Whitehead quote, please send it my way. I enjoy collecting such things.

Alioscia's Preply

Thank you for sharing the interview. I am reading it with great interest.

By the way, do you know that what Bryce DeWitt said about mathematics is itself a quote (maybe involuntary) from Whitehead? Whitehead said the same thing in the introduction to algebra, if I recall. It is interesting that also Hannah Arendt shares a similar opinion. It would be great to find some time to talk about this and other philosophical topics!

Alioscia's Reply

This is the quote I promised. It is in *Introduction to Mathematics*, around page 60 (depending on the editions). Whitehead is talking about the importance of symbolism in mathematics. He is showing an example in algebra where identities are very easy to state symbolically while they are difficult to explain in words. Then he says:

> This example shows that, by the aid of symbolism, we can make transitions in reasoning almost mechanically by the eye, which otherwise would call into play the higher faculties of the brain.
>
> It is a profoundly erroneous truism, repeated by all copy-books and by eminent people when they are making speeches, that we should cultivate the habit of thinking of what we are doing. The precise opposite is the case. Civilization advances by extending the number of important operations which we can perform without thinking about them. Operations of thought are like cavalry charges in a battle — they are strictly limited in number, they require fresh horses, and must only be made at decisive moments.

24-02-11 *Whitehead on James, 4* (to A. Hamma)

And here are the lines from the preface of *Process and Reality*. My false memory made it much more detailed than it really was.

> Among the contemporary schools of thought, my obligations to the English and American Realists are obvious. In this connection, I should like especially to mention professor T. P. Nunn, of the University of London. His anticipations, in the *Proceedings of the Aristotelian Society*, of some of the doctrines of recent Realism, do not appear to be sufficiently well known.
>
> I am also greatly indebted to Bergson, William James, and John Dewey. One of my preoccupations has been to rescue their type of thought from the charge of anti-intellectualism, which rightly or wrongly has been associated with it. . . .

It's good to know one's intellectual grandfathers. As a coincidence, I had only recently started studying the "American New Realists" (to which Whitehead is undoubtedly referring to), particularly (at the moment) the writings of Ralph Barton Perry.

24-02-11 *The Manic and the Depressive* (to H. C. von Baeyer)

If Gleick could get away with *The Information* for the title of his book, I figure I can get away with using inappropriate definite articles in my own titles. (This is not stroking: I don't like the title. It could be that when I read the book, I will find it to be very natural and suggestive for the work, but at the moment, to my eye, it sticks out like a sore toe.)

Anyway, to THE subject. Oh, a soberer mood started to set in last night while Kiki and I looked over the house plans and details, and I started—as I knew I would eventually—to admit defeat to myself. With honesty, I am no position to see this kind of thing through at the moment.

Any institute would have to be three, four, five, six years down the road. It would take so much effort to build the idea, the support, the funding, the logistical infrastructure. The real concern for me (the one that put me into the manic panic) is that the property be secured before some feckless investor or vacation-homer buy it and do more damage.

You are right that the essential idea is James's aura. That land and house and barn that he purchased for $750 is the geographical center of his aura. He died in the house; it was where he wanted to die. If ghosts live, as James had so hoped to gain evidence for with his dozens of interviews with mediums and his presidency of the American Society for Psychical Research, then his ghost lives in that house. The left horn of my own dilemma is that I feel I am possibly the only person alive who could channel that ghost to the good of physics. It may be lunatic, but it is a responsibility I feel. But the sharper right horn remains: I am in no position to buy that property on the fly like this.

Your question about the architecture is not lost on me, and certainly not Kiki. We have pored over the photos and the listing, looking for clues of this and that. There are certainly places where we can see that the ceiling has been dropped (kitchen being the most obvious example, but also the red-walled foyer). On the other hand, it looks that quite some of the original does remain. Only direct eyesight, and Kiki's expertise, with a physical visit will give me a truly accurate picture though.

You might have a look yourself. The realtor posted the old listing for me: [...] The little camera at the top of the webpage (for viewing photos) appears to be redundant—there are no more photos than are on the first page—but if you click on the paper clip, you'll find house plans and a survey of the farm. Here's a sketch of the outside from 1902 for some comparison: `http://www.uky.edu/~eushe2/Pajares/JamesChocorua.html`. And this photo gives some sense of how the stone walls and hay field have stayed the same since James and Royce gave a similar pose in 1903:

`http://www.flickr.com/photos/wjsymposium/5269017904/in/set-72157625617564916/`.

On the other hand, the James house is clearly not preserved as accurately as this other (though much younger) house in the area: [...] The wood trim really speaks for itself. (And—though it pierces my heart—Kiki has made it clear to me that she finds this house much more interesting than James's own.)

One thing that becomes clear by comparing the two listings (and some others), is that the owners of the James home know that they are selling his ghost more than the house. The +$270,000 / -1274 sq ft differences say a lot. (But there is a -33 acre difference too, and maybe I should give that more credit.)

I also know by direct experience that your remark, "starting with a building often ends in disappointment," is accurate. But then I remind myself of your other remark that the issue is an aura, not a building. Maybe I am particularly sensitive to this (or particularly illusioned by this) because it all comes just after my visit to Jerusalem's Old City. In the end, it is not as much the building as the ghost. Think of how holy the Western Wall is, the mere remnant of a temple though it be.

Woe is me. Kiki and I will certainly go see the thing if it is still up for grabs by the time the March Meeting comes and goes. But it'll almost surely be a painful act of self-flagellation.

The only Rutherfordity I know is, "To any young man who utters the word 'universe' in my laboratory, I ask him to pack his bags and leave." (Or something along those lines.)

On your suggestion about the James Society, one would think that they would have already been in action, given their symposium in Chocorua last year. Delving further into the documentation the realtor made available, I note that the owners actually put the house on the market three days before they gave the guided tour of it. Surely the appropriate people were all abuzz with the news, and the sellers wanted it that way.

von Baeyerism 66 : *The place seems to be halfway on a straight line between my brother-in-law in Kennebunk, ME, and my cousin in Weybridge VT. A lovely place.*

You never thought about moving closer to your family, did you?

25-02-11 *Home and Home* (to W. G. Demopoulos)

I'm glad to hear that you're back home!! It's much earlier than I expected actually. I wish you the best possible recovery.

Let me give you some light reading for your iPad; maybe some of it will make you chuckle. (My guess is you're spending time in bed mostly?) The first attachment below answers your question about my talk in Jerusalem. The next two (of many more than that that I won't bore you with) detail a bit of my crazy quest to find some way to buy William James's Chocorua, New Hampshire home. If you have any ideas, feel free to suggest! [See 23-02-11 and 24-02-11 notes "The Very Improbable Tamworth Iron Works" and "The Manic and the Depressive" to H. C. von Baeyer.]

Somewhere in my reading in Israel I made a note on "effects" (either from Schiller or Perry or, perhaps, the new realist consortium) that I wanted to send to you. I will try to dig it up tomorrow. (Unfortunately my wife is forcing me to ski this morning . . .)

25-02-11 *The Biggest and Best* (to I. Belfer)

Belferism 1 : *Can you think of indispensable highlights of the quantum-information era (like your statement about the inauguration of QIT as a real field after 1993)?*

Off the top of my head:

1973: Alexander Holevo proves his famous bound that limits the information that can be retrieved from a quantum system

1979: Holevo points out that the classical capacity of a quantum channel is nonadditive; Holevo's discovery was essentially unrecognized, until it was rediscovered by Asher Peres and Bill Wootters in 1991, when it became the problem that motivated quantum teleportation later in 1993

1982: Richard Feynman's paper "Simulating Physics with Computers" (in my opinion still one of the speculations closest to being correct on what powers quantum computation)

1982: Wootters and Zurek, and independently Dieks, point out the quantum no cloning theorem

1984: Bennett and Brassard's first quantum key distribution protocol

1985: David Deutsch's paper "Quantum theory, the Church-Turing principle and the universal quantum computer" (first example of a non-quantum-simulation problem that gets a speed up from quantum equipment)

1992: Ben Schumacher invents the idea of a qubit on a drive to the Columbus, OH airport, in conversation with Bill Wootters; finally appears first in a 1995 paper (with a 1993 submission date), but had long been a common term in the community by then

1993: Quantum Teleportation

1994: Peter Shor's factoring algorithm (many would mark the beginning of the field here; but I stick with 1993 as being deeper conceptually)

1995: Peter Shor and independently Andrew Steane discover the ideas of quantum error correction and fault tolerance (enabling the idea that quantum computers can actually be built)

1995: Cirac and Zoller's paper "Quantum Computations with Cold Trapped Ions" (the first serious implementation for quantum computation ever proposed)

2000: Robert Raussendorf and Hans Briegel's paper "Quantum Computing via Measurements Only"

2001: Fuchs's paper "Quantum Foundations in the Light of Quantum Information" points out the true path to progress in the quantum foundations debate! ☺

At least that's what comes to mind as I have my first cups of coffee this morning.

26-02-11 *Feelings of Guilt* (to the QBies)

PPS. I refined the definitions of QBoom and QBlooey (with no off color jokes this time either):

- **QBoom** – cf. *kaboom*; the sought-for deanthropocentrized distillate of quantum measurement that QBism imagines powering the world. William James called it "pure experience," where "new being comes in local spots and patches which add themselves or stay away at random, independently of the rest." John Wheeler asked, "Is the entirety of existence, rather than built on particles or fields or multidimensional geometry, built on billions upon billions of elementary quantum phenomena, those elementary acts of 'observer-participancy'? ... Is what took place at the big bang the consequence of billions upon billions of these elementary 'acts of observer-participancy'?" In place of a Big Bang, the QBist wonders whether it might not be myriads and myriads of little QBooms!

- **QBlooey** – cf. *kablooey*; a QBist slur on the usual conception of the Big Bang, where the universe had its one and only creative moment at the very beginning.

27-02-11 *Home and Home, 2* (to W. G. Demopoulos)

By the way, not to worry, the version of the story I told at my talk, was a lighthearted, gentle version targeted to make people laugh. Everything said with a smile. I didn't say all the business about the postmodern sauce and his newfound Bohmianism, etc.

You are correct that the instrumentalism remark was confined to the conference. Sorry to conflate two stories into one in the email I forwarded to you. I wasn't bothered by the charge of instrumentalism; it was the "postmodern sauce" that got under my skin. It's a general symptom of philosophers that they ignore that there are NEW equations and theorems behind this interpretative effort. And to hear the charge of it being a "postmodern sauce" just went too far.

But now you catch me off guard yourself:

Demopoulosism 46: *I'd be very surprised if you can get anything out of neutral monism but would love to talk with you about it; in my view it's not that different from what underlies "flash ontologies." What is distinctive about the BDP point of view is its eschewal of ontology, its embrace of a moderate instrumentalism which, however, is* not *anti-realist.*

Flash ontologies?!?!?!? Not at all!!!! You should expect something much more subtle from me than that by this point in our relationship! I can see I've got my work cut out for me to try to explain myself (more precisely explain my desires). Alright, I take the challenge. But you'll have to wait some time for me to compose myself.

Stay away from germs for the next 83 days!

Bill's Reply

> Looking forward to hearing more from you on neutral monism.
>
> I don't think NM's connection with Flash Ontologies is obvious, and I may be very mistaken about this. But the development of NM in *Analysis of Matter* by Russell is very suggestive of a connection. I appreciate that you don't care for Russell and that you're more interested in James's version. But R's development tries hard to spell out James's metaphors. Let me know what you come up with.
>
> As for Hilary's remark about "PoMo sauce," I'm not sure what he had in mind. Perhaps the notion that instrumentalism can be derived from a more general relativism? I think this is not entirely alien to your view of it and its connection with personalism, but I say this only to insert a further sense of urgency in you to write further on this stuff (pardon the pun)!

27-02-11 *Pragmatism in Sweden* (to Å. Ericsson)

... a gift for Sunday morning!

[See: Sami Pihlström, "Nordic Pragmatism," Euro. J. Pragma. and Amer. Phil. **2**(1); online at `http://lnx.journalofpragmatism.eu/`.]

27-02-11 *SAAP Membership* (to W. T. Myers)

How I wish I could attend your annual meeting, now that I've discovered the society. But March 13, I head to Tulane U to give their Clifford lectures this year, and then immediately after that it's to Dallas for the big American Physical Society meeting—I organized the part for the Topical Group on Quantum Information this year (being the chair), and we're having over 400 talks! The point is, though in principle I could get to the SAAP meeting, it hits at an awfully bad time for me. I'll try my best to be at next year's meeting.

Seeing the link to your webpage below, I got snoopy, and I'm glad I did. It's good to meet another University of Texas alumnus. UT Austin is without doubt my spiritual home. (I tell some of the story in my answer to Question 1 in the interview I sent you yesterday.)

I'm also quite intrigued by your research. Where I am headed with my own is to develop a kind of "process physics" behind quantum measurement theory, though I haven't been so much in the habit of calling it that. (I do so for you.) You can see some of the yet-very-vague thoughts I am having at the end of this recent paper of mine `http://arxiv.org/abs/1003.5209` and also at the end of the interview. They will certainly be beefed up one day or another.

I would be much appreciative if you could send me a copy of your PhD dissertation. From the abstract you post, it seems to be just what the doctor ordered. An electronic version of it would be just fine.

27-02-11 *Lenovo Machine* (to C. M. Caves)

Random things now.

1) I found these words in some old correspondence from you a couple days ago, and I really liked the way you put it:

> [QBism takes] a three-pronged approach to subjectivity and objectivity: (i) quantum states and probabilities are wholly subjective; (ii) system attributes are wholly objective; and (iii) measurement outcomes are where the rubber meets the road, i.e., where subjective and objective meet to produce something that is not under the control of the agent, but is also not out there in the world.

Very eloquent.

2) I really wish you were more a fan of William James and old-school pragmatism. I discovered last week that his farm in Chocorua, NH will be on the market in the Spring (it was on the market in the Fall, and taken off for the Winter). It's the house in which James died. The 4387 sq ft house is relatively historically intact (though a lot of updates) and barn still there, but with swimming pool now in addition. 44 acres of land, 39 of it thick forest, and beautiful hay fields and rock walls still there. Quiet secluded place. Perfect for a QBistic Institute!! (Imagine carving 5 little bungalows into the forest, a lecture room, etc.) 20 minutes from good skiing. 1.5 hours to Portland, ME and Portsmouth, NH. $895,000 asking … but probably overpriced by $100K. I've been wracking my brain trying to figure out a way I could secure the place till said institute could become a reality. Know any philanthropists with deep pockets for pragmatism or QBism or both? Seriously; I'm not joking.

Glad to hear you're back in Oz. You always seem happiest there.

28-02-11 *Home and Home, 3* (to W. G. Demopoulos)

Demopoulosism 47: *I think this is not entirely alien to your view of it and its connection with personalism, but I say this only to insert a further sense of urgency in you to write further on this stuff (pardon the pun)!*

Sadly, I didn't catch the pun! What is it?

Demopoulosism 48: *Russell, and I thought James as well, call the neutral constituents of the world which are neither mental nor physical, "stuff."*

28-02-11 *xQIT Meeting* (to MIT)

Title and abstract below.

> Title: Charting the Shape of Quantum State Space
>
> Abstract: Physicists have become accustomed to the idea that a theory's content is always most transparent when written in coordinate-free language. Sometimes though

the choice of a good coordinate system can be quite useful for settling deep conceptual issues. This is particularly so for an information-oriented or Bayesian approach to quantum foundations: One good coordinate system may be worth more than a hundred blue-in-the-face arguments. This talk will motivate and chronicle the search for one such class of coordinate systems for finite dimensional operator spaces, the so-called Symmetric Informationally Complete (SIC, pronounced "seek") measurements. The desired class will take little more than five minutes to define, but the quest to construct these objects will carry us down a 35 year journey, with the most frenzied activity only recently. Beyond this, we will turn the tables and discuss how one might hope to get the formal content of quantum mechanics out of the very existence of such a coordinate system. It has to do with seeing the Born Rule as a "tiny" modification to the Bayesian Law of Total Probability.

28-02-11 *Postmodern Smoke* (to C. M. Caves)

Cavesism 86: *As for pragmatism, here's my problem. Any approach that doesn't recognize that there is a hierarchy of concepts and truths—and not just a set of utilities—has problems, in my view. Clearly, the truths of the human experience are special to us; they don't even apply to other life on earth. The truths of life that has evolved on earth are special to earth; they wouldn't apply generally to life on other planets. The truths of physics might apply to the whole Universe. And I use truths here advisedly, willing to discuss the right word and concept for what I'm talking about.*

Last July at QCMC I got the clear impression that you don't have any ability to assert the fact or truth of climate change, and if that's where pragmatism takes you, well, I ain't going there. It could just be that you're too wrapped up in your professional and personal concerns to have much of a position on climate change, but I got the impression that even if you did have a position, you wouldn't be willing to assert it in the face of the deniers. And that just doesn't cut it, in my view.

I wish I had never given you that Rorty book. I actually tried to divert your thought away from it in advance, by writing "old-school pragmatism" (to take your mind back to the metaphysical club days) but that was far too small a fin in the water if it was meaningful to you at all. Sorry, I didn't mean to rile you.

Below is a little bit I wrote on what I think is the issue at hand here (it comes from Max's interview).

Of course, I believe global warming is a clear and present danger. Just ask Kiki about my own rants. And I am appalled that much of the rest of the world does not acknowledge it (certainly not America's politicians). The world is very likely at a turning point, and its fate will be decided by whether we do something about it.

The issue will be decided at the point "where the rubber meets the road" as you nicely put it. (I see no difference between the quantum and all the rest of life.) We act because of our beliefs, and nature grants passage of the belief *or not* by its power.

If I understand you correctly, the gulf between us might be this. I think you would like to say of some beliefs, they are correct, right here and right now, before any action has been made based upon them (and regardless of whether any action will ever be based on them at all). The truth of an idea is a timeless, tenseless thing on such a conception. Whereas I wish to reserve "right" and "wrong" to the sense of "only time will tell." That is, "right" and "wrong" are always bestowed (to a belief, proposition, sentence, policy, quantum state assignment) after the *fact*. Like that great scene in one of the *Lord of the Rings* movies, where Gandalf says commandingly to the balrog, "You shall not pass!" The universe made a decision at just that point, I would say, not before.

Question 6: Quantum probabilities: subjective or objective?

"Subjective" is such a frightening word. All our lives we are taught that science strives for objectivity. Science is not a game of opinions, we are told. That diamond is harder than calcite is no one's *opinion*! Mr. Mohs identified such a fact once, and it has been on the books ever since.

In much the same way, quantum theory has been on the books since 1925, and it doesn't appear that it will be leaving any time soon. That isn't lessened in any way by being honest of quantum theory's *subject matter*: That, on the QBist view, it is purely a calculus for checking the consistency of one's personal probabilities. If by subjective probabilities one means probabilities that find their only source in the agent who has assigned them, then, yes, quantum probabilities are subjective probabilities. They represent an agent's attempt to quantify his beliefs to the extent he can articulate them.

Why should this role for quantum theory—that it is a calculus in the service of improving subjective degrees of belief—be a frightening one? I don't know, but a revulsion or fear does seem to be the reaction of many if not most upon hearing it. It is as if it is a demotion or a slap in the face of this once grand and majestic theory. Of course QBism thinks just the opposite: For the QBist, the lesson that the structure of quantum theory calls out to be interpreted in *only* this way is that the world is an unimaginably rich one in comparison to the reductionist dream. It says that the world has excitement, risk, and adventure at its very core.

Perhaps the source of the fear is like I was taught of "that marijuana" in my little Texas town: Use it once, and it will be the first step in an unstoppable slide to harder drugs. If quantum probabilities are once accepted as subjective, somewhere down the line Mr. Mohs' scale will have to disappear in a great puff of postmodern smoke. There will be no way to enforce a distinction between fact and fiction, and the world will be anything our silly imaginations make up for it!

The first symptom is already there in a much more limited question: If quantum probabilities are subjective, why would an agent not make them up to be anything he wants? Why not pull them from thin air? The defense to this little question is the same as the defense against the "inevitable" postmodern horrors. My colleague Marcus Appleby put his finger on the issue sharply when he once said, "You know, it is *really hard* to believe something you don't actually believe!" Why would one assign arbitrary probabilities—ones that have nothing to do with one's previous thoughts and experiences—if the whole point of the calculus is to make the best decisions one can? The issue is as simple as that.

28-02-11 *On the Lighter Side* (to H. B. Geyer)

And now for something on the lighter side. I wasn't lying when I said that I was impressed with your fund-raising skills.

Just last week, I learned that my philosophical hero William James's farm is up for sale, and I have been going crazy thinking how I might secure it with the ultimate intent of turning it into a small New England version of something like STIAS (though with the focus being on the intersection between pragmatist philosophy and physics, QBism, and further such things).

For instance, I wrote this description to my old advisor Carlton Caves:

> I really wish you were more a fan of William James and old-school pragmatism. I discovered last week that his farm in Chocorua, NH will be on the market in the Spring (it was on the market in the Fall, and taken off for the Winter). It's the house in which James died. The 4387 sq ft house is relatively historically intact (though a lot of updates) and barn still there, but with swimming pool now in addition. 44 acres of land, 39 of it thick forest, and beautiful hay fields and rock walls still there. Quiet secluded place. Perfect for a QBistic Institute!! (Imagine carving 5 little bungalows into the forest, a lecture room, etc.) 20 minutes from good skiing. 1.5 hours to Portland, ME and Portsmouth, NH. $895,000 asking … but probably overpriced by $100K. I've been wracking my brain trying to figure out a way I could secure the place till said institute could become a reality. Know any philanthropists with deep pockets for pragmatism or QBism or both? Seriously; I'm not joking.

And here was part of a further explanation to another friend (H. C. von Baeyer):

> Any institute would have to be three, four, five, six years down the road. It would take so much effort to build the idea, the support, the funding, the logistical infrastructure. The real concern for me (the one that put me into the manic panic) is that the property be secured before some feckless investor or vacation-homer buy it and do more damage.

And I've sent out more than handful of significantly more detailed pleas as well. (None to any avail … of course.)

So maybe let me ask your advice. If you were confronted with a similar situation, how would you tackle it? What would be your first ideas for building up a funding effort (in a short timescale)? Any advice you could give would be most appreciated.

01-03-11 *Addiction Control* (to H. C. von Baeyer)

Just dropping in to give you an update. In case you're wondering about the title, I suppose it struck me because I've been watching and reading so much in the news on Charlie Sheen the last couple of days—I've never seen anyone go so over the top, and I've found it strangely fascinating. Of course, I also flinch in those moments when I think about the impending train wreck: I fear something really bad is going to happen in his private hours.

But on to my own addiction!! I haven't shaken it completely, but I feel I have regained control back to a relatively healthy level. It started by removing the house listing from the open tabs in my browser. That was a start. Then I stuffed the house plans and property map into my backpack, out of sight and off my desk. Now, every day, I think about Chocorua a little less. A feeling of health is starting to return.

On other matters, you never answered on whether you might come out to play the next time Marcus is in town. Having visitor money sometimes burns a hole in one's pocket. Mostly though, I'm trying to look ahead a little and get some inspiration … in a kind of "neutral stuff" study group. I want to know more about Pauli's thoughts there and his influences, etc. Not quite sure what questions I ought to ask, but if we get the ball rolling maybe it'll go somewhere. "Neutral stuff" is surely a better addiction.

01-03-11 *Entities and Effects* (to W. G. Demopoulos)

I was just looking at the wiki article on entity realism (because of some little debate on climate change that I'm having with Carlton Caves at the moment), and this line caught my eye: "[E]ntity realism claims that the theoretical entities that feature in scientific theories, e.g. 'electrons', should be regarded as real if and only if they refer to phenomena that can be routinely used to create effects in domains that can be investigated independently." It caused me to think of your "effects" of course, and I wondered whether you had ever thought about how your own position compares and contrasts to entity realism.

01-03-11 *Whitehead, Dewey, and a Potent Premise* (to W. T. Myers)

Thanks for the article. Reading it made for a very pleasant lunch in my office. I do wish you could send me the complete thesis; I would read it with just as much relish. It'd make a nice addition to my big Palazzo del QBismo library of pragmatism (nearly 700 volumes on the philosophical side of the room). If you find that you can still print it out, I would love it if you mailed me a copy. I've got a lot to learn about Whitehead's relation to the other things that feel right to me (*pieces* of James, Dewey, Schiller, and *parts* of the new realists), and I'd like to make the process as painless as possible.

Reading your quote of Whitehead on the "category of the ultimate" reminded me faintly of something I said at the start of a lecture once. Quote pasted in below. [See 07-02-11 note "The 2011 QBist Challenge" to the QBies.] My colleague, Appleby, and I have been debating a replacement for the word "FACT" used there; the common usage of the word is out of place with respect to the rest of the passage, which makes it clear that I am talking about some kind of "active entity" (i.e., not something "dead" as one usually thinks of facts).

Sure, you can share the interview; I don't mind. (The reason for writing is so that people might read.)

Bill's Preply

> I've attached an chapter I wrote for the *Hanbook of Whiteheadian Process Thought* on the intellectual relationship between Whitehead and Dewey. This piece distills my thinking on Dewey's metaphysics in a much neater fashion that my dissertation. Also, I have never distilled my dissertation into a single .doc file, so it's messy to send. I wrote the thing in Wordstar, and I'm still not sure it's all been converted properly. I need to tend to that at some point. But, the meat of the interpretation is in this article.
>
> I checked out the interview piece you sent me, and that is fascinating. Would you object to my sharing it will a few of my colleagues? I know a couple of folks who would find that of great interest, folks who know more about the physics than I do.
>
> I hope that some day you'll find your way to our meeting. It's a pluralistic, very congenial group of scholars. The meeting truly represents the highlight of my academic year.
>
> Off to teach Leibniz!

01-03-11 *Second Rant* (to M. A. Graydon & L. Hardy)

I've been watching the Charlie Sheen case very closely this week, and I think I've learned a pointer or two from him. Thus I launch into my second rant.

The quote below is from William James and refers to the German philosophers of his time. But in my mind, I often shift its relevance to be about much of modern-day "philosophy of science":

> In a subject like philosophy it is really fatal to lose connexion with the open air of human nature, and to think in terms of shop-tradition only. In Germany the forms are so professionalized that anybody who has gained a teaching chair and written a book, however distorted and eccentric, has the legal right to figure forever in the history of the subject like a fly in amber. All later comers have the duty of quoting him and measuring their opinions with his opinion. Such are the rules of the professorial game — they think and write from each other and for each other and at each other exclusively. With this exclusion of the open air all true perspective gets lost, extremes and oddities count as much as sanities, and command the same attention …

"Extremes and oddities count as much as sanities, and command the same attention …" Over and over I feel like I see that when I watch philosophers of science discuss physics.

02-03-11 *New Scientist? … New Sensationalist, Maybe!* (to H. C. von Baeyer)

First you wrote:

von Baeyerism 67: *After Valerie Jamieson, physics feature editor for* The New Scientist*, had made very positive noises about an article by me about QBism, I heard nothing from her for a while. Finally I phoned her, and found out that she is quite knowledgable (reads* `arXiv` *and has seen some of my work and yours), but basically of the opinion that quantum mechanics has done well all these years, and unless you have a fully worked out new formulation, there's not much interest.*

Then, because of our new emailing system, I noticed that New Scientist has been sending notices of the latest issues to my gmail account … I guess for some time. (Never noticed that before … I usually only use that account when I'm on the road and can't get into my PI account. But with the new system, there's not so much noise on my gmail account. Difficult to explain …) Anyway, the point is, I read this:

> **This week's top stories: The search for quantum loopholes**
>
> Can the universe really be as weird as quantum theory suggests? Einstein suggested that loopholes might let us escape its more baffling implications and now ingenious experiments are finding out if they actually exist …

Therein is the key! You've got to say that QBism makes the world look really, really weird! You've got to make all those other doofus interpretations look tame in comparison.

You mean man's free will has the potential to shape reality itself? How weird, how fantastic, how UNscientific is that! You mean our actions actually matter? Things can be moved to places that they might not have been otherwise? The world is made of a stuff that's neither matter nor mind?!? … Ahh, these people are worthless … Not much different than every news outlet in the land updating us on Charlie Sheen's sick mind with each of his latest "tweets."

03-03-11 *Normalizing Fiducial Stability Groups* (to M. A. Graydon)

I was very impressed by all that you did and all that you understood, of course. I never expected so much; I just wanted to alert you all to the thing before I ripped into the guy.

A much more subtle and interesting case will be Matt Leifer's paper, "Retrodiction In Quantum Bayesianism, Or Why Wigner's Friend Is Fuchs' Enemy," which I will handle gently and with great care. In case you're interested it's attached. I am amazed that Matt gets so much about the program (well, the subjective Bayesian part), but doesn't seem to absorb at all the American pragmatist side of it, i.e., the personal character of the measurement outcomes. Or the way Carl Caves got close to getting it right with:

> "[QBism takes] a three-pronged approach to subjectivity and objectivity: (i) quantum states and probabilities are wholly subjective; (ii) system attributes are wholly objective; and (iii) measurement outcomes are where the rubber meets the road, i.e., where subjective and objective meet to produce something that is not under the control of the agent, but is also not out there in the world."

Independently of that though, Matt's formalism is worth much thinking about.

03-03-11 *My Consternating Collaborator* (to R. Schack)

Section 5 starts out thus:

> The reflection principle is a constraint on an agent's beliefs about her future probability assignments.

Why, why, why do you write like that?!?!?! I would not be able to hold you to it in a court of law, but I am personally confident that the average reader will read it exactly in the way that caused all the confusion over van Fraassen to begin with. I.e., that the reflection principle demands that the agent set her present beliefs by her supposed future probabilities. Our task, as I recall, was to try to blunt that debate a little.

Something else would surely do better justice. Maybe for instance, "The reflection principle is a constraint on the relation between an agent's present beliefs of her future beliefs and those future beliefs themselves." I'll think some more; there's still much room for improvement.

Oh, I get frustrated so easily by iniquitous choices of words: Words that cause the very damage that we are trying mend. Your draft has been more riddled with these kinds of things than usual.

Oh, I'm frustrated, and I've got to finish this damned thing. Your Dutch book argument in Section 4 still has me totally confused. It looks to me like the paragraph starting "So let us first assume. . . " is a dangling appendage, not needed at all for the actual Dutch book argument that kicks in at the following paragraph. I am probably misunderstanding something very deeply, but *then I blame it on your exposition* in that section.

Can we talk or Skype tomorrow? I think I desperately need this.

Take my grumpiness with a grain of salt. Attached is the latest draft.

04-03-11 *Wherefrom Such Confidence* (to Å. Ericsson & M. A. Graydon)

I've got no good answer except to say, "From a long, hard fight." I have a collection of letters on my website titled *The Post-Växjö Phase Transition: Knowledge → Information → Belief → Pragmatic Commitment* that documents the gradual change of my view on quantum states. (But it only picks up from the days when I was already calling them knowledge; ten years before that I was thinking they were objective propensities of some sort.) Here's the file: `http://www.perimeterinstitute.ca/personal/cfuchs/PhaseTransition.pdf`.

But I don't really know what it shows but a soul in torment. Or an oyster trying to remove the grains of sand from its shell. No one argued more strongly that quantum states were personalist

Bayesian than me—more and more determinedly, in fact, as I checked my own consistency and found my failings therein. (In fact, as far as I know, no one had ever argued for such an extreme view at all.) No one led me; it was my own push and shove that got me there. You'll see it in every letter of exasperation I wrote to Caves, Schack, and Mermin as they fought back and I tried more and more to put together a convincing picture.

The conception was my baby, and maybe that's why I love it and have confidence in it like no one else.

04-03-11 *Among the World's Religions* (to Å. Ericsson)

Thanks for your thoughtful note. I've read it and reread it and two times beyond that (and I'll probably read it again), as I've been building and tending this fire for Kiki's return tonight. She's out at some kind of spousal dinner, and the children are away at a girl scout camp.

Particularly, I keep coming back to these lines:

Åsa-ism 2: *The thing is that I find my beliefs as based on something real outside myself, they are not crucially dependent on my philosophizing and it includes an explanation to how my beliefs can be strong. To me it is consistent in a way I can't see how anything I think about physics ever could be. So then I find it amazing you can believe, if not as much as I, still close to it. It's not a criticism, it is a conundrum to me.*

[OK, I fixed your spelling.]

I'm not sure you'll understand it, but my mind kept coming back to the image of Napoleon's coronation as Emperor. He somehow convinced the pope to travel from Rome to crown him in Notre Dame Cathedral rather than at the Vatican. (Something unheard of before.) At the very last moment, in front of hundreds of witnesses, as the pope reached for the crown to place it on Napoleon's head, Napoleon grabbed it and placed it on his head himself. "I crown myself Emperor!"

It's not unconnected to the last note I sent you. "I build my own religion." Contemplating the holy mystery of quantum theory is my personal sacrament. It seeps into every aspect of my life like a religion does. See for instance, the first two lines of an old proposal I wrote for Caltech once upon a time (attached). Better still, see the story "Some Things Should Not Pass" from 16 February 2002 on page 150 of the web document I noted to you earlier. All these things are tied together in my mind. Quantum theory is the closest I've yet come to an articulated expression of what I strive to say in the line, "For a small time we have the chance to move around and determine our courses as we please—to leave a trail behind us." See also Footnote 33 (page 20) in `http://arxiv.org/abs/1003.5209`.

I do hope I am not hurting your feelings with my jesting with you. In my mind it is playful, though I fear that I do not always convey it that way to you. I think long and hard, contemplating our disparate positions on religion. You are a catalyst to me, even if I don't reciprocate to you. I'm just saying I value these discussions.

06-03-11 *WTB, Reason and Emotion* (to H. Yadsan-Appleby)

I am sorry to take so long to reply to you. I wanted to get home to look at some quotes before responding, and then once I got home, I became ill (as you have probably heard).

I am glad that you are enjoying O'Connell's book [*William James on the Courage to Believe*] and finding some new thoughts in James's philosophy.

At the moment, I can't help you much with the meanings of "forced", "living", "momentous", etc., as it has been a long time since I've read the appropriate essays, but I think you will find the definitions clear enough if you read the actual essay "The Will to Believe".

In fact here is what I suggest as a study plan. I would say the next thing you should read is precisely "The Will to Believe". And then follow that with the magnificent, particularly deep (but harder to read) essay, "The Sentiment of Rationality."

On one of your points:

Hulya-ism 1: *I never accepted or even understood the split between reason and faith (or reasoning and feeling emotions).*

I wanted to bring your attention to a nice paragraph from Robert D. Richardson's biography of James:

> About a month after he sent off his "Remarks on Spencer's Definition of Mind as Correspondence," James sent off, in January 1878, an article called "The Sentiment of Rationality." He later described it as "the first chapter of a psychological work on the motives which lead men to philosophize," and he noted ruefully that it might better have been called "The Psychology of Philosophizing."
>
> We may, at this distance, prefer the original title, if only for its fresh and unorthodox, not to say brash, announcement that rationality is at bottom a feeling. Not a matter of logic or math, not reasoning or ratios, not induction, deduction, or syllogism, not something higher than and detached from the senses, not the opposite of a feeling or emotion—rationality is itself a feeling or emotion. He might even have called the essay "The Feeling of Rationality." He begins by asking how we recognize the rationality of a conception, and he answers, "By certain subjective marks, that is, a strong feeling of ease, peace, rest." He amplifies, saying, "This feeling of the sufficiency of the present moment, of its absoluteness—the absence of all need to explain it, account for it or justify it—is what I call the Sentiment of Rationality."

This sounds a lot to me like what you were saying above, and I suspect you would have quite an agreement with it. So I think when you are prepared for it, and your mind has gotten a bit accustomed to James's peculiar writing style, you would get much out of reading "The Sentiment of Rationality". It is a wide-ranging essay on much, more than this subject.

07-03-11 *Einstein, Indeed!* (to R. W. Spekkens)

> There can <u>*be*</u> no difference that <u>*makes*</u> no difference.
>
> — William James, "What Pragmatism Means" (1907)

[underlines mine for still extra emphasis, italics his own]

Rob's Reply

> Ah, but Einstein took that principle (in the form of the equivalence of acceleration and gravitational force) and derived general relativity! Surely no amount of prose can provide a better *justification* of the principle than that.

07-03-11 *The Battle of the Eyeballs* (to J. B. Hartle)

I was amused by the no-eyeball sign in your talk today. See attached for my dueling point of view. [See Figure 6 in "QBism, the Perimeter of Quantum Bayesianism," `arXiv:1003.5209v1`.]

I enjoyed your talk, and I'm glad I was able to get there: Seeing it teaches me that there's a bit of common groundwork between us for this little piece of formalism I want to show you tomorrow or Wednesday.

08-03-11 *Quantum Systems 101* (to the QBies)

"If the universe is a quantum mechanical system, then it has a quantum state." – J. B. Hartle

See the first two minutes of Jim Hartle's talk today, `http://pirsa.org/11030103/`.

Here's an essay question for you QBies: What does it mean to be a quantum mechanical system?

08-03-11 *Teaching the Teacher?* (to M. A. Graydon)

Graydonism 5: *I have attached the absolute latest version of the paper. A while back I tried to defend my opening sentence. Jim Hartle said something yesterday which might help my cause:*

A physical theory provides reliable probabilities for betting on the regularities of the universe.
— James Hartle (`PIRSA:11020124`)

Jim's statement is too "objectivist" about probabilities for my taste. Reliable? By whose measure? With respect to whose priors? (Don't fool yourself into thinking you can define such a notion without a prior.) Regularities? Is that a sneaky way of endowing nature with propensities?

My present thinking is that probability theory is a calculus for flagging "inconsistency"—nothing more. By my view, quantum theory does not *provide* probabilities any more than the raw axioms of probability theory do. Probabilities come out of a hat that no theory itself has yet entered (i.e., the subjective agent).

I expand on this view in Section III of `http://arxiv.org/abs/1003.5209` and my answer to Question 4 in the attached. [See "Interview with a Quantum Bayesian," `arXiv:1207.2141v1`.]

Thanks for the offer of a tutorial. I am going to try to get in early tomorrow morning to hopefully give Hartle a session on the urgleichung. Drop me a note when you're in the building, and I'll let you know if I'm free. Unfortunately, I have to chair the colloquium tomorrow; so that time will be taken up.

PS. Rüdiger tells me that Logue's book *Projective Probability* makes a good case for the idea (in opposition to E. T. Jaynes and the objective Bayesians) that nothing more than coherence is needed for making probability assignments useful tools. I can lend you the book if you wish.

10-03-11 *Late Great Johnny Ace* (to M. A. Graydon)

On a cold December evening
I was walking through the Christmas tide
When a stranger came up and asked me
If I'd heard John Lennon had died
And the two of us went to this bar
And we stayed to close the place

And every song we played
Was for the late great Johnny Ace, yeah, yeah, yeah

— Paul Simon, "The Late Great Johnny Ace"

Your choice of photos for your Beamer example took me down memory lane. I remember well the day John Lennon died. I heard it on the radio driving home from school. I had literally just gotten into the Beatles a few days previous, after reading the *Playboy* interview of John and Yoko (sifting through my brother-in-law's stash of the magazine). It was devastating; I recorded as much Beatles music as I could directly off the radio. No internet, no YouTube back then; no record store in my hometown.

I later named my first golden retriever Albert Winston Fuchs. Albert was for Albert Einstein; Winston was for John Lennon.

I remember being home from school sick the day Elvis Presley died. A ticker scrolled across the bottom of the TV screen saying "Elvis Aaron Presley, the king of rock and roll, is dead at age of 42."

I don't remember Hunter Thompson dying, but my records show that I was somewhere between a talk in College Park, Maryland and a talk in Wrocław, Poland.

10-03-11 *Your Reference* (to J. B. Hartle)

Thanks for the little audience yesterday. I enjoyed it. Also, I'm glad to learn that you're now thinking of probabilities in (kind of) Bayesian ways. As it happens, I'm just starting to slap together a contribution for a conference proceedings, where I've got a footnote which reads, "This is what particularly distinguishes QBism from several other Bayesian approaches to quantum probability. See Refs. `\cite{...}` for details of the other approaches." Footnote 2 in the attached.

If you have further papers than the one I cited [J. B. Hartle, "Quantum Mechanics with Extended Probabilities," Phys. Rev. A **78**, 012108 (2008)], or I didn't cite the most appropriate one and there is something better, please let me know.

13-03-11 *Hurried Remarks* (to M. A. Graydon)

Greetings from Chicago airport. I just had a pleasant time reading your paper (in the Toronto airport and then on the flight). Åsa and I are in the Admiral's Club, and she's off having a bowl of soup (that she paid for), and I of course am having my complimentary beer (because I'm cheap).

Your paper is extremely well written … much like I had expected. Beautiful clarity and organization. (And I'm not making this up.)

My only worry at all is that there is somehow still not enough "oomph" in the first page, abstract, and conclusion. Why is the question you're tackling here important? I feel the reader ought to be left with a memorable sentence in his mind long after the details of your construction have slipped away from him. Indeed, could you find one sentence that would really help draw potential readers in? I don't know, maybe something like: "Ultimately one would like to use the developing technologies of quantum information science to test the validity of complex over quaternionic quantum mechanics. Before that, however, one must have a clear conception of the relations and contrasts between the theories with respect to the full apparatus of quantum information theory — particularly, with respect to generalized quantum measurements and quantum operations, not *merely* projective measurements and unitary operations as has been worked out previously `\cite{Hermie,Joe,Peter}`. This paper fills that gap in the literature." It's not very elegant or finely tuned (or one sentence long either!), but I just wanted to throw something down before Åsa and I have to leave.

13-03-11 *Quaternions for Jesse Jackson* (to M. A. Graydon)

Now I'm writing you from the airplane! First time I've been able to get online on a flight in years. (Several years ago, it was briefly legal, but then the FAA squashed it for quite some time.) Now, for $9.95 I'm in internet bliss.

Funny thing: I was in seat 3E and Åsa was in 6A. I came back to the guy who was sitting beside her and said, "I'll trade you an aisle seat for an aisle seat so that I can sit beside my colleague. If it'll sweeten the pot for you, Jesse Jackson is sitting in the seat beside me." You see, our Åsa is much more important than any politician!

But wouldn't you know, she later said something that made me realize: If I had only been sitting beside him, I'm might have hit him up for help in buying the William James house. Darn, foiled again!

But then again, I'm on the internet! And thus, I can read things like this: `http://query.nytimes.com/gst/fullpage.html?res=9B0DE7DC113CF93AA15752C1A961948260&sec=&spon=&pagewanted=5`. I guess I did the right thing after all.

So let me get back to your notes!

15-03-11 *Itamar's Volume* (to R. Schack)

I guess Meir's "urgent" is now putting me on edge. I hope you've made progress.

I am now up at 3:30 AM working on talk number 2. I'm not sure talk number 1 did much for the general populace, but it did spur a long discussion with Samson Abramsky and Adam Brandenburger. (Samson was the Clifford lecturer three years ago.) Interesting seeing the tensions in Samson. He does take us seriously, but he's got the usual spook, "The Bayesian view is attractive, surely the best notion of probability in many situations [implicitly, he was meaning 'not in physics'], but surely the probabilities in quantum mechanics come about through 'nature's regularities'."

Today, the de Finetti theorem, fidelity, and no-broadcasting will be the main fare.

19-03-11 *Congratulations. Question* (to R. B. Griffiths)

I'm sorry for the delay. I just delivered this year's Clifford lectures at Tulane U in New Orleans, and now I'm having a brief respite with my 82-year-old mom in South Texas on my way to Dallas (for the APS March meeting).

Thanks for the congratulations; it means a lot to me to hear from you.

Griffithsism 2: *I am trying to write up an item where I would like to put a reference to your opinion that quantum information is [only] about the outcomes of future measurements, assuming that I am not mixing you up with someone else. Is there a good accessible reference?*

No, you're probably not mixing me up with anyone else, and *in a sense* that is right. *However*, my students did recently point me to a passage in one of your 2009 papers where you write:

> It seems almost inevitable that if quantum mechanics is incompatible with "local realism" (a term which in many minds is essentially synonymous with Bell inequalities)

> but is still somehow local, then it is realism that must be discarded, and one finds serious arguments to this effect [38]. But if the world of atoms is unreal, what shall we say of the macroscopic objects which most physicists think are composed of such atoms? Are the things we see around us (not to mention we ourselves) unreal?

That does *not* characterize my position now, and never has, though I'm getting better and better at articulating what it is that I am trying to get after. Let me thus send you to this paper: `http://arxiv.org/abs/1003.5209`. It is probably better read as a whole, but if you want to go straight to the metaphysics (i.e., what is real), go to Section's VI an VII on page 19. It is not at all that there is no microscopic (or macroscopic) reality—it never has been—it is that there is too much of it. As one philosopher once put it, it is that "The world is not sentence shaped." Give the reading a chance; at the least I hope you'll find it entertaining. Also, if you back up to Section V, you'll find my own take on the significance of the Bell inequalities.

Finally, let me attach an interview that is not posted yet. [See "Interview with a Quantum Bayesian," `arXiv:1207.2141v1`.] I hope it tackles some of these same issues adequately. In fact these two documents together, I believe, make the clearest and most complete statements yet of what it is that I see as my research program.

I wish I were going to see you at the March meeting, but as far as I can tell you won't be. It'd be nice to discuss these things with you when they might be fresh on your mind. Would you be interested in visiting the Perimeter Institute sometime this year? I could arrange it.

20-03-11 *Notes on Norsen* (to N. Argaman)

Let me start by quoting a passage from William James's essay "The Will to Believe":

> Let us give the name of *hypothesis* to anything that may be proposed to our belief; and just as the electricians speak of live and dead wires, let us speak of any hypothesis as either *live* or *dead*. A live hypothesis is one which appeals as a real possibility to him to whom it is proposed. If I ask you to believe in the Mahdi, the notion makes no electric connection with your nature,—it refuses to scintillate with any credibility at all. As an hypothesis it is completely dead. To an Arab, however (even if he be not one of the Mahdi's followers), the hypothesis is among the mind's possibilities: it is alive. This shows that deadness and liveness in an hypothesis are not intrinsic properties, but relations to the individual thinker. They are measured by his willingness to act. The maximum of liveness in an hypothesis means willingness to act irrevocably. Practically, that means belief; but there is some believing tendency wherever there is willingness to act at all.

The truth is, at the moment, I've just got no energy for the nonlocality debate. Nonlocality is just not a "live hypothesis" for me. And when I've got so many other urgent matters to take care of (like the Clifford lectures I gave last week or the 400 talks I've organized for the APS March meeting this week), it seems a special burden to even think the slightest amount about it.

I'm sorry; I just can't muster the steam. The best I can say is that Section V of `http://arxiv.org/abs/1003.5209` already speaks my piece on the subject. At the moment, I've got nothing else to say.

But, look, good luck in your journey.

21-03-11 *In Place of Regularities* (to S. Abramsky)

I've spent the early morning hours today reviewing a lot of things from last week ... not only my iniquities, but also the discussion we had at the reception where I shunned your phrase "regularities of nature". Anyway, in that regard, I hope you'll sometime have the chance to read my paper `http://arxiv.org/abs/1003.5209` particularly Section VI, starting on page 19. I'm not quite sure how to put what it's striving at in a pithy way, but I do know that it is an attempt to put something else—"things in common"—in place of your "regularities".

27-03-11 *NY Anytime Soon?* (to A. Plotnitsky)

I've been spending the morning re-organizing my bookshelf to make room for 18 new purchases. (Photo of one of the two walls in case, I haven't shown you before.) And I came across my two copies of *Complementarity*! (The signed paperback you gave me and the hard cover in perfect condition that I found sometime later.) It sent me on a trail of thinking about our discussions, but it also made me wonder whether you'll be in New York anytime soon, visiting Paula or your sister. Maybe you're even there now? I ask because there is a $300 set I'd like to purchase from The Strand, but I'm wondering what kind of condition it's really in. And I'd only want to trust a real book lover's opinion.

Oh, if you look in the middle column near the arm of the chair, you'll see the two copies of *Complementarity*.

Will you be in Växjö this year? It'd be nice to see you again and talk more extensively with you than I have the last couple of years. Recently, I've taken a renewed interest in Bohr and his complementarity. It has to do with my SIC work actually. The key is that one can always think of quantum state space as arising from an algebra with *only* two generators (Weyl-Heisenberg group). Appleby particularly has me starting to wonder whether Bohr saw this very fact ... in his own (to me usually inscrutable) way. I'd love to pick your brain on this!

Hope you're doing well. All the best .... Wait, let me attach a new composition that you might enjoy. It's an interview for Max Schlosshauer's new book. David Mermin wrote one too; but maybe you should ask him for it directly if you'd like to read it.

28-03-11 *Stream O'* (to M. Schlosshauer)

Schlosshauerism 36: *I was just reading a long essay about Joyce's* Ulysses*, and I learned that it was William James who coined the term "stream of consciousness." I had no idea! This James is really one hell of a personality. As they'd say in sports, "Go James!"*

- big bang
- multiverse
- stream of consciousness
- turtles all the way down
- block universe

Yep, he's the man. Have I told you how I've become obsessed with purchasing his old farm (beautiful big house and 44 acres of forest) and turning it into an institute for law-without-law studies? All I need to do is gather $895K quickly and then figure out a source of sustaining funds. Yeah right!

29-03-11 *NY Anytime Soon?, 2* (to A. Plotnitsky)

Thanks so much for being willing to look into this. The item is a four volume set titled, *Cambridge School of Pragmatism* edited by John R. Shook and Andre de Tienne. The price is $300 and the only description is "very good," with no further details.

Please tell me what you think of it, as you have a chance. If it sounds like it's roughly of the condition of any anything else on my bookshelf (no pencil markings, rips in the cover, etc.), then I will probably order it from them. I hope this is not too much trouble on you.

Thanks so much for sending me your draft. I absolutely love the title "Dark Materials to Create More Worlds"!! It's just chock full of good stuff. And I so love that Milton quote. I'm drawn to it. "His dark materials to create more worlds." I'm fascinated with that line particularly, even if I ultimately want to misuse it for my purposes: I want to think of "quantum systems" as the "dark materials," rather than the "chance" that spews from them in a quantum measurement setting. In my mind it has a kind of connection to a Pauli quote in the attached excerpt from my new samizdat (the one starting at the end of page 525 about quantum measurement being like a "black mass"). [See 19-06-05 note "Philosopher's Stone" to G. L. Comer.] When things cool down for me a bit, I'd like to continue this discussion on "dark materials".

30-03-11 *A Thought I Would Call Religious* (to the QBies)

> I must say one word about the extraordinarily intimate and important character which the phenomenon of effort assumes in our own eyes as individual men. Of course we measure ourselves by many standards. Our strength and our intelligence, our wealth and even our good luck, are things which warm our heart and make us feel ourselves a match for life. But deeper than all such things, and able to suffice unto itself without them, is the sense of the amount of effort which we can put forth. Those are, after all, but effects, products, and reflections of the outer world within. But the effort seems to belong to an altogether different realm, as if it were the substantive thing which we *are*, and those were but externals which we *carry*. If the 'searching of our heart and reins' be the purpose of this human drama, then what is sought seems to be what effort we can make. He who can make none is but a shadow; he who can make much is a hero. The huge world that girdles us about puts all sorts of questions to us, and tests us in all sorts of ways. Some of the tests we meet by actions that are easy, and some of the questions we answer in articulately formulated words. But the deepest question that is ever asked admits of no reply but the dumb turning of the will and tightening of our heartstrings as we say, *"Yes, I will even have it so!"* When a dreadful object is presented, or when life as a whole turns up its dark abysses to our view, then the worthless ones among us lose their hold on the situation altogether, and either escape from its difficulties by averting their attention, or if they cannot do that, collapse into yielding masses of plaintiveness and fear. The effort required for facing and consenting to such objects is beyond their power to make. But the heroic mind does differently. To it, too, the objects are sinister and dreadful, unwelcome, incompatible with wished-for things. But it can face them if necessary, without for that losing its hold upon the rest of life. The world thus finds in the heroic man its worthy match and mate; and the effort which he is able to put forth to hold himself erect and keep his heart unshaken is the direct measure of his worth and function in the game of human life. He can *stand* this Universe. He can meet it and keep up his faith in it in presence of those same features which lay his weaker brethren

low. He can still find a zest in it, not by 'ostrich-like forgetfulness,' but by pure inward willingness to face it with those deterrent objects there. And hereby he makes himself one of the masters and the lords of life. He must be counted with henceforth; he forms a part of human destiny. ...

Thus not only our morality but our religion, so far as the latter is deliberate, depend on the effort which we can make. *"Will you or won't you have it so?"* is the most probing question we are ever asked; we are asked it every hour of the day, and about the largest as well as the smallest, the most theoretical as well as the most practical, things. We answer by *consents or non-consents* and not by words. What wonder that these dumb responses should seem our deepest organs of communication with the nature of things! What wonder if the effort demanded by them be the measure of our worth as men! What wonder if the amount which we accord of it were the one strictly underived and original contribution which we make to the world!

William James

Psychology, A Briefer Course

31-03-11 *A Good Day's Impressions* (to D. Fraser)

I want to thank you again for yesterday's opportunity; I much enjoyed the experience. And actually one of the strongest impressions I got was in getting to know you better: You had a grasp of things, and my own idiosyncrasies, that impressed me much. Genuinely, if much had been the same, but [Philosophy Professor X, not at U. Waterloo] (for instance) had led the discussion, I don't think I would have encountered such understanding. That is not to say *agreement*, which you may not have had (i.e., I well expect a face of neutrality from a lecturer in a situation such as this), but what sets you apart in my mind is that you did not give the impression of hearing only non sequiturs spewing from me (as indeed so many philosophers portray of me). That built a sense of camaraderie, and that's much better for getting to the heart of these matters. I can learn something from that, and I appreciate it.

Doreen's Preply

I'm teaching an undergrad/grad seminar on philosophy of QM this term and I'm wondering whether you'd be willing to make a guest appearance near the end of term. I've scheduled (most of) two weeks on information-theoretic approaches to QM and it would be great to have you for the second of the two weeks. I will assign the students readings to complete in advance and the class will be devoted to 15 minute presentations by some of the students and discussion. The readings are listed as TBA in the syllabus; I'm thinking that I will assign most of Chris Timpson's survey chapter in the *Ashgate Companion to Contemporary Physics* and then a paper of yours and Chris' paper "QB: A Study." But I'm open to suggestions! [...]

I've attached the schedule of readings for the course. I have 12 undergrads evenly split between honors philosophy majors and (mostly) math/physics majors, 3 philosophy grad students and a handful of auditors. I'm using Albert's textbook—which has the virtue of being accessible to the philosophy audience—and supplementing it with additional readings. Teaching this course to the diverse audience is a bit of a challenge, but we've already had some good exchanges between the philosophers and the physicists.

04-04-11 *Les Papillons* (to H. C. von Baeyer)

It is 10:20 AM in Waterloo, and the review meeting will start at 11:00. I'm sitting at home, hiding, and my stomach is in butterflies, almost to the point of nausea. I thought it might help to write you; so here I am.

This morning, I have been using the coffee mug Emma bought for her mother at the Eiffel tower, when we were there two years ago. Somehow, the mug has gotten transferred to my nearly sole use of it, and it's generally considered one of "dad's mugs" now. It's got two scenes of the tower, and Katie was looking at it pretty thoroughly this morning at breakfast. I started to tell her stories of how she will be so impressed when she walks around a corner and spies it for the first time.

How I wish I were in the city with you today. I could so use a good serious discussion on QBism, Pauli, and alchemy again. And to do it in the right atmosphere would cap it all off. Or this would nice: Sitting in some nice Parisian café reading Boutroux and Renouvier! Ahh, the possibilities you have in your life.

Let me come back to the issue of criticism of QBism. Attached is a file I put together for the review. One faculty member had asked if there were any published criticisms of QBism. So it mostly contains the ones you've already seen, but there are some other ones in there as well:

1. D. Styer, S. Sobottka, W. Holladay, T. A. Brun and R. B. Griffiths, P. Harris, letters to editor in "Quantum Theory—Interpretation, Formulation, Inspiration," Phys. Tod. **53**(9), 11 (2000).

2. A. Hagar, "A Philosopher Looks at Quantum Information Theory," Phil. Sci. **70**, 752 (2003).

3. P. Grangier, "Contextual Objectivity and Quantum Holism," `arXiv:quant-ph/0301001v2` (2003).

4. E. Dennis and T. Norsen, "Quantum Theory: Interpretation Cannot be Avoided," `arXiv:quant-ph/0408178v1` (2004).

5. L. Marchildon, "Why Should We Interpret Quantum Mechanics?," Found. Phys. **34**, 1453 (2004).

6. M. Ferrero, D. Salgado, and J. L. Sánchez-Gómez, "Is the Epistemic View of Quantum Mechanics Incomplete?," Found. Phys. **34**, 1993 (2004).

7. A. Hagar, "Quantum Information—What Price Realism?," Int. J. Quant. Info. **3**, 165 (2005).

8. A. Hagar and M. Hemmo, "Explaining the Unobserved—Why Quantum Mechanics Ain't Only About Information," Found. Phys. **36**, 1295 (2006).

9. U. Mohrhoff, "Objective Probability and Quantum Fuzziness," `arXiv:quant-ph/0703035v1` (2007).

10. D. Wallace, "The Quantum Measurement Problem: State of Play," `arXiv:0712.0149v1 [quant-ph]` (2007).

11. V. Palge and T. Konrad, "A Remark on Fuchs' Bayesian Interpretation of Quantum Mechanics," Stud. Hist. Phil. Sci. Part B **39**, 273 (2008).

12. C. J. Timpson, "Quantum Bayesianism: A Study", Stud. Hist. Phil. Mod. Phys. **39**, 579 (2008).

13. A. Stairs, "A Loose and Separate Certainty: Caves, Fuchs and Schack on Quantum Probability One," to appear in Stud. Hist. Phil. Mod. Phys.

14. M. S. Leifer, "Retrodiction In Quantum Bayesianism, Or Why Wigner's Friend Is Fuchs' Enemy"

15. S. Friederich, "How to spell out the epistemic conception of quantum states," `arXiv:1101.1975v1` (2011).

16. J. Bub, "Quantum probabilities as degrees of belief," Stud. Hist. Phil. Mod. Phys. **38**, 232 (2007).

17. G. Jaeger, *Entanglement, Information, and the Interpretation of Quantum Mechanics*, (Springer, 2009).

Probably the best recommendations in there are Bub's paper and Stairs's paper. Their email addresses are `jbub@umd.edu` and `stairs@umd.edu` in case you want to get the papers directly from them. (I know that Allen's paper was refereed, and so have may have changed somewhat since I last saw it.) I'll attach the latest version of Matt Leifer's paper, though he said a day or two ago that he is expecting to substantially revise it. The key thing that Matt misses is the two-levels of personalism, but I expect that the article might be enlightening otherwise. (I haven't actually read it yet.)

Maybe in the next note, I'll send you the bigger presentation I made up for Lucien. Who knows, maybe some of that will be useful for your own QBism summary. It's not too very braggarty (compared to some of my other documents) ... mostly factual.

Thanks again for the nice note the other evening. There are times when little compliments help a lot.

07-04-11 *The Betterized Mermin* (to R. W. Spekkens & M. S. Leifer)

I dug this up yesterday, but then I forgot to send it to you. It is the article where David Mermin thinks he does a better job than he did on the three-man BFM paper.

I've got to say, I'm much taken with your program ... even though there are several philosophical divides from my own program ... the deepest being that quantum theory is a *generalization* of probability theory. You know that I would now say it is standard personalist probability theory (which includes for me, if I need to say so explicitly, van Fraassen's reflection principle)—not a generalization at all—with some structure about counterfactuals added to it. I.e., it is a more particular structure than abstract probability theory. But that doesn't get in the way of my appreciating this great formalism you're developing and all the things you're able to tackle with it.

07-04-11 *Huangjun Zhu Visiting at the Moment* (to R. W. Spekkens & M. S. Leifer)

Huangjun Zhu gave his talk today, and it was quite good. (Giulio was there, and you may consult him for an independent opinion.) But the talk can be viewed on PIRSA already: `http://pirsa.org/11040105/`.

Only they left out my dramatic introduction for Huangjun, telling the story of how Hermann Weyl became excited after a visit from Max Born in September 1925, where Born revealed that imposing the canonical commutation relations for position and momentum might be the key axiom for a new quantum mechanics. Weyl's excitement led him to discovering the Weyl commutation relations, so that he could reframe the Born-Jordan assumption in terms of bounded operators, but more importantly, so that he could define a quantum theory for finite systems. Weyl's letter announcing the discovery was dated 27 September 1925; the very same day the Born-Jordan paper—the very first to write down the full theory of quantum mechanics—was submitted for publication. After telling the tale, I returned to the subject of Huangjun to say that he would present a result that shows the deep connection between SIC POVMs in prime dimension and that very starting point of quantum theory.

08-04-11 *Thinking of You* (to W. G. Demopoulos)

Demopoulosism 49: *I recently ran across a reference to Santayana's* Character and Opinion*. In general, I find him a bit to literary for my taste, but this is not to say he isn't insightful. The book (based on lectures given in California) contains a chapter on James that I thought hit some nails on their heads pretty well. Have a look—there's a new edition, but I'd have to look up the reference again—which I'm happy to do if you can't find it—here it is, edited by James Seaton.*

That's one Santayana book I don't have; I'll be on the lookout for it.

I read your Poincaré introduction this morning with my coffee. It was nice. I really liked the passage you plucked out:

> . . . . . . more than a dozen processes, entirely independent and that I would not be able to enumerate without tiring you, lead us to the same result. If there were more or less molecules per gram, the brightness of the blue sky would be entirely different; incandescent bodies would radiate more or radiate less, etc.; one has to admit, we see atoms.

That's the style of reasoning that leads us to epistemic states. That's why I've always liked this passage from Spekkens's original toy model paper.

> We shall argue for the superiority of the epistemic view over the ontic view by demonstrating how a great number of quantum phenomena that are mysterious from the ontic viewpoint, appear natural from the epistemic viewpoint. These phenomena include [about a million things]. . . . The greater the number of phenomena that appear mysterious from an ontic perspective but natural from an epistemic perspective, the more convincing the latter viewpoint becomes. . . .
>
> Of course, a proponent of the ontic view might argue that the phenomena in question are not mysterious *if* one abandons certain preconceived notions about physical reality. The challenge we offer to such a person is to present a few simple *physical* principles by the light of which all of these phenomena become conceptually intuitive (and not merely mathematical consequences of the formalism) within a framework wherein the quantum state is an ontic state. Our impression is that this challenge cannot be met. By contrast, a single information-theoretic principle, which imposes a constraint on the amount of knowledge one can have about any system, is sufficient to derive all of these phenomena in the context of a simple toy theory, as we shall demonstrate.

I'll go back to the actual Poincaré paper later.

18-04-11 *If Words Have Meaning* (to N. D. Mermin)

Attached is a copy of the one article out there that cites your nonworld nonview paper. Have you seen it before? It's quite something.

I was just re-reading this passage in your paper:

> This is the quintessential quantum situation: How can you tell whether your experiment is giving you information about the object of study (particle), the apparatus (box), or some combination of the two?
>
> Figure 2 shows a way to tell. Suppose you can test particles before they get to the box, and on the basis of the results of the test you can predict in advance which bin each particle ends up in. Clearly that rules out the possibility that the box disposes randomly of the particle. Indeed, if you can specify in which bin the particle ends up before it even reaches the box, then, if words have any meaning at all, we can say that two kinds of particles enter the box: those that always get put into the left bin, and those that always get put into the right bin. We can call them particles of type L and particles of type R. There are indeed two kinds of particle, and the box is sorting them out.

So, I see our debate on "certainty" started all the way back in 1992.

18-04-11 *If Words Have Meaning, 2* (to N. D. Mermin)

Merminition 230: *Apropos of the content, all I'm doing there is repeating the EPR reality criterion (which is, after all, nothing but common sense.) The debate started in 1935.*

Aha! So you indirectly admit that the trouble with Bell is the assumption of the EPR criterion after all! For, you write in the text:

> Remarkably—amazingly—Bohr was right to do so. But this did not become clear for nearly thirty years. Since the overwhelming majority of physicists believed that Bohr had refuted Einstein in 1935, the remarkable 1964 paper by John Bell showing that Bohr was right to keep his head in the sand, had negligible impact on practicing physicists. Within the last decade, however, there has been a revival of interest in the foundations of quantum mechanics, primarily because experimental techniques are now available that reveal a far greater variety of exotic quantum phenomena than were available to the founders of the theory sixty-five years ago. In 1988 Greenberger, Horne, and Zeilinger came up with a spectacular refinement of the EPR argument and its refutation by Bell, that I shall now describe.

and

> It was thirty years before John Bell pointed out that there were other aspects of the EPR situation (involving the behavior of the particles at additional types of boxes through which they might be sent) which, though not contradicting in any way the data on which EPR based their conclusion, were nevertheless inconsistent with each particle possessing both 1-ness and 2-ness. I shall not pursue Bell's argument, because I believe the argument found a few years ago by GHZ is even more dramatic and compelling.

Shame, shame.

18-04-11 *The One Underived and Original Contribution We Make to the World* **(to N. D. Mermin and C. M. Caves)**

Attached is the promised quote on the will by William James. I felt like copying it in again.

> [I] must say one word about the extraordinarily intimate and important character which the phenomenon of effort assumes in our own eyes as individual men. Of course we measure ourselves by many standards. Our strength and our intelligence, our wealth and even our good luck, are things which warm our heart and make us feel ourselves a match for life. But deeper than all such things, and able to suffice unto itself without them, is the sense of the amount of effort which we can put forth. Those are, after all, but effects, products, and reflections of the outer world within. But the effort seems to belong to an altogether different realm, as if it were the substantive thing which we *are*, and those were but externals which we *carry*. If the "searching of our heart and reins" be the purpose of this human drama, then what is sought seems to be what effort we can make. He who can make none is but a shadow; he who can make much is a hero.
>
> The huge world that girdles us about puts all sorts of questions to us, and tests us in all sorts of ways. Some of the tests we meet by actions that are easy, and some of the questions we answer in articulately formulated words. But the deepest question that is ever asked admits of no reply but the dumb turning of the will and tightening of our heartstrings as we say, *"Yes, I will even have it so!"* When a dreadful object is presented, or when life as a whole turns up its dark abysses to our view, then the worthless ones among us lose their hold on the situation altogether, and either escape from its difficulties by averting their attention, or if they cannot do that, collapse into yielding masses of plaintiveness and fear. The effort required for facing and consenting to such objects is beyond their power to make. But the heroic mind does differently. To it, too, the objects are sinister and dreadful, unwelcome, incompatible with wished-for things. But it can face them if necessary, without for that losing its hold upon the rest of life. The world thus finds in the heroic man its worthy match and mate; and the effort which he is able to put forth to hold himself erect and keep his heart unshaken is the direct measure of his worth and function in the game of human life. He can *stand* this Universe. He can meet it and keep up his faith in it in presence of those same features which lay his weaker brethren low. He can still find a zest in it, not by "ostrich-like forgetfulness," but by pure inward willingness to face the world with those deterrent objects there. And hereby he becomes one of the masters and the lords of life. He must be counted with henceforth; he forms a part of human destiny. . . .
>
> Thus not only our morality but our religion, so far as the latter is deliberate, depend on the effort which we can make. *"Will you or won't you have it so?"* is the most probing question we are ever asked; we are asked it every hour of the day, and about the largest as well as the smallest, the most theoretical as well as the most practical, things. We answer by *consents* or *non-consents* and not by words. What wonder that these dumb responses should seem our deepest organs of communication with the nature of things! What wonder if the effort demanded by them be the measure of our worth as men! What wonder if the amount which we accord of it be the one strictly underived and original contribution which we make to the world!

21-04-11 *Quick Question* (to H. C. von Baeyer)

von Baeyerism 68 : *Reading Timpson has been very useful. I do have a question though. One of his principal complaints is the "explanatory deficit" of QBism – that you cannot match conventional quantum theory in explaining the thermal conductivity of lithium, for example. In particular, he claims: "When we are disallowing the possibility of any descriptive theory below this kind of level of generality it seems that there is little we can say about why macroscopic objects have the properties they do. . . ."*

Are you indeed disallowing this possibility? I thought that the Born rule and the Schr. equation are the twin pillars of conventional q.m., and that you have succeeded in a finding a new role for the former, but have not yet found the replacement for the latter*. (Timpson, in his last sentence, does allow for the possibility that you may resolve this problem.)*

I am not expecting the Schr. equation to be translated literally into the language of Bayesian probabilities. It may look quite different in character – just as your Born rule does now. But is the Schr. equation disallowed*?*

If Timpson is right, and it is *disallowed, where do the actual numbers that go into the Born rule come from?*

Well, they don't come from the Schrödinger equation itself: They come from the largest practical Bayesian web of beliefs. Do a word search on the term "mesh" in `http://arxiv.org/abs/1003.5209` and that will take you to some of the appropriate discussions. And do the same in the attached document. In fact, the discussion there might be better anyway:

> Now, our path back to quantum theory is complete because I want to say this: A quantum state just *is* a probability assignment. The particular character of the quantum world places new, physically-influenced consistency requirements on our mesh of beliefs (like the second equation in my answer to Q2), but in the end, even quantum probabilities must port into probability theory more generally. A quantum state assignment is only one element in a much larger Bayesian mesh of beliefs each agent inevitably uses for his calculations. It is a numerical *commitment* to how he will gamble and make his decisions when he plans to interact with a quantum system.

But back to the meaning of the Schrödinger equation. In the end, I want to say it too is a nothing else than a consistency requirement on various beliefs, just like the urgleichung. In fact, ultimately I want to say it just *is* the urgleichung in a different guise. See footnote 22 on page 13 of the `arXiv` paper. Rob Spekkens and Matt Leifer have some outstanding formal work coming out along these lines; there might be some discussions in there that will help you (forgetting about the new formalism). I will see if they are willing to share the draft at this stage (it may be too rough still).

Also, maybe I haven't sent you this little summary article for the QCMC proceedings, attached. It has a little discussion of unitarity as well. . . . Oh, maybe it doesn't (I just discover). But I'll send it to you anyway.

On this business of "explanatory power," my best recommendation for preparing to answer is to read Louis Menand's book *The Metaphysical Club*. It's the very best book I read in 2003 (and I read a lot of books that year). Here's a passage from it on the subject:

> The world is filled with unique things. In order to deal with the world, though, we have to make generalizations. On what should we base our generalizations? One answer, and it seems the obvious answer, is that we should base them on the characteristics things have in common. No individual horse is completely identical to any other

horse; no poem is identical to any other poem. But all things we call horses, and all things we call poems, share certain properties, and if we make those properties the basis for generalizations, we have one way of "doing things" with horses or poems—of distinguishing a horse from a zebra, for example, or of judging whether a particular poem is a good poem or a bad poem. These common properties can be visible features or they can be invisible qualities; in either case, we create an idea of a "horse" or a "poem," or of "horseness" or "poetry," by retaining the characteristics found in all horses or poems and ignoring characteristics that make one horse or poem different from another. We even out, or bracket, the variations among individuals for the sake of constructing a general type.

Darwin's fundamental insight as a biologist was that among groups of sexually reproducing organisms, the variations are much more important than the similarities. "Natural selection," his name for the mechanism of evolutionary development that he codiscovered with Alfred Russel Wallace, is the process by which individual characteristics that are more favorable to reproductive success are "chosen," because they are passed on from one generation to the next, over characteristics that are less favorable. Darwin regretted that the word "selection" suggested an intention: natural selection is a blind process, because the conditions to which the organism must adapt in order to survive are never the same. In periods of drought, when seeds are hard to find, finches that happen to have long narrow beaks, good for foraging, will be favored over finches with broad powerful beaks: more of their offspring will survive and reproduce. In periods of abundance, when seeds are large and their shells are hard, the broad-beaked finches will hold the adaptive advantage. "Finchness" is a variable, not a constant.

Darwin thought that variations do not arise because organisms need them (which is essentially what Lamarck had argued). He thought that variations occur by chance, and that chance determines their adaptive utility. In all seasons it happens that some finches are born with marginally longer and narrower beaks than others, just as children of the same parents are not all exactly the same height. In certain environmental conditions, a narrower beak may have positive or negative survival value, but in other conditions—for example, when seeds are plentiful and finches are few—it may make no difference. The "selection" of favorable characteristics is therefore neither designed nor progressive. No intelligence, divine or otherwise, determines in advance the relative value of individual variations, and there is no ideal type of "finch," or essence of "finchness," toward which adaptive changes are leading.

Natural selection is a law that explains *why* changes occur in nature—because, as Darwin and Wallace both realized after reading, independently, Thomas Malthus's *Essay on the Principle of Population* (1798), if all members of a group of sexually reproducing organisms were equally well adapted, the population of the group would quickly outgrow the resources available to sustain it. Since some members of the group must die, the individuals whose slight differences give them an adaptive edge are more likely to survive. Evolution is simply the incidental by-product of material struggle, not its goal. Organisms don't struggle because they must evolve; they evolve because they must struggle. Natural selection also explains *how* changes occur in nature—by the relative reproductive success of the marginally better adapted. But natural selection does not dictate *what* those changes shall be. It is a process without mind.

A way of thinking that regards individual differences as inessential departures from a general type is therefore not well suited for dealing with the natural world. A general type is fixed, determinate, and uniform; the world Darwin described is characterized by

> chance, change, and difference—all the attributes general types are designed to leave out. In emphasizing the particularity of individual organisms, Darwin did not conclude that species do not exist. He only concluded that species are what they appear to be: ideas, which are provisionally useful for naming groups of interacting individuals. "I look at the term species," he wrote, "as one arbitrarily given for the sake of convenience to a set of individuals closely resembling each other .... [I]t does not essentially differ from the term variety, which is given to less distinct and more fluctuating forms. The term variety, again, in comparison with mere individual differences, is also applied arbitrarily, and for mere convenience sake." Difference goes all the way down.

And maybe this one is good to see too, though it may not completely reflect to my own beliefs on the subject. In any case there is so much food for thought in the book. And wonderfully written.

> Darwinism was a scandal to many Laplaceans. In the Laplacean worldview, randomness is only appearance; in the Darwinian, it is closer to a fact of nature—in some respects, it is *the* fact of nature. Herschel, the man who had helped introduce Quetelet to British readers, wrote in 1850 that if all the literature of Europe were to perish and only Laplace's *Système du monde* and *Essai sur les probabilités* remained, "they would suffice to convey to the latest posterity an impression of the intellectual greatness of the age which could produce them, surpassing that afforded by all the monuments antiquity has left us." But when *On the Origin of Species* appeared, in 1859, he ridiculed Darwin's theory as "the law of higgledy-pigglety."[228]
>
> What does it mean to say we "know" something in a world in which things happen higgledy-pigglety? Virtually all of Charles Peirce's work—an enormous body of writing on logic, semiotics, mathematics, astronomy, metrology, physics, psychology, and philosophy, large portions of it unpublished or unfinished—was devoted to this question. His answer had many parts, and fitting them all together—in a form consistent with his belief in the existence of a personal God—became the burden of his life. But one part of his answer was that in a universe in which events are uncertain and perception is fallible, knowing cannot be a matter of an individual mind "mirroring" reality. Each mind reflects differently—even the same mind reflects differently at different moments—and in any case reality doesn't stand still long enough to be accurately mirrored. Peirce's conclusion was that knowledge must therefore be social. It was his most important contribution to American thought, and when he recalled, late in life, how he came to formulate it, he described it—fittingly—as the product of a group. This was the conversation society he formed with William James, Oliver Wendell Holmes, Jr., and a few others in Cambridge in 1872, the group known as the Metaphysical Club.

We start our little pilgrimage to Chocorua this afternoon. But I will contemplate more on a better answer to you ... in between contemplating the words I'll use for various job enquiries.

21-04-11 *Quick Question, 2* (to H. C. von Baeyer)

I probably didn't send you the right passage from *The Metaphysical Club*. I know there is something more closely related to your question; it's a question of finding it. I will try my best to give you a proper answer this weekend.

[228]Charles Darwin to Charles Lyell, December 10, 1859: "I have heard by round about channel that Herschel says my Book 'is the law of higgledy-pigglety'".

21-04-11 *Quick Question, 3* (to H. C. von Baeyer)

von Baeyerism 69: *In particular, he claims: "When we are disallowing the possibility of any descriptive theory below this kind of level of generality it seems that there is little we can say about why macroscopic objects have the properties they do...."*

Maybe another way to make a stab at answering you is to suggest you read James's essay "The Sentiment of Rationality". Here's one place to get it online: `http://www.archive.org/details/sentimentration02jamegoog`.

28-04-11 *A Standing Ovation for Reductionism* (to M. A. Graydon)

Graydonism 6: *Here is a transcription of part of the discussion during Christopher Stubbs's colloquium last week. I remember coming home with these words burning in my mind, and I wanted to preserve them as a reminder.*

> **Christopher Stubbs:** I mean, it's funny. I came of age in the 1980s. I went to grad school when the standard model of particle physics was built, right. And running coupling constants, and grand unification, and that had a very definite impact on my outlook as a physicist. Now, whether that was a good or a bad thing, I don't yet know. I crave in my deepest, darkest soul that there is some fundamental theory of everything that is uniquely determined by some symmetry principle that we don't yet understand, and things are the way they are because they had no choice. I want it to be that way in the most fervent [way] you can imagine.
>
> ****ovation for reductionism****
>
> But, just because I want it to be that way doesn't mean it's so. I am personally doing everything I can to continue to push the reductionist paradigm, because that's how I was brought up.
>
> **Neil Turok:** You said that this program [reductionism] has worked for centuries and why do we give it up now for something which doesn't actually give any prediction at all. It's more like: forget about science, let's all go and do art and music ... I mean that's what I see the anthropic tendency, or spiritualism, dare I say it.

So I guess it didn't help my case to write a section titled "Quantum Gravity in the Light of Nonreductionism" in my PI research proposal?

Fires, floods, and bad philosophy—who would have thought the world could carry such destructive tendencies?

29-04-11 *Questions* (to I. Bengtsson)

Bengtssonism 3: *In the not-too-distant future I should give a talk where I make the point that it is not true — as Bogdan Mielnik, who is to be celebrated, used to complain — that all Hilbert spaces are alike. Because the dimension matters a lot. I think you sometimes say things that goes in the direction that it is the dimension that really reflects the underlying reality? If you recognize that sentence, can you direct me to the place where you say it most forcefully?*

With regard to some spirited writing of mine on dimension, perhaps see Sections VI, VII, and VIII of `http://arxiv.org/abs/1003.5209`. Another good place to look is to look at the story titled “The More and the Modest” starting on pdf file page 883 (alternatively text page 874) on `http://www.perimeterinstitute.ca/personal/cfuchs/nSamizdat-2.pdf`. Beyond that, do searches on “capacity” and “alchemy” and “degree of detachedness” and you’ll find any number of crazy things.

30-04-11 *Abstract for Conceptual Foundations Conference* (to G. Chiribella)

OK, I bend. Below is my title and abstract . . . and some evidence that there is no such clean line between science and emotions as the reductionism-lovers might want in a world picture.

> Title: Some Negative Remarks on Operational Approaches to Quantum Theory
>
> Abstract: Over the last 10 years there has been an explosion of “operational reconstructions” of quantum theory. This is great stuff: For, through it, we come to see the myriad ways in which the quantum formalism can be chopped into primitives and, through clever toil, brought back together to form a smooth whole. An image of an IQ-Block puzzle comes to mind, `http://www.prismenferngl as.de/iqblock_e.htm` [“THE IQ-BLOCK, or a game for big and small children you could get addicted to”]. There is no doubt that this is invaluable work, particularly for our understanding of the intricate connections between so many quantum information protocols. But to me, it seems to miss the mark for an ultimate understanding of quantum theory; I am left hungry. I still want to know what strange property of matter forces this formalism upon our information accounting. To play on something Einstein once wrote to Max Born, “The quantum reconstructions are certainly imposing. But an inner voice tells me that they are not yet the real thing. The reconstructions say a lot, but do not really bring us any closer to the secret of the ‘old one’.” In this talk, I hope to expand on these points and convey some sense of why I am fascinated with the problem of the symmetric informationally complete POVMs to an extent greater than axiomatic reconstructions

02-05-11 *A JTF William James Center?* (to H. S. Choi)

I have been informed that you will be visiting Perimeter Institute May 9 and will be scheduled to meet with me for part of that time. I look forward to meeting you and telling you all about what Appleby and I plan to do with our new JTF grant. I wish Appleby could be around to meet you as well—you would be impressed by his breadth of knowledge—but he will be in the UK at that time.

The reason I write you today, however, has to do with something completely independent of Perimeter Institute concerns. My wife and I have been thinking for some years about how we might run a small research center (a place to mingle quantum physics, philosophy and theology and excite a synergy between them). For the first time a clear-cut opportunity has arisen that might put our vision within reach.

This is because I happen to know that William James’s home and property in Chocorua, New Hampshire will soon go on the market. It is a home built in 1842, the very year James was born, and the home in which he died in 1910. It is where he wrote the major part of his *Principles of*

Psychology and had debates with Josiah Royce and Charles Sanders Peirce and any number of other philosophers. When James first discovered the property in 1886, he wrote to his brother Henry, "It is only 4 hours from Boston by rail and 1 hour's drive from the station. Few neighbors, but good ones; hotel a mile off. If this is a dream, let me, at least indulge it a week or so longer!"

Well, I can tell you, the home remains every bit as dreamlike to this day, though now it is only a 2 hour 20 minute drive from Boston Logan airport. My wife and I have made one trip to the property already and have inspected it in detail. (My wife is an expert on old homes; please see the attached article on QBism House.) I will make a second reconnaissance trip this Thursday to gather more information and photos of the surrounding forest and lakes.

The home is 4,400 sq feet in size and immaculate in condition; very well maintained with a brand new roof. The house has an easy, elegant charm and flow to it conducive to all that is needed of a research station: spaces for private work, areas for conversation, solitude for contemplation. Some distance away from the house, in the yard, there is a rather large artist studio that James built for one of his sons—it could easily be converted into a conference room.

The property comes with 44 acres of land, mostly forest, and abuts a NH state forest. Chocorua Lake is a two minute walk away. The town of North Conway is a 15 minute drive (smaller villages in between) and has a variety of restaurants and cafes that puts our downtown Waterloo (where Perimeter Institute is situated) to shame. The region is filled with hiking trails, ski slopes, lakes, opportunities for cycling, fishing, and much more.

The owners already had the home on the market last Fall, but took it off the market for the Winter. They plan to put it back on the market again very soon, for Springtime viewing. When they last listed it, they were asking $895,000.

The vision I presently have for the research center is that it would provide workspace and interaction space for three postgraduate researchers (two year terms each)—one a physicist, one a philosopher, one a theologian—as well as the director (myself, whose own research spans the first two subjects). These four researchers would all live off-site. The bedrooms of the home would be used to house up to three visiting researchers at any one time, with stays ranging from 1 week to 3 months. From time to time, conferences could also be hosted by the center. A key component of the idea is to excite a synergy between the three subjects in a setting nearly perfect for their mixture. What the James home in particular brings to the table is an automatic branding for the product.

As I wrote in the QBism House article, "Ideas, like children, need homes where they can be loved, nurtured, and raised to independence."

Anyway, this is all in the way of introduction. When you visit Waterloo, I wonder whether I might be able to schedule some time with you outside of the Perimeter Institute setting to discuss all this? The late afternoon or early evening May 8 or May 9 would be ideal from my point of view: We could sit on the porch of QBism House if the weather cooperates. It is an atmosphere congenial for discussions of this nature (see attached *Maclean's Magazine* article discussing Perimeter Institute, QBism House, and William James, all in one go).

I could have a reasonably polished presentation prepared for you by then . . . with photos, maps, a discussion of the other institutes that I would like to model this one upon (drawing elements from the Traunkirchen Academy, Schloss Dagstuhl, and the Stellenbosch Institute for Advanced Study, all of which I know intimately), a proposed advisory board, funding models, the deep connection between William James's views and Sir John Templeton's, etc., etc.

I realize that I am not approaching the JTF through the standard funding portal (nor during the standard funding timeline) by sending this note, but there is some urgency in me to try to remove the house from the market on fear of another (inevitable) buyer. If I could get some guidance from you and the John Templeton Foundation, it would be invaluable to me.

To give you a bit more sense of where I'm coming from with the connections I see between William James's philosophy, John Wheeler's post-Everettian cosmology (that "the Big Bang is here, all around us" in moments of constant creation), and my Quantum Bayesian interpretation of quantum mechanics, please see the attached recent interview—particularly my answers to Questions 8, 13, 14, and 17. [See "Interview with a Quantum Bayesian," `arXiv:1207.2141v1`.]

Finally I attach a current CV. It speaks to some of my credentials for seeing a project like this through (for instance detailing my chairmanship of the 1,100-member APS topical group on quantum information, the many conferences I've organized, my recognition in the community through my citation record, etc., etc.). [...]

I apologize for this very long note, so please let me summarize:

1) The old William James house in Chocorua, NH will soon be on market.

2) I have a vision to turn it into a research center for quantum physicists, philosophers, and theologians to live and learn from each other and make great discoveries on its grounds.

3) The key point of the James house is that it comes with a "brand" that will be recognized by philosophers, theologians, psychologists, and many more beyond that the world around.

4) Might we meet and discuss this in more detail outside your Perimeter Institute schedule during your visit to Waterloo? I would appreciate any guidance the John Templeton Foundation could give me.

Thank you much for your time,

11-05-11 *Thinking of You* (to H. J. Bernstein)

I hope everything went well yesterday. You were in my thoughts yesterday, as your "realit*t*y" has been in my thoughts for years. Few understand how malleable the world is as you. Keep making it happen.

Mon travail est purement autobiographique. Il n'y est question que de moi et de ce qui m'est proche. C'est une tentative de mise en mémoire. Je travaille à partir des gens qui m'intéressent, qui m'importent, à qui je pense, dans le décor des pièces que j'habite, que je connais.

— *Lucian Freud*